# Debtor protection and business dynamism

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**Abstract**

We study the effect of debtor protection on business dynamism. We find that greater debtor protection, in the form of more lenient personal bankruptcy laws, increases firm entry only in sectors requiring low startup capital. We also find that debtor protection increases firm exit and job destruction rates among young small firms. This negative effect takes three years to materialize, and is persistent in time. Finally, we provide evidence consistent with two mechanisms underlying these changes in business dynamism: a reduction in credit supply and entry of lower quality firms following increases in debtor protection.

*Keywords*: Debtor Protection, Personal Bankruptcy, Entrepreneurship, Firm Dynamics, Business Dynamism

*JEL Classification*: G33, K35, M13

## 1. Introduction

Business dynamism—the dynamics of firm entry and exit—is a fundamental driver of job creation, innovation, and productivity growth (Davis, Haltiwanger and Schuh, 1996; Decker, Haltiwanger, Jarmin and Miranda, 2014; Council of Economic Advisers, 2016). In this paper we investigate how changes in debtor protection provided by U.S. personal bankruptcy law affect firm entry and exit, as well as job creation and destruction. In particular, we exploit the staggered timing of state laws amending personal bankruptcy exemptions which are the maximum asset value that individuals can legally protect from creditors under Chapter 7.

Bankruptcy exemptions provide entrepreneurs with insurance against the financial consequences of business failures. This wealth insurance can be particularly important for entrepreneurs, since it preserves the upside potential of their ventures while limiting the cost of failure. For this reason, a debtor-friendly bankruptcy regime can induce individuals to become entrepreneurs and thus lead to higher rates of firm entry and job creation (Kihlstrom and Laffont, 1979; Fan and White, 2003). However, if the new firms are being created by marginal entrepreneurs, then we could also see a reduction in the average quality of the businesses created. This can in turn translate into higher failure rates.

On the other hand, a growing literature shows that such wealth insurance comes at a cost. When debtor protection increases, then creditors can expect to recover less from defaulting borrowers.[1] Therefore, banks could reduce credit availability in response to the moral hazard problems induced by the exemptions (Fay, Hurst and White, 2002). Indeed, there is evidence that banks reduce credit availability to households and young firms in response to the moral hazard problems induced by the exemptions, making these affected firms more likely to fail (Gropp, Sholz and White, 1997; Cerqueiro and Penas, 2017). Since many small firms

[1] The trade-off between the insurance mechanism and borrowing costs is investigated for example in Akyol and Athreya (2011), Grant (2010), Jia (2015), Mankart and Rodano (2015) and Severino and Brown (2017).

rely on external debt to fund their ventures (Berger and Udell, 1995; Robb and Robinson, 2014), a reduction in credit availability may also hamper entrepreneurship. A reduction in credit availability may therefore reduce the rate of firm entry, especially in industries with high capital requirements, as well as increase the rate of exit.

How debtor protection affects firm entry and exit—and consequently job creation and destruction—is therefore an empirical question. In this paper we aim not only to provide an answer to that question, but also to pin down some of the mechanisms through which debtor protection affects entry and exit. To this end, we use data on firm and employment dynamics for the period 1994-2013 from the Longitudinal Business Database (LBD) and from the Business Dynamics Statistics (BDS). Both datasets are maintained by the U.S. Census Bureau. The Census' LBD is a restricted dataset that required Special Sworn Status to access. It is a longitudinal database of business establishments and firms that provides annual employment for every private-sector, U.S. establishment with at least one employee. The BDS data provide statistics compiled from the LBD. We complement these data with bankruptcy exemption limits, which we hand-collect from individual state codes for our entire sample period.

Our empirical strategy is to estimate the effect on firm entry and exit (as well as on job creation and destruction) of staggered changes in state exemption levels, which during our sample period are frequent and large in magnitude. We address endogeneity concerns in several ways. First, because exemptions could be correlated with other state specific economic shocks, we analyze the effects of exemptions separately for firms with 1-4 employees and firms with 5-20 employees. Personal bankruptcy law should mainly affect the first group because these very small firms are more likely to depend on debt raised by the firm owner and thus more exposed to any shocks to personal credit resulting from changes to Chapter 7 laws.[2] The second

[2] Personal bankruptcy law applies directly to firms of unlimited liability form, and it can affect small corporations either because banks often require these firms' owners to personally guarantee loans or because owners take personal loans that they use to finance the firm.

group provides a good counterfactual because these are also small firms with similar exposure to statewide economic shocks, but less dependent on personal debt financing. Second, we also estimate specifications with state×year fixed effects, which absorb any economic shocks affecting a state at any given point in time. In these saturated specifications we identify the differential effect of exemptions on firm entry or exit across different types of firms (by industry and birth cohort). Third, to rule out reverse causality, we investigate the year-by-year response of our dependent variables to a change in exemptions. There we show that there is no evidence of "pre-trends", that is, business dynamism is reacting to changes in exemptions and not the other way around.

We obtain several findings. First, panel regressions at the state-year level show that an increase in exemptions has no significant effect on aggregate firm entry and job creation. Our finding thus contrasts with the cross-sectional evidence in Fan and White (2003), who show that business ownership rates increase significantly with the level of exemptions. When using within-state variation across industries with low versus high entry barriers, we find that exemptions induce firm entry in industries with low entry barriers. However, the estimated effect remains economically small.

Second, we find that a higher exemption level increases the rates of firm exit and job destruction for small firms (with 1-4 employees). We find no such effects for firms with 5-20 employees. The estimated effect on job destruction that we obtain for the group of smallest firms is economically relevant. Our estimates indicate that raising exemptions by 50% (the average change in our sample period) increases the rate of job destruction by 0.16 percentage points. Given that the median number of employees (across all states and years) is 1.5 million, this estimate represents 2,400 additional job losses annually resulting from small firms closing in response to higher exemption levels.

Third, we investigate potential mechanisms behind the increase in firm exits and the resultant job losses. We start by analyzing how these effects vary across firms of different ages. We find that the increase in firm exits and job losses is concentrated among the group of youngest firms (1-5 years old). We find no significant effect for the oldest firms (15 or more years old).

We argue that such patterns are consistent with two possible mechanisms. The first is that the exemptions reduce access to credit especially to young businesses, which are more opaque to creditors, making them more likely to fail (as in Cerqueiro, and Penas, 2017). The second is that the exemptions attract a worse pool of entrepreneurs, which are quickly driven out of business. The evidence appears to corroborate both mechanisms.[3] In particular, we find that firms created following a change in exemptions experience significantly higher rates of failure than those created before. This result holds in a triple-differences setting that controls for state×year, state×cohort, and cohort×year fixed effects. We thus conclude that increases in exemptions foster the creation of firms with lower success potential. This result contrasts with evidence obtained from reforms in other countries that affected entrepreneurship (see for example Hombert et al., 2017).

Finally, we ask whether an increase in exemptions is indeed reducing the number of small firms and jobs in the economy. The results described above are based on employer data, which account for about 20% of the total number of firms in the U.S. The drop in employer firms we find could be potentially offset by the creation of smaller businesses without paid employees. However, using data from the Census Nonemployer Statistics, we find that this is not the case. An increase in exemptions also reduces the net creation rate of non-employer

[3] Two other mechanisms could explain the increase in firm exit rates: (i) an increase in strategic defaults, given that the financial benefit from filing for bankruptcy increases with exemptions, and (ii) an increase in firm competition from more entry. The fact that we only find an increase in exit rates for the younger firms does not provide support for these alternative mechanisms. We elaborate more on this in Section 5.3.

firms, and especially in industries with high entry barriers. We thus conclude that both the number of employer and non-employer firms falls.

Our study adds to a growing literature that analyzes how debtor protection affects entrepreneurship. In a cross-sectional study, Fan and White (2003) find that the probability of homeowners owning businesses is 35 percent higher if they live in states with unlimited rather than low exemptions (see also Mathur, 2015). Armour and Cumming (2008) find similar evidence for European and North-American countries in a study that analyzes the effect on self-employment of bankruptcy laws that protect debtors. Ersahin, Irani, and Waldock (2017) use Census data to investigate the effect of fraudulent transfer laws on entrepreneurship and find that reducing debtor protection leads to a decline in entrepreneurial activity. In contrast with all these results, Cumming and Li (2013) find a decline in entrepreneurial activity after exemptions increase (except for states with low homestead exemption levels). Our paper builds off of these existing studies by showing that stronger debtor protection can lead to a decline in entrepreneurial activity of very young firms, and also by exploring possible mechanisms – including a decline in credit supply and a deterioration of the pool of entrepreneurs – that help explain this finding.

Our paper is also related to a recent literature that aims to understand not only which factors promote entrepreneurship, but also how those factors affect the characteristics and quality of the firms that are created (e.g., Andersen and Nielsen, 2012; Hvide and Panos, 2014; and Hombert et al., 2017). Some of this literature also studies the effect of different types of financing, including bank financing, credit cards and venture capital (e.g., Bozkaya and Kerr, 2014; Chatterji and Seamans, 2012; Cole, Cumming and Li, 2016). Finally, our analysis also relates to, and builds off of, the literature that studies entry and exit patterns using U.S. Census data (e.g., Dunne, Roberts, and Samuelson, 1989; Davis, Haltiwanger, and Schuh, 1996; Haltiwanger, Jarmin, and Miranda, 2006; Kerr and Nanda, 2009; Pugsley and Sahin, 2018).

The paper proceeds as follows. In Section 2 we explain the institutional setting. In Section 3 we describe the data we use. In Section 4 we present our empirical methodology. Section 5 describes our results and Section 6 concludes.

## 2. U.S. personal bankruptcy law

There are two different personal bankruptcy procedures in the U.S., Chapter 7 and Chapter 13, and debtors are allowed to choose between them. When an individual files for bankruptcy, all collection efforts by creditors terminate. Under Chapter 13, the debtors' wealth is exempted, but they must propose a repayment plan. This plan typically involves using a proportion of the debtor's future earnings over a five-year period to repay debt. The law prescribes that the repayment plan must give creditors the same amount they would receive under Chapter 7, but no more.

Under Chapter 7, all of the debtor's future earnings are exempt from the obligation to repay; this is known as the "fresh start" principle. Roughly, 70% of total bankruptcy filings in the U.S. are under Chapter 7. In a Chapter 7 filing, debtors must turn over any unsecured assets they own above a predetermined exemption level (the secured debts cannot be discharged). The "fresh start" is mandated by Federal law, and applies throughout the U.S. In 1978, Congress adopted a uniform federal bankruptcy exemption, but gave the states the right to opt out and to adopt their own exemption levels. By the beginning of the 1980s, two-thirds of the states had opted out. The wealth exemptions vary widely across states as a result.

We hand-collect the exemptions from individual state codes. There are two main types of exemptions: for equity in owner-occupied residences (the homestead exemption), and for various other types of personal assets (the personal property exemption). Homestead exemptions specify a dollar amount of equity that the debtor is entitled to protect in the event of bankruptcy. Personal property exemptions may apply to assets as diverse as cash, deposits, the bible, other books, musical instruments, burial plots, family portraits, clothing, wedding

rings, other jewelry, furniture, guns, pets, cattle, crops, motor vehicles, health aids, and food. In many states, however, the law leaves unspecified the value of many of these assets.

Table 1 displays the exemption limits by state for 1994 and 2013. State exemptions include the homestead and personal property exemptions. The homestead exemptions are quantitatively more important than the personal property exemptions for most states. Some states have unlimited homestead exemptions. For personal property exemptions, the values only include assets that in all states have a maximum dollar amount to be exempted: jewelry, motor vehicle, cash and deposits, and a "wildcard" (an exemption that applies to any property).

During our sample period, 41 states enacted laws to raise their exemption levels. Although the median dollar value change in state exemptions during our sample period was $10,000, there is ample variation around this figure. Twelve states raised their exemption by at least $100,000, while ten states experienced increases of at least $50,000 and lower than $100,000.[4] The states that experienced smaller increases in exemptions typically have statutory provisions that mandate adjustments in the value of exemptions based on inflation. No state has reduced the exemption levels in nominal terms during our sample period.[5]

### 2.1. What drives differences in exemptions?

Hynes, Malani and Posner (2004) provide a detailed history of U.S. property exemption laws. Those authors also include an analysis of the determinants of exemption levels. They report that the single most robust predictor of a state's exemption levels during 1975-1996 is its historic (1920) levels of exemptions. In the methodology that follows, we make sure to

[4] DC adopted in 2001 an unlimited homestead exemption level. In our empirical analyses we assign a value of $1,000,000 to states with unlimited exemptions.

[5] The Bankruptcy Abuse Prevention and Consumer Protection Act (BAPCPA) of 2005 introduced a means test that limited the bankruptcy options for individuals. Under BAPCPA, only filers whose income over the previous six months is below the median for their state can file for Chapter 7 bankruptcy. This reform should not affect directly business owners because the U.S. Bankruptcy Code explicitly prescribes that the means test applies to "consumer Chapter 7 cases." Entrepreneurs can file for Chapter 7 without being subject to the means test restriction as long as they have mainly business debts.

include state fixed effects to control for any state-specific historic factors such as the historic levels of exemptions. Consequently, what is relevant in our empirical setting is to understand what drives changes in state exemptions.

Cerqueiro and Penas (2016) collect information from the legislative discussions surrounding the state laws that amended exemption limits during 2004-2011. They document that the main argument in favor of higher exemptions is the increase in house prices. Since in most states exemptions are not updated regularly, steady increases in house prices reduce the real level of protection that debtors obtain from filing for bankruptcy.[6] Other arguments invoked by proponents are the increase in medical costs and the higher exemption levels offered by surrounding states.

A more recent study by Severino and Brown (2017) provides empirical evidence that corroborates these views. In particular, they analyze the correlation between changes in state exemptions and several covariates, such as changes in house prices, bankruptcy filing rates, medical expenses, and other economic variables. They find that lagged changes in house prices is the most important determinant of subsequent changes in exemptions. The other determinants have almost no predictive power on changes in exemptions.

Our empirical strategy, which we explain in detail in Section 4, is designed to account for the potential correlation between changes in exemptions and house prices (as well as other state-level economic factors).

## 3. Data and variables

### 3.1. Employer data

[6] Legislative discussions about amending exemption limits are often promoted by local associations of realtors and homebuilders. See Cerqueiro and Penas (2016) (Appendix A) for details.

We obtain data on firm and employment dynamics for the period 1994-2013 from the Longitudinal Business Database (LBD) and from the Business Dynamics Statistics (BDS). Both datasets are maintained by the U.S. Census Bureau. The LBD is a restricted-access (requiring Special Sworn Status) longitudinal database of business establishments and firms that provides annual employments for every private-sector, U.S. establishment with at least one employee. The LBD also contains information on the industry, physical location, and establishment age. The BDS data provide statistics compiled from the LBD.[7] The BDS provides for each state annual measures of firm dynamics, such as firm startups and closures, and job creation and destruction. These measures are aggregated according to several firm characteristics, including size, age and year of birth (cohort).

Table 2 lists the variables used and provides some aggregate statistics from the BDS database. Data are at the state-year level for the period 1994-2013. Since the exemptions should affect mainly small business ventures, we focus our attention on firms with 1-4 employees (the smallest size cell available in the BDS) in panel A. In our regressions we also analyze firms with 5-20 employees as a counterfactual (panel B).

We use the following variables. *Firm births* is the count of firms born during the last 12 months. *Firm birth rate* is the count of firms born during the last 12 months as a percentage of the number of existing firms in the previous year. *Job creation* is the count of jobs created by firm births during the last 12 months. *Job creation rate* is the same variable expressed as a percentage of total employment in the previous year. *Firm exits* is the count of firms that have exited in their entirety during the last 12 months, while *Firm exit rate* is the same variable expressed as percentage of the total number of firms in the previous year. *Job destruction* is the count of employment associated with firm exits, while *Job destruction* rate is the same variable expressed as a percentage of total employment in the previous year. We also analyze

[7] See Jarmin and Miranda (2002) for details.

the net increase in firms and jobs, which we express both as counts (*Net firm creation* and *Net job creation*) and changes with respect to the previous year levels (*Net firm creation rate* and *Net job creation rate*).

### 3.2. Non-employer data

We obtain non-employer data from the Census Nonemployer Statistics (NES) for the period 1997-2013 (these data have been collected annually only since 1997). NES is an annual series that provides statistics on U.S. businesses with no paid employees or payroll, that are subject to federal income taxes, and have receipts of $1,000 or more ($1 or more for the Construction sector). These data originate from statistical information obtained through business income tax records that the Census Bureau obtains from the Internal Revenue Service. The data consist of the number of businesses by industry. Since there is no information on entry and exit, we can only study net firm creation. We provide descriptive statistics for net firm creation in panel C of Table 2.

### 3.3. Other data sources

We supplement these data with other state-level variables that we obtain from several sources. First, we hand-collect data on personal bankruptcy exemptions for each state and year from individual state legal codes. Our main variable of interest, *Exemptions*, equals the sum of the homestead exemption and the personal property exemptions in the state (see Section 2 for institutional details and Table 1 for the exemption amounts). Second, we control for state-year changes in house prices using the S&P Case Shiller Index. As argued in Section 2, changes in real estate prices are known to be a main driver of exemption changes. Third, we obtain from the Census Bureau annual state median income to control for economic conditions.

## 4. Empirical methodology

To investigate how changes in state exemption levels affect firm dynamics, we run the following panel regression model using state-year data for the period 1994-2013:

$$y_{s,t}=\alpha_s+\alpha_t+\beta Exemption_{s,t}+\delta Controls_{s,t}+\varepsilon_{s,t},$$

where *s* indexes state of location, *t* indexes time, *y* is the dependent variable, *Exemption* is the exemption level (in logs), *Controls* is a set of state-varying control variables, and $\varepsilon$ is an error term. $\alpha_s$ and $\alpha_t$ are vectors of state and year fixed effects, respectively. State fixed effects control for fixed differences in entry or exit across states, due to factors such as state economic size. State fixed effects also help control for potentially endogeneous levels of bankruptcy exemptions across states (recall that Hynes, Malani and Posner (2004) show that the single biggest determinant of bankruptcy exemption levels are historic (circa 1920) levels of exemptions). The year effects control for aggregate economic shocks. We cluster standard errors at the state level to address the serial correlation concerns in Bertrand, Duflo and Mullainathan (2004).

We note that this empirical set-up is richer than the typical difference-in-differences regression, which splits pre and post reform outcomes using a binary indicator for reform occurrence. In contrast, we allow the magnitude of treatment to depend on the nominal increase in exemption level. That is, we assume that the larger the increase in state exemptions, the larger the effect should be on entry and exit rates. Second, the staggered timing of the exemptions implies that our control group includes not only states that never passed exemption laws, but also states that changed exemptions before or will change exemptions later on, alleviating endogeneity concerns.

Identification in the above regression model relies on the two main assumptions for differences-in-differences models. The first assumption is that changes in firm dynamics are

due to changes in exemptions and not to other state-level economic shocks. The second is that states not passing exemption laws provide a good counterfactual, or the "parallel trends" assumption. We address these potential endogeneity concerns with these assumptions in several ways.

First, we estimate Equation 1 separately for two groups of firms: very small firms with 1-4 employees, and firms with 5-20 employees. We argue that personal bankruptcy law, which allows individuals to protect their assets from creditors, should affect mainly the first group because these very small firms are more likely to depend on debt raised by the firm owner. If changes in economic conditions affect similarly all small businesses located in a given state, the second group provides a good counterfactual. By comparing the effects of exemptions across these two types of firms, we thus address the concern that changes in exemptions may be correlated with other state-specific economic trends.[8]

Second, when analyzing firm entry we also exploit variation across industries with different entry barriers within a given state:

$$y_{s,i,t}=\alpha_{s,i}+\alpha_i\times t+\alpha_{s,t}+\beta \text{Exemptions}_{s,t}\times \text{EntBarrier}_i+\varepsilon_{s,i,t}.$$

*EntBarrier* denotes whether the industry has high or low entry barriers. Equation 2 includes state-industry type fixed effects ($\alpha_{s,i}$), state-year fixed effects ($\alpha_{s,t}$), and accounts for differential trends across industry types ($\alpha_x \times t$). Consequently, Equation 2 allows us to identify only the differential effect of exemptions on entry across industries with high and low entry barriers. This specification provides better identification of the effect of exemptions because the state-year fixed effects soak up any statewide changes in firm entry and thus mitigate the concern

[8] We also estimate triple differences models saturated with high-dimensional fixed effects (state-year, state-firm size, and year-firm size). We present these results in Appendix 2.

that the exemptions might be correlated with other statewide economic shocks. We note that the identification strategy used in Equation 2 is similar to that used, for instance, in Cetorelli and Strahan (2006).

Third, we examine the dynamic response of our dependent variables to the changes in exemptions. We do so by replacing in Equation 1 the *Exemptions* variable by leads and lags of the exemption laws. The lag variables allow us to assess the presence of pre-trends while the lead variables allow us to analyze how firm dynamics adjust in response to the change in exemptions. These results are depicted in Figure 1 and described in Section 5.3 below.

## 5. Results

### 5.1. Exemptions, firm entry, and job creation

In Table 3 we study the effect of exemptions on firm entry and job creation using state-year level data from the Census BDS for the period 1994-2013. The dependent variables are the rate of firm entry (columns 1 and 2) and the rate of job creation (columns 3 and 4). Results are similar if we use instead the log of the number of firms entering and jobs created as dependent variables (see Appendix 1). For each dependent variable we estimate two specifications. The first specification in columns 1 and 3 focuses on the smaller firms (1-4 employees), while the second specification in columns 2 and 4 focuses on the larger firms (5-20 employees). As argued above, personal bankruptcy law should affect mainly the entry of very small businesses. In contrast, economic conditions are likely to affect all small firms. By comparing the effects of exemptions across these two groups of firms, we intend to provide an identification test. Finding no effect of exemptions on the larger firms would rule out the possibility that exemptions are correlated with state-specific economic shocks. We include in all specifications state fixed effects, year fixed effects, and control for the state's change in house price index, and log of median income. We cluster standard errors at the state level.

The results show that the exemptions have little effect on aggregate firm entry or job creation. All estimated effects are statistically insignificant and economically small. We also note that the control variables work as expected. In particular, our results confirm the importance of the housing collateral channel both for firm entry and job creation, as documented in Adelino, Schoar and Severino (2015), Corradin and Popov (2015), Ersahin and Irani (2015), Kerr, Kerr and Nanda (2015), and Schmalz, Sraer and Thesmar (2017).

Our finding contrasts with the cross-sectional evidence in Fan and White (2003), who show that business ownership rates increase with the level of exemptions (see also the cross-country evidence in Armour and Cumming, 2008).[9] More broadly, our results do not provide empirical support to the popular view that a pro debtor personal bankruptcy regime can be an effective tool to promote firm entry and job creation (Audretsch, 2007).

#### *5.1.1. Firm entry across industries*

We next exploit within-state variation across industries to investigate how any new entrants are distributed across economic sectors. Although we found no effect of exemptions on aggregate firm entry, it could still be that exemptions induce entry in industries with low barriers to entry. To test this hypothesis, we estimate regressions at the state-industry-year level in which we compare the effect of exemptions on entry across sectors with high versus low entry barriers in a given state and year.

We present the results in Table 4. The data source is the restricted-access Census LBD for the period 1994-2013. The dependent variable is the log of firm births. We saturate the regressions with state-year fixed effects, state-industry fixed effects, and differential linear time trends across industries. For that reason, we can only identify the interaction of the exemption

[9] Whereas Fan and White (2003) exploit cross-sectional variation in state exemptions, our panel data allows us to use variation in exemptions over time in a given state. We believe our approach, which controls for state and year fixed effects, is more robust to potential endogeneity problems. In addition, our use of public and non-public data from the LBD of the U.S. Census Bureau allows us to tease out various mechanisms underlying our main finding.

variable with the type of industry (high versus low entry barriers). We define entry barriers based on the amount of capital needed to set up a firm in a particular industry (as in Adelino et al., 2015). In particular, an industry with low entry barriers is one with below-median startup capital needs. We cluster standard errors at the state level.

If exemptions deter firm entry by reducing entrepreneurs' access to external financing, this effect should be less pronounced in industries with low start-up capital needs. Consistent with this view, Table 4 shows that the exemptions induce relatively more firm entry in industries with low entry barriers. The estimated coefficient for the interaction of the exemptions with the dummy that indicates an industry with low entry capital is positive and significant at the 5% level. However, the estimated effect is economically small. Taking the average change in exemptions during our sample period (50%), the estimated coefficient indicates an increase in firm entry of around 0.28% in industries with low (relative to high) startup costs.[10] In terms of firm counts, this effect corresponds to the additional creation of 198 firms in a median-sized state (which is populated by about 68,700 firms).

We also note that this specification provides better identification of the effect of exemptions because it compares firm entry rates across different sectors in a given state and year. Indeed, the state-year fixed effects soak up any statewide changes in firm entry and thus mitigate the concern that the exemptions might be correlated with other statewide economic shocks.[11]

### 5.2. Exemptions, firm exit, and job destruction

[10] To access economic significance of the exemptions we multiply the estimated coefficient by ln(1.5)~0.4, where 1.5 corresponds to a 50% increase in exemptions.

[11] Following Kerr and Nanda (2009), we also analyze the entry of new start-up firms (single-unit) relative to the creation of new establishments by existing companies (multi-unit) in a given state, industry, and year. The identifying assumption in this alternative model is that the exemptions should affect mostly the smaller firms (single unit), which are the ones more likely to rely on personal loans for financing. However, we find no significant differences in entry between single-unit and multi-unit firms. This is consistent with our earlier finding in Table 3, which also shows no effect of exemptions on entry for any of size categories analyzed.

In Table 5 we study the effect of exemptions on firm exit and job destruction using state-year level data from the Census BDS for the period 1994-2013. The dependent variables are the firm exit rate (columns 1 and 2) and job destruction rate (columns 3 and 4). As before, we present split sample results for firms with 1-4 employees (in columns 1 and 3) and firms with 5-20 employees (in columns 2 and 4). All regressions include state fixed effects, year fixed effects, and control for the state's change in house price index and log of median income. We cluster standard errors at the state level.

The results show that the exemptions lead to significantly higher firm exit and job destruction rates only for firms with 1-4 employees. The estimated effects in columns 1 and 3 are not only statistically significant, but also economically relevant. For example, the point estimate in column 1 indicates that raising exemptions by 50% (the average change in our sample period) increases the exit rate of small firms by 0.14 percentage points (about 0.8% of the unconditional sample mean of this variable, for the smallest firms). With respect to the increase in job destruction rate, the economic effect is comparable in magnitude. The estimated increase in job destruction rate for a 50% increase in exemptions is 0.16 percentage points. This effect represents 0.5% of the unconditional sample mean of this variable for the smallest firms. Given that the median number of employees (across all states and years) is 1.5 million, our estimate indicates 2,400 additional job losses resulting from small firm exits. Results on the state-level control variables broadly confirm that firm exit is associated with deteriorating economic conditions, such as declining real estate prices.

The fact that we find no effect of exemptions on the exit rate for the group of larger firms (in columns 2 and 4) corroborates our empirical strategy. Personal bankruptcy law applies to personal debts and hence it should affect mainly very small firms that typically raise debt financing via their owners. If exemptions were picking some confounding economy-wide shocks that also drive firm exit, we should also see higher exit rates among the group of larger

firms (which are also fairly small in size). We perform additional tests described below that help us further tighten our identification of the effect of exemptions on firm exit, and rule out other potential endogeneity concerns. In particular, we attempt to identify the mechanism driving our results by exploiting variation in firm age and in year of birth. In addition, we carefully investigate the dynamic adjustment of those variables around the changes in exemptions to confirm a causal interpretation of our results.

### 5.3. What drives firm exit?

Several distinct mechanisms might explain the increase in exit rates. First, a higher exemption level increases the financial benefit of filing for bankruptcy and makes individuals more likely to file for bankruptcy (Fay, Hurst and White, 2002). Therefore the increase in exit rates we observe could be due to strategic defaults.[12] We call this the strategic default effect. Second, if the exemptions increase firm entry, then they may also increase competition in the product market and consequently lead to higher firm exit rates. We call this the competition effect. Third, prior evidence shows that higher exemptions reduce access to credit to small businesses. In particular, Cerqueiro and Penas (2017) show using data from the Kauffman Firm Survey that higher exemptions reduce credit availability to very young firms whose owners are not wealthy and makes them more likely to fail. We call this the credit supply effect. Fourth, higher exemptions may attract a worse pool of entrepreneurs, which are more likely to fail. Hombert et al. (2016) argue that providing entrepreneurs with downside risk protection (such as a more lenient bankruptcy regime) has two effects on the quality of the businesses created. On the one hand, some able and risk averse individuals are more likely to become entrepreneurs. On the other hand, individuals who have low expectations about the success of

[12] Fay et al. (2002) measure the financial benefit from filing for bankruptcy as the value of debt discharged in bankruptcy minus the value of nonexempt assets that households would have to give up in bankruptcy. They show that the main driver of a higher likelihood of filing for bankruptcy is the amount of debt discharged rather than the amount of exempt assets.

their venture are also more likely to become entrepreneurs. Hombert et al. (2016) further show that the latter effect dominates if the original pool of entrepreneurs is heterogenous. We call this the pool effect.

One common implication of the two latter mechanisms is that failure rates should be disproportionally higher for younger firms than for older firms following an increase in exemptions. In the case of the credit supply effect, young businesses are more opaque to creditors, and thus creditors are less likely to lend to them (relative to older, established businesses), and thus younger businesses are more likely to fail. In the case of the pool effect, younger firms (recent entrants) are on average worse quality than older firms (older entrants), and thus we expect younger firms to fail at a higher rate. In the next subsection we test whether the data corroborate this intuition.

#### *5.3.1. Analysis of age cohorts*

We analyze in Table 6 how exit rates vary with firm age. Data are from the Census BDS for the period 1994-2013 and include only firms with 1-4 employees. The dependent variables are the firm exit rate (in columns 1-3) and the job destruction rate (in columns 4-6). We assess the effect of exemptions for three different age groups (1-5 years, 6-15 years, and more than 15 years old). The estimated regressions are otherwise similar to those in Table 5, i.e. they all include state fixed effects, year fixed effects, and the same set of control variables. We cluster standard errors at the state level.

The results in Table 6 show that the effect of exemptions on small firm exit and job destruction fades with firm age. The youngest group of firms (in columns 1 and 4) experience a sharp increase in firm exit and job destruction rates. We obtain much smaller estimated coefficients for the second age group (in columns 2 and 5), and only the effect on job destruction rate remains significant at the 10% level. For the oldest group of firms (in columns 3 and 6) we find virtually no effect of exemptions on their exit rates.

*5.3.1.1. Dynamics for different age cohorts*

We also investigate the dynamic behavior of firm exit around the event date to confirm a causal interpretation of our results. We are particularly interested in whether there are significant changes in firm exit preceding the exemption laws, and whether the adjustment seems sensible. Figure 1 displays the dynamics separately for firms aged 1-5 years (top panel) and firms with 6 or more years (bottom panel).

The Figure plots point estimates of coefficients (with one standard deviation bands) of leading and lagging indicators of the exemption laws around the year of the event. The dependent variable is the exit rate of small firms and we estimate the same specification as in columns 1-3 of Table 6. For states that raise their exemption limit more than once, we plot dynamics for the largest change. Figure 1 shows a large, positive, persistent, and significant effect of exemptions on the exit rates of small and young firms (top panel), and no meaningful effect for older firms (bottom panel).

This result is consistent with our previous findings in Table 6 and provides additional evidence that the exemptions are not picking up other economy-wide shocks. The figure also shows no significant effect of the exemptions on small firm exit prior to the passage of the laws, confirming that exit rates are reacting to a change in exemptions and not the other way around. Moreover, the effect of the exemptions on the exit rate of young firms is not immediate; it peaks after three years and the effect persists with time.

Our results confirm that younger firms drive the increase in firm exit and job loss rates we document. These results are consistent with both the credit supply effect and the pool effect. On the one hand, younger—and thus more opaque—firms could suffer a stronger reduction in credit availability as exemptions increase, as shown in Cerqueiro and Penas (2017). On the other hand, these younger firms could be of lower quality if the exemptions attract a worse pool of entrepreneurs. In order to further investigate the role of the latter mechanism, we next

compare exit rates of firms created before and after an increase in exemptions.

### *5.3.2. Analysis of birth cohorts*

We collect from the Census BDS data at the state-cohort-year level for the same period of analysis (1994-2013). We include only firms aged from 1 to 5 years, for two reasons. First, the BDS provides the exact age only for these five cohorts.[13] For the older cohorts we are not always able to identify whether the firms were created before or after a change in exemptions and hence we prefer to discard them.[14] Second, our previous results show that the exemptions affect only the group of younger firms we analyze.

We identify for each state the year (if any) in which exemptions were raised. For states raising exemptions multiple times, we select the largest change. In order to compare the exit rates of firms created before and after a legal change, we create two versions of the variable *Post-Law cohort.* The first version (*binary treatment*) is an indicator variable that equals one if a given firm cohort in a certain state was created after an exemption law, and zero otherwise. To see how this variable works, take for example the increase in exemptions in Colorado in the year 2000. In a given year (say, 2003) we can compare the exit rates of firms created before and after the year of the law. In this case the variable *PostLaw cohort (binary treatment)* equals zero for firms that were born in 1998 and 1999, and equals one for firms born in 2000, 2001, and 2002. The second version *(treatment intensity)* is its continuous counterpart, which we calculate replacing the ones of the binary variable by the change in log exemptions. For states that never raised exemptions during our sample period, both *Post-Law cohort* variables are set to zero.

[13] The BDS provides the 10 following age bins: 1, 2, 3, 4, 5, 6-10, 11-15, 16-20, 21-25, and 26 or more years.

[14] We note, however, that results are similar if we consider all cohorts in our analysis. The results are available from the authors upon request.

If higher exemption levels attract lower quality entrepreneurs as posited by the pool effect mechanism, then the firms that are created after an exemption law should be more likely to fail than those created before. We put this intuition to a formal test in Table 7. The dependent variable is the exit rate of small firms (with 1-4 employees). The explanatory variables of interest are the *Post-Law cohort* measures, either in its binary version (in columns 1, 2, 4 and 5) or continuous version (in column 3). To maintain consistency with our earlier regressions, we cluster standard errors at the state level.

In column 1 we include only state, cohort, and year fixed effects. The estimated coefficient for the variable *Post-Law cohort (binary treatment)* is obtained from comparing the average exit rates of firms created before and after a change in exemptions, after conditioning on covariates. The coefficient is positive and significant, indicating that firms that enter following a change in exemptions experience significantly higher rates of exit than those created before.

While this result suggests that increases in exemptions foster the entry of lower quality firms that are more likely to go out of business, one might worry that other state-level factors that affect firm survival may be also changing at the same time. To address this concern, we add in column 2 state×year fixed effects that force comparison of post-law and pre-law cohorts within a given state and year. The coefficient on the variable *Post-Law cohort (binary treatment)* drops in magnitude but remains statistically significant. In column 3 we show that the finding holds when we allow exit rates of firms that enter after a legal change to depend on the intensity of the treatment (i.e., by how much exemptions were increased).

In columns 1-3 we considered all changes in exemptions irrespective of their magnitude. As documented in Table 1, some states raised their exemptions by small amounts, which should be unlikely to have important real effects. For this reason, in column 4 we consider only exemptions increases of at least $50,000 (and discard from the estimation sample

all exemption changes smaller than this value). The estimated coefficient we obtain is quantitively similar to the one obtained in column 2 that uses the full set of exemption changes, indicating that our results are not driven by the smaller exemption adjustments. In column 5 we show that our main finding holds even if we fully saturate our regression model with state×year, state×cohort, and cohort×year fixed effects. The results in Table 7 altogether provide strong evidence in support of the pool effect.

### 5.4. Are firms shrinking in size or disappearing?

Our findings thus far show that while an increase in exemptions has no significant effect on firm entry or job creation, it leads to higher firm exit and job destruction rates among firms with less than five employees. Do these results mean that a higher exemption level reduces the equilibrium number of small firms and jobs at small firms in the economy? Not necessarily, because the datasets we analyzed contain only employer firms, which according to Census data turn out to represent only about 20% of the total number of firms in the U.S. For example, it could be that higher exemptions induce individuals to shift to self-employment by creating very small firms with no paid employees. This mechanism implies that small firms, rather than being fewer, might be simply getting smaller.

We investigate this possibility in Table 8 using also non-employer data from the Census Nonemployer Statistics (NES). One drawback of this database is that we can only study net firm creation, because the database does not contain information on entry and exit. So, we start by assessing for comparison purposes the economic effect of exemptions on net firm creation and net job creation for the *employer* firms in Panel A. We compute these variables as the change in the number of firms or jobs between two consecutive years divided by the previous year levels. To maintain consistency with our previous analysis, we present the same sample split results (for firms with 1-4 employees and 5-20 employees), include state and year fixed effects and the same set of control variables, and cluster standard errors at the state level

The estimated coefficients for the exemptions variable are fairly close to the difference between the coefficients we found for firm entry (in Table 3) and firm exit (Table 5). The point estimate in column 1 indicates that raising exemptions by 50% reduces the rate of net creation of firms with less than five employees by 0.14 percentage points, which equals 24% of total net firm creation (of the same size) during our sample period. The estimated increase in net job destruction rate in column 3 for a 50% increase in exemptions is 0.12 percentage points. This effect represents 7% of the unconditional sample mean of this variable for the smallest firms.

In Panel B we use data from the Census NES to analyze whether the drop in the number of small firms with paid employees was compensated by an increase in the number of *non-employer* firms. Data are at the state-year level (in column 1) and state-industry-year level (column 2), for the period 1997-2013.[15] The dependent variable is the percent change in the number of non-employer firms between two consecutive years. Standard errors are clustered at the state level and shown in brackets.

Column 1 shows that exemptions also reduce the number of non-employer firms. In spite of the shorter time period, the estimated effect is marginally significant at the 10% level. Its economic magnitude is comparable to our previous result (in column 1 of Panel A). The point estimate in column 1 indicates that raising exemptions by 50% reduces the net firm creation rate by 0.1 percentage point, which equals 4.2% of the average rate of net creation of non-employer firms during our estimation period.

What explains the drop in non-employer firms? One possible channel discussed above is a reduction in credit supply to entrepreneurs. We investigate this potential channel by comparing the effect of exemptions on firm creation rates across sectors with high versus low entry barriers in a given state and year (as we did in Table 4). An industry with low entry barriers is one with below-median startup capital needs. As argued before, an increase in

[15] Nonemployer statistics have been collected annually only since 1997.

exemptions should affect more negatively sectors that are more capital-dependent. We saturate the regression in column 2 with fixed effects at the state-year, state-industry, and industry-year. Therefore, we can only identify the interaction of the exemption variable with the indicator of an industry with low entry barriers. The estimated coefficient is positive and statistically significant at the 10% level. Thus, the decline in the number of non-employer firms we found in column 1 appears to be driven by industries with larger startup capital needs.

Altogether, the findings in Table 8 allow us to conclude that exemptions reduce both the number of small employer and non-employer firms operating in the economy. These results are important given the growing evidence that small firms contribute disproportionally to new job creation (Haltiwanger, Jarmin, and Miranda, 2013; Adelino, Ma, Robinson, 2017). In addition, the findings on non-employer firms help us to rule out the alternative explanation that entrepreneurs are leaving small employer firms to start non-employer firms in response to changes in debtor protection.

### 5.5. Additional robustness tests

We also conducted a variety of other tests to systematically rule out other alternative explanations. To see how sensitive our results are to the inclusion of other relevant public policy variables that might also affect business dynamism, we collected several indicators of Economic Freedom as published by the Frasier Institute and used for example in Cumming and Li (2013). In particular, we collected the three main indicators used by Cumming and Li (2013): (1) Government Spending Index, (2) Taxation Index, and (3) Labor Freedom Index. Since these indices are calculated for each state and year, we start by analyzing their correlation with exemptions. It turns out that they are virtually uncorrelated (correlations of changes in exemptions and changes in each of the indexes is 1% or lower). Our next step is to include these indexes as control variables in regressions on firm entry, job creation, firm exit and job destruction. Controlling for these determinants of economic freedom does not alter our main

results, however, the Labor Freedom Index does appear to be an important determinant of business dynamism.[16]

Next, we conduct tests to see how sensitive our results are to the financial crisis period. To that end, we added to our main regressions an interaction of the exemptions with a variable that equals one during 2008-2010, and zero otherwise (we define the 2008-2010 years as the crisis period following Cumming and Li (2013)). We also perform a set of regressions in which we include an interaction between the exemptions variable with a dummy that equals one for the states that experienced the largest drop in house prices during the 2008-2010. In particular, we pick for each crisis year the states in the bottom quartile of the distribution of changes in the house price index. That is, we compare for every crisis year the effect of the exemptions across the 13 states that suffered the largest drop in house prices with the other 38 states. The results shown in Appendix 3 indicate that in states with the largest drop in house prices the negative effect of exemptions is amplified, consistent with the collateral channel exacerbating the effect of the increase in debtor protection (Adelino et al, 2015, and Schmaltz et al, 2017).

## 6. Conclusion

We study the effect of debtor protection on firm entry and exit dynamics. We find that more lenient personal bankruptcy laws increase firm entry only in sectors with low startup capital needs. We also find that debtor protection increases firm exit and job destruction rates among very small firms. These negative effects hold mainly for very young firms, take three years to materialize, and are persistent in time. Finally, we find that exit rates of firms born after the increase in debtor protection are higher than exit rates of firms born before. Our results suggest that the mechanisms affecting firm dynamics include both a decrease in credit supply and the entry of lower quality firms (that later exit) following an increase in debtor protection.

[16] Tables of results are available on request from the authors.

Our study contributes to existing literatures on bankruptcy, entrepreneurship and business dynamism. Our study also has implications for current policy discussions around the decline in business dynamism (e.g., Hathaway and Litan, 2014; Council of Economic Advisers, 2016; Shambaugh, Nunn, and Liu, 2018). Notably, our results suggest that calls to use higher levels of debtor protection to encourage more firm entry could perversely lead to higher firm exit in some cases.

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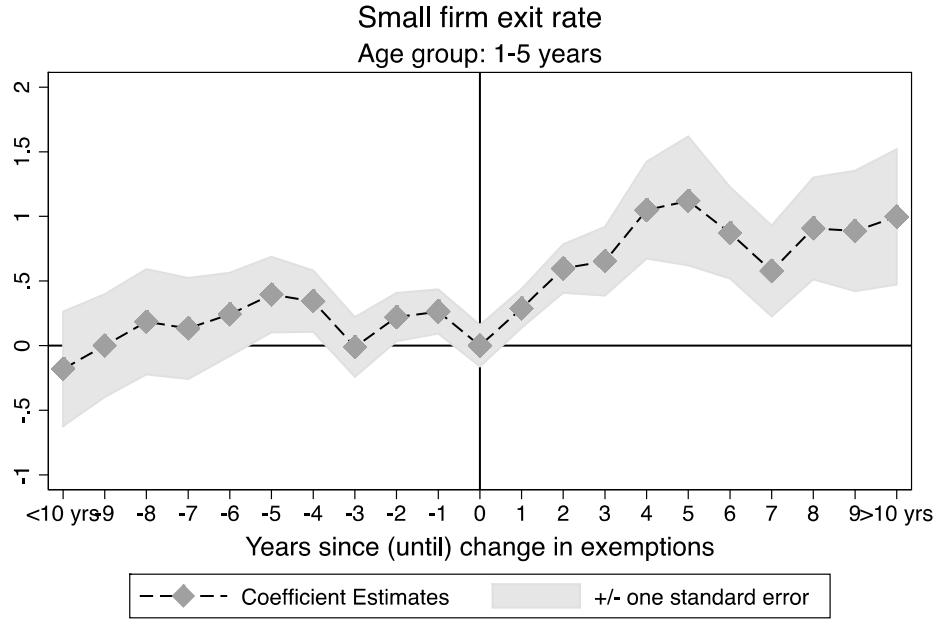


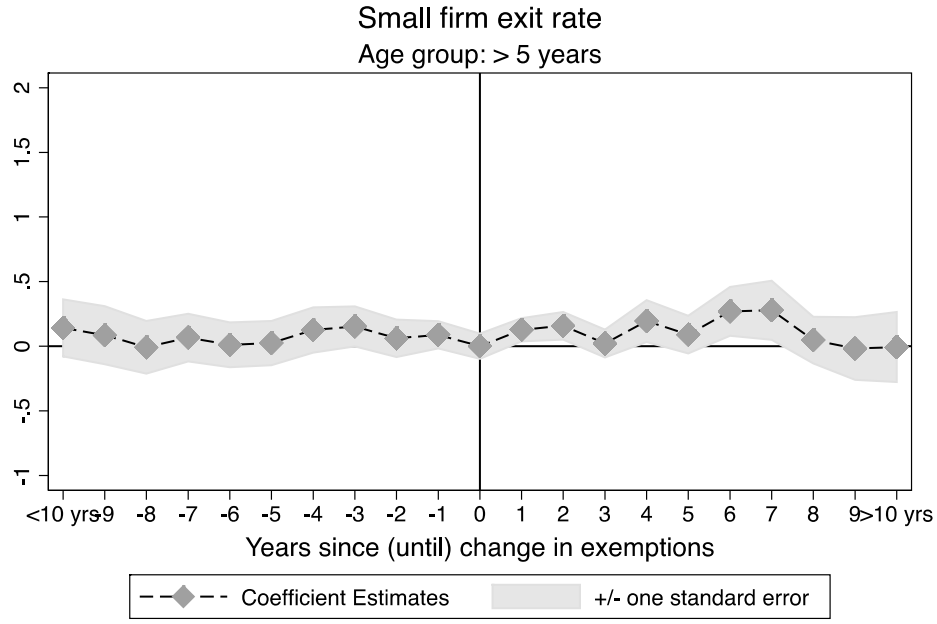


**Figure 1 – Dynamic effect of the exemption laws on small firm exit rate**

The Figure plots point estimates of leading and lagging indicators of the exemption laws around the event on small firm death rates. Small firms have 1-4 employees. The top panel contains estimates for the sample of younger firms (1-5 years), and the bottom panel contains estimates for the sample of older firms (>5 years). The regressions are similar to those displayed in Table 6. For states that raise their exemption limit more than once, we consider the largest change.

## Table 1 – Bankruptcy exemptions by state in 1994 and 2013

State exemptions include the homestead and personal property exemptions. Personal property exemptions contain the following assets: jewelry, motor vehicle, cash and deposits, and a "wildcard" (an exemption that applies to any property). "Unlimited" refers to states with unlimited homestead exemptions. In our empirical analyses we assign a value of $1,000,000 to states with unlimited exemptions.

| State | State exemptions ($) | | Years exemptions changed |
|---|---|---|---|
| | 1994 | 2013 | |
| Alabama | 16,000 | 16,000 | |
| Alaska | 71,500 | 87,480 | 1999, 2004, 2008, 2012 |
| Arizona | 103,300 | 160,300 | 2001, 2004 |
| Arkansas | Unlimited | Unlimited | |
| California | 78,700 | 110,525 | 1995, 2003, 2007, 2009, 2010, 2013 |
| Colorado | 63,000 | 134,000 | 2000, 2007 |
| Connecticut | 155,000 | 159,000 | 2007 |
| Delaware | 5,000 | 180,000 | 2005, 2010, 2011, 2012 |
| District Of Columbia | 38,400 | Unlimited | 1999, 2001 |
| Florida | Unlimited | Unlimited | |
| Georgia | 13,800 | 52,200 | 2001, 2012 |
| Hawaii | 38,400 | 58,850 | 1998, 2001, 2004, 2007, 2010, 2013 |
| Idaho | 53,500 | 117,600 | 1999, 2006, 2008, 2010 |
| Illinois | 21,400 | 42,800 | 2006 |
| Indiana | 23,200 | 54,600 | 2005, 2010 |
| Iowa | Unlimited | Unlimited | |
| Kansas | Unlimited | Unlimited | |
| Kentucky | 23,000 | 58,850 | 2005, 2007, 2010, 2013 |
| Louisiana | 22,500 | 42,500 | 2000, 2009 |
| Maine | 17,300 | 107,300 | 1995, 2001, 2003, 2008 |
| Maryland | 11,000 | 44,975 | 2004, 2010, 2013 |
| Massachusetts | 102,650 | 524,450 | 2000, 2004, 2011 |
| Michigan | 38,400 | 58,850 | 1998, 2001, 2004, 2007, 2010, 2013 |
| Minnesota | 206,400 | 399,200 | 1996, 1998, 2004, 2006, 2007, 2008, 2010, 2012 |
| Mississippi | 95,000 | 95,000 | |
| Missouri | 11,650 | 21,450 | 2003, 2004 |
| Montana | 91,400 | 514,000 | 1997, 1999, 2001, 2007 |
| Nebraska | 10,000 | 64,800 | 1997, 2007 |
| Nevada | 98,000 | 592,000 | 1995, 1997, 2003, 2005, 2007 |

| State | State exemptions ($) | | Years exemptions changed |
|---|---|---|---|
| | 1994 | 2013 | |
| New Hampshire | 63,000 | 225,000 | 1995, 1997, 2002, 2004 |
| New Jersey | 38,400 | 58,850 | 1998, 2001, 2004, 2007, 2010, 2013 |
| New Mexico | 67,000 | 127,000 | 2007 |
| New York | 29,800 | 320,000 | 2005, 2011 |
| North Carolina | 23,000 | 77,000 | 2006, 2009 |
| North Dakota | 86,200 | 110,450 | 2009 |
| Ohio | 9,400 | 146,700 | 2008, 2010, 2013 |
| Oklahoma | Unlimited | Unlimited | |
| Oregon | 55,800 | 75,400 | 2006, 2009 |
| Pennsylvania | 38,400 | 58,850 | 1998, 2001, 2004, 2007, 2010, 2013 |
| Rhode Island | 38,400 | 541,000 | 1998, 1999, 2001, 2004, 2006, 2008, 2012, 2013 |
| South Carolina | 13,000 | 125,775 | 2006, 2008, 2010, 2012 |
| South Dakota | Unlimited | Unlimited | |
| Tennessee | 15,500 | 27,500 | 2010 |
| Texas | Unlimited | Unlimited | |
| Utah | 13,000 | 66,000 | 1997, 1999, 2013 |
| Vermont | 76,200 | 266,200 | 1996, 2009 |
| Virginia | 20,000 | 20,000 | |
| Washington | 39,000 | 144,500 | 1998, 1999, 2002, 2007, 2011 |
| West Virginia | 19,200 | 58,400 | 1996, 2002 |
| Wisconsin | 54,400 | 192,000 | 2009 |
| Wyoming | 24,000 | 50,000 | 1996, 2012 |

**Table 2- Descriptive statistics**

Panel A: Firms with 1-4 employees (BDS). Period: 1994-2013.

| Variable | Mean | Standard Deviation | Min | Max |
|---|---|---|---|---|
| *Firm and job creation* | | | | |
| Firm entry (thous.) | 9.963 | 11.776 | 0.678 | 75.672 |
| Firm entry rate | 0.186 | 0.034 | 0.109 | 0.311 |
| Job creation (thous.) | 35.696 | 41.238 | 3.097 | 261.829 |
| Job creation rate | 0.310 | 0.048 | 0.213 | 0.483 |
| *Firm and job destruction* | | | | |
| Firm exits (thous.) | 9.244 | 10.694 | 0.883 | 71.000 |
| Firm exit rate | 0.173 | 0.020 | 0.127 | 0.273 |
| Job destruction (thous.) | 33.684 | 38.165 | 3.500 | 253.097 |
| Job destruction rate | 0.293 | 0.028 | 0.218 | 0.436 |
| *Net firm and job creation* | | | | |
| Net firm creation (thous.) | 0.377 | 1.466 | -11.019 | 12.264 |
| Net firm creation rate | 0.006 | 0.020 | -0.067 | 0.101 |
| Net job creation (thous.) | 2.012 | 6.881 | -55.920 | 43.581 |
| Net job creation rate | 0.017 | 0.044 | -0.152 | 0.143 |

Panel B: Firms with 5-20 employees (BDS). Period: 1994-2013.

| Variable | Mean | Standard Deviation | Min | Max |
|---|---|---|---|---|
| *Firm and job creation* | | | | |
| Firm entry (thous.) | 1.410 | 1.677 | 0.102 | 12.581 |
| Firm entry rate | 0.080 | 0.019 | 0.041 | 0.165 |
| Job creation (thous.) | 55.749 | 63.181 | 6.003 | 418.527 |
| Job creation rate | 0.377 | 0.054 | 0.247 | 0.594 |
| *Firm and job destruction* | | | | |
| Firm exits (thous.) | 1.340 | 1.582 | 0.100 | 11.720 |
| Firm exit rate | 0.076 | 0.016 | 0.039 | 0.145 |
| Job destruction (thous.) | 52.557 | 59.264 | 5.847 | 410.506 |
| Job destruction rate | 0.354 | 0.042 | 0.261 | 0.598 |
| *Net firm and job creation* | | | | |
| Net firm creation (thous.) | 0.079 | 0.681 | -5.883 | 3.617 |
| Net firm creation rate | 0.003 | 0.017 | -0.056 | 0.061 |
| Net job creation (thous.) | 3.192 | 11.565 | -119.743 | 79.004 |
| Net job creation rate | 0.023 | 0.054 | -0.256 | 0.197 |

Panel C: Non-employer firms (NES). Period: 1997-2013.

| Variable | Mean | Standard Deviation | Min | Max |
|---|---|---|---|---|
| *Net firm creation* | | | | |
| Net firm creation (thous.) | 9.272 | 17.716 | -68.726 | 128.668 |
| Net firm creation rate | 0.022 | 0.024 | -0.198 | 0.215 |

**Table 3 – Firm entry and job creation**

Data are at the state-year level from the Census BDS for the period 1994-2013. *Firm entry rate* is the number of firms that enter in year t divided by the number of firms in year t-1. *Job creation rate* is the number of jobs created in year t divided by total employment in year t. Standard errors are clustered at the state level and shown in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

| Dependent variable: | Firm entry rate | | Job creation rate | |
|---|---|---|---|---|
| Firm size: | 1-4 employees | 5-20 employees | 1-4 employees | 5-20 employees |
| | (1) | (2) | (3) | (4) |
| Log(Exemptions) | -0.00105 | -0.000741 | -0.000719 | -0.00310 |
| | [0.00162] | [0.000736] | [0.00187] | [0.00259] |
| Change in House Price Index | 0.0959*** | 0.0280*** | 0.141*** | 0.155*** |
| | [0.0110] | [0.00777] | [0.0148] | [0.0255] |
| Log(Median income) | 0.0253* | 0.00889 | 0.0404** | 0.0479* |
| | [0.0138] | [0.00772] | [0.0181] | [0.0263] |
| State FE | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes |
| Observations | 969 | 969 | 969 | 969 |
| R-squared | 0.932 | 0.914 | 0.941 | 0.902 |

**Table 4 – Firm entry across industries with different entry barriers**

Data are at the state-year-industry level from the Census LBD for the period 1994-2013. The dependent variable is the log of the number of firms that enter in year t. Industries with low entry barriers have below-median capital needs to set up a new firm. Standard errors are clustered at the state level and shown in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Numbers have been rounded to the closest 4 digits to comply with Census disclosure requirements.

| Dependent variable: | Firm entry |
|---|---|
| Log(Exemptions) × Low entry barrier | 0.0071** |
| | [0.0029] |
| State × Year FE | Yes |
| State × Industry FE | Yes |
| Industry linear time trends | Yes |
| Observations (approximate) | 22,000 |
| R-squared | 0.985 |

**Table 5 – Firm exit and job destruction**

Data are at the state-year level from the Census BDS for the period 1994-2013. *Firm exit rate* is the number of firms exiting in year t divided by the number of firms in year t-1. *Job destruction rate* is the number of jobs destroyed in year t divided by total employment in year t-1. Standard errors are clustered at the state level and shown in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

| Dependent variable: | Firm exit rate | | Job destruction rate | |
|---|---|---|---|---|
| Firm size: | 1-4 employees | 5-20 employees | 1-4 employees | 5-20 employees |
| | (1) | (2) | (3) | (4) |
| Log(Exemptions) | 0.00336** | 0.000281 | 0.00397** | 0.000373 |
| | [0.00152] | [0.000724] | [0.00157] | [0.00230] |
| Change in House Price Index | -0.0868*** | -0.0310*** | -0.121*** | -0.164*** |
| | [0.0101] | [0.00638] | [0.0146] | [0.0257] |
| Log(Median income) | -0.0126* | -0.00633 | -0.0126 | -0.0121 |
| | [0.00723] | [0.00513] | [0.00957] | [0.0181] |
| State FE | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes |
| Observations | 969 | 969 | 969 | 969 |
| R-squared | 0.860 | 0.892 | 0.852 | 0.822 |

**Table 6 – Firm exit and job destruction across age bins**

Data are at the state-year level from the Census BDS for the period 1994-2013. The sample includes only firms with 1-4 employees. *Firm exit rate* is the number of firms exiting in year t divided by the number of firms in year t-1. *Job destruction rate* is the number of jobs destroyed in year t divided by total employment in year t-1. Standard errors are clustered at the state level and shown in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

| Dependent variable: | Firm exit rate | | | Job destruction rate | | |
|---|---|---|---|---|---|---|
| Age group: | 1-5 years | 6-15 years | >15 years | 1-5 years | 6-15 years | >15 years |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Log(Exemptions) | 0.00526** | 0.00152 | 0.000885 | 0.00602** | 0.00245* | 0.00119 |
| | [0.00227] | [0.00134] | [0.00124] | [0.00246] | [0.00145] | [0.00191] |
| Change in House Price Index | -0.121*** | -0.0723*** | -0.0245*** | -0.149*** | -0.101*** | -0.0400*** |
| | [0.0146] | [0.0106] | [0.00739] | [0.0181] | [0.0176] | [0.0102] |
| Log(Median income) | -0.0202** | -0.0253*** | -0.0127 | -0.0253* | -0.0193* | -0.0120 |
| | [0.00959] | [0.00831] | [0.00798] | [0.0134] | [0.0115] | [0.00998] |
| State FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 969 | 969 | 969 | 969 | 969 | 969 |
| R-squared | 0.801 | 0.782 | 0.739 | 0.823 | 0.785 | 0.782 |

**Table 7 – Firm exit rates of pre-law and post-law birth cohorts**

Data are at the state-cohort-year level from the Census BDS for the period 1994-2013. The sample includes only firms with 1-4 employees and with 1-5 years of existence. *Firm exit rate* is the number of firms exiting in year t divided by the number of firms in year t-1. *Post-Law cohort (binary treatment)* equals one if that firm cohort was created after an exemption law, and zero otherwise. For states that never raised exemptions during our sample period, *Post-Law cohort (binary treatment)* is set to zero. *Post-Law cohort (treatment intensity)* is defined similarly with the difference that the ones are replaced by the magnitude of the exemption changes (in logs). For states raising exemptions multiple times, we select the largest change. Standard errors are clustered at the state level and shown in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

| Dependent variable: | Firm exit rate | | | | |
|---|---|---|---|---|---|
| Exemption changes: | All | All | All | ≥ $50,000 | ≥ $50,000 |
| | (1) | (2) | (3) | (4) | (5) |
| Post-law cohort (binary treatment) | 0.00819*** | 0.00400** | | 0.00471* | 0.00437** |
| | [0.00219] | [0.00164] | | [0.00256] | [0.00211] |
| Post-law cohort (treatment intensity) | | | 0.000350** | | |
| | | | [0.000158] | | |
| State FE | Yes | No | No | No | No |
| Cohort FE | Yes | Yes | Yes | Yes | No |
| Year FE | Yes | No | No | No | No |
| State × Year FE | No | Yes | Yes | Yes | Yes |
| State × Cohort FE | No | No | No | No | Yes |
| Cohort × Year FE | No | No | No | No | Yes |
| Observations | 5,100 | 5,100 | 5,100 | 2,700 | 2,700 |
| R-squared | 0.893 | 0.939 | 0.939 | 0.939 | 0.957 |

**Table 8 – Net firm creation and employment growth**

Panel A – Employer firms

Data are at the state-year level from the Census BDS for the period 1994-2013. *Net firm creation rate* and *Net job creation rate* are computed as the change in the number of firms or jobs between two consecutive years divided by the previous year levels. Standard errors are clustered at the state level and shown in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

| Dependent variable: | Net firm creation rate | | Net job creation rate | |
|---|---|---|---|---|
| Firm size: | 1-4 employees | 5-20 employees | 1-4 employees | 5-20 employees |
| | (1) | (2) | (3) | (4) |
| Log(Exemptions) | -0.00357* | -0.00158 | -0.00289** | -0.00142 |
| | [0.00190] | [0.00133] | [0.00137] | [0.00158] |
| Change in House Price Index | 0.109*** | 0.101*** | 0.109*** | 0.108*** |
| | [0.0136] | [0.0114] | [0.0132] | [0.0152] |
| Log(Median income) | 0.0202** | 0.0209* | 0.0216* | 0.0206 |
| | [0.00932] | [0.0121] | [0.0117] | [0.0132] |
| State FE | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes |
| Observations | 969 | 969 | 969 | 969 |
| R-squared | 0.721 | 0.725 | 0.628 | 0.665 |

Panel B – Nonemployer firms

Nonemployer data at the state-year level (column 1) and at the state-industry-level (column 2) are from the Census NES for the period 1997-2013. *Net firm creation rate* is the change in the number of firms between two consecutive years divided by the previous year level. Industries with low entry barriers have below-median capital needs to set up a new firm. Standard errors are clustered at the state level and shown in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

| Dependent variable: | Net firm creation rate | |
|---|---|---|
| | (1) | (2) |
| Log(Exemptions) | -0.00232* | |
| | [0.00117] | |
| Log(Exemptions) × Low entry barrier | | 0.00769* |
| | | [0.00409] |
| State controls | Yes | |
| State FE | Yes | |
| Year FE | Yes | |
| State × Year FE | | Yes |
| State × Industry FE | | Yes |
| Year × Industry FE | | Yes |
| Observations | 765 | 12,803 |
| R-squared | 0.625 | 0.441 |

# APPENDICES

## Appendix 1 – Functional form (logs instead of rates)

Panel A – Entry and Job creation (analogous to Table 3)

Data are at the state-year level from the Census BDS for the period 1994-2013. *Log firm entry* is the log number of firms that enter in year t. *Log jobs created* is the log number of jobs created in year t. Standard errors are clustered at the state level and shown in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

| Dependent variable: | Log(Firm entry) | | Log(Jobs created) | |
|---|---|---|---|---|
| Firm size: | 1-4 employees | 5-20 employees | 1-4 employees | 5-20 employees |
| | (1) | (2) | (3) | (4) |
| Log(Exemptions) | 0.00599 | -0.000851 | 0.00887 | -0.00364 |
| | [0.0141] | [0.0109] | [0.0107] | [0.00773] |
| Change in House Price Index | 0.166** | 0.179* | 0.222*** | 0.347*** |
| | [0.0802] | [0.0951] | [0.0683] | [0.0740] |
| Log(Median income) | 0.153 | 0.205* | 0.165** | 0.237*** |
| | [0.101] | [0.108] | [0.0813] | [0.0865] |
| State FE | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes |
| Observations | 969 | 969 | 969 | 969 |
| R-squared | 0.995 | 0.994 | 0.997 | 0.997 |

Panel B – Firm exit and job destruction (analogous to Table 5)

Data are at the state-year level from the Census BDS for the period 1994-2013. *Log firm exit* is the log number of firms exiting in year t. *Log jobs lost* is the log number of jobs destroyed in year t. Standard errors are clustered at the state level and shown in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

| Dependent variable: | Log(Firm exits) | | Log(Jobs lost) | |
|---|---|---|---|---|
| Firm size: | 1-4 employees | 5-20 employees | 1-4 employees | 5-20 employees |
| | (1) | (2) | (3) | (4) |
| Log(Exemptions) | 0.0289* | 0.00447 | 0.0238* | 0.00464 |
| | [0.0172] | [0.0171] | [0.0136] | [0.0133] |
| Change in House Price Index | -0.647*** | -0.416*** | -0.521*** | -0.386*** |
| | [0.0747] | [0.109] | [0.0701] | [0.0850] |
| Log(Median income) | -0.0645 | -0.0112 | -0.0204 | 0.0688 |
| | [0.0892] | [0.0830] | [0.0757] | [0.0718] |
| State FE | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes |
| Observations | 969 | 969 | 969 | 969 |
| R-squared | 0.995 | 0.994 | 0.996 | 0.996 |

## Appendix 2 – Within state-year analysis instead of sample splits

Data are at the state-year level from the Census BDS for the period 1994-2013. *Firm entry rate* is the number of firms that enter in year t divided by the number of firms in year t-1. *Job creation rate* is the number of jobs created in year t divided by total employment in year t. *Firm exit rate* is the number of firms exiting in year t divided by the number of firms in year t-1. *Job destruction rate* is the number of jobs destroyed in year t divided by total employment in year t-1. Standard errors are clustered at the state level and shown in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

| Dependent variable: | Firm entry rate | Job creation rate | Firm exit rate | Job destruction rate |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Log(Exemptions) × (1-4 employees) | -0.000047 | 0.00201 | 0.00250* | 0.00327** |
| | [0.171] | [0.155] | [0.129] | [0.147] |
| State × Year FE | Yes | Yes | Yes | Yes |
| State × Size group FE | Yes | Yes | Yes | Yes |
| Year × Size group FE | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes |
| Observations | 1938 | 1938 | 1938 | 1938 |
| R-squared | 0.99 | 0.99 | 0.99 | 0.98 |

## Appendix 3 – Exemptions and business dynamism when house prices tank

Data are at the state-year level from the Census BDS for the period 1994-2013. *Firm entry rate* is the number of firms that enter in year t divided by the number of firms in year t-1. *Job creation rate* is the number of jobs created in year t divided by total employment in year t. *Firm exit rate* is the number of firms exiting in year t divided by the number of firms in year t-1. *Job destruction rate* is the number of jobs destroyed in year t divided by total employment in year t-1. *Housing bust* equals one for states with changes in the house price index in the bottom quartile during 2008-2010, and zero otherwise. Standard errors are clustered at the state level and shown in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

| Dependent variable: | Firm entry rate (1-4 employees) | Job creation rate (1-4 employees) | Firm exit rate (1-4 employees) | Job destruction rate (1-4 employees) |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Log(Exemptions) | -0.00171 | -0.00175 | 0.00471*** | 0.00530*** |
| | [0.00188] | [0.00208] | [0.00116] | [0.00138] |
| Log(Exemptions) × Housing bust | -0.000371* | -0.000672** | 0.000711*** | 0.000973*** |
| | [0.000214] | [0.000278] | [0.000189] | [0.000291] |
| Change in House Price Index | 0.0904*** | 0.132*** | -0.0742*** | -0.107*** |
| | [0.00990] | [0.0127] | [0.0111] | [0.0141] |
| Log(Median income) | 0.0212 | 0.0361* | -0.00949 | -0.00934 |
| | [0.0145] | [0.0190] | [0.00688] | [0.00955] |
| State FE | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes |
| Observations | 969 | 969 | 969 | 969 |
| R-squared | 0.940 | 0.947 | 0.868 | 0.861 |