Debtor protection and firm dynamics

Geraldo Cerqueiro, María Fabiana Penas, and Robert Seamans*

November 23, 2017

PRELIMINARY DRAFT

Abstract

We study the effect of debtor protection on firm entry and exit dynamics. We find that more lenient personal bankruptcy increase firm entry only in sectors with low entry barriers. We also find that debtor protection increases small firm exit rates and job destruction, and that this effect is stronger for very young firms. This negative effect takes three years to materialize and is persistent in time. Finally, we find that there is no difference between exit rates of firms born before and after the change in debtor protection. Our results overall indicate that the main mechanism affecting firm dynamics is a reduction in credit supply to the smallest and youngest firms.

Keywords: Debtor Protection, Personal Bankruptcy, Entrepreneurship, Firm dynamics

---

*Cerqueiro: Católica-Lisbon School of Business and Economics (email: geraldo.cerqueiro@ucp.pt); Penas: Escuela de Negocios, Universidad Torcuato Di Tella (email: fpenas@utdt.edu); Seamans: Stern School of Business, New York University (email: rseamans@stern.nyu.edu). Any opinions and conclusions expressed herein are those of the authors and do not necessarily represent the views of the U.S. Census Bureau. All results have been reviewed to ensure that no confidential information is disclosed. This research uses data from the Census Bureau's Longitudinal Employer Household Dynamics Program, which was partially supported by the following National Science Foundation Grants SES-9978093, SES-0339191 and ITR-0427889; National Institute on Aging Grant AG018854; and grants from the Alfred P. Sloan Foundation.

1. Introduction

Firm dynamics are a fundamental driver of job creation, innovation, and productivity growth. In this paper we investigate how statewide 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 variation across states in personal bankruptcy exemptions, i.e. the maximum asset value that individuals can legally protect from creditors under Chapter 7.

A higher exemption level provides additional wealth insurance to debtors, because it reduces the asset value that creditors can seize in bankruptcy. This wealth insurance can be particularly important for entrepreneurship, 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 increase the rate of firm creation. 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. In particular, there is evidence that in response to the moral hazard problems induced by the exemptions banks reduce credit availability to households (Gropp et al., 1997) and very young firms (Cerqueiro and Penas, 2017), making these affected firms more likely to fail. 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 an empirical question. In this paper we aim not only to provide an answer to that question, but also to pin down the exact mechanism 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 LBD is a restricted-access longitudinal database of business establishments and firms that provides annual employments for every private-sector, US 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 changes in state exemption levels, which during our sample period are frequent and large in magnitude. We address the concern that firm dynamics could also be correlated with other state-specific economic shocks in several ways. First, we compare the effects of exemptions on small and large firms. Large firms provide a good counterfactual because personal bankruptcy should affect only the small firms. Second, we also estimate specifications with state-year fixed effects, in which we identify the differential effect of exemptions on firm entry across industries with high and low entry barriers.[1] Third, we investigate the year-by-year response of our dependent variables to a change in exemptions to assess the plausibility of the common trend assumption. Fourth, we also assess the effect of the exemptions by comparing contiguous county pairs located on opposite sides of a state border. This alternative methodology directly addresses the concern that unobserved time-varying local economic conditions might correlate with the exemptions and thereby bias our results.[2]

[1] 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). An industry with low entry barriers is one with below-median startup capital needs.

[2] We use County Business Patterns data to estimate these county-level regressions.

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 creation 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 one to four employees). We find no such effects for large firms (with at least 5 employees). The estimated effect on job destruction we obtain for the group of small firms is particularly relevant. Our estimates indicate that raising exemptions by 50% (the average change in our sample period) increases their rate of job destruction by 0.18 percentage points (or 7.5% of its unconditional sample mean). Given that the median number of employees (across all states and years) is 1.5 million, this estimate indicates 2,700 additional job losses resulting from small firm closures.

Third, we investigate the mechanism behind the increase in firm deaths and the resultant job losses we documented for small firms. We start by analyzing how these effects vary across different age bins. We find that the increase in firm closures and job losses is concentrated among the group of youngest firms (with no more than 5 years). We find no significant effect for the oldest firms (with 15 or more years). We argue that such patterns are consistent with two possible mechanisms. The first is that the exemptions reduce access to credit to small businesses, 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 (churning entry). We obtain additional evidence that points to the former mechanism.

In particular, we find that firms created before or after an increase in exemptions are equally likely to fail and extinguish jobs. This result is not consistent with a decline in the average quality of firms that are created after an increase exemptions. What seems to matter is that the affected firms are small and young. Our results therefore point to the tightening of credit constraints as the likely explanation for our results.

Our study adds to a growing literature that analyzes how debtor protection affects entrepreneurship. 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. Closest to our study is Ersahin, Irani, and Waldock (2017), who also use Census data to investigate the effect of fraudulent transfer laws on entrepreneurship. They find that strengthening creditor protection leads to a decline in entrepreneurial activity. We complement their work by examining an alternative form of asset protection. In particular, while in a fraudulent transfer the debtor transfers property to another party in order to place it out of the reach of a creditor, personal bankruptcy allows indebted individuals to discharge their unsecured their debts and shelter from creditors assets up to the predetermined exemption value. The fact that our results are qualitatively contrary to theirs points to important differences in the way the relevant mechanisms (i.e., wealth insurance and credit supply) operate across the two settings.

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).

The paper proceeds as follows. In Section 2 we explain the institutional framework. 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 – 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 have 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. 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.

3. Data and variables

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.[3] The LBD is a restricted-access longitudinal database of business establishments and firms that provides annual employments for every private-sector, US establishment with at least one employee. The LBD also contains information on the industry, physical location, and establishment age. The BDS data provide

[3] In robustness tests we also use county-level firm and employment counts from the County Business Patterns (CBP).

statistics compiled from the LBD.[4] 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 descriptive statistics for our sample for the period 1994-2013. Data are at the state-year level. *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 deaths* is the count of firms that have exited in their entirety during the last 12 months, while *Firm death 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 deaths, while *Job destruction* rate is the same variable expressed as a percentage of total employment in the previous year.

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 details). Second, we control for changes in house prices using the S&P Case Shiller Index. Third, we obtain from the Census Bureau the state median income to control for economic conditions.

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

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:

$$y_{s,t} = \alpha_s + \alpha_t + \beta Exemption_{s,t} + \delta Controls_{s,t} + \varepsilon_{s,t}, \qquad (1)$$

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 ε is an error term. α_s and α_t are vectors of state and year fixed effects, respectively. State fixed effects control for fixed differences in entry across states, due to factors such as state economic size. The year effects control for aggregate economic shocks. We cluster standard errors at the state level to address the serial correlation concerns in Bertrand et al. (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 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.

Identification in the above regression model relies on changes in states exemption levels having a causal impact on our measures of firm dynamics. Our empirical methodology builds on two key assumptions. The first is that changes in firm dynamics are due to changes in exemptions (and not to other state-level economic shocks). The second is the parallel trends assumption.

We address these concerns in several ways. First, we estimate Equation 1 separately for small and large firms. Small firms have 1-4 employees, while large firms have at least 5 employees. Personal bankruptcy law should affect mainly the group of small firms, because it allows individuals to protect their assets from creditors. Since changes in economic conditions are likely to affect all firms, the group of large firms offers a good counterfactual. By comparing the effects of exemptions on small and large firms, we thus address the concern that changes in exemptions may be correlated with other state-specific economic trends.

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

$$y_{s,i,t} = \alpha_{s,i} + \alpha_i \times t + \alpha_{s,t} + \beta Exemptions_{s,t} \times EntBarrier_i + \varepsilon_{s,i,t}. \tag{2}$$

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 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.

Fourth, we compare contiguous county-pairs across state borders as in Huang (2008), Heider and Ljungqvist (2013), and Severino and Brown (2017). This alternative empirical methodology addresses concerns of potential bias due to unobserved time-varying local economic conditions that might correlate with exemption changes. In this analysis we assess the effect of exemptions on firm and employment counts rather than on entry and exit, since county-level data are available in the CBP but not in the BDS.

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 log of firm births (columns 1 to 3) and the log of jobs created by new firms (columns 4 to 6).[5] We analyze the effects of exemptions on total firm entry and total job creation (in columns 1 and 4, respectively). We also compare the effects of exemptions on small firms (in columns 2 and 5) and large firms (in columns 3 and 6). Small firms have 1-4 employees, while large firms have more than 4 employees. Personal bankruptcy law should affect mainly the creation of small businesses. In contrast, economic conditions are likely to affect all firms. By comparing the effects of exemptions on small and large firms, we thus address the concern that changes in exemptions may be correlated with other state-specific economic trends. We include in all specifications state fixed effects, year fixed effects, and control for

[5] We obtain similar results if we use as dependent variables the number of firms born as a proportion of existing firms and the number of jobs created as a proportion of existing employment.

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 no effect on firm entry or job creation. All estimated effects are statistically insignificant and economically small. This conclusion holds both in aggregate as well as for the groups of small and large firms. Our finding thus contrasts with the cross-sectional evidence in Fan and White (2003), who show that business ownership rates increase with the level of exemptions. We also note that the control variables work as expected. Both firm and job creation increase with real estate prices and median income.

5.1.1. Firm entry and industry effects

We exploit within-state variation across industries to investigate how the 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 competitive 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 again 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 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.

Table 4 shows that the exemptions induce firm creation 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 cost 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 0.36% in industries with low (relative to high) startup costs. In terms of firm counts, this effect corresponds to the additional creation of 244 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 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.[6]

5.2. Exemptions, firm exit, and job destruction

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 fraction of existing firms that have exited in their entirety (columns 1 to 3) and the fraction of existing jobs that were lost due to firm deaths (columns 4 to 6). As before, we analyze the effect of exemptions on the entire population of firms (columns 1 and 4) and present sample split results for small firms (in columns 2 and 5) and large firms (in columns 3 and 6). Small firms have 1-4 employees, while large firms have more than 4 employees. We include in all specifications state fixed effects, year fixed effects, and control for the state's,

[6] 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 both small and large firms.

change in house price index, and log of median income. We cluster standard errors at the state level.

Columns 1 and 3 show that the exemptions lead to higher firm death and job destruction rates. Only the first effect is statistically significant. More interestingly, the subsequent columns show that the increases in firm exit and job destruction are entirely driven by the sample of small firms. The estimated effects in column 2 and 5 are not only highly statistically significant, but also economically relevant. For example, the point estimate in column 2 indicates that raising exemptions by 50% (the average change in our sample period) increases the exit rate of small firms by 0.16 percentage points (or 2% of the unconditional sample mean of this variable). With respect to the increase in job destruction rate, although the point estimate in column 5 is comparable in magnitude, the effect is economically even more important. The estimated increase in job destruction rate for a 50% increase in exemptions is 0.18 percentage points. This effect represents 7.5% of the unconditional sample mean of this variable. Given that the median number of employees (across all states and years) is 1.5 million, our estimate indicates 2,700 additional job losses resulting from small firm closures.

Regarding the state-level control variables, the results broadly confirm that firm exit is associated with deteriorating economic conditions, such as declining real estate prices. However, our estimates indicate that the evolution of house prices appears to matter mostly for the small firms. This finding is consistent with the importance of the collateral lending channel for small businesses.[7]

The fact that we find no effect of exemptions on the closure rate of large firms corroborates our empirical strategy, since personal bankruptcy law should not have a direct effect on those firms. If exemptions were picking some confounding economy-wide shocks

[7] See for example Adelino, Schoar and Severino (2015), Corradin and Popov (2015), Ersahin and Irani (2015), Kerr, Kerr and Nanda (2015), and Schmalz, Sraer and Thesmar (2017).

that also drive firm exit, we should also see higher exit rates among the large firms (recall that our definition of a large firm is one with more than five employees). In any case, we perform below three additional types of tests that help us to tighten further our identification of the effect of exemptions on firm exit. First, we attempt to identify the mechanism driving our results by exploiting variation in firm age and in their year of birth. Second, we carefully investigate the dynamic adjustment of those variables around the changes in exemptions to confirm a causal interpretation of our results. Third, we estimate an alternative empirical model that relies on comparing contiguous county pairs located on opposite sides of a state border.

5.2.1. Credit constraints or pool effect? Age and cohort analysis

Distinct mechanisms can explain the increase in failure rates. One potential explanation is that the exemptions reduce access to credit to small businesses, making them more likely to fail (Cerqueiro, and Penas, 2017). Another possibility is that the exemptions attract a worse pool of entrepreneurs, which are quickly driven out of business (churning entry). One common implication of these two mechanisms is that failure rates should be disproportionally higher for younger firms than for older firms following an increase in exemptions.

This is precisely what we test in Table 6. Data are from the Census BDS for the period 1994-2013 and include only small firms (which have 1-4 employees). The dependent variables are the exit rate of small firms (in columns 1 to 3) and the job destruction rate of small firms (in columns 4 to 6).[8] We assess the effect of exemptions for three different age groups (1 to 5 years, 6 to 15 years, and more than 15 years old). The estimated regressions are otherwise similar to those in Table 5: they all include state fixed effects, year fixed

[8] The denominator in both variables is the total number of firms in the same state and in the previous year.

effects, and the same set of control variables (that we omit for brevity). 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 death and job destruction rates. Moving from the first to the second age group (in columns 2 and 5) halves the estimated coefficients, which nevertheless remain statistically significant. For the oldest group of firms (in columns 3 and 6) we find no effect of exemptions on their exit rates.

Our results confirm that younger firms drive the increase in firm exit and job loss rates we documented. Consequently, the two mechanisms discussed above so far offer plausible explanations for our results. On the one hand, younger – and thus more opaque – firms could suffer a stronger reduction in credit availability as exemptions increase. On the other hand, these younger firms could be of lower quality if the exemptions attract a worse pool of entrepreneurs. To disentangle between these two explanations, we next use variation across firm cohorts.

In particular, we collect from the Census BDS data at the state-cohort-year level for the same period of analysis. We identify for each state the first year (if any) in which exemptions were raised. Then, we create an indicator variable (*PostLaw*) that equals one if a given firm cohort was created after an exemption law, and zero otherwise.[9] If higher exemption levels attract lower quality entrepreneurs, 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, where we interact the exemptions variable with the *PostLaw* indicator. We note that all specifications shown include state fixed effects, year fixed effects,

[9] Take for example the increase in exemptions in Colorado in the year 2000. In a given year (say, 2003) there groups of firms belonging to different cohorts. In this case the variable *PostLaw* equals one for firms that were born in 2001, 2002, and 2003. The variable equals zero for all earlier cohorts.

cohort fixed effects, and the same set of control variables. We cluster standard errors at the state level.

The results in Table 7 speak against the adverse pool story. Firms created before or after an increase in exemptions are equally likely to fail and extinguish jobs. What seems to matter is that the affected firms are small and young. Our results therefore point to the tightening of credit constraints as the likely explanation for our results.

5.2.2. Firm exit dynamics

In this section we 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 to 5 (top panel) and firms with 6 or more years (bottom panel).

The Figure plots point estimates of coefficients (with and 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 to 3 of Table 6. For states that raise their exemption limit more than once, we plot dynamics for the first 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 only after three years and the effect persists with time.

5.3. Comparing contiguous counties along state borders

In Table 8 we assess the effect of the exemptions by comparing contiguous county pairs located on opposite sides of a state border. We use county-level data on establishment and employment counts from the CBP, because the BDS provides data aggregated at the state level. The corresponding dependent variables are the log of the number of firms (in columns 1 to 3) and the log of employment (in columns 4 to 6) in a given county and year. Our estimation sample contains 1308 county-pairs for the time period 1998-2003. We note that all contiguous counties located in the same state are dropped from our sample because exemptions vary only across states. All regressions shown include the same set of control variables. We cluster standard errors at the state level.

We estimate three specifications. The first one contains year fixed effects. The second one adds county-pair fixed effects, which uses only the variation in exemptions within each contiguous border county pair. The third specification contains county-pair×year fixed effects. While in the first specification all counties that do not pass exemptions in a given year are in the control group, the second and third specifications reduce the control group to contiguous border counties. Moreover, the third specification further controls for any time-varying local shocks affecting any given county-pair. Consequently, the last specification should better control for local economic conditions.

The results in Table 8 show that exemptions reduce the number of both operating firms (in columns 1 to 3) and available jobs (in columns 4 to 6). The point estimate in column 3 indicates that raising exemptions by 50% (the average change in our sample period) increases the number of firms by 1.25%. With respect to employment, although the point estimate in column 6 is only marginally significant, the effect is also economically important. The estimated decrease in employment for a 50% increase in exemptions is almost 1 percent.

Besides corroborating our empirical strategy, these results also confirm our earlier finding of exemptions having a negative effect on economic activity both in terms of firms and jobs.

6. Conclusion

We study the effect of debtor protection on firm entry and exit dynamics. We find that more lenient personal bankruptcy increase firm entry only in sectors with low entry barriers. We also find that debtor protection increases small firm exit rates and job destruction, and that this effect is stronger for very young firms. This negative effect takes three years to materialize and is persistent in time. Finally, we find that there is no difference between exit rates of firms born before and after the change in debtor protection. Our results overall indicate that the main mechanism affecting firm dynamics is a reduction in credit supply to the smallest and youngest firms.

References

Adelino, M., A. Schoar, and F. Severino. 2015. “House Prices, collateral and self-employment,” *Journal of Financial Economics*, 117, 288-306.

Armour, J., and D. Cumming. 2008. “Bankruptcy law and entrepreneurship,” *American Law and Economics Review*, 10, 303-350.

Berger, A., and L. Black. 2011. “Bank size, lending technologies, and small business finance,” *Journal of Banking and Finance*, 35, 724-773.

Berger, A., G. Cerqueiro, and F. Penas. 2011. “Does debtor protection really protect debtors? Evidence from the small business credit market,” *Journal of Banking and Finance*, 35, 1843-1857.

Berger, A., N. Miller, M. Petersen, R. Rajan, and J. Stein. 2005. “Does function follow organizational form? Evidence from the lending practices of large and small banks,” *Journal of Financial Economics*, 76, 237–269.

Berger, A., R. Rosen, and G. Udell. 2007. “Does market size structure affect competition? The case of small business lending,” *Journal of Banking and Finance*, 31, 11–33.

Berkowitz, J., and M. White. 2004. “Bankruptcy and small firms’ access to credit,” *Rand Journal of Economics*, 35, 69-84.

Bertrand, M., E. Duflo, and S. Mullainathan, 2004. “How Much Should We Trust Differences-in-Differences Estimates?,” *Quarterly Journal of Economics*, 119, 249-275.

Cerqueiro, G., D. Hegde, R. Seamans, and F. Penas. Forthcoming. “Debtor Rights, Credit Supply, and Innovation,” *Management Science*.

Cerqueiro, G., and F. Penas. 2017. “How does personal bankruptcy law affect start-ups? ” *Review of Financial Studies*, 30, 7, 2523-2554.

Cetorelli, N., and P. Strahan. 2006. “Finance as a barrier to entry: Bank competition and industry structure in U.S. local markets,” *Journal of Finance*, 61, 437–461.

Corradin, S., and A. Popov. 2015. “House prices, home equity borrowing, and entrepreneurship,” *Review of Financial Studies, 28*, 2399-2428.

Davis, S., J. Haltiwanger, R. Jarmin, and J. Miranda. 2006. “Volatility and dispersion in business growth rates: publicly traded versus privately held firms,” *NBER Working Papers* #12354.

Davis, S., J. Haltiwanger, and S. Schuh. 1996. “Job Creation and Destruction,” MIT Press, Cambridge, MA.

Dunne, T., M. Roberts, and L. Samuelson. 1989. “Patterns of firm entry and exit in US manufacturing industries,” *RAND Journal of Economics* 19, 495–515.

Ersahin, N., and R. Irani. 2015. “Collateral values and corporate employment,” Working paper.

Ersahin, N., R. Irani, and K. Waldock. 2017. “Creditor rights and entrepreneurship: Evidence from Fraudulent Transfer Law,” Working paper.

Fan, W., and M. White. 2003. “Personal bankruptcy and the level of entrepreneurial activity,” *Journal of Law and Economics*, 46, 543-68.

Fay, S., E. Hurst, and M. White. 2002. “The household bankruptcy decision,” *American Economic Review*, 92, 706-718.

Filer, L., and J. Fisher. 2005. “The consumption effects associated with filing for personal bankruptcy,” *Southern Economic Journal*, 71, 837-854.

Glaeser, E., and W. Kerr. 2009. “Local industrial conditions and entrepreneurship: how much of the spatial distribution can we explain?”*Journal of Economics and Management Strategy*, 18, 623-663.

Grant, C. 2010. “Evidence on the insurance effect of bankruptcy exemptions,” *Journal of Banking & Finance*, 34, 2247–2254.

Gropp, R., J. Scholz, and M. White. 1997. “Personal bankruptcy and credit supply and demand,” *Quarterly Journal of Economics*, 112, 217-251.

Haltiwanger, J., R. Jarmin, and J. Miranda. 2010. “Who creates jobs? Small vs. large vs. young”, *NBER working paper No. 16300.*

Han, S., and G. Li. 2011. “Household borrowing after personal bankruptcy,” *Journal of Money, Credit and Banking*, 43, 491–517.

Heider, F., and A. Ljungqvist. 2015. “As certain as debt and taxes: Estimating the tax sensitivity of leverage from state tax changes,” *Journal of Financial Economics*, 118, 684-712.

Huang, R. 2008. ‘‘Evaluating the real effect of branch banking deregulation: Comparing contiguous counties across U.S. state borders,’’ *Journal of Financial Economics*, 87, 678-705.

Jarmin, R., and J. Miranda. 2002. “The longitudinal business database,” *Center for Economic Studies Discussion Paper CES-WP-02-17*.

Kerr, W., and R. Nanda. 2009. "Democratizing entry: Banking deregulations, financing constraints, and entrepreneurship," *Journal of Financial Economics*, 94, 124-149.

Kerr, S., W. Kerr, and R. Nanda. 2015. "House money and entrepreneurship," *NBER working paper*, No. 21458.

Kihlstrom, R., and J.-J. Laffont. 1979. “A general equilibrium entrepreneurial theory of firm formation based on risk aversion,” *Journal of Political Economy*, 87, 719-748.

Mathur, A. (2009) “Spatial model of the impact of bankruptcy law on entrepreneurship.” *Spatial Economic Analysis*, 4.

Rice, T., and P. Strahan. 2010. “Does credit competition affect small-firm finance?” *Journal of Finance*, 65, 861–889.

Severino, F., and M. Brown. 2017. “Personal bankruptcy protection and household debt,” Working paper.

Schmalz, M., D. Sraer, and D. Thesmar. 2017. “Housing collateral and entrepreneurship,” *Journal of Finance*, 72, 99-132.

Shane, S. 2008. “The illusions of entrepreneurship, the costly myths that entrepreneurs, investors, and policy makers live by”, New Haven, Yale University Press.

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.

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
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

State	State exemptions ($)		Years exemptions changed
	1994	2013	
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

Data are at the state-year level from the Census BDS for the period 1994-2013.

Variable	Mean	Standard deviation	Min	Max
Firm entry and job creation				
Firm births (thous.)	14.12	16.29	1.38	104.93
Firm birth rate (in %)	11.12	2.12	6.61	19.93
Job creation (thous.)	122.58	145.32	7.66	1,015.75
Job creation rate (in %)	5.61	1.31	2.92	9.85
Firm deaths and job destruction				
Firm deaths (thous.)	8.08	9.41	0.765	62.30
Firm death rate (in %)	7.81	1.07	5.02	12.60
Job destruction (thous.)	51.88	62.86	3.692	415.53
Job destruction rate (in %)	2.36	0.46	1.13	5.99
Other state-level variables				
Exemptions (thous. dollars)	225.79	339.44	5.00	1,000.00
Change in House Price Index	0.04	0.06	-0.21	0.33
Median income (thous. dollars)	44.11	9.20	23.56	71.84

Table 3 – Exemptions, firm entry and job creation

Data are at the state-year level from the Census BDS for the period 1994-2013. The dependent variables are log-transformed. Small firms have 1-4 employees, while large firms have at least employees. 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 births			Jobs created by entrants		
Firm size:	All	Small	Large	All	Small	Large
	(1)	(2)	(3)	(4)	(5)	(6)
Log(Exemptions)	0.00897	0.00599	0.00770	0.00439	0.00982	0.00334
	[0.0111]	[0.0141]	[0.0113]	[0.0113]	[0.0122]	[0.0120]
Change in House Price Index	0.155**	0.166**	0.134*	0.277***	0.230***	0.259***
	[0.0740]	[0.0802]	[0.0754]	[0.0838]	[0.0769]	[0.0848]
Log(Median income)	0.192**	0.153	0.242**	0.278**	0.162*	0.260**
	[0.0929]	[0.101]	[0.102]	[0.106]	[0.0935]	[0.115]
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	969	969	969	969	969	969
R-squared	0.997	0.995	0.996	0.993	0.996	0.991

Table 4 – Exemptions and firm entry: within-state analysis

Data are at the state-year-industry level from the Census LBD for the period 1994-2013. The dependent variable is log-transformed. Industries with high entry barriers have above-median capital needs to set up a new firm. Standard errors are clustered at the county 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 digit to comply with Census disclosure requirements.

	(1)
Dependent variable:	Firm births
Log(Exemptions) × Low Barrier	0.0071**
	[0.0029]
State-year fixed effects	Yes
State-industry fixed effects	Yes
Industry linear time trends	Yes
Observations	22,000
R-squared	0.985

Table 5 – Exemptions and firm exit

Data are at the state-year level from the Census BDS for the period 1994-2013. Small firms have 1-4 employees, while large firms have at least 5 employees. 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 death rate (%)			Job destruction rate (%)		
Firm size:	All	Small	Large	All	Small	Large
	(1)	(2)	(3)	(4)	(5)	(6)
Log(Exemptions)	0.00178***	0.321***	0.00236	0.000233	0.357***	-0.00597
	[0.000578]	[0.0980]	[0.0128]	[0.000189]	[0.108]	[0.0140]
Change in House Price Index	-0.0466***	-5.571***	-0.505**	-0.0104***	-6.134***	-0.370**
	[0.00635]	[1.048]	[0.195]	[0.00244]	[1.456]	[0.171]
Log(Median income)	-0.00759**	-0.0865	-0.196	-0.00559***	0.419	-0.363***
	[0.00374]	[0.678]	[0.133]	[0.00184]	[1.017]	[0.119]
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	969	969	969	969	969	969
R-squared	0.908	0.912	0.909	0.862	0.902	0.858

Table 6 – Exemptions and small firm exit: The role of firm age

Data are at the state-year level from the Census BDS for the period 1994-2013. The sample includes only small firms (with 1-4 employees). State controls include the change in the house price index and the log of the state median income. 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:	Small firm death rate (%)			Small firm 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)	0.454**	0.229***	0.0695	0.490**	0.264**	0.0462
	[0.183]	[0.0845]	[0.0995]	[0.206]	[0.104]	[0.135]
State controls	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	969	969	969	969	969	969
R-squared	0.865	0.770	0.786	0.845	0.752	0.614

Table 7 – Exemptions and small firm exit by cohort

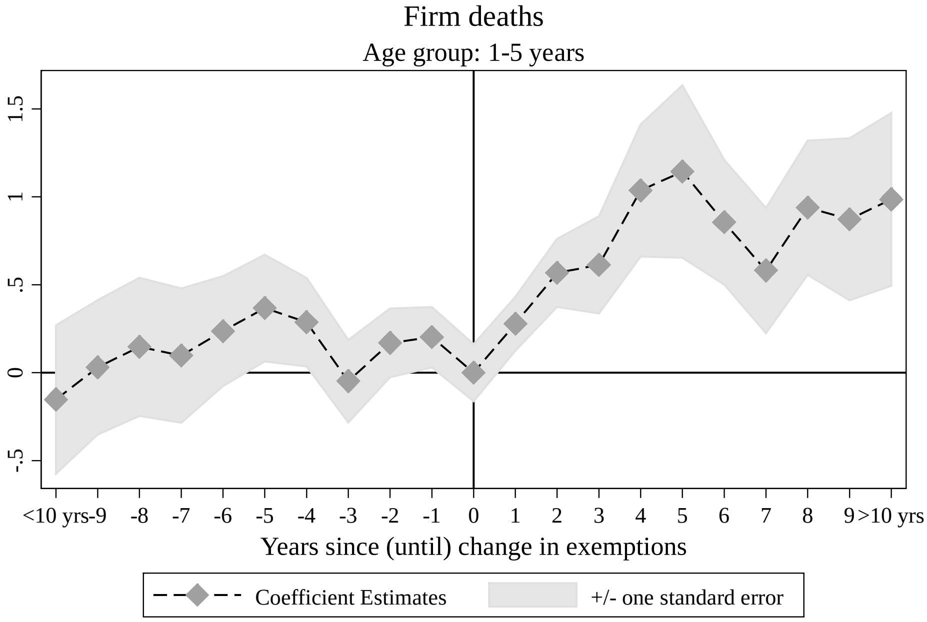
Data are at the state-cohort-year level from the Census BDS for the period 1994-2013. The sample includes only small firms (with 1-4 employees). State controls include the change in the house price index and the log of the state median income. The variable *PostLaw* equals one if the cohort was created after a change in exemptions, and zero otherwise. For states that changed exemptions multiple times, we consider the first 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 death rate (%)		Job destruction rate (%)	
	(1)	(2)	(5)	(6)
Log(Exemptions)	0.503***	0.464**	0.541***	0.478**
	[0.177]	[0.194]	[0.192]	[0.211]
Log(Exemptions) × PostLaw		0.0560		0.156
		[0.236]		[0.253]
PostLaw		-0.539		-1.752
		[2.669]		[2.847]
State controls	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes
Cohort fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Observations	4,845	4,845	4,845	4,845
R-squared	0.775	0.775	0.766	0.766

Table 8 – Using contiguous border counties as controls

Data are the county-pair-year level from the Census CBP for the period 1994-2013. The sample contains 1,308 county-pais. State controls include the change in the house price index and the log of the state median income. 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(Number of firms)			Log(Number of jobs)		
	(1)	(2)	(3)	(4)	(5)	(6)
Log(Exemptions)	-0.128***	-0.0268***	-0.0309***	-0.175***	-0.0154*	-0.0203*
	[0.0148]	[0.00622]	[0.00886]	[0.0183]	[0.00866]	[0.0122]
State controls	Yes	Yes	Yes	Yes	Yes	Yes
County-pair fixed effects	No	Yes	No	No	Yes	No
Year fixed effects	Yes	Yes	No	Yes	Yes	No
County-pair×Year fixed effects	No	No	Yes	No	No	Yes
Observations	49,369	49,369	49,369	49,369	49,369	49,369
R-squared	0.071	0.751	0.753	0.052	0.708	0.732



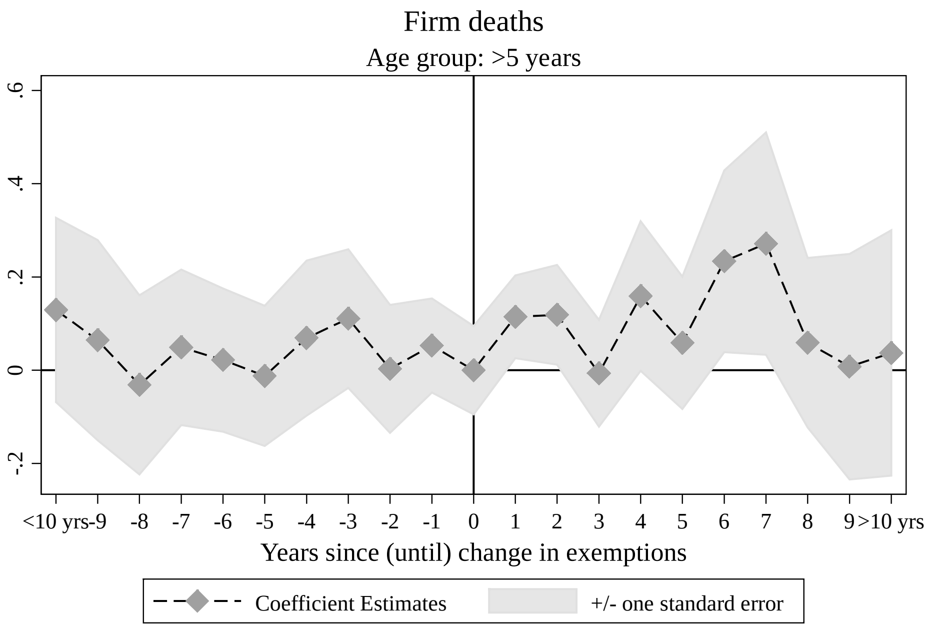


Figure 1 – Dynamic effect of the exemption laws on small firm exits

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 first change.