Personal Bankruptcy Law and Entrepreneurship

by

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Abstract

We study the effect of debtor protection on firm entry and exit dynamics. We find that more lenient personal bankruptcy laws lead to higher firm entry, especially in sectors with low entry barriers. We also find that debtor protection increases firm exit rates and that this effect is independent of firm age. Our results overall indicate that changes in debtor protection affect firm dynamics.

Keyword*:* Debtor Protection, Personal Bankruptcy, Entrepreneurship

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1. Introduction

This paper investigates how statewide changes in debtor protection provided by U.S. personal bankruptcy law affect firm entry and exit dynamics. We assess the effects of personal bankruptcy law on entrepreneurship rates, the size and industry distribution of incumbent firms, and on business closures. We thus aim to paint a complete picture of how personal bankruptcy law shapes the market structure across all industries.

Our analysis uses state-level bankruptcy homestead and personal exemptions as a measure of debtor protection. An exemption limit is 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. Our analysis exploits the passage of multiple state laws since the beginning of the 1990s that increased exemption levels. While personal bankruptcy law is designed for consumers, it also affects small businesses. On the one hand, it affects unlimited liability firms, as the firm owners are legally liable for the firm's debts. One the other hand, it can also affect small limited liability firms, since lenders often require the owners of these firms to personally guarantee their firms' loans and since the firm owners use funds borrowed at the personal level to finance the firm.

A growing literature analyzes how the exemptions affect the credit market. On the supply side, banks could reduce credit availability in response to the moral hazard problems induced by the exemptions (Fay et al., 2002). There is evidence that banks anticipate moral hazard and opportunistic behavior from borrowers in high exemption states by reducing credit availability to households (Gropp, Scholz, and White, 1997), and to small and medium enterprises (Berkowitz and White, 2004; Berger, Cerqueiro, and Penas, 2011). In a recent study, Cerqueiro and Penas (2017) use the Kauffman Firm Survey to investigate the effect of statewide changes in exemptions on a representative sample of US start-ups. They find that

firms owned by entrepreneurs whose assets become fully protected in bankruptcy obtain less bank credit, hire fewer employees and, become more likely to fail. Cerqueiro, Hegde, Penas, and Seamans (2017) find that personal bankruptcy regimes that provide stronger debtor protection decrease the number and the quality of patents produced by small firms and that these effects are amplified in industries with a high dependence on external financing.

On the demand side, the wealth insurance provided by exemptions may induce risk-averse borrowers to increase their demand for personal credit (Gropp, Scholz, and White, 1997; Cerqueiro and Penas, 2017). Related to this demand-side effect of the exemptions, two studies analyze how debtor protection affects entrepreneurship. In their theoretical model, Fan and White (2003) show that high debtor protection levels provide partial wealth insurance, which makes potential entrepreneurs who are risk averse more likely to choose self-employment (see also Kihlstrom and Laffont, 1979). Fan and White (2003) use the Survey of Income and Program Participation to test this prediction, and find that the probability of homeowners owning businesses is 35 percent higher if they live in states with unlimited rather than low exemptions. 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. Our paper relates more closely to the stream of literature that analyzes how debtor protection affects entrepreneurship. In particular, we study how the exemptions affect firm entry and exit dynamics. Besides focusing on rates of firm creation and business closures, we propose to investigate how the exemptions affect the size and industry distribution of incumbent firms. Therefore 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).

Our study employs several databases. We hand-collect the bankruptcy exemptions from individual state codes for the period 1994-2013. Our empirical strategy exploits changes in state exemption levels, which have been more frequent and larger in magnitude since the end of the 1990s. We combine the exemptions with the Longitudinal Business Database (LBD), which provides annual employments for every private-sector US establishment with payroll. We supplement these data with state-level control variables (house prices, median income, and labor force).

Our research design allows us to address several previously unexplored issues related to debtor protection. In particular, the data allows us to study how debtor protection affects entry, firm size and industry distribution, and firm exit. Existing studies have typically relied on cross sectional data; cross-sectional results are subject to the criticism that the measures of debtor protection may be correlated with other omitted state or country-level characteristics. A few more recent studies have relied on the Kauffman Firm Survey (KFS) panel data. However, the panel structure of the KFS focuses on a single cohort of businesses and therefore does not allow for a study of entry dynamics.

We provide three main findings. First, we find that the exemptions lead to higher firm entry. In order to address the potential concern that the exemptions might be correlated with other economic shocks, we compare the entry of new start-up firms relative to the creation of new establishments by existing companies in a given state and year (as in Kerr and Nanda, 2009). We find that the exemptions have a disproportionally larger effect on firm entry by inducing individuals to create new businesses.

Second, we find that the exemptions induce firm creation especially in industries with low entry barriers. We find, however, no effect of the exemptions on firm entry size (measured by the number of employees).

Third, we find that the exemptions increase firm exit rates. We argue that this could result from the increase in competition that results from higher entry. An alternative explanation is that the supply effect dominates, reducing credit availability for small firms, and making them more likely to fail. The fact that we find no difference in firm failure rates across firms of different ages lends support to the former mechanism.

The paper proceeds as follows. In the next section we explain the institutional framework. In Section 3 we describe the data we use. In Section 4 we explain the key hypotheses we test, as well as the proposed identification strategies. 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 (1994-2013), 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

Our main dataset is the Longitudinal Business Database (LBD), which is provided by the U.S. Census Bureau. The LBD 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. We use LBD data from 1994 to 2013.

We supplement the LBD with several 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). We also analyze homestead exemptions separately. Second, we control for changes in house prices using the S&P Case Shiller Index. Third, we collect from the Bureau of Labor Statistics the number of working individuals in each state to control for the economic size of the state. Fourth, we obtain from the Census Bureau the state median income to control for economic conditions.

Table 2 provides descriptive statistics for our sample.

4. Hypotheses and empirical methodology

4.1. Exemptions and firm entry

The wealth insurance provided by exemptions may induce more risk-averse borrowers to become self-employed (Kihlstrom and Laffont, 1979; Fan and White, 2003; Armour and Cumming, 2008). At the same time, banks could reduce credit availability to individuals in response to the moral hazard problems induced by the exemptions (Gropp, Scholz, and White, 1997; Cerqueiro and Penas (2017)). If the former wealth insurance effect dominates the latter credit supply, higher exemptions should positively affect the creation of new businesses. Accordingly, our first hypothesis is:

(H1) Higher exemptions lead to higher firm entry.

To test H1, we analyze whether changes in state exemption levels increased entry during the same sample period, using the following panel regression:

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

where *s* indexes state of location, *t* indexes time, *Entry* is the number of entering establishments (in logs), *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 changes in entry. We cluster standard errors at the state level to address the serial correlation concerns in Bertrand et al. (2004).

Identification in the above regression model relies on changes in states exemption levels having a causal impact on entry rates. 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.

However, the above specification is subject to the criticism that higher entry could be due to a state-level economic boom (and not necessarily to the increase in exemptions). To address this criticism, we propose also to compare the entry of new start-up firms with the

creation of new establishments by existing firms using industry-level data (as in Kerr and Nanda, 2009).[1] The regression model we estimate is:

$$Entry_{s,i,x,t} = \alpha_{s,i,x} + \alpha_x \times t + \alpha_{s,i,t} + \beta Exemptions_{s,t} \times Multi\text{-}unit_x + \varepsilon_{s,i,x,t}, \quad (2)$$

Multi-unit denotes whether entry is a new establishment from an existing firm or a new firm, and *i* denotes two-digit industry sectors. This econometric specification includes state-industry-entry type fixed effects ($\alpha_{s,i,x}$), accounts for differential trends across entrant type ($\alpha_x \times t$), and includes state-industry-year fixed effects ($\alpha_{s,i,t}$). Since the exemptions vary at the state and year levels, we can only identify its interaction with entrant type. Therefore the coefficient of interest β measures the effect of a change in state exemptions on new firm entry relative to the response of facility expansions.

We expect that an increase in exemptions will affect entry by very small firms (the ones that may rely on personal loans for financing). If the average effect on entry is positive, we expect this positive effect to be larger for the smallest firms (single units) than for the larger firms (multi unit). Moreover, by assessing the differential effect of exemptions across different types of entry, we can better control for any changes in the state economic environment that should affect all firms.

4.2. Exemptions, industry effects, and entrant size

Evidence from other regulatory reforms (e.g., Cetorelli and Strahan, 2006; Kerr and Nanda, 2009) indicates that these reforms may affect economic activity through persistent effects on the firm-size distribution. For instance, Cetorelli and Strahan (2006) show that

[1] This is the same approach used by Kerr and Nanda (2009) who use the LBD to study the effect of bank branching deregulation on new firm entry and exit.

deregulation in the U.S bank industry led to an increase in the number of establishments in operation, reduced average firm size, and increased the share of small establishments.

To the extent that exemptions should induce more entrepreneurs to create businesses, it is then important to understand whether these higher entry rates have persistent effects on economic activity. Do these higher entry rates translate into increased competition and development due to Schumpeterian creative destruction? Or do these higher entry rates reflect a massive increase in churning, whereby the marginal entrants firms fail shortly after founding and fail to impact the existing market structure? Moreover, do these new entrants distribute uniformly across all economic sectors? Or do they tend to concentrate in competitive industries with lower barriers to entry and high rates of failure?

With these questions in mind, we formulate two hypotheses that focus on separate mechanisms through which exemptions could affect firm market structure. We build on the existing evidence (Shane, 2008) that regulatory changes that lower barriers to entry typically stimulate the creation of smaller enterprises distributed disproportionately in competitive industries with high rates of failure. Moreover, a reduction in credit availability can also force entrepreneurs to operate smaller firms (Cerqueiro and Penas, 2017). Consequently we hypothesize that:

(H2) Higher exemptions reduce average firm entry size, and

(H3) Higher exemptions lead to disproportionate firm creation in highly competitive industries.

To test H2 we propose to estimate panel regressions of the form:

$$y_{s,t} = \alpha_s + \alpha_t + \beta Exemption_{s,t} + \delta Controls_{s,t} + \varepsilon_{s,t}, \quad (3)$$

where in separate regressions *y* equals average firm entry size (we use the number of employees to measure firm size).

To test H3 we propose to estimate the following panel regression:

$$Entry_{s,i,t} = \alpha_{s,i} + \alpha_i \times t + \alpha_{s,t} + \beta Exemptions_{s,t} \times EntBarrier_i + \varepsilon_{s,i,t}. \quad (4)$$

EntBarrier denotes whether the industry has high or low entry barriers. Equation 4 includes state-industry type fixed effects ($\alpha_{s,i}$), state-year fixed effects ($\alpha_{s,t}$), and accounts for differential trends across industry types ($\alpha_i \times t$). Consequently, Equation 4 allows us to identify only the differential effect of exemptions on entry across industries with high and low entry barriers. We note that the identification strategy used in Equation 4 is similar to that used, for instance, in Cetorelli and Strahan (2006).

4.3. Exemptions and exit rates

We argued above that higher exemptions could increase rates of firm creation if the wealth insurance effect dominates the negative credit supply channel. We argue that each of these channels can also have an independent effect on firm exit. On the one hand, higher firm entry can lead to higher firm exit via the increase in competition (in the Schumpeterian sense). In this case we should an increase in the exit rates of both entrants and incumbents. On the other hand, the reduction in credit availability is likely to be greater for smaller and younger, making them more likely to fail.

We analyze firm exit in two steps. First we test the hypothesis:

(H4) Higher exemptions lead to higher failure rates.

In order to study how the exemptions affect firm exit, we propose to estimate a panel regression similar to Equation 1, but where the dependent variable *Exit* is the number of exiting establishments (in logs):

$$Exit_{s,t} = \alpha_s + \alpha_t + \beta Exemption_{s,t} + \delta Controls_{s,t} + \varepsilon_{s,t}. \quad (5)$$

Second, we test whether exemptions affect firm exit via a competition effect that affects all firms or by a reduction in credit availability that affects mostly younger firms. We propose to do so by comparing exit rates of firms across different age buckets:

$$Exit_{s,i,k,t} = \alpha_{s,i,t} + \beta_k Exemptions_{s,t} + \varepsilon_{s,i,k,t}, \text{ for each of } k \text{ age buckets} \quad (6)$$

where *k* denotes age bucket (e.g.: 1-2 years old, 3-5 years old, etc.).

The identification behind this model is the following. An increase in state exemptions should increase competition for all firms in the state. As a result, changes in exit rates driven by an increase in competition should be differenced out when we compare young incumbent firms (e.g., 3-5 years old) with the new entrants (e.g., 1-2 years old). In this case the estimated coefficients β should be similar across all age bins. In contrast, a reduction in credit availability is more likely to affect younger firms, which tend to be smaller and more opaque. In this case, the estimated coefficient β should be significantly larger for small *k*.

5. Results

5.1. Exemptions and firm entry

In Table 3 we study the effect of exemptions on firm entry at the state-year level for the period 1994-2013. The two dependent variables are the log of establishment counts (columns 1 and 2) and the log of establishment births (columns 3 and 4). We include in all specifications

state fixed effects, year fixed effects, and control for the state's labor force, median income, and house price index. We cluster standard errors at the state level.

The results confirm our first hypothesis that the exemptions are associated with higher entry rates, expressed both in terms of establishment counts and births. The estimated coefficients are significant and economically relevant. For instance, the point estimate in column 4 indicates that doubling the exemption level increases the rate of establishment birth by 1.3%.[2]

One potential concern with this empirical strategy is that the exemptions might be contemporaneous with other state-level shocks that are also correlated with firm entry. To address this criticism, 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 (as in Kerr and Nanda, 2009). The identifying assumption in this alternative model is that the exemptions should only affect the smaller firms (single unit) which are the ones more likely to rely on personal loans for financing.

The results in Table 4 corroborate this view. The analysis is at the state-year-industry-entry type level for the period 1994-2013. We saturate the regressions with state-industry-entry type fixed effects, differential trends across entrant type, and state-industry-year fixed effects. For this reason, we can only identify the interaction of the exemptions with entrant type. We cluster standard errors at the state level.

The interaction of the exemptions with an indicator of whether entry is made via the expansion of existing businesses relative to the creation of new ones is always negative. The estimated coefficients are statistically significant only for establishment counts. These results are important for at least two reasons. First, they confirm that the exemptions have a positive effect on firm entry by inducing individuals to create new businesses. Second, they directly

[2] The average exemption increase during our sample period is 85% of its starting value.

address the concern that the exemptions might be picking other statewide economic shocks and thus corroborate our empirical strategy.

5.2. Exemptions, industry effects, and entrant size

In Table 5 we study the effect of the exemptions on entrant size, which we measure with the number of firm employees (both in levels and in logs). Data are at the state-year level for the period 1994-2013. As before, we include in all specifications state fixed effects, year fixed effects, and control for the state's labor force, median income, and house price index. We cluster standard errors at the state level.

Although all estimated coefficients are positive, they are statistically insignificant in all specification except the first. These results do not support our hypothesis that the exemptions should reduce the average size of entrant firms. However, we must note that one important limitation of our dataset is that it includes only establishments with employees. To see why this matters, suppose the exemptions induce individuals to create small businesses that do not hire any workers. Our analysis would be unable to identify such a reduction in average startup size, since the LBD does not contain information on non-employer firms.

We next investigate how the new entrants are distributed across economic sectors. In particular, we hypothesize that the exemptions should induce entry especially in competitive industries with low barriers to entry and that experience high rates of failure. To test this hypothesis, we estimate regressions at the state-year-industry 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 6. The dependent variables are the log of establishment counts (in columns 1 and 2) and the log of establishment births (in columns 3 and 4). 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 variables with the type of industry (high versus low startup costs). We cluster standard errors at the state level.

The estimates displayed in Table 6 indicate that the exemptions induce firm creation especially in industries with low entry barriers. The estimated coefficients for the interactions of the exemptions with the dummy that indicates an industry with high entry cost are always negative. Moreover, the coefficients are statistically significant in specifications 1 and 3, which consider state changes only in homestead exemptions. We also note that since these regressions difference out any statewide changes in firm entry and thus mitigate the concern that the exemptions might be correlated with other statewide economic shocks.

5.3. Exemptions and exit rates

Our results indicate that the exemptions foster the creation of new firms and especially in easy-to-enter industries. Next, we investigate how the exemptions affect firm exit. Table 7 presents regression results in which the dependent variable is the number of firm closures (in logs) in a given state and year. The specifications include state fixed effects, year fixed effects, and the same set of control variables as before (labor force, median income, and house price index). We cluster standard errors at the state level.

Consistent with our hypothesis, we find that the exemptions lead to higher failure rates. The estimated effects are economically relevant and statistically significant. For example, the estimated coefficient in column 2 indicates that doubling the exemption level increases failure rates by 3%.

Distinct mechanisms can explain the increase in failure rates. One potential explanation is that the exemptions reduce the availability of credit to entrepreneurs, making them more likely to fail (Cerqueiro and Penas, 2017). One implication of this mechanism is that failure rates should be disproportionally higher for younger firms following an increase in

exemptions.[3] Another possibility is that the higher entry rates lead to more competition, and this naturally (that is, in a Schumpeterian sense) leads to more exits due to the increase in competition. In this case we would expect to see an increase in failure rates for firms of all ages. We attempt to disentangle these two potential mechanisms by analyzing whether exit rates depend on firm age. We present the results in Table 8. Specifically, we use data at the establishment-year level and assess the effect of exemptions on firm exit (in log) for different age groups (all firms, 1 or 2 year old firms, 3 to 5 year old firms, 6 to 10 year old firms, and 11 or more years old). For this analysis, we include state-year and state-industry fixed effects (as for Table 6), and also age category indicators (i.e., indicators for 1 year old, 2 year old, 3 years old, etc). In panel A we focus on the homestead exemptions, while in panel B we focus on all personal property exemptions.

All estimated coefficients are positive (albeit estimated imprecisely). More importantly, the coefficients do not appear to vary by firm age. Therefore our evidence suggests that the exemptions lead to higher firm exit via an increase in overall competition.

6. Conclusion

We study the effect of changes in state bankruptcy exemptions on firm entry and exit dynamics. We find that the exemptions lead to higher firm entry, especially in sectors with low entry barriers. We also compare the creation of new firms with the expansion of existing firms and find that the increase in firm entry is driven mainly by the creation of new businesses.

We also find that the exemptions increase firm exit rates. We argue that this could result from the increase in competition that results from higher entry or from a reduction in credit availability for small firms that makes them more likely to fail. The fact that we find no

[3] An alternative mechanism that would give the same prediction is that exemptions attract a worse pool of entrepreneurs, which are quickly driven out of business (churning entry).

difference in firm failure rates across firms of different ages lends support to the former mechanism.

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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. The number of observations is approximately 1,000. Numbers have been rounded to the closest 4 digit to comply with Census disclosure requirements.

Variable	Mean	Standard deviation
Dependent variables		
Births	15,880	19,850
Deaths	13,020	17,460
Counts	166,300	175,700
Employment	4.953	2.545
State Exemptions		
Homestead	213,500	342,100
Exemptions	222,900	339,100
State-level control variables		
Labor force	2,855,000	3,111,000
Median Income	43,650	9,022
House Price Index	0.0354	0.0587

Table 3 – Exemptions and firm entry

Data are at the state-year level from the Longitudinal Business Database for the period 1994-2013. State controls include the labor force, median income, and house price index. 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)	(2)	(3)	(4)
Dependent variable:	Log(Counts)	Log(Counts)	Log(Births)	Log(Births)
Log(Homestead)	0.0038*		0.0019	
	[0.0020]		[0.0031]	
Log(Exemptions)		0.0175**		0.0188*
		[0.0085]		[0.0105]
State controls	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Observations	1,000	1,000	1,000	1,000
R-squared	0.685	0.693	0.938	0.938

Table 4 – Exemptions and firm entry: single versus multi-unit firms

Data are at the state-year-industry-entry type level from the Longitudinal Business Database for the period 1994-2013. Multi-unit firms have more than one establishment and single-unit firms have only one establishment. 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)	(2)	(3)	(4)
Dependent variable:	Log(Counts)	Log(Counts)	Log(Births)	Log(Births)
Log(Homestead) × Multi-unit	-0.0043**		-0.0061	
	[0.0019]		[0.0064]	
Log(Exemptions) × Multi-unit		-0.0128*		-0.0089
		[0.0066]		[0.0177]
State-industry-entry type fixed effects	Yes	Yes	Yes	Yes
State-industry-year fixed effects	Yes	Yes	Yes	Yes
Entry type linear time trends	Yes	Yes	Yes	Yes
Observations	46,000	46,000	46,000	46,000
R-squared	0.997	0.997	0.982	0.982

Table 5 – Exemptions and firm size

Data are at the state-year level from the Longitudinal Business Database for the period 1994-2013. State controls include the labor force, median income, and house price index. 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)	(2)	(3)	(4)
Dependent variable:	Employment	Log (Employment)	Employment	Log (Employment)
Log(Homestead)	0.0636*	0.0052		
	[0.0323]	[0.0038]		
Log(Exemptions)			0.1496	0.0011
			[0.1385]	[0.0141]
State controls	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Observations	1,000	1,000	1,000	1,000
R-squared	0.277	0.396	0.277	0.395

Table 6 – Exemptions and firm entry in industries with high versus low entry barriers

Data are at the state-year-industry level from the Longitudinal Business Database for the period 1994-2013. 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)	(2)	(3)	(4)
Dependent variable:	Log(Counts)	Log(Counts)	Log(Births)	Log(Births)
Log(Homestead) × HighBarrier	-0.0025**		-0.0071**	
	[0.0010]		[0.0029]	
Log(Exemptions) × HighBarrier		-0.0021		-0.0140
		[0.0131]		[0.0129]
State-year fixed effects	Yes	Yes	Yes	Yes
State-industry fixed effects	Yes	Yes	Yes	Yes
Industry linear time trends	Yes	Yes	Yes	Yes
Observations	22,000	22,000	22,000	22,000
R-squared	0.443	0.998	0.985	0.985

Table 7 – Exemptions and firm exit

Data are at the state-year level from the Longitudinal Business Database for the period 1994-2013. State controls include the labor force, median income, and house price index. 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)	(2)
Dependent variable:	Log(Exits)	Log(Exits)
Log(Homestead)	0.0110**	
	[0.0044]	
Log(Exemptions)		0.0425***
		[0.0118]
State controls	Yes	Yes
State fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Observations	1,000	1,000
R-squared	0.914	0.918

Table 8 – Exemptions and firm exit by firm age

Data are at the establishment-year level from the Longitudinal Business Database for the period 1994-2013. 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.

Panel A: Homestead exemptions

Dependent variable:	Log(Exits)				
	(1)	(2)	(3)	(4)	(5)
Age group:	All firms	1-2 years	3-5 years	6-10 years	≥11 years
Log(Homestead)	0.0051	0.0046	0.0028	0.0043	0.0046
	[0.0042]	[0.0073]	[0.0025]	[0.0032]	[0.0036]
State-year fixed effects	Yes	Yes	Yes	Yes	Yes
State-industry fixed effects	Yes	Yes	Yes	Yes	Yes
Age category indicators	Yes	Yes	Yes	Yes	Yes
Observations	615,000	44,000	67,000	111,000	393,000
R-squared	0.836	0.922	0.942	0.918	0.765

Panel B: Total exemptions

Dependent variable:	Log(Exits)				
	(1)	(2)	(3)	(4)	(5)
Age group:	All firms	1-2 years	3-5 years	6-10 years	≥11 years
Log(Exemptions)	0.0142	0.0135	0.0179	0.0174	0.0100
	[0.0093]	[0.0162]	[0.0122]	[0.0112]	[0.0095]
State-year fixed effects	Yes	Yes	Yes	Yes	Yes
State-industry fixed effects	Yes	Yes	Yes	Yes	Yes
Age category indicators	Yes	Yes	Yes	Yes	Yes
Observations	615,000	44,000	67,000	111,000	393,000
R-squared	0.836	0.922	0.942	0.918	0.765