How does personal bankruptcy law affect start-ups?*

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Abstract

We exploit state-level changes in the amount of personal wealth individuals can protect under Chapter 7 personal bankruptcy to analyze the causal effect of debtor protection on the financing structure and performance of a representative panel of U.S start-up firms. We show that a higher level of debtor protection reduces the availability of credit, employment, operating efficiency, and survival rate of firms owned by low-wealth entrepreneurs. We find no such negative effects for firms owned by high-wealth entrepreneurs, who still have large amounts of assets unprotected under the new bankruptcy regime. Our evidence actually indicates that these wealthier entrepreneurs expand their businesses by increasing employment. Our results are consistent with theories that predict that debtor-friendly bankruptcy regimes redistribute credit from the less wealthy to the more wealthy individuals.

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1. Introduction

Entrepreneurs require adequate funding in order to successfully run their businesses.[1] An entrepreneur's borrowing capacity depends on the amount of personal wealth that creditors can seize in case the entrepreneur fails. Therefore, borrowing capacity depends not only on how much wealth the entrepreneur has, but also on how much of that wealth the entrepreneur is entitled to keep in bankruptcy. A more debtor-friendly bankruptcy regime reduces the amount of assets creditors can seize in bankruptcy, and thereby it could reduce entrepreneurs' access to credit and hurt the performance of their businesses.

In this paper, we exploit state-level changes in U.S. personal bankruptcy law to study the causal effect of debtor protection on the credit availability, employment, and performance of young firms. We use the Kauffman Firm Survey (KFS), a representative panel of U.S. start-ups that began operations in 2004, and which we follow until 2009. To quantify the importance of liquidity constraints, we analyze the effects of changes in debtor protection on entrepreneurs with different levels of initial wealth.

We analyze changes in *personal* bankruptcy law because it applies directly to all personal liabilities and guarantees of firm owners. Whether a firm owner is liable or not for the firm's debts depends on the legal form of the business organization. In an unlimited liability firm, all debts of the firm are personal, since there is no legal distinction between the firm and the owner. If the firm has instead the limited liability form, the owner is not liable for the firm's debts. However, almost half of the owners of limited liability firms in our sample report that they borrow at the personal level to

[1] Borrowing constraints could force start-ups to operate at a suboptimal scale, to grow slower, and to fail more often. See, for instance, Holtz-Eakin et al., (1994), Blanchflower and Oswald (1998), Cabral and Mata (2003), Albuquerque and Hopenhayn (2004), and Fracassi et al. (forthcoming).

finance the firm's operations. This confirms anecdotal evidence that personal bank borrowing is an important source of financing for young firms.

Specifically, we study the increases of Chapter 7 exemption limits introduced in several U.S. states during our sample period. Most individuals in the U.S. file for personal bankruptcy under Chapter 7. Under this Chapter, debtors keep their future income, but they must turn over any unsecured assets they own above a predetermined exemption limit. The exemption limit is the maximum amount of the borrower's personal assets that is protected from creditors, and therefore it provides a precise measure of debtor protection.

Our empirical methodology exploits these staggered changes in state exemptions. The panel structure of our data allows us to include firm fixed effects that control for time-invariant differences between entrepreneurs, firms, and states. These firm fixed effects address the concern, among others, that states with high exemption levels might attract a worse pool of entrepreneurs (Hombert et al., 2013). In order to remove the effects of potentially confounding state-level shocks, we also exploit the differential effects of the exemption laws across entrepreneurs with different levels of personal wealth (Gropp et al., 1997, and Lilienfield-Toal and Mookherjee, 2008).

We consider three groups of entrepreneurs according to how much unprotected (or pledgeable) wealth they have: *No wealth, Low wealth,* and *High wealth.*[2] *No wealth* entrepreneurs are likely to have all their wealth already protected under the old exemption limit, and therefore an increase in exemptions should not

[2] The names we assign to these groups are based on how much unprotected (or pledgeable) wealth entrepreneurs are likely to have, rather than on their actual wealth. At the start of our sample, all states have positive exemption limits, implying that all entrepreneurs in our sample start with part of their wealth protected (provided they have any). We construct the wealth groups using the five net worth ranges provided in the KFS. The *No wealth* group includes entrepreneurs with negative or zero net worth and entrepreneurs with net worth lower than $50,000. The *Low wealth* group includes entrepreneurs with net worth between $50,000 and $250,000. The *High wealth* group includes entrepreneurs with net worth of more than $250,000.

affect this group. *Low wealth* entrepreneurs are likely left without unprotected assets after the increase in the exemption limit. This reduction in pledgeable assets could reduce their access to credit. *High wealth* entrepreneurs are likely to still have substantial pledgeable wealth under the new, higher, exemption limit. For this reason, they should be less affected by the exemptions than the previous group.[3]

We obtain strong empirical support for these predictions.

First, we find that the exemptions have no significant effect on start-ups owned by entrepreneurs in the *No wealth* group. This result confirms our view that these entrepreneurs should provide a good placebo group for our analysis.

Second, for entrepreneurs who are left without pledgeable assets (i.e., the *Low wealth* group), the increase in exemptions has a significant negative impact on the financing, employment, and performance of their firms. In particular, we find that these entrepreneurs permanently reduce the inflow of *personal* credit they obtain to finance the firm by about 6% for every $10,000 increase in the exemption limit. The reduction in personal credit is driven by a reduction in both credit card financing and term loans. Importantly, as expected, we do not find any effects of the exemptions on the inflow of *business* credit (i.e., loans obtained in the name of the firm). This result is important in terms of identification as it rules out the possibility that our finding on personal credit is driven by contemporaneous local economic shocks rather than by the exemption laws. With respect to employment, we find that following an increase in exemptions, firms owned by *Low wealth* entrepreneurs reduce their labor force significantly and become less likely to be employers. In addition, these firms generate fewer revenues, have lower operating efficiency (which we measure as average

[3] This is a partial equilibrium argument. A general equilibrium argument is provided by Lilienfeld-Toal and Mookherjee (2008) who show that a higher exemption level redistributes credit from the less wealthy towards the wealthiest entrepreneurs, since these are the only individuals in the economy left with pledgeable wealth.

revenue per employee), and become more likely to fail. These findings indicate that tighter credit constraints force these firms to operate at a suboptimal scale, making these firms more vulnerable to failure.

Third, we obtain a modest positive effect on the financing and employment of start-ups owned by *High wealth* entrepreneurs. In particular, these entrepreneurs are granted increases in their personal credit cards limits. This result is consistent with a redistribution of credit towards the wealthiest entrepreneurs, as predicted in Lilienfeld-Toal and Mookherjee (2008) and documented in a cross-sectional study of consumer credit by Gropp et al. (1997). We also find that *High wealth* entrepreneurs increase their labor force significantly, though there is no improvement in the firm's performance. We interpret the increase in employment as resulting from the additional wealth insurance, as in Kihlstrom and Laffont (1979).[4]

Our paper makes two important contributions to the existing literature. First, we significantly improve on the empirical identification of the effects of the exemptions on bank financing. While previous studies use cross-sectional variation in exemption levels (Gropp et al., 1997, Berkowitz and White, 2004; Berger et al., 2011), our paper is the first to exploit the effect of state laws that increased exemption levels, allowing us to control for unobserved heterogeneity across entrepreneurs, firms, and states. Our study is thus related to a fast growing a literature that studies the causal effect of changes in the financial and regulatory environment on entrepreneurship (Djankov et al. (2002), Cetorelli and Strahan (2006), Klapper et al. (2006), Bertrand et al. (2007), Kerr and Nanda (2009), and Hombert et al., 2013).

[4] Kihlstrom and Laffont (1979) develop a general equilibrium model where entrepreneurial decisions depend on the individual's level of risk aversion. They show that more risk-averse individuals become workers, while less risk-averse individuals become entrepreneurs. Moreover, the less risk-averse entrepreneurs increase their exposure to business risk by hiring more employees and by increasing the size of their ventures.

Second, our paper studies the effect of debtor protection not only on start-ups' financing structure, but also on several indicators of real performance, such as employment, operating efficiency, and survival. Our study makes therefore a step forward by analyzing whether credit market frictions triggered by changes in exemptions actually affect young firms' real outcomes.

Our results also have important policy implications. Start-ups have been traditionally important job creators in the U.S. (Haltiwanger et al., 2013). However, there is a growing concern that the number of jobs created by start-ups has declined in recent years.[5] Our findings contribute to a broader debate on the role of regulation and institutions in promoting job creation and economic growth. In particular, policy makers have embraced the view that debtor-friendly bankruptcy laws could enhance entrepreneurial activity and spur economic growth (see Audretsch, 2007; Ederer and Manso 2011). Instead, our results indicate that debtor protection could limit the growth of an important component of the entrepreneurial sector of the economy.

Our study also highlights an unintended consequence in the design of personal bankruptcy law. The most popular arguments in favor of lenient bankruptcy rules are the protection of debtors against unfortunate events, such as illness or job loss, and the preservation of their ex post incentives to work. However, our results indicate that lenient personal bankruptcy laws make the less wealthy entrepreneurs more likely to fail.

The paper is organized as follows. Section 2 details the institutional background of U.S. personal bankruptcy law. In Section 3, we review the related literature and develop our hypotheses. We present our empirical methodology in Section 4. The dataset and the variables used in our analysis are described in Section

[5]For instance, see: http://www.economist.com/news/business/21587778-americas-engines-growth-are-misfiring-badly-not-open-business (retrieved 22 May 2014)

5. Section 6 presents the results and Section 7 provides some robustness tests. Section 8 concludes.

2. Institutional setting

a. U.S. Personal Bankruptcy Law

When an individual files for bankruptcy, all collection efforts by creditors must terminate. There are two separate personal bankruptcy procedures in the U.S., named Chapter 7 (a liquidation procedure) and Chapter 13 (a reorganization procedure). Under Chapter 7, filers keep all their future income but they must turn over any unsecured assets they own above the relevant state's exemption limits.[6] The bankruptcy trustee uses these nonexempt assets to repay debt. As explained below, the exemption limits vary widely across both states and time. Under Chapter 13, debtors can keep all of their assets, but they must propose to creditors a repayment plan. This plan typically involves using a portion of the debtor's future earnings over a five-year period to repay debt.

Before 2005, debtors were allowed to choose between Chapters 7 and 13. Around 70 percent of all bankruptcy filings were made under Chapter 7 (White, 2007a). Debtors had an incentive to choose Chapter 7 over Chapter 13 whenever they had few nonexempt assets. In this way, debtors maximized their financial benefit from filing for bankruptcy, because they were able to preserve both their current assets and their future income. But this also means that the system permitted that most bankrupt individuals had no obligation to repay from future income, irrespective of how high their incomes were.

[6] Most unsecured debts, including credit card and personal loans, are discharged in bankruptcy. In contrast, mortgages and other secured loans cannot be discharged. However, filing for bankruptcy often delays creditors from repossessing the collateral, because they must first obtain the bankruptcy trustee's permission to seize the assets. The probability of bankruptcy should thus reduce the value of both unsecured and secured claims.

The Bankruptcy Abuse Prevention and Consumer Protection Act (BAPCPA) of 2005 aimed at preventing borrowers from abusing the bankruptcy regime. This legal reform essentially introduced a *means test* that changed the bankruptcy options for individuals (but not for business owners, as we explain below). Under BAPCPA, only filers whose income over the *previous six months* is below the median for their state can file for Chapter 7 bankruptcy. Higher income debtors with sufficient means can only file for Chapter 13 bankruptcy.[7] Otherwise, the provisions in Chapter 7 remain essentially unchanged. In particular, the state exemption limits remain in effect, and Chapter 7 bankruptcy filers are only obliged to turn over to creditors their nonexempt assets.

As noted in White (2007b), the effects of BAPCPA on small business owners should be particularly modest. The U.S. Bankruptcy Code explicitly refers 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. And even if an entrepreneur did not qualify for Chapter 7, White (2007b) presents a variety of strategies that debtors can pursue in order to either bypass the means test or reduce their obligation to repay. For instance, debtors at higher income levels can pass the means test by filing when their average income over the previous six months is low. In short, the 2005 reform should not change the way exemptions affect indebted *entrepreneurs,* even if they have high asset and income levels.

[7] Another major change in the 2005 law is that debtors are no longer allowed to propose their own Chapter 13 repayment plan. BAPCPA implemented a procedure based on debtors' disposable income that determines how much they must repay. Other significant changes of BAPCPA are that all filers must undergo six months of mandatory credit counseling, provide additional documentation, and pay higher filing fees.

b. Bankruptcy exemptions

Under Chapter 7, debtors are allowed to keep certain assets in bankruptcy up to the state's predefined exemption limits. A higher exemption level provides additional wealth insurance to debtors, because it reduces the asset value that creditors can seize in bankruptcy. Although the Bankruptcy Reform Act of 1978 established a uniform national set of exemptions, it allowed states to opt out and set their own exemption levels. About three quarters of the states opted out (Hynes, Malani, and Posner, 2004). As a result, exemption limits vary widely across states.[8]

There are several different categories of asset exemptions. The most important is the homestead exemption, which provides protection for equity in the debtor's family residence. The homestead exemption varies from a few thousand dollars to unlimited. Lower exemption amounts are also available for various other types of personal property, such as clothing, furniture, cattle, guns, and motor vehicles. Many states offer wildcard exemptions that allow debtors to retain any personal property up to a specified dollar amount. The types of personal assets specified in the law vary considerably across states and many of these assets have unspecified exemption amounts. It is therefore unfeasible to include all personal assets specified in these various state laws. Similar to Gropp et al. (1997), our measure of personal property exemptions includes only assets that have specific dollar amounts in most states: jewelry, motor vehicles, cash and deposits, and the wildcard exemption. In our empirical analysis, we use a measure of state exemptions that combines the homestead exemption and the personal property exemptions.

[8] Several states allow their residents to choose between the state and the federal exemptions. In these cases, we selected the option that grants the claimant the highest exemption level. In some states, married couples are allowed to double the amount of the exemption when filing for bankruptcy together (called "doubling"). We have doubled all amounts except in those cases where bankruptcy law explicitly prohibits "doubling."

c. State laws amending bankruptcy exemptions

During 2005-2009, multiple states enacted laws that increased their exemption levels. These laws can dictate an increase in the homestead exemption, in the personal property exemptions, or in both. In most cases, the same law amends the exemption limits for various assets (e.g., homestead and motor vehicle). Table 1A shows that many states have changed their exemption levels during the sample period.[9] Moreover, some states raise exemptions more than once (e.g., Idaho in 2006 and 2008). Table 1B shows that there is significant variation in the exemption amounts. The states in the lowest bracket made very small changes to their exemption limits (below $5000). These small changes typically reflect statutory increases in the nominal value of exemptions based on inflation. On the other end, eighteen states experienced increases of at least $25,000, and six states experienced very large increases in their exemption levels of at least $100,000.

d. The political economy of exemption laws

We are unaware of any study that investigates the political context behind the amendments in exemption limits that occurred in several states during 2005-2009. Anecdotal evidence we obtained from the legislative discussions preceding the laws that amended exemption limits highlights three supporting arguments: the increase in house prices, the increase in medical costs, and the higher exemption levels offered by other states.[10] In light of this evidence, we cannot rule out that changing state economic conditions may have led to the passage of these laws. This raises obvious concerns regarding the identification of the effect of the exemptions. Our empirical

[9] In some cases, there is a one-year gap between the law's approval date and when the law becomes effective. In this case, the date we assign to the exemption change is the year in which the law becomes effective.

[10] Appendix A discusses these arguments in more detail.

strategy, which we explain in detail in Section 4, was designed to address these concerns.

3. Related literature and hypotheses

a. Exemptions and the credit market

A higher exemption level makes borrowers more likely to file for personal bankruptcy and it reduces the amount of assets creditors can seize in bankruptcy. Moreover, it also increases the potential for opportunistic behavior by borrowers (Fay et al., 2002). Several papers find cross-sectional evidence consistent with banks reducing the supply of credit in response to the moral hazard problem. In particular, these papers find that in states with high exemptions banks are more likely to turn down loan applications from households (Gropp et al., 1997) and from SMEs (Berkowitz and White, 2004; Berger et al., 2011).

If a higher exemption level reduces entrepreneurs' ability to secure external financing, then it should also affect their real decisions. An important literature shows that financial constraints may force entrepreneurs to inefficiently reduce the scale of their ventures, harming their performance and making them more vulnerable to failure (Evans and Jovanovic, 1989; Holtz-Eakin et al., 1994; Hurst and Lusardi, 2004; Kerr and Nanda, 2010).[11] We therefore contend that the credit market channel should have a negative effect on a firm's size and performance.

b. Exemptions and wealth insurance

Entrepreneurs face the risk associated with their firms' activities. A higher exemption level allows entrepreneurs to shelter more assets in bankruptcy, and hence

[11] Cerqueiro et al. (2014) study the effect of exemptions on patenting by small firms. They find that an increase in exemptions reduces the number of patents produced by small firms, a result which is consistent with innovation being negatively affected by a reduction in credit availability.

it decreases their exposure to business risk. Gropp, Scholz, and White (1997) argue that the insurance provided by the exemption should lead risk-averse individuals to demand more credit. Kihlstrom and Laffont (1979) develop a general equilibrium model where entrepreneurial decisions depend on the individual's level of risk aversion. They show that more risk-averse individuals become workers, while less risk-averse individuals become entrepreneurs. Moreover, the less risk-averse entrepreneurs increase their exposition to business risk by hiring more employees and by increasing the size of their ventures. A higher exemption level reduces the risk associated with entrepreneurship, and therefore it should: (i) make individuals more likely to become self-employed and, (ii) increase entrepreneurs' willingness to increase employment and expand their businesses. Consistent with the first prediction, Fan and White (2003), and Armour and Cumming (2008) document that debtor-friendly personal bankruptcy regimes have substantially higher self-employment rates. We are not aware of any direct evidence linking debtor-protection to either firm employment or firm size.

In sum, the insurance mechanism predicts that following increases in exemptions, entrepreneurs should be more willing to obtain external financing and to expand the scale of their businesses by hiring more employees.

c. Credit market versus insurance: The role of entrepreneurs' wealth

Which of the two above channels dominates should depend on the wealth level of the entrepreneur. Gropp et al. (1997) document, using the 1983 Survey of Consumer Finances, that the amount of debt held by high asset households is positively related to bankruptcy exemptions, while the amount of debt of low asset households is negatively related to the level of exemptions. In light of these findings,

they conclude that high exemptions redistribute credit from the less wealthy towards the more wealthy individuals.

This redistribution effect finds theoretical support in Lilienfeld-Toal and Mookherjee (2008), who study the optimal design of personal bankruptcy law in a general equilibrium setting with contracts. A debtor-friendly regime reduces the amount of assets individuals can credibly pledge to creditors. However, this limited liability constraint is more binding for individuals with low wealth, who have few or no assets left to pledge, than for high wealth individuals, who still have pledgeable assets. As a result, their model predicts that a debtor-friendly regime reduces the debt capacity of low wealth individuals by more than that of high wealth individuals.

This redistribution mechanism is important for two reasons. First, it alerts us of the fact that how the exemptions affect start-ups should depend on the entrepreneur's level of wealth. Second, it provides us with theoretical guidance to appropriately identify the effect of the exemptions. In light of the discussion above, the credit channel should dominate for low wealth entrepreneurs, while the insurance mechanism should dominate for the high wealth entrepreneurs.

4. Empirical methodology

We explain our identification strategy in two steps. Consider the following panel regression model:

$$y_{ist} = \alpha_i + \alpha_t + \beta\ Exemp_{st} + \delta Z_{st} + \gamma X_{it} + u_{ist} \qquad (1)$$

where *i* indexes firms, *s* indexes state, *t* indexes time, y_{ist} is the dependent variable, α_i and α_t are firm and year fixed effects, $Exemp_{st}$ is the exemption amount in state *s* at time *t*, Z_{st} are state control-level variables, X_{it} are firm-level control variables, and u_{ist} is an error term.

The year fixed effects control for aggregate shocks. The firm fixed effects control for all time-invariant heterogeneity at the firm and state level. Therefore, these fixed effects ensure that our identification of the exemptions effect comes entirely from changes in state exemption levels. In contrast to the previous literature (e.g., Gropp et al., 1997), we discard the vast cross-state variation in exemption levels and thus the possibility that differences in exemption levels might be picking other state-level characteristics. For instance, one might worry that states with high exemptions could attract less skilled (marginal) entrepreneurs who ex ante benefit more from the insurance provided by the exemptions.[12] If these marginal entrepreneurs find it harder to obtain external financing and if their firms are more likely to underperform, then a cross-state analysis of the exemptions could yield biased estimates of their effects on firm financing and performance. In particular, one might conclude that high exemption levels reduce firm financing and cause them to underperform, while in reality that effect is driven by the lower quality of firms in high exemption states. The inclusion of firm fixed effects mitigates such concerns.

The coefficient β measures the effect of the exemption laws. The following example illustrates how we identify this parameter. Rhode Island (RI) passed a law in 2006 raising the state's homestead exemption from $200,000 to $300,000. Suppose that we wish to analyze the effect of the law on bank financing. We could obtain such an estimate by simply subtracting the level of bank financing after 2006 from the one before 2006 for each firm located in RI. However, a contemporaneous change in credit market conditions in RI, for example, may have affected bank financing for all firms. To help control for changing economic conditions, we could use a control state that did not raise exemptions in the same year, such as Connecticut (CT). If firms in

[12] Fan and White (2003), and Armour and Cumming (2008) document that generous personal bankruptcy systems increase substantially the probability that an individual becomes self-employed.

CT were exposed to similar credit market conditions, their change in bank financing would measure the effect of such aggregate shocks. We can then compare the difference in bank financing obtained by firms in RI before and after 2006 with the same difference in CT. The difference of those two differences would therefore serve as estimate of the effect of the increase in exemptions in RI.

Equation (1) has two additional virtues that are not readily visible in the above two-state example. First, the regression model accounts for the fact that we have several exemption laws staggered during our sample period. Consequently, our "control" group is not restricted to states that never raised exemptions. Equation (1) implicitly takes as the control group all firms located in states not changing exemptions at time *t*, even if they changed exemptions before or will change exemptions later on. Second, the regression model exploits variation in the dollar amounts by which exemption limits are amended. The model implicitly assumes that the effect of an exemption law increases proportionally with the size of the limit change. The variation in the intensity of the "treatment effect" provides better identification than the standard binary treatment outcome (i.e., whether a legal change occurred or not).

One important concern not addressed in Equation (1) is that local economic shocks could be correlated with the passage of the exemption laws. For instance, suppose that an adverse shock hit only RI at the same time it passed the exemption law. Our results would be biased towards finding a negative effect of the exemptions on firm financing, since the local economic shock would be correlated with both the exemption law and local banking market conditions. To control for such changing economic conditions, we exploit differential effects of the exemptions on

entrepreneurs with different levels of wealth (*No wealth*, *Low wealth*, and *High wealth*).[13] The regression model we estimate is:

$$\begin{aligned} y_{ist} = {} & \alpha_t \times NoWealth_i + \beta^{NoWealth} Exemp_{st} \times NoWealth_i \\ & + \alpha_t \times LowWealth_i + \beta^{LowWealth} Exemp_{st} \times LowWealth_i \\ & + \alpha_t \times HighWealth_i + \beta^{HighWealth} Exemp_{st} \times HighWealth_i \\ & + \alpha_i + \delta Z_{st} + \gamma X_{it} + u_{ist}. \end{aligned} \tag{2}$$

The other variables are defined similarly as in Equation (1). Importantly, the model contains year fixed effects interacted with each of the wealth groups. These year–wealth fixed effects control for any aggregate shocks affecting entrepreneurs with different levels of wealth.

We use regression model (2) to estimate the effect of the exemptions for each of the wealth groups, as well as to test for differential effects between groups. The differential effects are crucial in our identification strategy for two reasons. First, they filter out the effects of local economic shocks affecting *all* firms in the state passing the exemption law. Second, these differential effects allow us to identify the effect of the exemptions in accordance with theory (Lilienfield-Toal and Mookherjee, 2008).

We estimate Equation (2) to examine the effect of changes in exemptions on bank financing, firm employment, firm revenue, and firm efficiency. When analyzing firm exit, we estimate a multiperiod logit regression where the dependent variable equals zero if the firm is alive in year *t*, and equals one if the firm stopped its operations in year *t*.[14] This multiperiod logit regression is similar to Equation (2), except that we are forced to drop the firm fixed effects due to the incidental parameter problem. In exchange, we control for differences across firms and entrepreneurs with industry dummies and with several characteristics of the entrepreneur, such as

[13] Section 5 describes the wealth groups in detail.

[14] The multiperiod logit models in our firm exit regressions are equivalent to discrete-time hazard models (Shumway, 2001).

education and experience (see Table A1 for the complete list of variables). In addition, we add state fixed effects in our survival regressions to ensure that identification of the exemptions comes only from changes in state exemptions.

5. Data and variables

This paper uses confidential data from the Kauffman Firm Survey (KFS). The KFS is a longitudinal survey that collected information for a sample of 4,928 start-ups that began operations in 2004 in the United States and that are followed annually. We use six years of data (2004-2009). The KFS contains detailed information on the financial injections these firms receive in each year. The survey also provides detailed information on the firm, such as its credit history, geographic location, industry, and information on the owners, such as experience, education, gender, race, age, and net worth. The KFS uses weights that make it representative of the population of start-ups in the US, and all of our analyses use these weights.[15]

Table 2 provides definitions of variables and summary statistics for the period 2004-2009. Panel A focuses on the dependent variables (bank financing, employment, and revenue and performance), while Panel B focuses on the explanatory variables (state exemptions, owner wealth, state time-varying controls, and firm time-varying controls). Below, we describe separately the variables in each group.

a. Bank financing

Robb and Robinson (2014) document that outside debt – most of which is obtained from banks – is the largest single financing category for the KFS start-ups. The KFS enables us to separate credit obtained in the name of the firm's owner that is

[15] Each year there is some loss in sample size because some firm owners cannot be located, refuse to respond to the follow-up survey wave, or stop operations. The KFS weights were designed to minimize potential non-response bias in the estimates. See DesRoches et al. (2011) for additional details of the KFS sample design.

used to finance the firm's operations (*Personal credit*) from credit obtained in the name of the firm (*Business credit*). The KFS also provides detailed information about different modes of personal bank financing, including credit cards and other bank loans. We observe both the lending balance and the credit limit of the credit cards that the entrepreneur claims to use to finance the firm's operations.[16] Finally, the variable *Other bank loans* measures the amount of personal credit obtained, excluding credit card financing, such as term loans.

b. Employment

We use two measures of firm employment. First, we use the number of full-time employees including the firm owner. About half of the firms in our sample report that they don't have employees. Therefore, we also create the dummy variable *Firm is employer* that indicates whether or not the firm has employees in the specific year.

c. Revenues and performance

We report firm revenue in thousands of dollars. We use this variable to create a measure of the firm's operating efficiency, which we define as the average revenue generated by each employee (including the firm owner). Our second measure of performance is firm failure. *Failed* is a dummy variable that indicates whether the firm went out of business in the current year.

d. Personal bankruptcy

Our main variable of interest is *Exemptions*, which equals the sum of the homestead exemption and the personal property exemption in the state.[17] We obtain the exemption values from individual state legal codes. Table 1A describes the timing

[16] Robb and Robinson (2014), and Chatterji and Seamans (2012) document the importance of credit card financing for nascent firms. Brown, Coates, and Severino (2014) find that higher bankruptcy exemptions increase the level of credit card debt held by households.

[17] For details on the different types of exemptions, see subsection II.b.

of the exemption laws, while Table 1B describes the distribution of the increase in exemption values.[18]

e. Owner wealth

In the KFS the entrepreneur's reported net worth can lie in one of five bins: (i) $0 or less, (ii) $1 to $50,000, (iii) $50,001 to $100,000, (iv) $100,001 to $250,000, and (v) $250,001 and up. Based on these bins, we construct three wealth groups that we call: *No wealth, Low wealth,* and *High wealth*. *No wealth* indicates that the net worth of the main firm owner is lower than $50,000, corresponding to the two lower intervals in the survey. *Low wealth* indicates that the net worth of the main firm owner ranges between $50,000 and $250,000, which includes intervals (iii) and (iv). *High wealth* indicates that the firm owner declared its net worth to be higher than $250,000, corresponding to interval (v) in the survey.

Two important issues deserve discussion. First, we note that the names we assign to these wealth groups are based on how much unprotected (or pledgeable) wealth entrepreneurs are likely to have, rather than their actual wealth. All entrepreneurs in our sample start with part of their wealth protected (provided they have any), because all states have positive exemption limits. For instance, although some entrepreneurs in the no wealth group may have some wealth, it is very likely that the current exemption level protects most or all of their wealth.

Second, the above survey question about the primary owner's personal net worth is not available in the KFS until its fourth follow-up survey (2008). Some firms in the 2004 wave went out of business in the following years. As a result, information about net worth is only available for about 53% of the initial sample. The missing

[18] There are no reductions in exemption limits during our sample period. In the descriptive statistics, we assign a value of $1 million to the states with unlimited homestead exemptions. This assumption is irrelevant for our empirical analysis because no state changed its homestead exemption level from or to unlimited during our sample period.

information about wealth in the early waves of the KFS raises several problems for our empirical analysis. First, the missing data greatly reduces the number of sampled firms. Second, restricting the analysis to the subsample of firms that survived to 2009 may raise concerns about sample selection. Third, dropping firms that fail during their first years of operation makes it almost unfeasible to study firm survival.

In order to deal with these issues, we estimate a predictive model of the entrepreneur's wealth group as a function of demographic characteristics of the entrepreneurs, location, local income and house prices (both at the zip-code level), and the legal form of the company. All these variables are measured as of 2004. In Appendix B, we describe the predictive model and the variables used.

The measure of owner wealth that we report in the paper combines the *reported* wealth group for firm owners who survive until 2008 with the *predicted* wealth group for firms that fail before 2008. As discussed below in our robustness tests, our results remain similar when we use either: (i) the reported wealth group (for the subsample of surviving firms), or (ii) the predicted wealth group (for all firms in our sample).

Table 3 displays summary statistics by wealth group.

f. State time-varying controls

We collect average state house prices from the Federal Housing Finance Agency to control for changing conditions in real estate markets. In order to control for changes in other economic conditions, we add the rate of unemployment and the state median household income, which we obtain respectively from the U.S. Bureau of Labor Statistics and the U.S. Census Bureau. Finally, we obtain firm entry rates from the Business Dynamics Statistics (Census Bureau), measured as the percent change in the number of establishments due to births. Higher entry rates could be

correlated with fiercer competition, which could negatively affect the performance of start-ups (Kerr and Nanda, 2009).

g. Firm time-varying controls

The KFS contains the commercial credit score class of the firm from Dun & Bradstreet, which ranges from 1 (minimum risk) to 5 (maximum risk). The credit scores are not available for about one fourth of our sample, because Dun & Bradstreet sometimes did not have enough information to produce a score. We decompose the credit score variable into a set of six mutually exclusive dummy variables, with the 'missing credit score dummy' as the omitted category.

6. Results

a. Bank financing: Personal credit and Business credit

We first study the effect of changes in state exemptions on start-ups' bank financing. The KFS distinguishes which bank loans used for business purposes are obtained in the name of the firm owner (*Personal credit*) or in the name of the business (*Business credit*). This distinction is important, because personal bankruptcy law only applies directly to personal liabilities of the entrepreneur.[19] Therefore, analyzing the effect of the exemptions on business credit serves as a good counterfactual.

In Table 4, we investigate how changes in exemptions affect the inflows of personal credit and business credit (both measured in logs). The exemptions are expressed in millions of dollars. We report the coefficients for the estimated effects of the exemptions for the three wealth groups (*No Wealth*, *Low wealth*, and *High*

[19] In firms with unlimited liability form, all credits are de facto personal, since there is no legal distinction between the firm and the owner. Consequently, the distinction between personal credit and business credit is meaningful only for limited liability firms. In our sample 44% of the owners of limited liability firms report that they borrow at the personal level to finance the firm's operations.

Wealth). At the bottom of the table, we also provide estimates of the differential effects between the *Low wealth* and each of the two other groups. These differential effects should filter out any economic shocks affecting all entrepreneurs in the state passing the exemption law. All the regressions reported in this section include firm fixed effects and separate year dummies for each wealth group. Also, we include several state controls (average house prices, median income, unemployment rate, and entry rate) and a full set of firm credit score dummies, where the omitted category is a missing credit score. Standard errors are clustered at the state level.

Column 1 of Table 4 shows that the exemptions reduced personal credit only for the *Low wealth* entrepreneurs. The respective coefficient indicates that these entrepreneurs permanently reduce the inflow of personal credit by six percent for a $10,000 increase in the exemption limit. This estimate is statistically significant and economically meaningful. It indicates, for instance that, as Rhode Island increased $100,000 its exemption limit, the inflow of personal credit to *Low wealth* entrepreneurs in this state decreased on average by 60%. We find an insignificant effect of the exemptions on the inflow of personal credit for both the lowest and highest wealth groups. The differential effects we obtain for these two groups are positive, significant, and sufficiently large to offset the negative coefficient obtained for the *Low wealth* group.

These findings indicate that the increase in exemptions reduce significantly the debt capacity of *Low wealth* entrepreneurs. Because most or all of their assets become protected under the new exemption limit, an increase in exemptions leaves these individuals without enough pledgeable wealth. Put differently, the exemptions impose a limited liability constraint on entrepreneurs that reduces their ability to obtain personal bank loans (Lilienfeld-Toal and Mookherjee, 2008). In contrast, the credit

channel is less important for the *High wealth* entrepreneurs who face a weaker limited liability constraint, because they still have plenty of unprotected assets remaining. Finally, increasing exemptions has no effect on the *No wealth* group, since these entrepreneurs had nothing to protect even under the previous exemption level. The coefficient of the *No wealth* group can therefore be considered as the result of a placebo test.

In Column 2 of Table 4, we find no significant effect of the exemptions on the inflow of business credit for any of the wealth groups. This is an important result. Business credit is a good counterfactual, because, while it should not be affected directly by personal bankruptcy law, it could still be susceptible to any economic shocks affecting the company. Consequently, finding no effect on business credit mitigates the concern that the exemptions might be correlated with other contemporaneous local shocks affecting a particular group of entrepreneurs. For instance, if the companies owned by *Low wealth* individuals were hit by some negative shock contemporaneous with the exemptions, then this shock would likely affect all types of bank financing. The fact that we obtain a significant effect of the exemptions only for personal credit strongly suggests that our estimates are indeed picking the effects of the exemption laws.

With respect to control variables, we highlight that firms with better credit scores obtain substantially larger inflows of both types of bank financing.[20] The estimated coefficients for the credit score dummies increase monotonically as we move towards the highest scores, and most coefficients are statistically significant. For instance, our estimates indicate that increasing the credit score to the highest level

[20] The credit scores contain information not only about the business, but also about the firm owners, such as past delinquencies. The inclusion of personal information explains why the credit scores matter also for personal credit.

(*Credit risk 1*) from the second highest level (*Credit risk 2*) increases the firm's inflow of personal loans and business loans by 30% and 22%, respectively.

b. Personal credit: Credit cards and Other bank loans

The level of detail of the KFS allows us to further analyze the two main types of personal bank loans used for business purposes: credit card financing and other personal loans. For the credit cards, we observe both the amount used and the credit limit. Other personal loans refer mainly to personal term loans.

Credit card financing is interesting for us for several reasons. First, it is a popular source of startup financing (Chatterji and Seamans, 2012; Robb and Robinson, 2014). Second, credit cards are important liquidity providers that entrepreneurs can tap to face temporary shocks. Third, and more importantly, personal bankruptcy law applies directly to unsecured lending, such as credit cards (Brown et al., 2014).

In Table 5, we investigate the effect of the exemptions on the credit card balance (Column 1), credit card limit (Column 2), and the inflow of other personal bank loans (Column 3). All three dependent variables are in logs. The exemption limits are expressed in millions of dollars. All specifications include firm fixed effects and separate year fixed effects for each wealth group. We also include average state house prices, other state variables (median income, unemployment rate, and entry rate), and a full set of credit score dummies (the omitted category is a missing credit score). Standard errors are clustered at the state level.

In Column 1, we show that credit card debt inflow decreased significantly only for the *Low wealth* group. The point estimate indicates that a $10,000 increase in the exemption limit leads to a reduction in credit card debt of almost four percent. We

obtain positive and significant differential effects for entrepreneurs in both the *High* and *Low wealth* groups.

The results in Column 2 indicate that the reduction in credit card financing for the *Low wealth* is essentially driven by a reduction in the credit card limit. This is consistent with our earlier finding that the exemptions reduce the supply of credit to *Low wealth* entrepreneurs. The remaining estimated coefficients are similar to those we report in Column 1, with the exception that the differential effect for the *High wealth* group becomes much stronger. In fact, our estimates indicate these entrepreneurs *increase* their credit card limit on average by 3.4% following a $10,000 increase in the exemption limit. This positive coefficient contrasts sharply with the reduction in credit card limit that *Low wealth* entrepreneurs suffer (a fall of 5.6% for a $10,000 increase in exemptions). We interpret this result as evidence that bankruptcy exemptions redistribute credit towards the wealthiest entrepreneurs (Gropp et al., 1997; Lilienfeld-Toal and Mookherjee, 2008).

In Column 3, we show that the exemptions reduce the inflow of other personal bank loans only to *Low wealth* entrepreneurs. The estimated total indicates that these entrepreneurs experience a permanent reduction in the inflow of other personal bank credit of 3.6% for a $10,000 increase in the exemption limit. The differential effects obtained for the two other wealth groups, albeit economically important, are not statistically significant.

In sum, how a debtor-friendly regime affects the financing of entrepreneurs seems to depend crucially on how much unprotected wealth they have left. Less wealthy entrepreneurs, who become mostly or entirely protected by the bankruptcy regime, suffer a reduction in personal credit. In contrast, the wealthiest entrepreneurs,

who retain large amounts of assets to pledge, are able to maintain – if not increase – their level of personal borrowing after an increase in exemptions.

We next investigate whether the changes in credit availability triggered by the change in exemptions affect firm employment, revenue, and performance.

c. Firm employment

The net effect of exemptions on firm employment should reflect the interplay between two competing forces. On the one hand, an increase in exemptions may reduce credit availability, which could prevent entrepreneurs from expanding their ventures or even force them to operate at a smaller scale. Our previous financing results indicate that this negative credit channel affects mainly *Low wealth* entrepreneurs. On the other hand, the wealth insurance provided by the exemptions could encourage entrepreneurs to expand their businesses and to hire more employees. This insurance mechanism could dominate for the *High wealth* entrepreneurs.

Column 1 of Table 6 estimates the impact of the exemption laws on the logarithm of the number of firm employees (including the firm owner). The exemption limits are expressed in millions of dollars. All specifications include firm fixed effects and separate year fixed effects for each wealth group. We also include average state house prices, other state variables (median income, unemployment rate, and entry rate), and a full set of credit score dummies (the omitted category is a missing credit score). Standard errors are clustered at the state level.

Following an increase in the exemption limit, firms owned by *Low wealth* entrepreneurs decrease their labor force significantly. The estimated coefficient indicates that these entrepreneurs reduced labor force on average by 0.7% for a $10,000 increase in the exemption limit. We interpret this reduction in labor force by

as evidence that tighter credit constraints force *Low wealth* entrepreneurs to operate at a smaller scale.

In contrast, we obtain a large and positive effect of the exemptions for the *High wealth* group. Specifically, these entrepreneurs increase labor force on average by 1.56% for a $10,000 increase in the exemption limit. This estimate is statistically significant and economically meaningful. For instance, it implies that following the $100,000 increase in exemptions in Rhode Island, *High wealth* entrepreneurs increase employment by almost 16%. We interpret this result as evidence that the insurance mechanism dominates the credit channel for the wealthiest entrepreneurs.

The above results are subject to the criticism that several start-ups in our sample do not hire employees, which results in a large concentration of zeros in our dependent variable. For this reason, we also analyze in Column 2 of Table 6 a linear probability model of the firm's decision to hire employees. The results broadly confirm our earlier findings. Following an increase in exemptions, *Low wealth* entrepreneurs become less likely to hire (the credit channel dominates), while *High wealth* entrepreneurs become more likely to hire (the insurance mechanism dominates). Finally, as expected, we find no significant effect of changes in exemptions in any of the specifications for the *No wealth* group.

d. Firm revenue, efficiency, and failure

Bankruptcy exemptions affect start-ups' financing opportunities and employment decisions. In Table 7, we analyze the real implications of the exemptions in terms of firm revenue and firm efficiency, which we define as revenue per employee. Both variables are expressed in logs. As before, the exemption limits are expressed in millions of dollars. All specifications include firm fixed effects and separate year fixed effects for each wealth group. We also include average state house

prices, other state variables (median income, unemployment rate, and entry rate), and a full set of credit score dummies (the omitted category is a missing credit score). Standard errors are clustered at the state level.

Column 1 shows that firms owned by *Low wealth* entrepreneurs suffer a steep decrease in revenues after an increase in exemptions. Specifically, their revenues decrease on average by almost five percent for a $10,000 increase in the exemption limit. Column 2 further shows that the downsizing of firms owned by *Low wealth* entrepreneurs also reduces their operating efficiency. We interpret these findings as evidence that credit constraints force these firms to operate at a smaller and below-optimal scale (Evans and Jovanovic, 1989).

An inefficient small firm that does not generate adequate revenue should be more prone to failure. In Table 8, we estimate a multiperiod logit model to test whether the passage of the exemption laws affects the probability of firm exit. The exemption limits are expressed in millions of dollars. Because we cannot have firm fixed effects, we include instead state fixed effects and add several firm and owner characteristics to control for time-invariant heterogeneity.[21] As in the previous models, we also include average state house prices, other state variables (median income, unemployment rate, and entry rate), and a full set of credit score dummies (the omitted category is a missing credit score). Standard errors are clustered at the state level.

The results show that firms owned by *Low wealth* entrepreneurs become more likely to fail following an increase in the exemptions. The estimated coefficient indicates that a $10,000 increase in the exemption limit raises the likelihood of failure by four percentage points. This result corroborates our previous findings of a negative

[21] See Table A1 for the complete list of variables.

effect of exemptions on revenue and efficiency for these entrepreneurs. Survival chances of firms owned by the other types of entrepreneurs are not significantly affected.

We note that the negative real effects we find for *Low wealth* entrepreneurs are unlikely due to adverse economic shocks hitting states that raised their exemption limits. As explained before, such economic shocks should also affect companies owned by *No wealth* and *High wealth* entrepreneurs located in the same state. We do not find such evidence. Instead, our results suggest that the reduction in credit supply triggered by the change in exemptions forces affected start-ups to operate at a smaller scale and makes them more likely to fail.

7. Robustness tests

a. Wealth group proxy

The survey question about the net worth of firm owners is introduced in the fourth wave of the KFS, which implies that such information is not available for firms that failed before 2008.[22] To avoid a drastic reduction in our sample size, we estimate the wealth group for these missing cases. Appendix B describes our estimation procedure in detail. The measure of owner wealth that we use in the main tables in the paper is thus a composite measure that combines the reported wealth group (for firm owners who survive until 2008) and the predicted wealth group (for firms that fail before 2008).

Our first robustness test is to re-estimate all our models only for the subsample of firm owners that reported their wealth group. The results, which we report in the Appendix (Tables A3–A6), are similar to those we present in the main tables of the

[22] The response rate for the net worth question is 97%.

paper. We could not estimate the survival regression, because those firms who failed (before 2008) are precisely those that do not report the wealth group.

Our second robustness test is to use the predictive model to estimate the entrepreneur's wealth group conditional on the 2004 covariates for *all* firm owners, even if they reported such information in the 2008 KFS. The results we obtain with these alternative wealth groups are similar to those we report in the paper, and therefore we choose not to report them, but are available upon request.

b. Excluding individual states

During our sample period, a few states raised their exemption limits more than once, and a few states experienced very large increases in their exemption limits (see tables 1A and 1B). For these reasons, it is important to test whether the effects of the exemptions we document are driven by a particular state.

In additional regressions, we find that our results are not overly influenced by any individual state. Specifically, we run, for each dependent variable, 51 separate regressions excluding one state at a time. Appendix figures A1 and A2 plot the coefficient estimates and 95-percent confidence intervals of the effect of exemptions on *Personal credit* and *Business credit*, respectively. We display the effects of the exemptions for the *High wealth* and *Low wealth* groups. The first coefficient in each graph displays the full sample coefficients that we reported in Table 4. All of the estimates are statistically indistinguishable from the full sample results, indicating that the estimated coefficients are stable across specifications.[23]

[23] We reach the same conclusion for the remaining dependent variables, and for the *No Wealth* group. These results are available upon request.

8. Conclusion

Recent evidence highlights that start-ups are important job creators in the U.S. In this paper, we show that recent state changes to personal bankruptcy exemption limits have important effects on the availability of credit, employment, and performance of local start-ups. When a state raises the amount of personal wealth that is protected in bankruptcy, start-ups owned by entrepreneurs that are left with few pledgeable assets suffer a strong reduction in credit availability. In turn, entrepreneurs with high wealth benefit from higher limits on their personal credit cards. Our results therefore indicate that more debtor-friendly bankruptcy regimes redistribute credit from less wealthy to more wealthy entrepreneurs, consistent with the theoretical predictions in Lilienfeld-Toal and Mookherjee (2008).

We also find strong evidence that these credit market frictions triggered by changes in exemptions actually affect young firms' real outcomes. In particular, while high-wealth entrepreneurs increase their labor force, the less wealthy entrepreneurs decrease the number of employees significantly and are less likely to become employers. Consequently, firms owned by less wealthy entrepreneurs perform worse after the increase in exemptions. These firms generate fewer revenues, have lower operating efficiency, and become more likely to fail.

Our results have important policy implications. A higher level of debtor protection reduces entrepreneurs' asset pledgeability. We show that this limited liability constraint reduces credit availability to these entrepreneurs, forcing them to operate their firms at a smaller scale, and making them more vulnerable to failure. Therefore our results confirm that access to capital is an important determinant of start-up growth and survival (Evans and Jovanovic, 1989; Holtz-Eakin et al., 1994). Our study also highlights an unintended consequence in the design of personal

bankruptcy law. The most popular arguments in favor of lenient bankruptcy rules are the protection of borrowers against unexpected events such as illness or job loss, and the preservation of their ex post incentives to work. However, our results indicate that lenient personal bankruptcy laws actually increase the failure probability of the less wealthy entrepreneurs.

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Table 1 – Personal bankruptcy law

Table 1A.
States changing bankruptcy exemption levels, 2005-2009.

Year	States
2005	DE, IN, KY, MO, NV, NY
2006	ID, IL, MN, NC, OR, RI
2007	CO, CT, HI, KY, MI, MN, MT, NE, NJ, NM, NV, PA, WA
2008	AK, ID, ME, MN, OH, RI, SC
2009	CA, NC, ND, OR, WI

Table 1B.
Distribution of changes in state exemption levels, 2005-2009.

Magnitude of exemption limit increase	States
\$5,000 > ΔExemption	MO, MN, CT, HI, MI, KY, PA, NJ, AK, ID
\$25,000 > ΔExemption ≥ \$5,000	KY, IN, NC, IL, OR, SC, RI, ND
\$100,000 > ΔExemption ≥ \$25,000	NY, SC, ID, NE, WA, NM, CO, MN, OH, ME, CA, NC
ΔExemption ≥ \$100,000	DE, NV, RI, MT, MN, WI

Table 2 – Definition of variables and summary statistics

The main dataset is the longitudinal Kauffman Firm Survey (KFS), which includes the 2004, 2005, 2006, 2007, 2008, and 2009 waves. Panel A focuses on the dependent variables, and Panel B focuses on the explanatory variables. We assign a value of $1 million to the states with unlimited homestead exemptions. The statistics displayed account for the KFS sampling weights.

Variable	Definition	Mean	Standard deviation
Panel A. – Dependent variables			
Bank financing			
Personal credit	Total bank financing in the firm owner's name ($000)	38.72	1118.95
Business credit	Total bank financing in the firm's name ($000)	37.28	1417.18
Credit card balance	Balance on personal credit cards ($000)	2.51	12.44
Credit card limit	Maximum credit limit on personal credit cards ($000)	7.71	56.28
Other bank loans	Total credit obtained via other personal loans ($000)	13.94	290.80
Employment			
Number of employees	Number of full-time employees including the firm owner.	3.90	10.31
Firm is employer	= 1 if the firm has external employees	0.49	0.50
Revenue and performance			
Revenue	Total revenue ($000)	450.50	6236.17
Efficiency	= Firm revenue / Number of employees	66.74	1892.81
Failed	= 1 if the firm is no longer in business	0.07	0.25

Variable	Definition	Mean	Standard deviation
Panel B. – Independent variables			
Personal bankruptcy			
Exemptions	Sum of the homestead and personal property bankruptcy exemptions ($000)	255.90	362.73
Owner wealth			
High wealth	= 1 if the net worth of the main firm owner >$250,000	0.48	0.50
Low wealth	= 1 if the net worth of the main firm owner is between $50,000 and $250,000	0.25	0.43
No wealth	= 1 if the net worth of the main firm owner < $50,000	0.28	0.45
State time-varying controls			
Average house price	Average house value in the state ($000)	253.59	121.28
Median income	Median household income in the state ($000)	49.20	6.94
Unemployment rate	Rate of unemployment in the state (in %)	5.81	1.96
Entry rate	Change in the number of establishment due to births (in %)	11.58	1.90
Firm time-varying controls			
Credit risk 1	= 1 if credit score percentile is in the range 91-100	0.03	0.18
Credit risk 2	= 1 if credit score percentile is in the range 71-90	0.14	0.35
Credit risk 3	= 1 if credit score percentile is in the range 31-70	0.44	0.50
Credit risk 4	= 1 if credit score percentile is in the range 11-30	0.15	0.36
Credit risk 5	= 1 if credit score percentile is in the range 1-10	0.09	0.29

Table 3 – Summary statistics by wealth group

The main dataset is the longitudinal Kauffman Firm Survey (KFS), which includes the 2004, 2005, 2006, 2007, 2008, and 2009 waves. We assign a value of $1 million to the states with unlimited homestead exemptions. The statistics displayed account for the KFS sampling weights.

	Means and (standard deviations)		
Owner wealth:	High wealth	Low wealth	No wealth
Bank financing			
Personal credit ($000)	68.80	18.74	35.92
	(1532.55)	(93.54)	(1437.47)
Business credit ($000)	88.16	10.29	8.11
	(2378.02)	(166.95)	(207.97)
Credit card balance ($000)	2.77	2.68	2.84
	(12.83)	(10.6)	(17.04)
Credit card limit ($000)	10.11	8.78	5.74
	(52.26)	(90.64)	(40.86)
Other bank loans ($000)	27.43	6.67	5.02
	(482.18)	(76.63)	(36.56)
Employment			
Number of employees	5.43	3.21	2.65
	(15.13)	(5.87)	(4.62)
Firm is employer	0.55	0.51	0.44
	(0.50)	(0.50)	(0.50)
Revenue and performance			
Revenue ($000)	811.62	382.53	137.07
	(7614.84)	(8972.99)	(1027.60)
Efficiency ($000)	88.16	104.84	26.49
	(575.60)	(4092.92)	(169.45)
Failed	0.05	0.01	0.05
	(0.22)	(0.11)	(0.22)
Personal bankruptcy			
Exemptions ($000)	236.18	234.73	284.30
	(347.89)	(346.59)	(385.88)
State time-varying controls			
Average house price ($000)	254.49	236.01	266.50
	(116.31)	(109.73)	(133.49)
Median income ($000)	49.98	48.70	48.89
	(7.28)	(6.59)	(6.71)
Unemployment rate (%)	5.70	5.85	5.91
	(1.89)	(1.96)	(2.04)
Entry rate (%)	11.50	11.42	11.73
	(1.78)	(1.88)	(1.99)
Firm time-varying controls			
Credit risk 1	0.05	0.03	0.02
	(0.21)	(0.18)	(0.13)
Credit risk 2	0.18	0.15	0.10
	(0.38)	(0.36)	(0.30)
Credit risk 3	0.47	0.47	0.41
	(0.50)	(0.50)	(0.49)
Credit risk 4	0.11	0.16	0.19
	(0.32)	(0.37)	(0.39)
Credit risk 5	0.06	0.07	0.12
	(0.23)	(0.26)	(0.32)

Table 4 – Bank financing: Personal credit and Business credit

The table displays coefficients from panel regression models of the natural log of one plus the dollar amounts of *Personal credit* and *Business credit*. *Exemptions* values are in millions of dollars. The estimated models contain firm fixed effects and year fixed effects interacted with the owner wealth groups. The dataset is the longitudinal version of the Kauffman Firm Survey (KFS), which includes the 2004, 2005, 2006, 2007, 2008, and 2009 waves. Our estimation takes into account the KFS sampling weights. Robust t-statistics (standard errors are clustered at the state level) are provided in parentheses. The symbols ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable:	(1) Log(Personal credit)	(2) Log(Business credit)
Exemptions		
Exemptions × No wealth	3.85	0.06
	(1.25)	(0.05)
Exemptions × Low wealth	-6.18**	0.40
	(-2.26)	(0.26)
Exemptions × High wealth	0.42	1.07
	(0.14)	(0.92)
State time-varying controls		
Average house price (Log)	-0.30	-0.88**
	(-0.47)	(-2.06)
Median income (Log)	0.83	1.60**
	(0.69)	(2.38)
Unemployment rate	0.034	-0.0027
	(0.47)	(-0.044)
Entry rate	0.18*	0.040
	(2.00)	(0.39)
Firm time-varying controls		
Credit risk 1	0.84***	0.60**
	(3.16)	(2.46)
Credit risk 2	0.58**	0.40***
	(2.62)	(3.09)
Credit risk 3	0.52***	0.35***
	(3.16)	(3.80)
Credit risk 4	0.30	0.16*
	(1.67)	(1.87)
Credit risk 5	-0.19	0.15
	(-0.86)	(0.92)
Firm fixed effects	Included	Included
Year × Owner wealth fixed effects	Included	Included
Number of observations	20,150	20,150
R-squared	0.02	0.01
Differential effects of Exemptions: Tests		
Low wealth – No wealth	-10.00***	0.34
	(-2.98)	(0.20)
Low wealth – High wealth	-6.60**	-0.67
	(-1.98)	(-0.36)

Table 5 – Personal credit: Credit cards and Other bank loans

The table displays coefficients from panel regression models of the natural log of one plus the dollar amounts of *Credit card balance*, *Credit card limit*, and *Other bank loans*. *Exemptions* values are in millions of dollars. The estimated models contain firm fixed effects and year fixed effects interacted with the owner wealth groups. The dataset is the longitudinal version of the Kauffman Firm Survey (KFS), which includes the 2004, 2005, 2006, 2007, 2008, and 2009 waves. Our estimation takes into account the KFS sampling weights. Robust t-statistics (standard errors are clustered at the state level) are provided in parentheses. The symbols ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable:	(1) Log(Credit card balance)	(2) Log(Credit card limit)	(3) Log(Other bank loans)
Exemptions			
Exemptions × No wealth	1.85	0.57	0.72
	(0.97)	(0.24)	(0.21)
Exemptions × Low wealth	-3.78**	-5.45***	-3.52**
	(-2.16)	(-2.69)	(-2.27)
Exemptions × High wealth	0.55	3.34**	-0.44
	(0.39)	(2.04)	(-0.29)
State time-varying controls			
Average house price (Log)	-0.69	0.14	-0.50
	(-1.14)	(0.18)	(-1.20)
Median income (Log)	1.45	1.81	-0.58
	(1.49)	(1.55)	(-0.73)
Unemployment rate	-0.023	-0.011	-0.037
	(-0.38)	(-0.13)	(-0.73)
Entry rate	0.028	-0.034	0.059
	(0.38)	(-0.37)	(1.27)
Firm time-varying controls			
Credit risk 1	-0.35**	-0.32	-0.13
	(-2.07)	(-1.27)	(-0.53)
Credit risk 2	-0.046	-0.081	0.22*
	(-0.32)	(-0.38)	(1.94)
Credit risk 3	0.026	-0.0028	0.31***
	(0.25)	(-0.017)	(3.03)
Credit risk 4	0.019	-0.079	0.25**
	(0.16)	(-0.55)	(2.30)
Credit risk 5	-0.28*	-0.38**	0.14
	(-1.71)	(-2.29)	(0.83)
Firm fixed effects	Included	Included	Included
Year × Owner wealth fixed effects	Included	Included	Included
Number of observations	20,150	20,150	20,150
R-squared	0.02	0.04	0.03
Differential effects of Exemptions: Tests			
Low wealth – No wealth	-5.63**	-6.02*	-4.24
	(-2.24)	(-1.82)	(-1.47)
Low wealth – High wealth	-4.33***	-8.79***	-3.08
	(-2.64)	(-4.08)	(-1.36)

Table 6 – Firm employment

The table displays coefficients from panel regression models of the natural log of the *Number of employees* and of the dummy *Firm is employer*. *Exemptions* values are in millions of dollars. The estimated models contain firm fixed effects and year fixed effects interacted with the owner wealth groups. The dataset is the longitudinal version of the Kauffman Firm Survey (KFS), which includes the 2004, 2005, 2006, 2007, 2008, and 2009 waves. Our estimation takes into account the KFS sampling weights. Robust t-statistics (standard errors are clustered at the state level) are provided in parentheses. The symbols ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable:	(1) Log(Number of employees)	(2) Firm is employer
Exemptions		
Exemptions × No wealth	-0.34	-0.10
	(-0.97)	(-0.32)
Exemptions × Low wealth	-0.69**	-0.56**
	(-2.06)	(-2.13)
Exemptions × High wealth	1.55***	0.71***
	(3.00)	(3.07)
State time-varying controls		
Average house price (Log)	-0.11	0.027
	(-1.24)	(0.44)
Median income (Log)	0.0099	0.0012
	(0.067)	(0.011)
Unemployment rate	-0.0076	0.0038
	(-0.64)	(0.47)
Entry rate	0.024*	0.012
	(1.79)	(1.32)
Firm time-varying controls		
Credit risk 1	0.19***	0.11***
	(5.57)	(4.99)
Credit risk 2	0.13***	0.075***
	(5.99)	(4.52)
Credit risk 3	0.084***	0.053***
	(4.43)	(3.88)
Credit risk 4	0.11***	0.047***
	(4.48)	(2.82)
Credit risk 5	0.0013	-0.040**
	(0.045)	(-1.97)
Firm fixed effects	Included	Included
Year × Owner wealth fixed effects	Included	Included
Number of observations	20,150	20,150
R-squared	0.08	0.03
Differential effects of Exemptions: Tests		
Low wealth – No wealth	-0.35	-0.47
	(-0.71)	(-1.18)
Low wealth – High wealth	-2.24***	-1.27***
	(-3.66)	(-3.64)

Table 7 – Firm revenue and efficiency

The table displays coefficients from panel regression models of the natural log of one plus the dollar amounts of *Revenue* and *Efficiency*. *Exemptions* values are in millions of dollars. The estimated models contain firm fixed effects and year fixed effects interacted with the owner wealth groups. The dataset is the longitudinal version of the Kauffman Firm Survey (KFS), which includes the 2004, 2005, 2006, 2007, 2008, and 2009 waves. Our estimation takes into account the KFS sampling weights. Robust t-statistics (standard errors are clustered at the state level) are provided in parentheses. The symbols ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)
Dependent variable:	Log(Revenue)	Log(Efficiency)
Exemptions		
Exemptions × No wealth	-0.95	-0.66
	(-0.22)	(-0.51)
Exemptions × Low wealth	-4.97***	-1.45**
	(-2.64)	(-2.20)
Exemptions × High wealth	0.50	-0.64
	(0.21)	(-0.97)
State time-varying controls		
Average house price (Log)	0.047	0.075
	(0.068)	(0.32)
Median income (Log)	3.37***	1.11***
	(2.72)	(2.69)
Unemployment rate	0.037	0.0083
	(0.39)	(0.27)
Entry rate	0.10	0.022
	(0.99)	(0.66)
Firm time-varying controls		
Credit risk 1	1.55***	0.60***
	(5.23)	(5.61)
Credit risk 2	1.28***	0.45***
	(6.69)	(7.01)
Credit risk 3	0.80***	0.29***
	(5.12)	(5.79)
Credit risk 4	0.86***	0.29***
	(4.73)	(5.06)
Credit risk 5	0.42*	0.21***
	(1.77)	(2.85)
Firm fixed effects	Included	Included
Year × Owner wealth fixed effects	Included	Included
Number of observations	20,150	20,150
R-squared	0.03	0.05
Differential effects of Exemptions: Tests		
Low wealth – No wealth	-4.03	-0.79
	(-0.81)	(-0.46)
Low wealth – High wealth	-5.48*	-0.81
	(-1.91)	(-0.88)

Table 8 – Firm exit

The table displays coefficients from a logit regression of firm failure. *Exemptions* values are in millions of dollars. The estimated model contains state fixed effects. Firm characteristics include legal form dummies and industry dummies. Owner characteristics include the number of hours worked, age dummies, a dummy indicating ownership of other businesses in the same industry, and education dummies. The dataset is the longitudinal version of the Kauffman Firm Survey (KFS), which includes the 2004, 2005, 2006, 2007, 2008, and 2009 waves. Our estimation takes into account the KFS sampling weights. Robust t-statistics (standard errors are clustered at the state level) are provided in parentheses. The symbols ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable:	Failed
Exemptions	
Exemptions × No wealth	0.05
	(0.03)
Exemptions × Low wealth	4.10***
	(3.21)
Exemptions × High wealth	1.61
	(1.56)
State time-varying controls	
Average house price (Log)	0.94***
	(3.42)
Median income (Log)	3.73***
	(8.95)
Unemployment rate	0.094***
	(4.78)
Entry rate	0.0034
	(0.090)
Firm time-varying controls	
Credit risk 1	-0.80***
	(-6.11)
Credit risk 2	-0.36***
	(-4.91)
Credit risk 3	-0.29***
	(-5.07)
Credit risk 4	-0.38***
	(-4.88)
Credit risk 5	0.22***
	(2.81)
State fixed effects	Included
Firm characteristics	Included
Owner characteristics	Included
Number of observations	19,765
Pseudo R-squared	0.10
Differential effects of Exemptions: Tests	
Low wealth – No wealth	4.05***
	(2.60)
Low wealth – High wealth	2.49**
	(2.18)

Appendix for

How does personal bankruptcy law affect start-ups?

A. State exemption laws

For some states, we were able to collect documentation that helps understand the motivation of the state laws that amended exemption limits. We obtained this evidence from various sources, including comments, reports, and public hearings on the proposed bills. According to the evidence collected, the proposals of an increase in the exemption limits were backed by three arguments.

The first and main argument is the gap between the homestead exemption value and current house prices. Proponents of the increase in the exemption levels argued that, since in most states the exemption levels are not updated regularly, sharp increases in house prices and inflation together eroded the homestead's purpose of protecting home ownership. In most cases, the discussion surrounding the approval of the bill focused indeed on the mode of determining a fair homestead value that reflected current house prices. For instance, such discussion is present in "Senate Bill 70", which proposed to increase the homestead exemption in Nevada from $125,000 to $200,000.[24] We note that the discussion was promoted by the Southern Nevada Homebuilders Association and by the local Realtors Association. These lobbyists based their case on the sharp increase in house prices in Las Vegas.

A second argument often used is that skyrocketing medical expenses increased the need of such protection by medical indebted households. This argument is consistent with the evidence in Domowitz and Sartain (1999), who find medical debt

[24] See comment by one attorney at http://law.unlv.edu/sites/default/files/SB70.pdf, accessed 22 January 2014.

to be one of the most important determinants of the consumer bankruptcy decision. The concern of soaring medical expenses is highlighted, for example, in the report on bill HB1805 to raise the homestead exemption in Washington from $40,000 to $100,000, and in the transcripts on proposal LB237 to increase the homestead exemption in Nebraska from $12,500 to $60,000.[25]

The third argument is that the state's exemption level is much lower than the exemptions offered by the other states. Brinig and Buckley (1996) argue that states use bankruptcy law to compete for "deadbeats," i.e., agents who cross state lines to avoid repayment of debts. These "deadbeats" make desirable immigrants, since they bring to the state assets to protect. For instance, the fact that Nebraska is surrounded by three states that have unlimited homestead exemptions (South Dakota, Iowa, and Kansas) may have contributed decisively to the increase in homestead exemption from $12,500 to $60,000, effective on January 2007.

Overall, it seems that the main purpose of increasing homestead exemption levels was to restore a reasonable level of insurance to debtors, which had been eroded by increasing house price values and medical costs. It is important to note that the discussion in the majority of the bills we analyzed was overly influenced by a well-identified group with clear private interests on litigation. The main promoters of the increase in exemption levels were attorneys, law firms, and local bar associations. Hynes et al. (2004) argue that lawyers have strong incentives to lobby in favor of generous debtor protection, as this increases bankruptcy and debt-related litigation. On the opposite side sat representatives of local associations of banks and collectors.

[25] See http://apps.leg.wa.gov/documents/billdocs/200708/Pdf/Bill%20Reports/House/1805.HBR.pdf and http://www.legislature.ne.gov/FloorDocs/100/PDF/Transcripts/Judiciary/2007-01-24.pdf, both accessed 22 January 2014.

B. Wealth group proxy

The entrepreneur's wealth group is a key variable in our analysis. However, information about entrepreneurial wealth is not available in the KFS until its fourth follow-up survey (2008). The entrepreneur's net worth is reported in one of five categories: (i) $0 or less, (ii) $1 to $50,000, (iii) $50,001 to $100,000, (iv) $100,001 to $250,000, and (v) $250,001 and up. Of the 4,928 firms in the baseline sample, some firms went out of business. As a result, information about net worth is only available for 2,650 firms (about 53% of the initial sample).

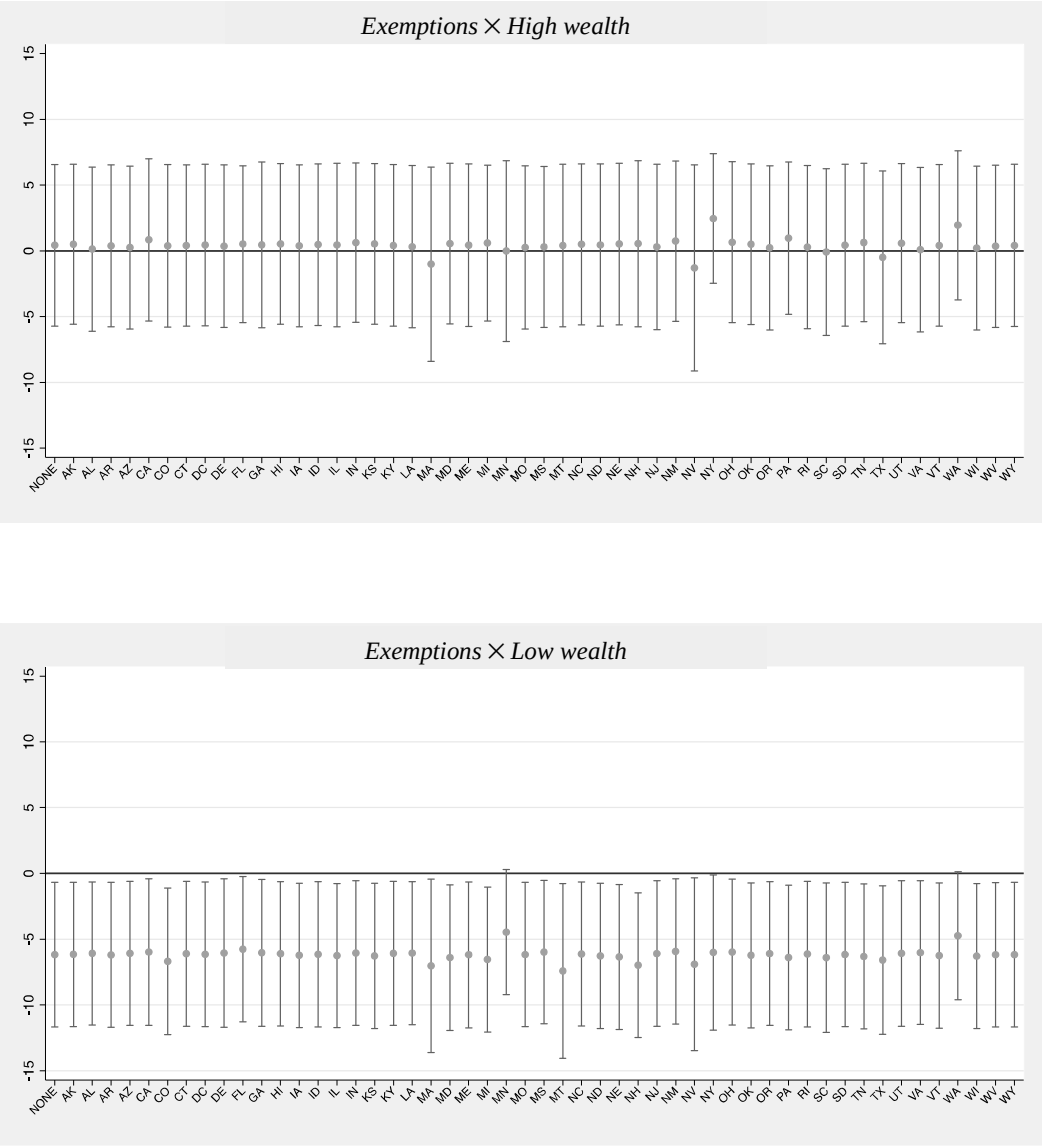
The missing information about wealth in the early waves of the KFS raises several problems for our empirical analysis. First, the missing data greatly reduces the number of sampled firms. Second, restricting the analysis to the subsample of firms that survived to 2008 may raise concerns about sample selection. Third, dropping firms that fail during their first years of operation makes it almost unfeasible to study firm survival. Below, we present a simple predictive model of the entrepreneur's wealth group that allows us to address each of these concerns.

The 2008 KFS reports entrepreneur wealth divided into five bins: (i) $0 or less, (ii) $1 to $50,000, (iii) $50,001 to $100,000, (iv) $100,001 to $250,000, and (v) $250,001 and up. We estimate a cross-sectional ordered logit regression model where the dependent variable is the wealth bin reported. An ordered model takes into account the ordinal nature of our wealth variable and hence it is more appropriate than multinomial models. In order to predict the entrepreneur's wealth group, we employ several covariates measured as of 2004, such as several demographic characteristics of the entrepreneur, location, economic information at the Zip-code level (obtained from the 2000 Census), and the legal form of the company. Table A1 lists all covariates used to predict the entrepreneur's wealth group.

In Table A2, we provide for each reported wealth group sample means and standard deviations for several of the model's explanatory variables. The table shows that there are substantial differences between wealth groups. Wealthier entrepreneurs have on average better human capital (i.e., they have more business experience and a higher education level) and they are less likely to be female or to belong to a minority group. As expected, *High wealth* entrepreneurs are located in zip codes with substantially higher average income and house prices. There are no significant differences between the two lower wealth groups with respect to location. Finally, wealthier entrepreneurs are less likely to adopt an unlimited liability form of organization.

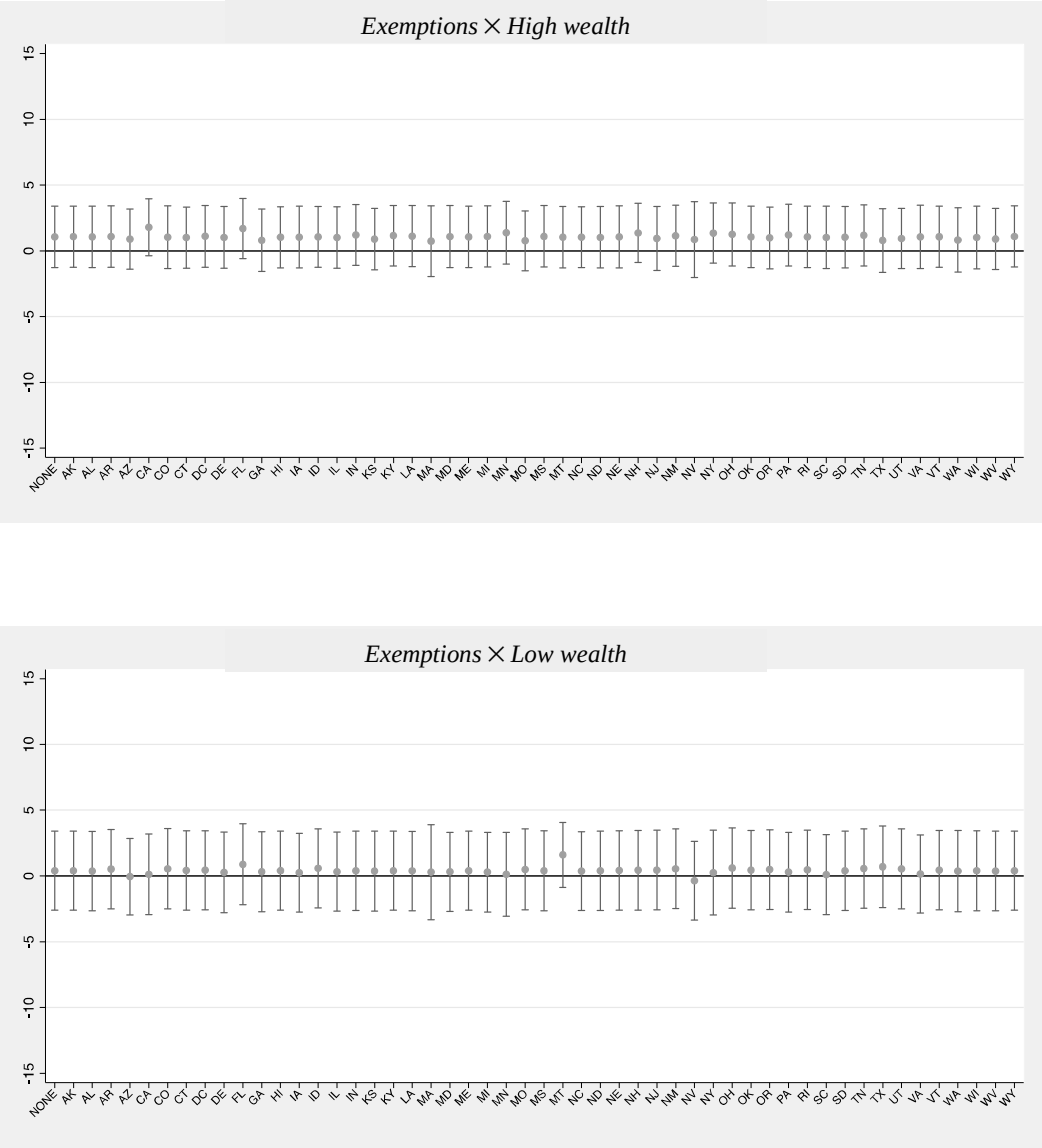
After the estimation procedure, we compute the matrix containing the predicted probabilities of an entrepreneur falling in each of the wealth bins. We assign entrepreneurs to the wealth groups using a cut-off of 50%. That is, if the predicted probability of an entrepreneur belonging to a given wealth bin is higher than 0.5, we assign the entrepreneur to the corresponding wealth group. If all predicted probabilities are lower than 0.5, we discard the firm from the sample. This methodology enables us to recover 1,000 firms for which the wealth of the owners was not originally available. Our final sample contains 3,600 firms.

Figure A1 – Effect of exemptions on personal credit excluding each state



Notes: The figures display the estimated coefficients and 95-percent confidence intervals of the impact of the exemptions, for the *High wealth* group (top) and for the *Low wealth* group (bottom) on *Log(Personal credit)*, using the same specification as Table 4 – Column 1 and in subsamples that exclude each state. The horizontal axis indicates the state that is excluded from the estimation sample. The first coefficient ("NONE") includes the full sample.

Figure A2 – Effect of exemptions on business credit excluding each state



Notes: The figures display the estimated coefficients and 95-percent confidence intervals of the impact of the exemptions, for the *High wealth* group (top) and for the *Low wealth* group (bottom) on *Log(Business credit)*, using the same specification as Table 4 – Column 2 and in subsamples that exclude each state. The horizontal axis indicates the state that is excluded from the estimation sample. The first coefficient ("NONE") includes the full sample.

Table A1 – Predictors of the firm owner's wealth group

The table list all variables used to predict the wealth group of firm owners. All variables are measured as of 2004. The dataset is the longitudinal version of the Kauffman Firm Survey (KFS), which includes the 2004, 2005, 2006, 2007, 2008, and 2009 waves.

Variable	Description
Owner demographics	
Education	Dummies: Less than 9th grade; Some high school, but no diploma; High school graduate; Technical, trade or vocational degree; Some college, but no degree; Associate's degree; Bachelor's degree; Some graduate school but no degree; Master's degree; Professional school or doctorate.
Age	Dummies: 18-24, 25-34, 35-44, 45-54, 55-64, 65-74, and 75 or more.
Years of experience	Years of work experience in the industry.
Number of businesses	Number of other businesses started.
Female	= 1 if the owner's gender is female.
Race	Dummies: Hispanic; American Indian or Alaska Native; Native Hawaiian or other Pacific Islander; Asian; Black or African American; White; Other.
Firm characteristics	
Industry	One digit SIC codes
Legal form	Dummies: Sole Proprietorship; Limited Liability Company; S-Corporation; C-Corporation; General Partnership; Limited Partnership; Other.
Zip-level information	
Average house value	Average house value in the firm's ZIP code (2000 Census)
Average income	Average income per household in the firm's ZIP code (2000 Census).
Location	
State	State dummies.
MSA	= 1 if firm is located in a MSA.

Table A2 – Summary statistics of wealth group predictors

The table displays summary statistics (means and standard deviations) of a subset of variables used to predict the wealth group (see Table A1 for the complete list). All predictors are measured as of 2004. The wealth groups are based on the net worth reported in the 2008 and 2009 KFS. The statistics displayed account for the KFS sampling weights.

	Means and (standard deviations)		
Reported owner wealth:	High wealth	Mid wealth	Low wealth
Owner experience			
Years of experience	14.49	12.59	10.34
	(11.57)	(10.68)	(9.04)
Number of businesses	1.18	0.84	0.77
	(2.75)	(1.64)	(1.77)
Owner education			
High school diploma	0.24	0.37	0.41
	(0.43)	(0.48)	(0.49)
College or technical degree	0.49	0.48	0.43
	(0.50)	(0.50)	(0.50)
Graduate degree	0.27	0.13	0.12
	(0.44)	(0.34)	(0.32)
Owner gender and race			
Female	0.28	0.30	0.31
	(0.45)	(0.46)	(0.46)
Minority	0.09	0.14	0.25
	(0.29)	(0.34)	(0.43)
Zip-level information			
Average house value ($000)	174.44	139.80	139.78
	(116.85)	(87.44)	(80.03)
Average income ($000)	53.74	47.95	47.37
	(20.23)	(16.65)	(17.12)
Legal form			
Unlimited liability	0.28	0.46	0.52
	(0.45)	(0.50)	(0.50)

Table A3 – Personal credit and Business credit (reported wealth group only)

This table replicates Table 4 of the paper using only reported wealth. The table displays coefficients from panel regression models of the natural log of one plus the dollar amounts of *Personal credit* and *Business credit*. *Exemptions* values are in millions of dollars. The estimated models contain firm fixed effects and year fixed effects interacted with the owner wealth groups. The dataset is the longitudinal version of the Kauffman Firm Survey (KFS), which includes the 2004, 2005, 2006, 2007, 2008, and 2009 waves. Our estimation takes into account the KFS sampling weights. Robust t-statistics (standard errors are clustered at the state level) are provided in parentheses. The symbols ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable:	(1) Log(Personal credit)	(2) Log(Business credit)
Exemptions		
Exemptions × Low wealth	1.33	-0.40
	(0.36)	(-0.26)
Exemptions × Mid wealth	-6.26**	0.30
	(-2.27)	(0.20)
Exemptions × High wealth	-0.66	-0.11
	(-0.30)	(-0.09)
State controls		
Average house price (Log)	-0.35	-0.85**
	(-0.46)	(-1.99)
Median income (Log)	0.45	1.53**
	(0.36)	(2.13)
Unemployment rate	0.003	-0.02
	(0.036)	(-0.28)
Entry rate	0.19*	0.04
	(1.68)	(0.32)
Firm controls		
Credit risk 1	0.66**	0.43
	(2.17)	(1.58)
Credit risk 2	0.52**	0.34**
	(2.03)	(2.22)
Credit risk 3	0.51***	0.28**
	(2.74)	(2.48)
Credit risk 4	0.39**	0.23**
	(2.20)	(2.05)
Credit risk 5	-0.065	0.23
	(-0.26)	(1.21)
Firm FE	Included	Included
Year × Owner wealth FE	Included	Included
Number of observations	15,948	15,948
R-squared	0.01	0.01
Differential effects of Exemptions: Tests		
Mid wealth – Low wealth	-7.60**	0.70
	(-2.07)	(0.36)
Mid wealth – High wealth	-5.60*	0.41
	(-1.79)	(0.20)

Table A4 – Credit cards and Other bank loans (reported wealth group only)

This table replicates Table 5 of the paper using only reported wealth. The table displays coefficients from panel regression models of the natural log of one plus the dollar amounts of *Credit card balance*, *Credit card limit*, and *Other bank loans*. *Exemptions* values are in millions of dollars. The estimated models contain firm fixed effects and year fixed effects interacted with the owner wealth groups. The dataset is the longitudinal version of the Kauffman Firm Survey (KFS), which includes the 2004, 2005, 2006, 2007, 2008, and 2009 waves. Our estimation takes into account the KFS sampling weights. Robust t-statistics (standard errors are clustered at the state level) are provided in parentheses. The symbols ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable:	(1) Log(Credit card balance)	(2) Log(Credit card limit)	(3) Log(Other bank loans)
Exemptions			
Exemptions × Low wealth	0.63	-1.57	0.48
	(0.31)	(-0.63)	(0.13)
Exemptions × Mid wealth	-3.91**	-5.56***	-3.48**
	(-2.21)	(-2.75)	(-2.24)
Exemptions × High wealth	0.06	3.87**	-1.14
	(0.05)	(1.97)	(-0.70)
State controls			
Average house price (Log)	-0.69	0.12	-0.45
	(-0.86)	(0.13)	(-0.88)
Median income (Log)	0.96	1.83	-0.36
	(0.95)	(1.36)	(-0.41)
Unemployment rate	-0.07	-0.05	-0.03
	(-1.02)	(-0.55)	(-0.43)
Entry rate	-0.01	-0.07	0.06
	(-0.07)	(-0.77)	(1.14)
Firm controls			
Credit risk 1	-0.53***	-0.59**	-0.23
	(-2.78)	(-2.03)	(-0.81)
Credit risk 2	-0.15	-0.23	0.24*
	(-0.83)	(-0.83)	(1.69)
Credit risk 3	-0.01	-0.07	0.33***
	(-0.09)	(-0.36)	(3.02)
Credit risk 4	-0.04	-0.19	0.28**
	(-0.27)	(-1.05)	(2.11)
Credit risk 5	-0.32	-0.47*	0.07
	(-1.53)	(-1.92)	(0.44)
Firm FE	Included	Included	Included
Year × Owner wealth FE	Included	Included	Included
Number of observations	15,948	15,948	15,948
R-squared	0.01	0.02	0.02
Differential effects of Exemptions: Tests			
Mid wealth – Low wealth	-4.54*	-3.99	-3.96
	(-1.67)	(-1.14)	(-1.23)
Mid wealth – High wealth	-3.97**	-9.43***	-2.34
	(-2.45)	(-3.79)	(-1.03)

Table A5 – Firm employment (reported wealth group only)

This table replicates Table 6 of the paper using only reported wealth. The table displays coefficients from panel regression models of the natural log of the *Number of employees* and of the dummy *Firm is employer*. *Exemptions* values are in millions of dollars. The estimated models contain firm fixed effects and year fixed effects interacted with the owner wealth groups. The dataset is the longitudinal version of the Kauffman Firm Survey (KFS), which includes the 2004, 2005, 2006, 2007, 2008, and 2009 waves. Our estimation takes into account the KFS sampling weights. Robust t-statistics (standard errors are clustered at the state level) are provided in parentheses. The symbols ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable:	(1) Log(Number of employees)	(2) Firm is employer
Exemptions		
Exemptions × Low wealth	-0.29	-0.21
	(-0.82)	(-0.68)
Exemptions × Mid wealth	-0.70**	-0.58***
	(-2.22)	(-2.79)
Exemptions × High wealth	0.98**	0.57***
	(1.97)	(2.41)
State controls		
Average house price (Log)	-0.04	0.05
	(-0.41)	(0.70)
Median income (Log)	0.02	-0.04
	(0.13)	(-0.39)
Unemployment rate	-0.01	0.01
	(-0.49)	(0.64)
Entry rate	0.02	0.01
	(1.22)	(1.41)
Firm controls		
Credit risk 1	0.20***	0.08***
	(6.23)	(2.87)
Credit risk 2	0.13***	0.05***
	(5.64)	(3.12)
Credit risk 3	0.07***	0.03*
	(3.33)	(1.87)
Credit risk 4	0.08**	0.03
	(2.53)	(1.26)
Credit risk 5	-0.02	-0.03
	(-0.80)	(-1.48)
Firm FE	Included	Included
Year × Owner wealth FE	Included	Included
Number of observations	15,948	15,948
R-squared	0.08	0.05
Differential effects of Exemptions: Tests		
Mid wealth – Low wealth	-0.41	-0.37
	(-0.83)	(-1.15)
Mid wealth – High wealth	-1.69***	-1.15***
	(-2.76)	(-3.99)

Table A6 – Firm revenue and efficiency (reported wealth group only)

This table replicates Table 7 of the paper using only reported wealth. The table displays coefficients from panel regression models of the natural log of one plus the dollar amounts of *Revenue* and *Efficiency*. *Exemptions* values are in millions of dollars. The estimated models contain firm fixed effects and year fixed effects interacted with the owner wealth groups. The dataset is the longitudinal version of the Kauffman Firm Survey (KFS), which includes the 2004, 2005, 2006, 2007, 2008, and 2009 waves. Our estimation takes into account the KFS sampling weights. Robust t-statistics (standard errors are clustered at the state level) are provided in parentheses. The symbols ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)
Dependent variable:	Log(Revenue)	Log(Efficiency)
Exemptions		
Exemptions × Low wealth	-4.34	-1.81
	(-1.30)	(-1.49)
Exemptions × Mid wealth	-5.03***	-1.46**
	(-2.71)	(-2.27)
Exemptions × High wealth	-2.69	-1.24
	(-0.91)	(-1.30)
State controls		
Average house price (Log)	0.08	0.11
	(0.14)	(0.55)
Median income (Log)	3.51***	1.11***
	(3.01)	(3.57)
Unemployment rate	0.06	0.02
	(0.72)	(0.53)
Entry rate	0.14*	0.03*
	(1.78)	(1.78)
Firm controls		
Credit risk 1	1.18***	0.50***
	(5.44)	(6.00)
Credit risk 2	1.06***	0.40***
	(5.39)	(5.91)
Credit risk 3	0.65***	0.26***
	(4.28)	(5.09)
Credit risk 4	0.87***	0.31***
	(5.14)	(5.06)
Credit risk 5	0.57**	0.27***
	(2.02)	(3.14)
Firm FE	Included	Included
Year × Owner wealth FE	Included	Included
Number of observations	15,948	15,948
R-squared	0.05	0.08
Differential effects of Exemptions: Tests		
Mid wealth – Low wealth	2.34	0.22
	(0.96)	(0.25)
Mid wealth – High wealth	0.69	-0.35
	(0.16)	(-0.23)