Debtor Rights, Credit Supply, and Innovation

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Abstract

Debtor-friendly laws can encourage innovation by reducing the cost of failure for innovators, but can also harm innovation if they tighten the availability of credit to innovators. We use state and year variation in U.S. personal bankruptcy laws, which affect the capital constraints of individual innovators and small firms, to investigate the effects of debtor protection on innovation. We find that stronger debtor protection *decreases* the total number of patent applications by small firms without increasing the average quality of their patents. The negative effect of debtor protection on innovation is amplified in industries with a high dependence on external finance and in concentrated banking markets. We also find that stronger debtor protection leads firms to innovations that can be categorized as "safer bets." Our results suggest that the negative credit supply effect of debtor protection can dominate the positive tolerance-for-failure effect and thus *reduce* innovation.

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1. Introduction

Innovation is now widely recognized as one of the key drivers of economic growth (Abramowitz, 1956; Solow, 1957; Romer, 1990; King and Levine, 1993; Acemoglu, 2008). A recent literature focuses on the role of institutional settings in shaping agents' incentives to innovate (Holmstrom, 1989; Kortum and Lerner, 2000; Landier, 2006; Manso, 2011; Nanda and Rhodes-Kropf, 2012). One important corollary of some of these studies is that institutions and laws that protect inventors when they fail encourage inventors to engage in risky experimentation, thereby fostering innovation. Consistent with this prediction, Tian and Wang (forthcoming) show that startup firms financed by more failure-tolerant VC investors are more innovative. Similarly, Acharya and Subramanian (2009) show that more debtor-friendly bankruptcy codes lead to higher corporate innovation.

In this paper, we argue that these tests of the effects of debtor protection are incomplete since they neglect how the institutional environment affects the incentives of financiers to fund risky ventures. To see this, suppose that a regulator attempts to stimulate innovation through the adoption of a lenient bankruptcy code. From the inventor's perspective, the legal change reduces the downside risk of experimentation because the new code protects the inventor in bankruptcy. This is the *tolerance-for-failure* effect. At the same time, however, the legal change also increases the exposure of creditors to the risk that the inventor may fail and default on her debt obligations. Thus, creditors may reduce the availability of funds to finance the inventor's risky projects. This is the *credit supply* effect. If this negative credit supply effect dominates the tolerance-for-failure effect, then more lenient bankruptcy codes could lower the debtors' levels of spending on risky activities and thereby lead to less innovation.

To examine the competing effects of debtor protection and credit supply on innovation, we study how state and year variation in U.S. personal bankruptcy law affects the amount and quality of innovative activity. To the best of our knowledge, our paper is the first to empirically investigate these competing effects. Under U.S. Chapter 7 Bankruptcy, debtors must turn over to their creditors any unsecured assets they own above a predetermined exemption limit. A higher exemption limit makes the institutional setting friendlier towards debtors, since it reduces the amount of assets that creditors can seize in bankruptcy.

We focus our study on personal bankruptcy and small firm innovation for several reasons. First, the exemption limits vary widely across states and time, providing us with a quasi-natural experiment. Second, a number of prior studies, including Gropp et al. (1997) and Berger et al. (2011), have shown that higher debtor protection, in the form of higher personal bankruptcy exemptions, leads to less credit available to borrowers. Third, tighter credit supply impacts not only individuals, but also small firms which often use personal liabilities and guarantees to finance their ventures.[1] Moreover, individual inventors and small firms depend heavily on banks as an importance source of financing, as they typically have limited access to other sources of external financing (Berger et al., 2011; Robb and Robinson, forthcoming). Finally, the focus on small firms complements Acharya and Subramanian (2009), who show that higher corporate debtor protection leads to more innovation in large firms. However, they focus on large firms where the credit supply effect may be less binding than it is for small firms, owing to the number

[1] The owners of an unincorporated (i.e., unlimited liability) firm are legally liable for the firm's debts, because there is no legal distinction between the company and its owners. Therefore, personal bankruptcy laws apply directly to unincorporated firms, such as sole proprietorships. In an incorporated (i.e., limited liability) firm, such as a corporation or a limited liability company (LLC), the firm is a separate entity from its owners. However, owners of incorporated businesses become personally liable for the business debts whenever they: (i) borrow at the personal level to finance the firm, or (ii) cosign or personally guarantee business loans. Evidence that bankruptcy exemptions affect both unincorporated firms and small incorporated businesses is provided in Berkowitz and White (2004), Berger et al. (2011) and Cerqueiro and Penas (2013). Cerqueiro and Penas (2013) also document that a large fraction of incorporated start-up companies use personal loans from their owners to finance the companies' operations.

of alternative sources of finance available to large firms such as debt and equity markets. In addition, small firms are an important source of innovation in their own right (Acs and Audretsch, 1988; Lerner, 2009).

We have several sets of findings. First, we find that the passage of state laws that raise exemption limits significantly decrease the number of patents filed by small firms. We obtain this result after controlling for state median income, house price index, and population, and after including fixed effects at the calendar year, state, and patent subclass levels, and patent subclass-specific trends. This finding also holds when we include patents assigned to individual inventors, or when we restrict our analysis to patents assigned to privately-held small firms. We also show that the decrease in patenting is driven by a reduction in the number of innovators rather than the innovative output per inventor.

Second, we find that the negative effect of debtor protection on patenting is amplified in industries with a high dependence on external finance and in concentrated banking markets. The latter result corroborates the growing evidence of a negative relationship between banking market concentration and innovation by small firms (Benfratello et al., 2008; Chava et al., forthcoming; Cornaggia et al., forthcoming). These two findings suggest that the decrease in patenting is driven by a reduction in credit availability in response to higher exemption limits.

Third, we analyze the effect of the exemptions on several commonly employed proxies for patent quality (Hall et al., 2001). Our measures of patent quality are its *impact* (based on the number of citations received), *originality* (based on how many different patent subclasses this patent cites), and *generality* (based on how many different patent subclasses cite this patent). We find no robust effect of the exemptions on the quality of the patents produced: Hence, the

decrease in patenting associated with higher exemption limits is not accompanied by an increase in the average quality of patents.

Fourth, we also analyze how the exemptions affect firm exploration and the riskiness of the projects firms undertake. To analyze riskiness, we examine how exemptions affect the upper and lower tails of the distribution of the patent citations (as in Chava et al., forthcoming). To analyze firm exploration, we examine how exemptions affect the number of different patent subclasses in which firms' patent. We find that an increase in exemptions reduces the incidence of extremely successful and unsuccessful patents, and makes affected firms less likely to patent across multiple subclasses. These results suggest that the exemptions lead to a particularly sharp decline in the supply of credit for riskier, more explorative projects and lead firms to take "safer bets". This result is consistent with the predictions in Nanda and Rhodes-Kropf (2012) that show that policies aimed at protecting innovators could result in fewer radical innovations. In short, more debtor friendly laws appear to *reduce* innovation and exploration.

Our analysis addresses several important identification-related concerns. First, the panel structure of our data enables us to use state fixed effects to control for unobserved heterogeneity across states and account for the possibility that high exemption states may attract a different (e.g., less innovative) pool of entrepreneurs. The evidence in Fan and White (2003) corroborates this possibility, as they find that individuals in high exemption states are substantially more likely to be self-employed (see also Armour and Cumming, 2008). Second, current or expected future state economic conditions could influence the passage of the exemption laws in a state. To tackle this concern, we compare the effects of exemption laws on small firms and large firms. Large firms provide an excellent counterfactual, because personal bankruptcy law should not affect this group of firms. We find no effect of the exemptions on innovation by large firms,

suggesting that our results are unlikely to be driven by state-specific changes in macroeconomic conditions. Finally, we exploit the timing of the exemptions and show that there is no statistically significant effect one to three years before their passage, thus confirming that our results are not driven by reverse causality.

The paper proceeds as follows. Section 2 overviews personal bankruptcy laws. Section 3 reviews the background literature on which we build our hypotheses. Section 4 describes the empirical strategy and the data. Section 5 presents the results. Section 6 concludes.

2. U.S. Personal bankruptcy law

When an individual files for personal bankruptcy, all collection efforts by creditors cease. In our sample period of 1995-2005, debtors in the U.S. could choose between two different personal bankruptcy procedures: Chapter 7 and Chapter 13. Under Chapter 13, debtors can keep all of their assets, but they must propose a repayment plan. This plan typically involves using a portion of the debtor's future earnings over a five-year period to repay debt. Repayment plans must give creditors the same amount they would receive under Chapter 7, but no more. Under Chapter 7, all of the debtor's future earnings are exempt from the obligation to repay – the "fresh start" principle. The "fresh start" is mandated by Federal law, and applies throughout the U.S. However, debtors who file for bankruptcy under Chapter 7 must turn over any unsecured assets they own above the relevant state's exemption limit.[2] Therefore, creditors cannot enforce claims against debtors' assets if the value of these assets is lower than the exemption limit. In 1978, Congress adopted a uniform federal bankruptcy exemption, but gave the states the right to opt out and to adopt their own exemption levels. By the beginning of the 1980s, two-thirds of states

[2] Although secured claims cannot be discharged under Chapter 7, the distinction between secured and unsecured credit is less meaningful in practice, since individuals are often able to arbitrage debts across categories (Gropp et al., 1997). For instance, debtors could borrow on their credit cards in order to reduce their mortgage debts.

had opted out. Exemption limits vary widely across states as a result.[3]

During our sample period, more than 70% of total bankruptcy filings were under Chapter 7. Debtors have an incentive to choose Chapter 7 rather than Chapter 13 whenever their assets are less than the exemption limit. This strategy maximizes debtors' financial benefit from filing, as they are able to preserve both their current assets and their future income. But even when debtors file for bankruptcy under Chapter 13, the exemption limits in Chapter 7 affect the amounts that these debtors are willing to repay (see Gropp et al., 1997). As a result, we focus solely on the state exemption limits.

There are generally two types of asset exemptions: for equity in owner-occupied residences (the homestead exemption), and for various other types of personal assets (the personal property exemption). Homestead exemptions specify a dollar amount of equity that the debtor is entitled to protect in the event of bankruptcy. Personal property exemptions apply to other specified assets, such as jewelry and motor vehicles. In many states, however, the law leaves unspecified the value of many of these assets. Our measure of personal property exemptions comprises only assets that have specific dollar amounts in all states: jewelry, motor vehicle, cash and deposits, and a "wildcard" (an exemption that applies to any property). In our empirical analysis we use an aggregate measure of state exemptions that includes both the homestead exemptions and the personal property exemptions.

During the period of our study, several states enacted laws that increased their exemption levels. We consider as an exemption increase any increase in either the homestead exemption or the personal property exemptions (in any of the asset categories mentioned above). Table 1A

[3] Some states allow their residents to choose between the state and the federal exemptions. In these cases, we selected the option which grants the claimant with 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."

provides information about the timing of the exemption laws. The date we assigned to the exemption change is the year that the law became effective. In some cases, there is a one year gap between the year the law is approved by the governor and the year the law becomes effective. Table 1A shows that with the exception of 2004 the exemption law changes are distributed relatively uniformly across our sample period. Table 1B shows that there is also substantial variation in the magnitude of the changes in the exemption limits. Both the staggered timing of the exemption laws and the variation in exemption amounts are important ingredients in our empirical strategy, which we explain in detail in a separate section.

We are not aware of any evidence that exemption laws were designed to promote entrepreneurship or innovation. Cerqueiro and Penas (2013) analyze the political discussions surrounding the exemption laws passed during 2004-2009 and do not find any evidence that would suggest that innovation policy led to the passage of these laws. According to their study, the argument most often invoked to support the increases in exemption values was the increase in house values. In our analysis, we explicitly account for house prices using state-specific house-price indices. In addition, Hynes, Malani and Posner (2004) find that the single biggest determinant of state exemption levels in the 1990s are exemption levels in the 1920s. In our analyses, we use state fixed effects to account for unobserved state specific factors that might potentially drive exemption levels and innovative activity.

3. Background literature

3.1. Bankruptcy exemptions, the credit market, and incentives to innovate

A higher exemption limit allows debtors to shield a higher asset value from creditors in bankruptcy. Therefore, an increase in the exemption limit should affect the *ex ante* incentives of

both debtors and creditors. On the one hand, a higher exemption level increases the asset value that debtors can keep in bankruptcy. Therefore, bankruptcy exemptions provide wealth insurance to individuals when such insurance is most valuable. Landier (2006) and Manso (2011) argue that institutional environments that are more tolerant towards failure provide greater incentives for innovation. In this sense, softer bankruptcy laws that protect entrepreneurs against failure could make these entrepreneurs more inclined to innovate ex ante. Consistent with this, prior empirical research suggests that higher exemptions increase the likelihood that an individual becomes an entrepreneur (Fan and White, 2003; Armour and Cumming, 2008).

On the other hand, a higher exemption level reduces the asset value that creditors can seize in bankruptcy. The higher the exemption level, the greater the incentive to default on outstanding loans and file for bankruptcy. Therefore, exemptions may give rise to opportunistic behavior on the part of borrowers and exacerbate moral hazard problems in the credit markets. If higher exemptions decrease credit availability to innovators, this may then reduce R&D and hamper innovation. Prior empirical evidence provides strong evidence that higher exemption levels reduce the availability of credit to both households and firms. Using the *Survey of Consumer Finances*, Gropp et al. (1997) find that households are more likely to be turned down for credit or discouraged from borrowing when they are located in states with higher bankruptcy exemptions. Lin and White (2001) use Home Mortgage Disclosure Act data and find that mortgage applicants are more likely to be turned down when exemptions are high.[4] Personal bankruptcy law applies directly to firms of unlimited liability form, and it can also affect small corporations in that banks often require owners of these firms to personally guarantee the loan. Berkowitz and White (2004), and Berger et al. (2011) use data from the *Surveys of Small*

[4] They explain that although bankruptcy exemptions apply only to unsecured debts, they could also affect mortgages, since debtors have incentives to arbitrage debts across categories to increase their financial benefit from bankruptcy. For instance, debtors could use credit card debt in order to reduce their mortgage debt.

Business Finance to show that credit availability to small and medium enterprises is lower in states with higher exemptions. These studies also find that higher exemptions increase loan denial rates, increase loan rates, and reduce the amount and maturity of these loans. Cerqueiro and Penas (2013) exploit state-level changes in exemption laws and data from the *Kaufman Firm Survey* to study the effect of the exemptions on the financing structure and size of start-ups. They find that an increase in exemptions reduces the inflow of bank financing to start-ups located in that state.

How the exemption laws should affect innovation is therefore ambiguous. The net effect should result from the interaction between the (positive) tolerance-for-failure effect and the (negative) credit supply effect. In a recent working paper, Nanda and Rhodes-Kropf (2012) model these two forces (i.e., tolerance-for-failure and the access to external funds) to determine the ex-ante strategy of a firm, investor or government that maximizes profits or promotes innovation. They find that a strategy that is failure tolerant will increase the entrepreneur's willingness to experiment but will decrease investors' willingness to fund experimentation. They show that financiers that are more tolerant towards failure endogenously choose to fund *less* radical innovations. They also show that strategies that are more tolerant will encourage innovation on the margin but will change what financiers are willing to fund, reducing innovation on the extensive margin.

The findings in Acharya and Subramanian (2009) suggest that large corporations in countries with more debtor-friendly corporate bankruptcy laws are more innovative. While this result is consistent with the tolerance-for-failure view, the focus of this study is on large firms for which the bank credit channel may be less important as these firms are less credit constrained

and have more access to other sources of funds than small and young firms, which are the focus of our study.[5]

3.2. Competition in the credit market and incentives to innovate

A growing literature analyzes how the structure of the banking market affects firms' innovative activities. High concentration in banking markets increases the market power of banks and, thereby, reduces credit supply. Benfratello et al. (2008) exploit a rich dataset of Italian firms and show that process innovations are less likely in provinces with more concentrated banking markets. Our empirical analysis also exploits spatial variation in a local measure of bank competition as in Benfratello et al. (2008) to determine whether the effect of exemptions on innovation depends on the characteristics of the credit market. Ng (2012), Chava et al. (2013) and Cornaggia et al. (forthcoming) exploit the deregulation in the U.S. banking industry, and also find that bank concentration is detrimental to innovation.

4. Empirical strategy and data

4.1. Empirical strategy

We use state-level changes in bankruptcy exemptions from 1995-2005 to identify the effect of debtor protection laws on innovation. We focus on this period because it is relatively free from contamination by other types of regulatory policies. In particular, the Riegle-Neal Interstate Banking and Branching Efficiency Act, which opened up nationwide acquisition of banks across state lines, was passed in 1994. A number of studies have linked the state-by-state bank branching deregulation that preceded the Riegle-Neal Act of 1994 to innovation and

[5] The recent study by Robb and Robinson (forthcoming) documents that bank financing is the most import source of external funding for start-ups during their first years of operation

entrepreneurial outcomes (e.g., Kerr and Nanda, 2009; Chava et al., 2013; Cornaggia et al., forthcoming). Second, the Bankruptcy Abuse Prevention and Consumer Protection Act (BAPCA) was passed in late 2005. According to Paik (2013), BAPCA introduced bankruptcy provisions that were more favorable to creditors.

Following a well-established tradition, we measure innovation using various patent-based proxies, described in detail in Section 4.2., and estimate Poisson models that take into account the count nature of our patent based measures (see Hausman, Hall, and Griliches, 1984). The model has the general form:

$$E[Innovation_{sct}] = exp\left(\alpha_s + \delta_c + \lambda_t + \beta Exemption_{st} + X_{st}\right), \qquad (1)$$

where *s* indexes the state, *c* indexes the patent subclass, *t* indexes calendar years, $Exemption_{st}$ is the exemption level in state *s* in year *t*, and X_{st} are other state-year variables. In some specifications, we also control for different trends across patent subclasses. The parameter of interest, β, measures the permanent effect of an increase in the state exemptions on innovation activity.

We emphasize that our empirical setting allows us to perform a more powerful test than the standard differences-in-differences (DID) approach. The DID approach typically compares the average outcome across the treated and control groups, before and after the treatment and thus provides an estimate of the outcome from comparing the means across four groups. Our methodology improves on this in two important ways.[6] First, we are able to achieve more convincing causal estimates by exploiting the staggered timing of the exemption laws (see Table 1A). To see this, consider a state that passes an exemption law at time *t*. Our control group is not

[6] Our empirical methodology is similar to that used in Amore et al. (forthcoming), Chava et al. (forthcoming), and Hombert and Matray (2012), who study the effect of the staggered deregulation in the U.S. banking industry on innovation.

restricted to states that never raised exemptions. Our methodology also 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. In practice, this means that any state that passes an exemption law belongs to both the treated and control groups. The staggered timing of the exemption laws provides variation across both states and time, reducing the concern that unobserved state-specific shocks affecting innovation drive our results.

Second, the different exemption laws passed represent in fact a wide range of treatment effects (see Table 1B). Thus, our methodology implicitly assumes that the magnitude of the treatment is proportional to the increase in the exemption value, i.e., a higher increase in exemption value should trigger a stronger reaction in innovative activities. This continuum in treatment effects allows us to achieve better identification than the standard binary treatment.

In Section 3, we argue that the expected net effect of the exemption laws on innovation should depend on the interplay between two opposing forces: The positive *tolerance-for-failure* effect versus the negative *credit supply* effect. In an attempt to tease out these effects, we follow two separate approaches. First, we compute an industry-level measure of external financial dependence, similar to Rajan and Zingales (1998), and compare the effects of the exemptions across industries with high and low dependence on external financing. A firm is highly dependent on external financing if a small fraction of its investments is financed by internal cash flows. We contend that firms that depend more heavily on external finance to fund their investments should be more negatively affected by the exemptions.

Second, we interact the exemptions with a local level measure of the competitiveness of the banking market. Recent evidence suggests that less competitive banking markets may reduce innovation (Benfratello et al., 2008; Ng, 2012; Chava et al., forthcoming; Cornaggia et al.,

forthcoming). In light of this evidence, we expect then that the negative impact of exemptions on innovation will be exacerbated in regions with less competitive banking markets.

4.2. Description of data

Table 2 defines all variables and Table 3 presents summary statistics. Our data set combines state-level bankruptcy exemptions with several different innovation measures. Data used to create the variable *Exemptions* has been hand-collected from individual state legal codes. The state exemption value is the sum of the homestead exemption and the personal property exemption, which includes jewelry, motor vehicle, cash and deposits, and the “wildcard” exemption.[7]

The innovation measures we use are based on patents. Patent-based measures are commonly used in applied research as indicators of inventive activity.[8] We gather data on patents from the U.S. Patent and Trademark Office (USPTO). For each patent, we retrieve the patentee (patent assignee), the location of the primary inventor associated with the patented invention, the filing date of the application, the technological subclass and class, and the number of citations received.

We retain only those patents that are assigned to U.S. non-government entities (which includes U.S-owned firms, both private and publicly listed) and identify “small firms” as companies with fewer than 500 employees at the time of patent grant (the rest are large companies). Small firms are those patentees that claimed the “small entity” status while filing their applications at the USPTO. Under 13 CFR 121.802(a), a patentee qualifies for small entity

[7] See Section 2 for details on the bankruptcy exemptions.

[8] See Griliches (1990) for a survey on the topic. More recent examples of studies that use patents to measure innovation include Acharya and Subramanian (2009), Hombert and Matray (2012), Amore et al. (forthcoming), Chava et al. (forthcoming).

status if its number of employees, including affiliates, does not exceed 500 persons. The small entity status allows individual inventors and small businesses to benefit from substantially lower patent application and other processing fees at the USPTO.[9] We then use information on the location of the inventor, year of patent application (which in most cases corresponds to date of invention), and technology subclass of the patent, and assign each patent to a state-year-subclass.

We construct several variables based on patent counts. *Patents by small firms* is the sum of patents applied for by small companies that are located in a given state, for a given subclass, for that year. *Patents by small firms & inventors* is the sum of patents applied for by small companies and individual inventors.[10] *Patents by private small firms* is the sum of patents applied for by small companies that are not publicly traded. The variable *Patents by large firms* is constructed for large companies following similar procedure as small companies.

Next, we disentangle the extensive and the intensive margins of patent output for small companies. *Number of small firms* is the count of unique identifiers of small companies that are patenting in a given state, subclass and year. *Ratio of small firm patents to firms* equals *Patents by small firms* divided by *Number of small firms*.

In addition to studying the effect of exemptions on patenting activity, we also investigate the effect on patent attributes for those patents that are filed. We consider two broad sets of variables. The first set focuses on the quality of innovation. *Patent impact* is the average of all

[9] For additional information, see the USPTO http://www.uspto.gov/web/offices/pac/mpep/s509.html and Graham and Hegde (2012).

[10] Individual inventors should also be affected by changes in state exemptions, as these are shocks related to personal bankruptcy. However, sometimes patents that are assigned to the "individual inventor" category at the USPTO are in fact owned by large corporations through reassignment. Furthermore, all individual inventors are assigned the same assignee code by the USPTO, thus making it difficult to distinguish one individual inventor from the other. However, only a small number of patents are assigned each year to USPTO's "individual inventor" category (less than 0.5% of all patents), while about 5% of all patents and 25% of all patents assigned to US business entities are assigned to the "US small firms" category. For these reasons, we focus primarily on patents assigned to the "small firms" category in our analysis.

citations within five years to patents (from the date of patent disclosure) filed in each state-year-subclass.[11] Several studies, including Trajtenberg (1989), Trajtenberg et al. (1997), Harhoff et al. (1999), Hall et al. (2005), validate the use of forward citations as indicators of the quality or economic value of patents. We also construct citations-based indices of patent originality and generality, as described in Hall et al. (2001). *Patent originality* is the state average of one minus the Herfindahl concentration index of the number of subclasses cited by the focal patents. A high score for this variable means that the patents in a given state cite previous patents that belong on average to a wide range of technology fields. In the same way, we define *Patent generality* as the average of one minus the Herfindahl index of the number of subclasses that cite a patent. A high value for this variable means that patents in a given state are cited by subsequent patents that belong to a wide range of technology fields.

Our second set of variables attempts to capture the riskiness of innovation projects and the degree of firm experimentation. To measure riskiness, we analyze the upper and lower tails of the distribution of the 5-year forward citations, by calculating the number of patents that belong to the top 25th citations percentile and bottom 25th citations percentile of all patents (including small companies, large companies, individual inventors and unassigned) filed in the same year and subclass (Chava et al. (forthcoming) also use this approach). The respective variables are *Most cited patents* and *Least cited patents*. Our measure of the amount of exploration in which a firm engages is a count of the number of technological subclasses in which a given firm files patents in a given state and year, based on the logic that a firm which works in multiple technological domains, rather than specializing in a few, performs more

[11] A U.S. patent can be cited by future patents only after the patent is publicly disclosed. U.S. patent applications filed before November 29, 2000 were disclosed (and thus could be cited) only after their grant, but patent applications filed after November 29, 2000 are typically disclosed at 18 months from application date.

explorative research. We average this count across all the firms patenting in a given state and year to create the variable *Firm exploration*.

In order to analyze the role of credit markets, we construct two variables. First, we compute the financial dependence index proposed by Rajan and Zingales (1998). We construct the external financial dependence measure using the population of Compustat firms over the period 1995-2005. For each firm-year, we compute the difference between capital expenditures and cash flow from operations, divided by capital expenditures. The variable *External financing dependence* equals the industry median of this ratio over the entire sample period. We then match industries to patent subclasses.

Our second credit market variable is *Bank market HHI*, the Herfindahl concentration index of market bank deposits at the MSA level. The HHI is the most standard measure of market power used in anti-trust analysis and in empirical research.[12] We focus on bank concentration at the MSA level rather than at the state level, since there are pronounced differences between MSAs within the same state.[13] Note that if an MSA crosses state boundaries, we separate it into multiple mutually exclusive MSAs so that each observation is assigned to a specific state. We operationalize the use of the HHI measure by assigning each MSA-year to one of ten mutually exclusive bins, where each bin accounts for a decile in the range of observable HHI.

We include several location specific time-varying variables in most regression models. From the Census we collect state-level data on *Median income* and *Population density*. We

[12] The HHI is extensively used in empirical research as a measure of bank market power (e.g., Black and Strahan, 2002; Cetorelli and Strahan, 2006).

[13] As an example, consider a potential entrepreneur located in Buffalo NY. Bank market characteristics in Buffalo NY are more important to this entrepreneur than bank market characteristics in New York City. In addition, we replicate the MSA-level findings at the state level (see Appendix Table 1).

obtain state-level *House price indexes* (HPI) from the Federal Housing Finance Agency (FHFA). The HPI (1995 = 100) is estimated using sales prices and appraisal data. Finally, we create a variable *Innovation intensity*, which is the lagged median number of patents applied for in a given year, and a variable *Subclass innovation intensity*, which is the lagged median number of patents applied for in a given year and subclass. We use these variables in robustness checks designed to replicate the approach used in Acharya and Subramanian (2009).

5. Results

5.1. Patent counts

We first investigate the effect of changes in state level bankruptcy exemptions on the number of patents filed in a given state, subclass and year. Table 4 presents the results of Poisson models; all standard errors are clustered at the state level to allow for geographic autocorrelation (Bertrand, Duflo and Mullainathan, 2004). The dependent variable in Columns 1-3 is the number of patents by small companies and individual inventors. Progressively more control variables are added across the three models. The coefficient on *Exemptions* is negative and statistically significant across all three models. In Column 4, we focus on our baseline specification that includes the number of patents by small companies only. In Column 5, we focus even more narrowly on the number of patents by small companies that are privately held. Publicly traded companies can rely on other sources of external finance, and public filings provide a higher level of transparency than do non-publicly traded companies. We therefore expect that bankruptcy exemptions have a larger effect for small privately held companies than for small publicly traded companies. The negative coefficient on *Exemptions* remains remarkably stable across the various models in Table 4, both in terms of magnitude and statistical significance, providing us with

initial evidence that bankruptcy exemptions reduce innovative activity. The coefficient estimates reported in Column 4 indicate that a $100,000 increase in the state exemption level leads on average to a permanent reduction in patents of 2.9%.

Before analyzing other effects of the exemption laws, we subject our empirical model to two important identification tests. First, we test whether the exemptions had any effect on innovation by large companies. Large companies provide a good counterfactual, because personal bankruptcy law should not affect this group of firms. The results in Table 5 show no significant effect of the exemptions on patents by large companies in any of the specifications. Therefore these results help corroborate our empirical strategy. For instance, they address the important concern that changes in exemptions could be correlated with other state-specific economic trends.

Second, as in Bertrand and Mullainathan (2003), and Acharya and Subramanian (2009), we exploit the timing of exemptions. To do this, we decompose our exemptions variable into three exemptions change indicators for the following time periods: from three years to one year before the exemption law (-3,-1), from the year the law became effective to three years after that (0,3), and four years after the law and beyond (≥4). In addition, we estimate separate regressions for small and large firms. Table 6 reports the corresponding results. Column 1 displays the estimates for small companies and Column 2 for large companies. We find no significant effect of the exemptions on innovation prior to the passage of the laws for either type of company. Consistent with our previous findings, we obtain a larger, negative, and significant effect of the exemptions on innovation by small firms, and no meaningful effect for the large firms.[14] This

[14] The long term effect of the exemptions on innovation by small firms is slightly smaller and becomes statistically insignificant. This may have to do with a right-censoring problem, i.e., the fact that some exemption laws were passed towards the end of our sample period, which may have reduced the model's power to identify their long term effects.

important finding further helps us rule out the possibility that our estimates are biased by the reverse causality of the exemptions.

5.2. Extensive vs. intensive margins

The reduction in innovation activity at the state-year-technology level we find could be the result of a reduction in either the number of innovative firms (i.e., the extensive margin) or the number of patents per innovative firm (i.e., the intensive margin). In Table 7, we report the results of the effect of exemptions on the number of unique small companies that patent (in Column 1) and on the ratio of small company patents to the number of unique companies (in Column 2). Column 1 shows that a higher exemption limit reduces the number of small companies that patent. Specifically, a $100,000 increase in the exemption limit is predicted to decrease the number of small companies that patent in the given subclass-year by 3.1%. In contrast, we find no effect of the exemptions on the intensive margin. These results provide some support to Nanda and Rhodes-Kropf (2012), who argue that debtor protection policies could reduce the equilibrium number of innovators, since financiers will restrict the number of projects they are willing to fund.

5.3. The credit market channel

Our evidence so far indicates that the net effect of an increase in exemptions is a reduction in the extent of innovation. In this subsection, our aim is to show empirically that the mechanism through which debtor protection negatively affects innovation is the reduction in credit availability. To this end, we rely on the existing evidence (reviewed in detail in Section 3.1) that high bankruptcy exemptions reduce credit availability to households (Gropp et al., 1997), small firms (Berkowitz and White, 2004; Berger et al., 2011) and start-up firms

(Cerqueiro and Penas, 2013). We also recall that these studies do not investigate whether this negative effect also affects innovative firms.

To provide evidence on the credit supply mechanism, we first investigate whether the negative effect of the exemptions is exacerbated in industries that depend more heavily on bank financing. Following a large literature, we use the financial dependence index developed by Rajan and Zingales (1998), and run separate regressions for industries with high and low external finance dependence. If the negative effect on innovation is due to a reduction in credit availability, then those industries that rely more on external funding should be more affected by the exemptions. The results in Table 8 provide some support for this conjecture. In Column 1, we find that the negative effect of the exemptions on small firm patents is small and insignificant for industries with low external finance dependence. Column 2 shows that the effect of the exemptions on small firm patents is larger and statistically significant for industries with high external finance dependence.

Next, we build on a body of evidence that shows that the availability of credit to innovators is lower in highly concentrated bank markets (see Section 3.2). We then test whether the negative effect from the changes in exemptions is amplified in concentrated banking markets. In order to take advantage of the rich variation in bank concentration across markets, all observations are now at the MSA, subclass and year level.[15] Table 9 presents estimates of the exemptions after incorporating the data on bank market concentration. Column 1 replicates our main estimation at this new level of analysis. The corresponding coefficient on *Exemptions* is similar in sign, magnitude and significance to our baseline estimate presented in Column 4 of Table 4. In Column 2, we include indicator variables for deciles of bank market concentration.

[15] We replicate the findings at the state level in Appendix Table 1. State level HHI is the average of HHI across all MSAs in the state.

Of note, the coefficient on the variable for the highest decile of bank market concentration is negative and statistically significant, indicating that patenting is substantially lower in highly concentrated bank markets.[16]

In Column 3, we include interactions between exemption levels and deciles of bank market concentration. Several interesting results emerge. First, across the distribution of bank market concentration, patenting activity decreases with higher exemptions. Second, the decrease is higher in highly concentrated bank markets. There appears to be a monotonic increase in the effect size when moving from bank market concentrations in the fifth through the tenth deciles. Wald tests of coefficient equality reveal that the coefficients on the interactions with indicators for the ninth and tenth deciles are statistically different from the interaction with the indicator for the first decile. Third, it is notable that the mean concentration value for deciles six to ten range from 0.16 to 0.47. These markets are considered moderately or highly concentrated per horizontal merger guidelines set out by the US Department of Justice.[17] The results in Table 8 provide support to the conjecture that concentrated markets exacerbate the reduction in credit supply to small firms triggered by increases in state exemption levels. These findings are consistent with Benfratello et al. (2008), Ng (2012), Cornaggia et al. (forthcoming), and Chava et al. (forthcoming), who also find that lower competition in the banking market is detrimental to innovation activity by small firms.

Overall, these results suggest that the exemption laws have a strong and negative effect on credit availability that prevents some small companies from undertaking innovation activities. The negative credit market effect seems to be particular acute in highly concentrated banking

[16] This is similar to the finding in Benfratello et al. (2008) who show that innovative activity is lower in highly concentrated bank markets in Italy.

[17] See http://www.justice.gov/atr/public/guidelines/hmg-2010.pdf, accessed June 25, 2013.

markets, and it seems to primarily affect firms that depend heavily on external financing.

5.4. Innovation quality

One could argue that while greater debtor relief reduces the extent of innovative activity, it may increase the average quality of innovative activity by forcing inventors to drop their low-quality projects. We thus analyze how changes in exemption levels affect several attributes related to the quality of innovation by using measures of patent quality based on the number of forward citations they receive over a period of five years from the date the patent applications are public and therefore available to be cited by future patents. Table 10 reports the estimated effect of exemptions on the 5-year forward citations received by patents. The estimates are obtained from Poisson regression models that include time-varying state-level controls and fixed effects at the state, subclass, and year levels. We cluster standard errors at the state level. All estimates in Table 10 show no statistically significant effect of the exemptions on patent quality. One problem with forward citation based measures is that the distribution of these variables is right-censored since later patents have a shorter window at a given point in time to gather citations from future patents. In order to minimize censoring bias, we estimate several specifications with alternative citation windows and by eliminating the later years from our sample for which censoring problems are more severe.[18] All these specifications do not qualitatively alter our findings.

Next, we analyze the effect of exemptions on our measure of invention originality, based on the number of subclasses cited by the focal patents. We report the results from OLS regression models in Table 11. The coefficient on exemptions is insignificant and close to zero, suggesting that higher exemption levels do not also affect innovation originality.

[18] See Appendix Table 2 for additional detail about the censoring problem.

Finally, we analyze the effect of exemptions on our measure of the generality of innovation based on the number of subclasses that cite the focal patents. Since the dependent variable is based on forward citations, we estimate several specifications with shorter estimation windows to mitigate the effects of censoring (see Appendix Table 2). Table 12 displays the corresponding estimates we obtain using OLS regression models. The coefficient on the exemptions variable appears to be positive across all specifications but statistically significant in only three cases. The results from Table 12 suggest that higher exemption levels may have a modest positive effect on patent generality.

5.5. Innovation riskiness and firm exploration

In this subsection, we analyze whether inventors change the risk profile of their innovative activity and the extent to which they undertake exploration in response to bankruptcy laws. We do so by focusing separately on the production of small-firm patents that are extremely well cited (i.e., with small-firm patents that are among the top-25% of patent quality measured by 5-year forward citations; the distribution includes patents assigned to all firms, including large corporations and foreign entities) and small-firm patents with extremely few cites (i.e., with 5-year forward citations in the bottom 25% of the distribution). As before, we obtain the estimates from Poisson regression models that include time-varying state-level controls and fixed effects at the state, subclass, and year levels. We cluster standard errors at the state level. As before, since the dependent variable is based on forward citations, we estimate several specifications with shorter estimation windows to rule out that censoring is driving any results (see Appendix Table 2). Table 13A presents our estimates of the effect of exemptions results on the number of patents in the upper tail of forward citations. Across most specifications, the coefficient on the variable *Exemptions* is negative and statistically significant. We find that a $100,000 increase in the

exemption limit reduces the number of small firm patents in the high citations group by a percentage that ranges between 5.8% and 9.1%. Table 13B presents the results for the lower tail of the forward citations' distribution. The coefficient on *Exemptions* is negative across all specifications, but not always statistically significant. The set of results presented in Tables 13A and 13B suggest that higher exemption levels decreases the extreme outcomes and thus riskiness of innovative activities pursued by small firms.

We next analyze how changes in exemption levels affect firm exploration. In particular, we focus on the effect of exemptions on the average number of subclasses in which firms in a given state patent. We report the results from an OLS regression model in Table 14. The number of observations is smaller because observations are at the state-year level rather than state-subclass-year level as before. The results show that an increase in exemptions makes affected firms less experimental on average. In particular, the estimate obtained indicates that a $100,000 increase in the exemption limit leads to a reduction of 2.5 subclasses in which firms patent. Our findings on the decline in innovation riskiness and firm exploration are consistent with the predictions in Nanda and Rhodes-Kropf (2012) that policies aimed at protecting innovators could result in fewer radical innovations.

5.6. Other robustness tests

Acharya and Subramanian (2009) propose a measure of innovation intensity that controls for a sector's propensity to innovate. In order to replicate their approach, we add in two measures of innovation intensity to our regression models. Results are reported in Table 15. Column 1 replicates the main results for comparison. Column 2 includes the variable *Innovation intensity*, which is the lagged median number of patents applied for in a given year. Column 3 includes the variable *Subclass innovation intensity*, which is the lagged median number of patents applied for

in a given year and subclass. Although these variables have positive and significant coefficients, the main results are virtually unaffected: the coefficient on *Exemptions* remains negative and statistically significant.

6. Conclusion

We study the effect of debtor-friendly laws on innovation. In a departure from prior literature, we highlight the idea that while debtor-friendly laws can encourage innovation by reducing the cost of failure for innovators, they can also harm innovation if they tighten the availability of credit to innovators. We use state and year variation in U.S. personal bankruptcy exemption limits, which affect the capital constraints of individual innovators and small firms, to investigate the effects of debtor protection on small firm innovation.

We have three main sets of results. First, we find that increases in bankruptcy exemption limits (i.e., increases in debtor protection) reduce the total number of patent applications by small firms. These results hold after implementing a careful identification strategy that addresses concerns about unobserved heterogeneity across states, changes in macroeconomic conditions within states, or reverse causality of the state laws. We also show that the reduction in innovation activity is driven by a reduction of innovation at the extensive margin (number of innovators) rather than the intensive margin (output per inventor). Second, we find that the negative effect of debtor protection on innovation is amplified in industries with a high dependence on external finance and in concentrated banking markets. These results suggest that the channel through which the exemptions negatively affect innovation activity is a reduction in credit supply to small firms. Third, we find evidence that firms are filing patents that can be categorized as "safer bets." This finding matches the predictions in Nanda and Rhodes-Kropf (2012) and corroborates

the idea that the exemptions decrease financiers' willingness to fund riskier ventures. We find no robust evidence that the exemptions affect the overall quality of the patents.

Our findings complement those in Acharya and Subramanian (2009), who find that debtor-friendly corporate bankruptcy codes foster innovation in large firms. In contrast, we study innovation activity by small firms, for which the information asymmetry problem in credit markets is particularly acute. Moreover, the agency problems caused by debtor friendly codes magnify the risky nature of investing in innovative activities. Overall, our results support the conjecture that the tolerance-for-failure effect from higher levels of debtor protection, that seem to explain the positive effect on large firms, is more than offset by a negative effect of debtor protection on bank credit availability to small firms. These results have important implications for policy makers considering policies to encourage innovation and growth. For example, recent policy efforts in Europe to introduce more lenient bankruptcy laws, such as *procedure de sauvegarde* in France, may ultimately lead to lower levels of innovation if credit supply tightens.

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Table 1A.
States changing bankruptcy exemption levels, 1995-2005.

Year	States changing exemption levels
1995	CA, ME, NV, NH
1996	MN, VT, WV, WY
1997	MT, NE, NV, NH, UT
1998	HI, MI, MN, NJ, PA, RI, SD, WA
1999	AK, DC, ID, MT, RI, UT, WA
2000	CO, DC, LA, MA
2001	AZ, GA, HI, ME, MI, MT, NJ, PA, RI
2002	NH, WA, WV
2003	CA, ME, MO, NV
2004	AK, AZ, HI, KY, MD, MA, MI, MN, MO, NH, NJ, PA, RI
2005	DE, IN, NV, NY, OK

Table 1B.
Distribution of changes in exemption levels, 1995-2005

Magnitude of change	States
Exemptions <$25K	AK, AZ, CA, DC, GA, HI, ID, IN, KY, LA, ME, MD, MI, MN, MO, MT, NE, NV, NH, NJ, OK, PA, RI, SD, UT, WA, WV, WY
$25K < Exemptions <$50K	CO, ME, MT, NV, NH
$50K < Exemptions <$100K	AZ, MT, NV, NY, RI, VT
$100K< Exemptions	DE, DC, MA, NV, NH

Table 2.
Definition of variables.

Variable	Definition
Personal Bankruptcy Law	
Exemptions	Sum of the homestead exemption and the personal property exemption (in $100,000s).
Patent Count	
Patents by Small Firms	Number of patents applied for by small firms (<500 employees).
Patents by Small Firms & Inventors	Number of patents applied for by small firms and by individual inventors.
Patents by Private Small Firms	Number of patents applied for by privately held small firms.
Patents by Large Firms	Number of patents applied for by large firms (≥500 employees).
Extensive vs. intensive margins	
Number of Small Firms	Number of small firms that applied for patents.
Ratio of Small Firm Patents to Firms	Ratio of *Patents by Small Firms* to *Number of Small Firms.*
Patent Quality	
Patent Impact	Average of all citations within five years to patents from the date of patent disclosure filed.
Patent Originality	Average of one minus the Herfindahl index of the number of subclasses cited by the patent.
Patent Generality	Average of one minus the Herfindahl index of the number of subclasses citing the patent.
Patent Riskiness and Firm Exploration	
Most Cited Patents	Number of patents in the top 25^{th} percentile of the distribution of five-year forward citations in a given subclass and year.
Least Cited Patents	Number of patents in the bottom 25^{th} percentile of the distribution of five-year forward citations in a given subclass and year.

Table 2.
Definition of variables.

Variable	Definition
Firm Exploration	Average number of subclasses in which firms patent in a given state and year.
Credit Market	
External Financing Dependence	Industry-average fraction of capital expenditures not financed with cash flows from operations.
Bank Market HHI	Herfindahl concentration index of market bank deposits at the MSA-state level.
Other Variables	
Median Income	State median income (in $10,000s).
Population Density	State population density.
House-Price Index	State House Price Index.
Innovation Intensity	Lagged median number of patents applied for in a given year.
Subclass Innovation Intensity	Lagged median number of patents applied for in a given year and subclass.

Table 3.
Summary statistics.

This table includes summary statistics of the variables used. Observations are at the subcategory-state-year level except for the number of subclasses in which firm patents, which is at the state-year level, and bank market HHI, which is at the MSA-subclass-year-level. See text for sources of data. Table 2 provides definitions of all variables.

Variable	Mean	Std. Dev.	Min	Max
Personal Bankruptcy Law				
Exemptions ($100,000s)	2.00	3.36	0.05	10.60
Patent Count				
Patents by Small Firms	10.04	31.23	0.00	881.00
Patents by Small Firms & Inventors	10.47	32.09	0.00	899.00
Patents by Private Small Firms	7.01	17.48	0.00	433.00
Patents by Large Firms	30.93	107.71	0.00	3100.00
Extensive vs. intensive margins				
Number of Small Firms	7.04	18.15	0.00	506.00
Ratio of Small Firm Patents to Firms	0.97	0.72	0.00	13.00
Patent Quality				
Patent Impact	1.33	2.08	0.00	76.50
Patent Originality	0.40	0.20	0.00	0.92
Patent Generality	0.20	0.16	0.00	0.90
Patent Riskiness and Firm Exploration				
Most Cited Patents	2.12	8.27	0.00	315.00
Least Cited Patents	4.43	12.77	0.00	312.00
Firm Exploration	1.23	0.12	1.00	1.92
Credit Market				
External Financing Dependence	0.17	0.62	-1.10	1.56
Bank Market HHI	0.16	0.08	0.03	1.00
Other Variables				
Median Income ($10,000s)	40.49	0.72	24.88	63.37
Population Density	182.69	249.38	1.06	1176.92
House-Price Index	129.33	26.89	88.96	268.18
Innovation Intensity	13.64	1.23	11.00	15.00
Subclass Innovation Intensity	19.57	17.60	2.00	87.50

Table 4.

Effect of personal bankruptcy exemptions on number of patents

This table reports results of regressions of number of patents on personal bankruptcy exemptions, using data from the USPTO for 1995-2005. Poisson models are used in all cases. The level of analysis is state-subclass-year. In Columns 1-3 the dependent variable is the count of patents for small companies and individual inventors in the state-subclass-year, where small company is defined as less than 500 employees. In Column 4 the dependent variable is the count of patents for small companies. In Column 5 the dependent variable is the count of patents for privately held small companies. Time-varying state specific variables include median income, house price index and population. Time-varying state specific variables and fixed effects for 10 calendar years, 50 states, and 36 patent subcategories are included in models as indicated. Robust standard errors are included in brackets and clustered at the state level. *Significant at 10%; **Significant at 5%; ***Significant at 1%.

	(1)	(2)	(3)	(4)	(5)
	Patents by Small Firms & Inventors			Patents by Small Firms	Patents by Private Small Firms
Exemptions	-0.022**	-0.028**	-0.028**	-0.029**	-0.026**
	[0.010]	[0.013]	[0.013]	[0.014]	[0.011]
Calendar Year Effects	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes
Time-Varying State Specific Variables	No	Yes	Yes	Yes	Yes
Subclass Fixed Effects	No	No	Yes	Yes	Yes
Subclass*Trend	No	No	Yes	Yes	Yes
Observations	19,800	19,800	19,800	19,800	19,800
Log Likelihood	-128595	-128588	-57873	-56961	-38529

Table 5.

Counterfactual: Effect of personal bankruptcy exemptions on large firms.

This table reports results of regressions of number of patents on personal bankruptcy exemptions, using data from the USPTO for 1995-2005. The level of analysis is state-subclass-year. The dependent variable is the count of patents for large companies in the state-subclass-year, where large company is defined as more than 500 employees. Time-varying state specific variables include median income, house price index, and population. Time-varying state specific variables and fixed effects for 10 calendar years, 50 states, and 36 patent subcategories are included in models as indicated. Robust standard errors are included in brackets and clustered at the state level. *Significant at 10%; **Significant at 5%; ***Significant at 1%.

	(1)	(2)	(3)
	Patents by Large Firms		
Exemptions	0.018	0.014	0.014
	[0.021]	[0.017]	[0.017]
Calendar Year Effects	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes
Time-Varying State Specific Variables	No	Yes	Yes
Subclass Fixed Effects	No	No	Yes
Subclass*Trend	No	No	Yes
Observations	19,800	19,800	19,800
Log Likelihood	-486640	-486158	-216945

Table 6.
Timing of exemptions.

This table reports results of regressions of number of patents on the timing of personal bankruptcy exemption laws, separately by firm size. Data is from the USPTO for 1995-2005. The level of analysis is state-subclass-year. The dependent variable in Column 1 is the count of patents for small companies in the state-subclass-year, where small company is defined as less than 500 employees. The dependent variable in Column 2 is the count of patents for large companies in the state-subclass-year, where large company is defined as more than 500 employees. Fixed effects for 10 calendar years, 50 states, and 36 patent subclasses are included in models as indicated. Robust standard errors are included in brackets and clustered at the state level. *Significant at 10%; **Significant at 5%; ***Significant at 1%.

	(1) Patents by Small Firms	(2) Patents by Large Firms
Exemption change (-3,-1)	-0.022	-0.024
	[0.027]	[0.051]
Exemption change (0,3)	-0.048**	0.012
	[0.024]	[0.043]
Exemption change (t≥4)	-0.022	-0.007
	[0.021]	[0.031]
Calendar year effects	Yes	Yes
Subclass fixed effects	Yes	Yes
State fixed effects	Yes	Yes
Observations	19,800	19,800

Table 7.

Extensive vs. intensive margins

This table explores the effect of personal bankruptcy exemptions on measures of extensive and intensive firm margins. Poisson models are used in all cases. The level of analysis is state-subclass-year. In Column 1 the dependent variable is the count of small companies that are actively patenting in the state-subclass-year, where small company is defined as less than 500 employees. In Column 2 the dependent variable is the ratio of patents filed in the state-subclass-year to the number of firms in the in the state-subclass-year. Time-varying state specific variables include median income, house price index, and population. Time-varying state specific variables and fixed effects for 10 calendar years, 50 states, and 36 patent subcategories are included in models as indicated. Robust standard errors are included in brackets and clustered at the state level. *Significant at 10%; **Significant at 5%; ***Significant at 1%.

	(1) Number of Small Firms	(2) Ratio of Small Firm Patents to Firms
Exemptions	-0.031**	0.003
	[0.013]	[0.017]
Calendar Year Effects	Yes	Yes
State Fixed Effects	Yes	Yes
Time-Varying State Specific Variables	Yes	Yes
Subclass Fixed Effects	Yes	Yes
Subclass*Trend	Yes	Yes
Observations	19,800	19,800
Log Likelihood	-43642	-20684

Table 8.

External financing dependence

This table reports results of regressions of number of patents on personal bankruptcy exemptions, using data from the USPTO for 1995-2005. Poisson models are used in all cases. The level of analysis is state-subclass-year. The dependent variable is the count of patents for small companies in the state-subclass-year, where small company is defined as less than 500 employees. We define external financing dependence as the fraction of capital expenditures not financed with cash flows from operations. We compute this measure along the lines of Rajan and Zingales (1998) using Compustat data. Column 1 includes subclasses with below median dependence on external finance; Column 2 includes subclasses with above median dependence on external finance. Time-varying state specific variables include median income, house price index, and population. Time-varying state specific variables and fixed effects for 10 calendar years, 50 states, and 36 patent subcategories are included in models as indicated. Robust standard errors are included in brackets and clustered at the state level. *Significant at 10%; **Significant at 5%; ***Significant at 1%.

	(1)	(2)
	Patents by Small Firms	
	Low External Financing Dependence	High External Financing Dependence
Exemptions	-0.017	-0.038***
	[0.014]	[0.012]
Calendar Year Effects	Yes	Yes
State Fixed Effects	Yes	Yes
Time-Varying State Specific Variables	Yes	Yes
Subclass Fixed Effects	Yes	Yes
Subclass*Trend	Yes	Yes
Observations	9,900	9,900
Log Likelihood	-24274	-27650

Table 9.

Concentration in local bank markets.

This table reports results of regressions of number of patents on personal bankruptcy exemptions and bank market concentration, using data from the USPTO for 1995-2005. The level of analysis is MSA-subclass-year. Note that if an MSA crosses state boundaries, it is separated into multiple mutually exclusive MSAs so that each observation can be assigned to a specific state. The dependent variable in all columns is the count of patents for small companies. In Columns 2-3, bank concentration levels are assigned to mutually exclusive decile categories. The category for the lowest decile is excluded. Poisson models are used in all cases. Time-varying state specific variables include median income, house price index, and population. Time-varying state specific variables and fixed effects for 10 calendar years, 414 MSAs, and 36 patent subclasses are included in all models. Robust standard errors are included in brackets and clustered at the MSA level. *Significant at 10%; **Significant at 5%; ***Significant at 1%.

	(1)	(2)	(3)
Exemptions	-0.026**	-0.025**	
	[0.013]	[0.012]	
Bank HHI Bin 2		0.009	-0.004
		[0.025]	[0.034]
Bank HHI Bin 3		0.020	0.030
		[0.038]	[0.047]
Bank HHI Bin 4		0.011	0.013
		[0.030]	[0.043]
Bank HHI Bin 5		-0.029	-0.027
		[0.031]	[0.043]
Bank HHI Bin 6		-0.007	0.004
		[0.036]	[0.050]
Bank HHI Bin 7		-0.041	-0.024
		[0.034]	[0.046]
Bank HHI Bin 8		-0.071	-0.04
		[0.045]	[0.057]
Bank HHI Bin 9		-0.060	-0.018
		[0.046]	[0.057]
Bank HHI Bin 10		-0.131**	-0.092
		[0.053]	[0.067]
Exemptions*Bank HHI Bin 1			-0.028**
			[0.011]
Exemptions*Bank HHI Bin 2			-0.021*
			[0.012]
Exemptions*Bank HHI Bin 3			-0.033**
			[0.013]
Exemptions*Bank HHI Bin 4			-0.028**
			[0.013]
Exemptions*Bank HHI Bin 5			-0.027*
			[0.014]
Exemptions*Bank HHI Bin 6			-0.033**
			[0.015]
Exemptions*Bank HHI Bin 7			-0.036***
			[0.013]
Exemptions*Bank HHI Bin 8			-0.040***
			[0.014]
Exemptions*Bank HHI Bin 9			-0.046***
			[0.014]
Exemptions*Bank HHI Bin 10			-0.048***
			[0.016]
Controls	Yes	Yes	Yes
Observations	163,944	163,944	163,944
Log Likelihood	-134655	-134631	-134617

Table 10.
Patent impact.

This table reports results of regressions of five-year forward citations on personal bankruptcy exemptions, using data from the USPTO for 1995-2005. Poisson models are used in all cases. The level of analysis is state-subclass-year. Column 1 uses data from the entire time period (1995-2005). Columns 2-7 use earlier end years, to investigate the sensitivity of results to a well-known problem with forward citations. Time-varying state specific variables include median income, house price index, and population. Time-varying state specific variables and fixed effects for 10 calendar years, 50 states, and 36 patent subcategories are included in models as indicated. Robust standard errors are included in brackets and clustered at the state level. *Significant at 10%; **Significant at 5%; ***Significant at 1%.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Year Sample	All	< 2005	< 2004	< 2003	< 2002	< 2001	< 2000
Exemptions	-0.027	-0.023	-0.021	0.010	-0.011	0.003	0.081
	[0.026]	[0.034]	[0.038]	[0.041]	[0.050]	[0.067]	[0.405]
Calendar Year Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time-Varying State Specific Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Subclass Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Subclass*Trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	19,800	18,000	16,200	14,400	12,600	10,800	9,000
Log Likelihood	-27598	-25560	-22992	-20149	-17018	-13861	-10746

Table 11.
Patent originality.

This table explores the effect of personal bankruptcy exemptions on patent originality. OLS models are used in all cases. In Column 1 the dependent variable is the number of subclasses cited by patents. The level of analysis is state-subclass-year. Time-varying state specific variables include median income, house price index, and population. Time-varying state specific variables and fixed effects for 10 calendar years, 50 states, and 36 patent subcategories are included in models as indicated. Robust standard errors are included in brackets and clustered at the state level. *Significant at 10%; **Significant at 5%; ***Significant at 1%.

	(1) Number of Subclasses Cited by Patent
Exemptions	0.006
	[0.007]
Calendar Year Effects	Yes
State Fixed Effects	Yes
Time-Varying State Specific Variables	Yes
Subclass Fixed Effects	Yes
Subclass*Trend	Yes
Observations	19,800
R-Squared	0.149

Table 12.

Patent generality.

This table reports results of regressions of the generality of patents on personal bankruptcy exemptions, using data from the USPTO for 1995-2005. OLS models are used in all cases. The level of analysis is state-subclass-year. Patents are considered more general if they receive forward citations from more subclasses. Column 1 uses data from the entire time period (1995-2005). Columns 2-7 use earlier end years, to investigate the sensitivity of results to a well-known problem with forward citations. Time-varying state specific variables include median income, house price index, and population. Time-varying state specific variables and fixed effects for 10 calendar years, 50 states, and 36 patent subclasses are included in models as indicated. Robust standard errors are included in brackets and clustered at the state level. *Significant at 10%; **Significant at 5%; ***Significant at 1%.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Year Sample	All	< 2005	< 2004	< 2003	< 2002	< 2001	< 2000
Exemptions	0.013	0.018	0.028**	0.031**	0.031*	0.016	0.018
	[0.010]	[0.011]	[0.012]	[0.013]	[0.017]	[0.013]	[0.030]
Calendar Year Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time-Varying State Specific Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Subclass Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Subclass*Trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	19,800	18,000	16,200	14,400	12,600	10,800	9,000
R-squared	0.297	0.245	0.207	0.190	0.181	0.177	0.172

Table 13A.

Most cited patents.

This table reports results of regressions of the top 25th percentile of five-year forward citations on personal bankruptcy exemptions, using data from the USPTO for 1995-2005. Poisson models are used in all cases. The level of analysis is state-subclass-year. Column 1 uses data from the entire time period (1995-2005). Columns 2-7 use earlier end years, to investigate the sensitivity of results to a well-known problem with forward citations. Time-varying state specific variables include median income, house price index, and population. Time-varying state specific variables and fixed effects for 10 calendar years, 50 states, and 36 patent subcategories are included in models as indicated. Robust standard errors are included in brackets and clustered at the state level. *Significant at 10%; **Significant at 5%; ***Significant at 1%.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Year Sample	All	< 2005	< 2004	< 2003	< 2002	< 2001	< 2000
Exemptions	-0.058***	-0.054***	-0.067***	-0.071***	-0.091***	-0.062*	0.108
	[0.010]	[0.012]	[0.016]	[0.018]	[0.026]	[0.035]	[0.273]
Calendar Year Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time-Varying State Specific Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Subclass Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Subclass*Trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	19,800	18,000	16,200	14,400	12,600	10,800	9,000
Log Likelihood	-25927	-23707	-21270	-18701	-16003	-13413	-11041

Table 13B.

Least cited patents.

This table reports results of regressions of the bottom 25th percentile of five-year forward citations on personal bankruptcy exemptions, using data from the USPTO for 1995-2005. Poisson models are used in all cases. The level of analysis is state-subclass-year. Column 1 uses data from the entire time period (1995-2005). Columns 2-7 use earlier end years, to investigate the sensitivity of results to a well-known problem with forward citations. Time-varying state specific variables include median income, house price index, and population. Time-varying state specific variables and fixed effects for 10 calendar years, 50 states, and 36 patent subcategories are included in models as indicated. Robust standard errors are included in brackets and clustered at the state level. *Significant at 10%; **Significant at 5%; ***Significant at 1%.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Year Sample	All	< 2005	< 2004	< 2003	< 2002	< 2001	< 2000
Exemptions	-0.024	-0.052**	-0.075*	-0.089*	-0.103*	-0.045	-0.337**
	[0.019]	[0.025]	[0.044]	[0.051]	[0.059]	[0.054]	[0.164]
Calendar Year Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time-Varying State Specific Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Subclass Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Subclass*Trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	19,800	18,000	16,200	14,400	12,600	10,800	9,000
Log Likelihood	-38568	-34940	-31426	-27937	-24293	-20702	-17214

Table 14.

Firm exploration

This table explores the effect of personal bankruptcy exemptions on experimentation. OLS models are used in all cases. In Column 1 the dependent variable is the number of subclasses in which firms patent. The level of analysis is state-year. Time-varying state specific variables include median income, house price index, and population. Time-varying state specific variables and fixed effects for 10 calendar years, 50 states, and 36 patent subcategories are included in models as indicated. Robust standard errors are included in brackets and clustered at the state level. *Significant at 10%; **Significant at 5%; ***Significant at 1%.

	(1) Number of Subclasses in which Firms Patent
Exemptions	-0.025*
	[0.014]
Calendar Year Effects	Yes
State Fixed Effects	Yes
Time-Varying State Specific Variables	Yes
Subclass Fixed Effects	No
Subclass*Trend	No
Observations	550
R-Squared	0.098

Table 15.

Effect of bankruptcy exemptions on number of patents, controlling for innovation intensity.

This table reports results of regressions of number of patents on personal bankruptcy exemptions and innovation intensity measures, using data from the USPTO for 1995-2005. Poisson models are used in all cases. The level of analysis is state-subclass-year. The dependent variable is the count of patents for small companies in the state-subclass-year, where small company is defined as less than 500 employees. Column 2 includes a lagged innovation intensity measure defined across all subclasses. Column 3 includes a lagged innovation intensity measure defined across a specific subclass. Time-varying state specific variables include median income, house price index, and population. Time-varying state specific variables and fixed effects for 10 calendar years, 50 states, and 36 patent subcategories are included in models as indicated. Robust standard errors are included in brackets and clustered at the state level. *Significant at 10%; **Significant at 5%; ***Significant at 1%.

	(1)	(2)	(3)
Exemptions	-0.029**	-0.029**	-0.029**
	[0.014]	[0.014]	[0.014]
Innovation Intensity (one year lag)		0.099***	
		[0.013]	
Subclass Innovation Intensity (one year lag)			0.007***
			[0.001]
Calendar Year Effects	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes
Time-Varying State Specific Variables	Yes	Yes	Yes
Subclass Fixed Effects	Yes	Yes	Yes
Subclass*Trend	Yes	Yes	Yes
Observations	19,800	19,800	19,800
Log Likelihood	-56961	-56961	-56883

Appendix Table 1.
Concentration in local bank markets (state-level).

This table reports results of regressions of number of patents on personal bankruptcy exemptions and bank market concentration, using data from the USPTO for 1995-2005. The level of analysis is state-subclass-year. The dependent variable in all columns is the count of patents for small companies. In Columns 2-3, bank concentration levels are assigned to mutually exclusive decile categories. The category for the lowest decile is excluded. Poisson models are used in all cases. Time-varying state specific variables include median income, house price index, and population. Time-varying state specific variables and fixed effects for 10 calendar years, 50 states, and 36 patent subclasses are included in all models. Robust standard errors are included in brackets and clustered at the state level. *Significant at 10%; **Significant at 5%; ***Significant at 1%.

	(1)	(2)	(3)
Exemptions	-0.036**	-0.040**	
	[0.018]	[0.016]	
Bank HHI Bin 2		0.015	0.023
		[0.024]	[0.025]
Bank HHI Bin 3		-0.057*	-0.055*
		[0.029]	[0.031]
Bank HHI Bin 4		0.019	0.023
		[0.045]	[0.050]
Bank HHI Bin 5		-0.035	-0.044
		[0.050]	[0.062]
Bank HHI Bin 6		-0.012	-0.011
		[0.052]	[0.063]
Bank HHI Bin 7		-0.041	-0.051
		[0.054]	[0.067]
Bank HHI Bin 8		0.000	0.019
		[0.062]	[0.074]
Bank HHI Bin 9		-0.072	-0.057
		[0.063]	[0.077]
Bank HHI Bin 10		-0.033	-0.017
		[0.060]	[0.069]
Exemptions*Bank HHI Bin 1			-0.039*
			[0.022]
Exemptions*Bank HHI Bin 2			-0.049**
			[0.020]
Exemptions*Bank HHI Bin 3			-0.041**
			[0.019]
Exemptions*Bank HHI Bin 4			-0.043**
			[0.019]
Exemptions*Bank HHI Bin 5			-0.039**
			[0.016]
Exemptions*Bank HHI Bin 6			-0.043**
			[0.017]
Exemptions*Bank HHI Bin 7			-0.036**
			[0.017]
Exemptions*Bank HHI Bin 8			-0.063***
			[0.020]
Exemptions*Bank HHI Bin 9			-0.065**
			[0.032]
Exemptions*Bank HHI Bin 10			-0.068***
			[0.020]
Controls	Yes	Yes	Yes
Observations	19,800	19,800	19,800
Log Likelihood	-51838	-51781	-51774

Appendix Table 2.
Effect of right censoring on forward citation measures.

This table provides year-by-year averages for four measures based on 5-year forward citations using data from USPTO, 1995-2005. Forward citations suffer a well-known censoring problem. The effect of censoring is especially apparent in the latter two-three years for each measure.

Year	5-year Forward Citations	Num. Patents with 5-Yr Fwd Cites in Top 25%	Num. Patents with 5-Yr Fwd Cites in Bottom 25%	Num. Subclasses that Cite Patent
1995	0.380	1.677	6.572	0.261
1996	0.696	1.481	4.694	0.264
1997	1.076	1.906	4.399	0.265
1998	1.400	1.925	3.837	0.253
1999	1.641	2.304	3.644	0.244
2000	1.772	2.449	4.200	0.227
2001	1.991	2.683	4.432	0.203
2002	1.895	2.628	4.246	0.182
2003	1.584	2.418	4.413	0.149
2004	1.282	2.044	4.108	0.103
2005	0.887	1.822	4.197	0.062