Creditor rights and innovation: Evidence from patent collateral

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Abstract

I show that patents are pledged as collateral to raise significant debt financing, and that the pledgeability of patents contributes to the financing of innovation. In 2013, 38% of US patenting firms had pledged their patents as collateral at some point, and these firms performed 20% of R&D and patenting in Compustat. Employing court decisions as a source of exogenous variation in creditor rights, I show that patenting companies raised more debt, and spent more on R&D, when creditor rights to patents strengthened. Subsequently, these companies exhibited a gradual increase in patenting output and the use of patents as collateral.

Keywords: Patents, innovation, corporate investment, law and finance.

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Innovation is critical to economic growth, but its financing is inhibited by problems of moral hazard and adverse selection. These frictions lead to credit rationing, increased costs of capital, and an inefficient level of innovation. In recent years, the use of patent portfolios as collateral for secured debt has become a common mechanism to mitigate these frictions. In this paper, I document the value of patent collateral for financing innovation among public firms. I also ask whether stronger creditor rights to patents primarily encourage or discourage financing and investment among patenting firms. The answer is unclear: Strong creditor rights may increase collateral value and thus financing capacity, but may also discourage risk-taking by allocating more bargaining power to creditors in the event of financial distress.

The link between creditor rights and innovation is important to understand because patents are a growing source of financing for innovative firms. I document that 16% of patents produced by US corporations have been pledged as collateral at some point, and the companies pledging them performed 20% of R&D and patenting in Compustat in 2013. These facts are surprising, given that innovative firms generally feature low tangibility and are thus often assumed to lack access to collateral. Patent portfolios are evidently an important exception to this rule. This observation suggests in turn that policy initiatives to increase the pledgeability of patents could alleviate financial constraints on innovation.

To investigate this possibility, and to document the above findings, I employ records from the United States Patent and Trademark Office (USPTO) on the use of patents as collateral. These records are part of the standard Assignments dataset, but have not been studied in prior work. I match these records to Compustat, then use this data to examine the characteristics of patents used as collateral. Patents pledged as collateral score highly on citation counts and generality. The firms that pledge them feature low tangibility, and cluster heavily in a small set of "high-tech" industries that account for most recent growth in aggregate R&D (as shown by Brown, Fazzari, and Petersen, 2009). Within-firm, total debt rises by roughly 4% of total assets in the quarter when a patent portfolio is pledged, and R&D expense is substantially above the firm's mean in this and subsequent quarters.

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These descriptive findings hint strongly that the collateral value of patents facilitates financing and investment for innovative firms. To establish a causal link, I study a natural experiment that increased patent collateral value via a strengthening of creditor rights.

This natural experiment consists of four court decisions, from 2002, 2003, 2007, and 2009, that clarified the legal status of patents nationwide. Specifically, they limited the extent to which patent law preempts property rights defined by state laws. For the most part, property rights are uniform across states, but one major exception is Delaware's pro-creditor Asset-Backed Securities Facilitation Act (ABSFA). The court decisions thus represented a relative strengthening of creditor rights for patenting firms incorporated in Delaware. Because the decisions only concerned patent law, they strengthened creditor rights *only* to patents, isolating their collateral value. My empirical strategy is a difference-in-difference that examines the evolution of financing and investment for Delaware- relative to non-Delaware-incorporated firms around the dates of the four court decisions.

The most important purpose of this natural experiment is to establish the quantitative importance of patent collateral, using quasi-experimental evidence that is more convincing than the descriptive correlations mentioned earlier. The magnitudes of this effect are important because conventional wisdom suggests that intangible assets have little if any collateral value. However, the results also serve a secondary purpose. As mentioned above, the optimal degree of creditor rights for innovative firms is an open question. Some prior empirical research (summarized below) argues that a creditor-friendly bankruptcy code discourages innovation. The theoretical arguments behind these findings focus on the challenge of motivating risky innovation. I find the opposite results, suggesting that investment by innovative firms in my sample is constrained more by a lack of collateral value.

I first document that the increase in patent collateral value, via strengthening of creditor rights, led to increased use of external finance in the form of debt. Treated firms' total debt levels, compared to untreated firms, rose by \$1 on average for every \$100 of total (book) assets in the two years on average following a court decision. Relative to a pre-event average debt-to-assets ratio of 0.27, this represented an increase in credit availability of roughly 4%, or \$25m given their average book assets of \$2.6bn. The increase in patent collateral values thus increased access to finance for innovative firms, as lenders evidently became more optimistic about the probability and speed of recovering patent collateral in default.

I next show that this increased borrowing translated into increased investment in innovation, as measured by the firm's spending on research and development (R&D). Treated firms' quarterly R&D spending rose by \$0.17 for every \$100 of total assets on average following a court decision. Combined with the financing effects documented above, this magnitude implies a pass-through of \$0.17 of annual R&D spending for each marginal \$1 of total debt. The R&D effect represents a 6.3% increase relative to pre-event average quarterly R&D-to-assets ratio of 0.027. Increasing the collateral value of patents thus alleviated credit constraints for investment in R&D. On the other hand, there was no increase in capital expenditures, a traditional measure of investment that is less important than R&D for innovative firms, and is more likely to yield tangible, easily-pledged collateral.

These findings stand up to a range of alternative approaches and robustness checks. The estimated effects are large even with fixed effects for the state of headquarters, the state of incorporation, or the interaction of these two. For firms in the high-tech industries mentioned above, the estimated financing effect is 40% larger, and the investment effect more than doubles. In event time, the effects developed shortly after the court decision dates, not as a differential trend beforehand, The estimates are similar for firms headquartered in each of the largest geographic clusters in the sample: the northeast, California, Texas/Colorado, and Florida/Georgia. The financing effect was absent for companies that had not yet produced patents, but was particularly strong for those that had previously pledged their patents as collateral, or that owned patents that had received many citations. Finally, none of this heterogeneity appears when comparing firms based on their *trademark* portfolios, instead of their patent portfolios, pinning down the legal implication of the court decisions studied.

Furthermore, the effects of the natural experiment were also stronger among firms that appeared, ex ante, more financially constrained. Dividing firms by their stock of tangible assets, tangibility ratio, payout policy, or profitability, I consistently find that firms with lower values of each measure exhibited larger increases in debt financing and R&D spending in response to the natural experiment. These findings echo the descriptive results earlier, which showed that the same firms were more likely to use patent collateral in the first place. Together, all these results demonstrate that the firms relying on patent collateral face significant financial constraints, and that an increase in the collateral value of their important *intangible* assets increased their ability to raise capital and invest it.

The increase in treated firms' investment also led to a subsequent increase in innovation output, as measured by new patent applications filed with the Patent Office. Between 2001 and 2013, treated firms produced about 19% more successful patent applications. This finding is robust to weighting patents by the number of forward citations they receive. I further show that firms in treated states were gradually more likely to pledge their patents as collateral following the court decisions, supporting the interpretation that the collateral value of those patents had increased. As with the financing and investment effects, these findings are not specific to any particular region, but rather are robust across the major geographic subdivisions of the sample.

My study contributes to a growing literature on the role of intangible assets in corporate finance. In contemporaneous work, Hochberg et al. (2017) match the USPTO collateral records to venture-capital data in order to study the market for venture lending, whereas I match the records to Compustat in order to analyze financial data. Loumioti (2013) uses Dealscan data to examine how redeployability and reputation affect the use of intangible assets as collateral, without focusing specifically on patents or investment. Amable et al. (2010) argue that the difficulty of enforcing security interests on patents is an important constraint on innovation, and Rampini and Viswanathan (2013) analyze more generally how limited enforcement determines collateral constraints and capital structure. Peters and Taylor (2016) show that q-theoretic models of investment perform substantially better when modified to account for intangible assets.

A theme in this literature is that innovation is more dependent on equity than debt financing (for example, Falato et al., 2013; Brown et al., 2013; and Brown et al., 2009). The interpretation has been that innovative firms are credit-constrained, with lack of access to collateral being one potential reason. My results are consistent with this view: Increasing the collateral value of patents enabled greater debt financing and investment by innovative firms. Similarly, Chava et al. (2017) show that borrowers with higher-quality patents receive lower spreads on bank loans; Farre-Mensa et al. (2016) show that a patent grant causally increases a startup's later success; and Saidi and Zaldokas (2017) show that the signaling value of a patent can substitute for a relationship lender's "soft" information.

My study also contributes to the literature on optimal creditor rights for innovative industries. Acharya and Subramanian (2009), Acharya et al. (2011), and Seifert and Gonenc (2012) show that countries with stronger creditor rights in corporate bankruptcy exhibit *less* secured financing and investment by innovative firms. One might conclude from these findings that the optimal strength of creditor rights must balance negative effects on innovative industries against positive effects in other industries.[1] In Section 4.6, I discuss how our divergent findings may be explained by differences in our institutional settings. Most importantly, the prior papers study a strengthening of creditor rights in settings where they are initially weak, whereas in my setting they are initially strong.

By construction, firms pledging patent portfolios have already performed the research necessary to produce those portfolios in the first place. Also, for data reasons, my analysis focuses on public companies, which perform the majority of corporate R&D in the United States (see Hirschey et al., 2012, Figure 3). Thus, my paper should be understood as studying innovation by relatively mature firms, not startups looking for venture capital.

[1] Theoretical arguments are provided in Acharya and Subramanian (2009) and Berkovitch et al. (1998). The general idea of "failure tolerance" is discussed in Manso (2011) and Tian and Wang (2011), among others. A parallel literature also shows that debtor-friendly provisions in *personal* bankruptcy law stimulate entrepreneurship and early-stage investment (e.g. Cerqueiro et al., 2016, Berger et al., 2011, Berkowitz and White, 2004), but the effects of personal and corporate bankruptcy laws are theoretically distinct.

1 Data

I first construct the dataset of patents pledged as collateral in the United States. When a lender accepts a patent as collateral, it can file a record of that fact with the USPTO, which provides public notice so that a third party cannot purchase the patent and subsequently claim ignorance of the security interest.[2] These records are entered into the USPTO Assignments dataset, which records the ownership of patents after they are issued. I download the Assignments data through the Google Patents website, and I assemble a dataset containing the date, patent number, and borrower and lender names for each pledge of patents as collateral for a loan. I match the patent collateral data to the NBER's *pdpass* firm identifier through a string comparison algorithm described in Appendix C.2.[3]

Next, I obtain the public dataset from the NBER Patent Data Project, which records patent grants, citations, and classifications, along with the *pdpass* identifiers. The vintage of the NBER data used in this project ends in 2006, but I extend it to the end of 2013 due to the relatively recent timing of the court decisions that I use for identification. To do this, I download the Grants and Applications datasets from the Google Patents website, and I match the post-2006 patents to pre-2006 *pdpass* identifiers using the same comparison algorithm mentioned in the previous paragraph. Using the Citations dataset, I also measure the total number of citations that each patent receives from subsequent patents granted through the end of 2013.[4]

Finally, for any analysis requiring financial data, I merge on Compustat *gvkey* identifiers, using the linkfile between *pdpass* and *gvkey* provided by the NBER Patent Data Project, and aggregate the dataset to a *gvkey*-quarter panel. I merge this data onto quarterly Compustat, dropping any *gvkey*-quarters that do not appear in Compustat. I restrict to companies

[2] Formally, the USPTO filings protect against a potential bona fide purchaser. They do not perfect the security interest, which is accomplished through state UCC filings for patents as with any other asset class.

[3] Serrano (2006), Serrano (2010), and Galasso et al. (2013) previously used the Assignments data to study the secondary market for patents, but did not use the collateral data.

[4] In an effort to mitigate the well-known right-truncation problem with patent citation counts (see Hall et al., 2001), I include only citations by patents granted within the first five years of the cited patent's term.

incorporated in the United States (FIC code "USA"). I remove any observations with zero, negative, or missing total assets (item ATQ). I calculate total debt as the sum of long-term debt (DLTTQ) and short-term debt (DLCQ), and I remove the acquired, in-process component of research and development expense (item RDIPQ). For any firm-years in which R&D is only reported in the fourth fiscal quarter, my extensive hand-checking has confirmed in every case that this fourth-quarter number is actually an annual total, so I divide this reported value over all four quarters in the fiscal year. Any remaining missing values of R&D or total debt are replaced with zero. To limit the influence of large firms, I scale all financial outcomes by total assets, and further winsorize these ratios at zero and 1.

2 Descriptive analysis of patent collateral

In this section, I use the patent collateral data to document several stylized facts suggesting that the pledgeability of patents contributes to the financing of innovation.

The first stylized fact documented in this section is that the frequency of patents being pledged as collateral has increased rapidly in recent years. Figure 1a traces the increase over time in the annual number of patents pledged, to over 40,000 patents per year in recent years. Figure 1b restates this growth as a proportion instead of a frequency: Among patents granted to US companies, about 15% from a given vintage are pledged as collateral within five years of being granted, a number that has also risen steadily over the past three decades. These findings of an upward aggregate trend in patent collateral usage are consistent with Loumioti (2013), who reports that the fraction of Dealscan lending collateralized primarily by intangible assets (such as patents) increased from 11% to 24% during 1997-2005, as well as with figures for venture-stage companies reported in Hochberg et al. (2017).

The next stylized fact documented in this section is that patents are more likely to be pledged as collateral if they appear more redeployable. Table 1 establishes this through patent-level regressions, where the outcome variable is an indicator for an individual patent ever being pledged as collateral. The first two columns show that pledged patents are highly-cited.[5] This evidence is consistent with Chava et al. (2017), who show that firms with more highly-cited patents are able to raise more debt financing, as well as Czarnitzki et al. (2014) and Farre-Mensa et al. (2016), who show that patents help small firms gain access to finance. However, the Patent Office data isolate the *collateral* value of highly-cited patents (they can attract buyers if the firm fails, thus providing downside protection for lenders) from their *information* value (firms producing them are likely to have profitable projects), in a way that was not possible in those previous papers.

As a different way to measure redeployability, the third column of Table 1 shows that a patent is more likely to be pledged as collateral when it scores highly on the "generality" measure of Hall et al. (2001): A one-standard-deviation increase in generality is associated with a 1.2% greater probability that the patent is pledged as collateral. Generality is defined as the dispersion of future citations to a patent across technology categories, and it is designed to reflect a wider range of industries building on the patent. Thus, like the raw citation count, it should be higher for patents with many potential purchasers. Altogether, the findings in the first three columns of Table 1 are consistent with studies emphasizing the general importance of redeployability to collateral value (e.g. Benmelech and Bergman, 2009).[6]

Column 4 of Table 1 further shows that publicly-owned companies are *less* likely to pledge their patents than private firms, possibly because they have relatively easier access to equity capital. Finally, Column 5 combines all these explanatory variables together, along with a quadratic time trend (to capture the aggregate pattern in Figure 1b), and shows that the magnitude and statistical significance of the marginal effects remains about the same.

Despite these marginal effects, the regressions in Table 1 achieve consistently low R^2 values. This is consistent with anecdotal evidence, which suggests that pledging patents is

[5] As noted in Section 1, I count only citations received within the first five years after the patent is granted, in an attempt to mitigate right-truncation problems.

[6] In fact, redeployability may not be a necessary condition for patent collateral to address financing frictions. Because patents are often core assets, pledging them as collateral may provide lenders with bargaining power through the credible ability to shut down the firm in distress, even if the patent has no potential buyers. However, I make no attempt to quantify this mechanism here.

a decision made at the level of the company (portfolio), not the individual patent. This likely reflects extreme heterogeneity in individual patents' values, and the adverse selection problems that would arise if the company only offered to pledge a few of them. From this point forward, I analyze the decision to pledge patents at the company level. I restrict to the matched *gvkey*-quarter panel in order to have reliable firm identifiers and financial data.

Table 2 continues the descriptive analysis, now at the firm level. The cross-section of firms in these regressions is sampled from Compustat in 2010q1, and is restricted to only firms that had at least one patent by that quarter. The outcome variable is an indicator for having pledged patents at any point up to that quarter. The firm's patents and citations are measured cumulatively up to that quarter. The financial variables are averaged over the prior ten years.

Column 1 of Table 2 shows that firms with more patents, and more highly-cited patents, are more likely to pledge them. The first relationship is probably largely mechanical, as the total collateral value of the portfolio should increase in the number of patents. The second is consistent with the patent-level findings of the previous table. The negative interaction term between these two effects suggests that the marginal predictive power of a citation decreases as the portfolio size grows, which also seems natural.

Column 2 shows that several common capital-structure regressors are also meaningful for predicting patent collateral usage. Some are consistent with standard empirical predictors of higher leverage ratios: Firms that pledge patents as collateral have low cash balances and are unlikely to pay dividends. Contrary to standard capital-structure results, these firms have *low* tangibility; but Column 4 adds fixed effects for 3-digit SIC industry, and finds that this relationship becomes weaker and insignificant, suggesting that it was mostly driven by across-industry variation. The other relationships survive the fixed effects with roughly unchanged magnitudes. These firm-level regressions attain better fit than the patent-level regressions did, with the full set of controls in Column 3 exhibiting an R^2 of 11%.

Building on the tangibility results in Table 2, the next stylized fact is that the *industries* in which patent collateral is used are industries in which patents themselves are likely to be important collateral, not merely an afterthought for firms that pledge tangible assets simultaneously. Figure 2 displays the top ten industries, identified by 3-digit SIC code, of Compustat companies that pledge their patents as collateral. These are all industries that feature low tangibility, but high rates of patenting. In the remainder of the paper, I will refer to these ten industries as "high-tech" industries.

The list includes all of the seven industries that were identified by Brown et al. (2009) as accounting for the growth in US R&D from 1994-2004. Unsurprisingly, then, Figure 3 further documents that firms which have pledged patents as collateral perform a significant share of R&D and patenting: In 2013, these firms accounted for 20% of both R&D dollars and patenting numbers by Compustat companies, and accounted for 38% of Compustat firms that filed patents. In sum, patents are used as collateral in low-tangibility industries that are important to aggregate corporate research investment.

To complement this industry-level breakdown, Figure 10 in the Internet Appendix lists the top 15 lenders in the sample, by number of deals since 1976. For this figure, I return to the full sample of firms (i.e. not just Compustat borrowers), in order not to miss the prominence of Silicon Valley Bank (SVB), the major player in the market for venture loans and the second-most-frequent lender in the sample after Bank of America. However, the remaining analysis in the paper focuses on Compustat, and therefore should be understood as studying debt financing for relatively mature firms, rather than the venture loans or growth capital in which SVB specializes.

The final stylized fact documented in this section is that patent-backed loans are associated with economically large amounts of both debt usage and intangible investment. I first demonstrate this fact in the cross-section. The results are summarized in Table 3, which uses the same sample and control variables as in Table 2, but moves the indicator for pledging patents as collateral from the left- to the right-hand side of the model, where it replaces the IP-related explanatory variables. The first three columns show that companies pledging patents as collateral feature significantly higher total debt as a fraction of total assets, even controlling for standard predictors of corporate leverage, and when restricting to the high-tech industries identified in Figure 2. For these high-tech industries, the last column also shows that firms pledging patents as collateral exhibit relatively high rates of R&D spending.

Complementing these cross-sectional results, I next show that patent collateral is associated with significant *within-firm* changes in debt usage and R&D investment. Figure 4 restricts to the "high-tech" industries identified in Figure 2, as in the last two columns of the previous table, and examines the quarterly within-firm dynamics of financing and investment around the date of a patent-backed loan. Formally, I regress total debt and R&D expense (both scaled by total assets) on a firm fixed effect, plus distributed lags of an indicator for pledging patents as collateral in a given quarter. Panel (a) shows that debt increases substantially in the quarter when a firm pledges its patents: The flow of long-term debt issuance and retirement increase by about 7% and 3% of firm assets respectively, for a net increase of 4%. Accordingly, the firm's level of total debt increases sharply by about 4% of total assets in the quarter in which patents are pledged. The increase in debt is not substituted by an increase in cash holdings, which in fact trend downward leading up the loan date (this trend is omitted to keep the figure from becoming cluttered).

Moreover, Panel (b) of Figure 4 shows that quarterly R&D investment rises in anticipation of the loan and peaks shortly after the loan quarter, exceeding the firm's mean level by about 1% of total assets for the rest of that year. (For comparison, the average quarterly R&D-to-assets ratio for firms in these industries is 3.9%.) In contrast, there is little change around the loan date in capital expenditures, suggesting that companies that are reliant on the collateral value of their intangible assets invest primarily in R&D.

These results demonstrate that patent collateral is associated with economically large amounts of credit and R&D spending, both cross-sectionally and within-firm. The mere fact that lenders bother to notify the USPTO of their collateral claims, coupled with the prevalence of these events in R&D-intensive, low-tangibility industries, both suggest that patents themselves are valuable collateral in these deals. However, the evidence does not yet prove that fact unambiguously. Most importantly, even for loans within high-tech industries, we cannot necessarily attribute all collateral value to patents, as opposed to other assets that may also have been pledged alongside them as part of the same loan agreement.

To isolate the collateral value of patents, I implement a natural experiment beginning in the next section. Its findings will demonstrate that patents are economically meaningful collateral. They will also address the deeper question, raised earlier, of the optimal degree of creditor-friendliness of the bankruptcy code for innovative firms.

3 Natural experiment: Strategy

I first overview the intuition of the strategy, then describe its components in detail:

My first source of variation is cross-sectional. Delaware features an unusual pro-creditor law governing secured transactions, so that firms incorporated in Delaware are subject to stronger creditor rights. This law is the only significant non-uniformity across states in property rights under the Uniform Commercial Code (UCC).

To study the effects of stronger creditor rights, one might then consider a comparison of firms incorporated in Delaware and elsewhere. However, this variation would not be enough, for two reasons. First, there are other differences across states aside from UCC property rights, and these differences may correlate with both the decision to incorporate in Delaware and corporate outcomes like debt financing, R&D, or patenting. Second, the law applies to all assets, and thus cannot uniquely identify the importance of patents as collateral.

I address both concerns through additional time-series variation in the perceived *importance* of state laws for the ownership of patents. Four federal court decisions have determined, in distinct settings, that federal patent law was relatively narrow in its intent to preempt state property law. These have led to a consensus that ownership of patents is primarily a matter of state law, as with other asset classes. My strategy is to study *changes* in financing and investment by Delaware-incorporated firms around these decision dates.

This strategy differences away any fixed effects of incorporation in Delaware, isolating only those characteristics that *changed* around the decision dates for Delaware-incorporated firms relative to other firms. The decisions only affected a narrow issue – the importance of state laws in governing the ownership of patents. Any other fixed characteristics of Delaware incorporation, such as its well-developed court system, are differenced away since these features did not change around the court decisions. Moreover, no asset class other than patents would have been affected by the court decisions, since they only interpreted patent law – not, for example, trademark law or bankruptcy law.

The following subsections describe the empirical approach in more detail: first, the pro-creditor law in question, second, the court decisions that elevated the role of state law for patents, and third, the sample construction and regression specifications. For more detail on the related legal issues, see Appendix F.

3.1 Pro-creditor state law

The pro-creditor law in question is Delaware's Asset-Backed Securities Facilitation Act (ABSFA), effective since January 2002. It allows borrowers to "sell" collateral to a special-purpose entity (SPE) that avoids the automatic stay if the borrower enters bankruptcy. It accomplishes this goal by stating explicitly that, if the parties to a transaction agree that it constitutes a sale and transfer of property rights, then it must be respected as such.[7,8]

Without a law like this one, bankruptcy judges have the authority to recharacterize the "sale" as a disguised financing, defeating the strategy.[9] This authority arises from the

[7]Section E of the Internet Appendix to the paper reproduces the full text of the law. See Kettering (2010) and Janger (2003) for discussion of the legal implications of this law, and Li et al. (2016) for a related empirical study of this and similar laws in other states. More generally, Scott and Smith (1986) show that lenders price loans to incorporate the ex-ante costs of recovery created by automatic stay protection and other features of the Bankruptcy Code.

[8]Strictly speaking, the transfer of collateral to an SPE is optional. However, as a secured borrower nears financial distress, lenders can likely demand the transfer to the SPE in exchange for not forcing the firm into default. This mechanism, modeled formally in Berkovitch et al. (1998), makes stronger creditor rights effectively mandatory even when they are nominally optional.

[9]For example, *Paloian v LaSalle*, the best-known true-sale case, has gone through multiple hearings to

bankruptcy court's goal of pursuing equitable outcomes in bankruptcy. However, bankruptcy courts cannot overrule state laws, which ultimately define property rights in commercial transactions. Thus, the risk of recharacterization only applies to the extent that property rights are ambiguous under state law. Delaware's law, by specifying those rights explicitly, represented an effort by the state to remove the risk of recharacterization in bankruptcy, and thus to expand the menu of contracts for firms incorporated there.

Historically, policymakers have sought to harmonize secured lending laws across states. The ABSFA has been the only major exception, and it has received scholarly criticism for interfering with this effort; see Janger (2003). Thus, it represents an ideal source of variation for this study, when combined with the court cases described in the next section: They increase the applicability of state property laws to patents, and the ABSFA is the only irregularity in this law for Delaware-incorporated firms as opposed to other firms.

A few other states have enacted similar laws with variations in wording, but they have attracted less scrutiny than Delaware's, making it difficult to judge their impact. For simplicity, I drop firms incorporated in those states: Alabama, Texas, Louisiana, Virginia, South Dakota, Nevada, Ohio, and North Carolina. This has little practical impact, as none of these states of incorporation comprises more than 2% of the sample, except Nevada at 6%.

3.2 Court decisions

All four decisions were by federal courts, not state courts (two by bankruptcy courts, and two by the Court of Appeals for the Federal Circuit). The four decisions turned on distinct but related legal issues. Each held, for the issue at hand, that patent law was relatively narrow and did not override state law.[10] The decisions are summarized briefly here, with

determine whether collateral for a revolving loan, held in an SPE, was actually bankruptcy-remote. A 2013 opinion in the case even suggests that Delaware's ABSFA could have aided the creditor if it existed in Illinois (the relevant jurisdiction). Especially relevant for this paper, Kieff and Paredes (2004) suggest that the ABSFA may increase the effectiveness of intellectual property SPEs.

[10]The relative importance of state versus federal law for patents was an uncertain and evolving issue during the 2000s. The issue arises because courts can rule that federal patent law implicitly preempts state law in certain cases. Similar issues arise with other asset classes: Copyright law, for example, has been

a more thorough discussion provided in Appendix F. The most important point to make is that all four addressed the specifics of patent law, and did not affect any other asset class.

The first, dated March 26, 2002, concerned the bona fide purchaser defense, which is generally governed by state law but is also mentioned in the Patent Act. An earlier decision (predating the ABSFA) had suggested a broad bona fide purchaser defense for patents compared to other asset classes, and thus a departure of patent law from state common law. This decision, on appeal, discarded that precedent, reversing the outcome. The court emphasized the importance of relevant state law (North Carolina) in reaching its decision.

The second, dated May 30, 2003, contained two separate findings both limiting the scope of patent law to preempt state law. First, the court held that state-level procedures for perfecting a security interest are sufficient for patents; a USPTO filing is not required.[11] Second, the court rejected a novel argument that patent law implicitly redefines a limited sale as a license. The court's reasoning on both subjects echoes the previous case: despite their federal regulation, patents are not exceptional assets in property or contract law.

The third, dated May 15, 2007, included three relevant rulings. First, the court disallowed an attempted foreclosure against patent collateral that ignored some requirements of California law and relied instead on USPTO filings. Second, it held that state-level procedures are *necessary* to perfect a security interest (complementing the above ruling that they are sufficient). Third, it held that USPTO filings do not allow one to perfect by possession, which would obviate the requirement to register the security interest at all.

The fourth, dated August 20, 2009, further addressed foreclosure. It held that foreclosing on a patent under state-law procedures successfully transfers ownership, independent of the

found to preempt state law quite broadly, contrasting the trend for patents (*In re Peregrine Entertainment*). Uncertainty about this issue has constrained the growth of the patent-backed loan market (Amable et al., 2010, Stevens, 2005).

[11] Under state law, one perfects a security interest through a public filing with the relevant Secretary of State. However, certain asset classes are excepted due to conflicting federal laws. For example, with copyrights, a security interest is properly perfected by filing with the federal Copyright Office. Ibrahim (2010), in a study of venture debt, notes that "perfecting security interests in software requires compliance with the Federal Copyright Act, which preempts state law and is more difficult for secured parties to comply with" compared to the procedures for other intellectual property.

assignment system created by the Patent Act. The decision also noted the public benefits of increasing patent pledgeability by harmonizing their treatment with other asset classes.

These decisions have been cited for the ways they changed perceptions about the applicability of state laws to patents. Shepard's Citations reports that they have been cited a total of 79 times by subsequent court decisions, including by all of the circuit courts, and 135 times by law reviews and treatises on the law, and each decision has received at least 20 citations across these categories. Unreported negative-binomial regressions show that this volume of citations is significantly greater, both economically and statistically, than the average for all decisions released by federal courts on the same days. Regarding the content of those citations, Appendix F provides references to each decision by legal scholars that explicitly reference the applicability of state laws to patents. Overall, the evidence suggests that the court decisions elevated, nationwide, the perceived extent to which state property laws govern the ownership of patent collateral.

3.3 Implementation

The court decisions elevated the importance of state property law for the ownership of patents, including in bankruptcy. This should be expected to have heterogeneous effects across the sample of firms, depending on whether they were incorporated in creditor-friendly Delaware. I exploit this setting as a natural experiment to examine the effect of strengthened creditor rights on patenting firms.

To implement a difference-in-difference approach, I isolate an event window extending 8 quarters before and after each court decision date. I extract all four of these event windows from the full panel, then "stack" them together and run a single regression, averaging the treatment effect across the four events (an approach previously taken, for example, by

Gormley et al., 2013). My specification is thus:

$$\begin{aligned} y_{iskq} &= \alpha \times \mathbb{1}\{DE\}_{sk} + \delta \times \mathbb{1}\{after\}_{kq} \\ &+ \beta \times \mathbb{1}\{DE\}_{sk} \times \mathbb{1}\{after\}_{kq} + \epsilon_{iskq} \end{aligned} \tag{1}$$

where i indexes firms, s indexes states of incorporation, $k \in \{1, 2, 3, 4\}$ indexes court decisions and their associated 8-quarter windows, and $q \in [-8, -8]$ indexes quarters in event time relative to the court decision date ($q = 0$) within a given window. α is a fixed effect for firms incorporated in Delaware, and δ is a post-event fixed effect for quarters that follow the court decision within a given event window.

The key quantity of interest is β, the coefficient on the interaction term between these fixed effects, interpreted as the causal impact of strengthened creditor rights. For event window k, this interaction term turns on for Delaware-incorporated firms in the quarters after the corresponding court decision is announced. Any confounding variation would have to involve a systematic change in the outcome variables for Delaware-incorporated relative to other firms around the decision dates. The specification also allows for many potential fixed effects, which I will explore in the empirical analysis.

Using the quarterly Compustat sample described earlier, I code all Delaware-incorporated firms as treated, and all other firms as untreated. The firm's state of incorporation is taken from Compustat's company file, and thus by design is fixed over time. As mentioned in Section 3.1, I drop from the sample firms incorporated in states with laws similar to Delaware's ABSFA. These comprise 12.4% of the sample. The resulting panel contains 11,212 unique firms, of which 65% are treated by virtue of being incorporated in Delaware. Figure 5 displays the geographic distribution of treated and untreated companies across states, revealing substantial geographic heterogeneity.

Table 5 summarizes several financial characteristics of treated and untreated firms as of the beginning of 2000 (or the firm's first sample quarter, if this is later than 2000Q1).

The subsamples are similar in size, tangibility, and patenting intensity, but Delaware firms exhibit higher levels of debt financing and R&D (scaled by assets), and a greater probability of having pledged patents as collateral. However, it is difficult to attribute this difference to any particular characteristic of Delaware, such as creditor rights. The goal of the difference-in-difference strategy is to identify the creditor-rights channel more tightly, under the key identifying assumption that the two subsets of firms would have followed parallel trends in the outcome variables absent the court decisions. I will corroborate this assumption by examining trends in treatment effects during pre-treatment and placebo periods.

3.4 Interpreting the estimated effects

Separately from parallel trends, one may be concerned by the clear nonrandomness of treatment. Firms choose to incorporate in Delaware, and Table 5 documents that this decision correlates with at least some observable characteristics. Granted the parallel-trends assumption, the concern here is not with interpreting the results causally, but rather with the possibility of heterogeneous treatment effects: If firms that benefit from stronger creditor rights systematically sort into Delaware in the first place, then their positive responses are not informative about other firms. Policies strengthening creditor rights might be good for Delaware firms, but not for the nation as a whole.

First, note that few sample companies, if any, could have sorted on the ABSFA law or the court decisions themselves, as nearly all sample companies appeared in Compustat before these events. The question is more generally what motivates firms to incorporate in Delaware. Legal scholars emphasize the benefits of contractual flexibility and a body of law that is widely understood by venture capitalists and institutional investors. The costs are real but relatively modest: taxes, fees, reporting requirements in multiple states, and potential travel obligations, as well as the costs of changing state later.[12]

While a full theory is beyond the scope of this paper, one might fairly characterize the

[12] See further discussion in Black (2007) and Carney and Shepherd (2009).

decision as a judgment call about the firm's growth prospects. Then the concern is that firms with stronger anticipated growth prospects, making them more likely to choose Delaware, are also more likely to respond positively to a strengthening of creditor rights.

This judgment is left to the reader. However, it bears repeating that this issue only arises in interpreting the estimates as average treatment effects (ATE) across the entire sample. Granted the parallel-trends assumption, we can at minimum interpret the results as an average treatment effect on the treated (ATT): the effect of stronger creditor rights on Delaware-incorporated public firms (which are the vast majority of the sample), or possibly on firms with strong growth prospects (which are responsible for most innovation). These are interesting quantities in their own right.

4 Natural experiment: Results

4.1 Financing and investment

In Column 1 of Table 6, I estimate specification (1) across the full Compustat sample, using the ratio of total debt to assets as the outcome variable. In the two-year window after a decision date, total debt increased by an average of roughly one percent of total assets, as captured by the coefficient on "Treated, after." Mean total assets among treated firms in 2000 was $2.6bn (see Table 5), so this translates to about $26m of additional debt per firm. This reflects a substantial amount of marginal credit released to the average firm from one court decision; the implied cumulative effect of all four decisions is four times as large.

Column 2 adds fixed effects for the company's state of headquarters, interacted with event-window dummies, to control for regional differences. The estimated effect on debt financing in this specification is nearly identical to Column 1. In Column 3, I restrict the sample to the "high-tech" industries identified in Figure 2, which typically exhibit low tangibility and leverage, yet also exhibit the highest frequency of pledging patents as collateral. The estimated effect for these firms is consistent with, and in fact 40% larger than, for the full sample. For this subsample, Column 4 adds a headquarters fixed effect as in Column 2, and Column 5 further interacts this with the firm's state of incorporation. Even at this tight level of identification, the effect remains statistically and economically significant.

Again, the interpretation of these results is that creditors of Delaware-incorporated firms became increasingly confident in the speed and probability with which they could foreclose against patent collateral in the event of default. They therefore became willing to extend additional credit to borrowers who had previously borrowed up to their maximum willingness to lend. If patent collateral was not economically important, we would observe no effect at all. If the primary effect of strengthening creditor rights was instead on firms' incentive to borrow, with credit constraints not playing a key role, we would observe a negative effect.

Next, I demonstrate that the marginal credit released by the court decisions flowed through to investment in research. Tables 7 repeats all the above analysis, changing only the outcome variable to quarterly R&D expense (instead of total debt) scaled by total assets. Column 1 shows that, across the full Compustat sample, quarterly R&D expense increased by about 0.17% of total assets following the court decisions, compared to a sample mean among treated firms of 2.7%. Given average total assets of $2.6bn, this represents about $4.4m of additional quarterly R&D spending per firm, or $17.6m annually.

The remaining columns of Table 7 perform the same robustness checks as before: The estimated quarterly increase in R&D spending survives fixed effects for state of headquarters, and rises to 0.39% of total assets in high-tech industries. In contrast, I show that capital expenditures exhibit no treatment effect around the event dates, in a set of results starting in Table 13 in the Internet Appendix to this paper. These provides a useful placebo, showing that the effect are specific to innovation investment. Table 15 in the Internet Appendix also demonstrates robustness of the debt effect to standard capital-structure controls.

These are all causal effects of strengthened creditor rights. If one further accepts the exclusion restriction that debt financing was the only mechanism by which this strengthening could have affected R&D spending, then we can divide the debt and R&D effects to calculate, for a marginal dollar of external finance, the fraction that is spent quarterly on R&D. Across all firms, this fraction is roughly 17% (= 0.00169/0.0102), and for high-tech firms specifically it is a larger 27% (= .00394/.0145). These results quantify the financial constraints affecting sample firms at the time of the court decisions.

Lerner and Seru (2015) point out that, in empirical innovation research, regional differences in research and patenting activity may swamp the magnitude of any other desired comparison (such as my natural experiment). Since my analysis is based on borrowers' state of incorporation, not their headquarters location, I can address this concern by estimating effects, for example, *within* the Northeast, or within California. In Tables 11 and 12 in the Internet Appendix, I show that the debt financing and R&D effects are strikingly robust, and statistically significant, across the four major regions in Figure 5 that feature a meaningful density of both treated and untreated firms. The important conclusion is that the estimated effects are not driven by one specific region, but rather reflect a generalizable effect of increasing patent collateral value via strengthened creditor rights.

Table 16 in the Internet Appendix separates the four decisions, and adds interaction terms specific to each one, to investigate heterogeneity in the treatment effects. The second decision is set as the baseline in this analysis, because it exhibits neither the greatest nor the smallest effect on either debt financing or R&D expense. The table shows that the second decision has statistically significant effects on its own, while none of the magnitudes of the other decisions are statistically distinguishable from the magnitude of the second one. For this reason, the four decisions will be discussed as a group, without attempting to draw inferences from any heterogeneity in their effects.

Finally, for the high-tech firms that exhibited the largest effects on debt financing and R&D spending, Figure 6 examines the parallel-trends assumption by interacting the main specification with a full set of quarter dummies and plotting the resulting regression coefficients. The difference between treated and untreated firms in terms of debt and R&D is fairly stable before the court decision dates. The treatment effects develop in event time shortly after the quarter of the court decision, not as part of a surrounding differential trend.

In contrast to Figure 4, which also displayed dynamics of financing and investment but from a descriptive perspective, Figure 6 can be interpreted as reflecting the causal effect of patent pledgeability on financing and investment. To clarify further the content of these plots, Figure 12 in the Internet Appendix reports a placebo analysis, in which the "event dates" are taken from the sample years in the sample with no court decision. The estimated event-time coefficients exhibit a flat trend, with none significantly different from zero.

4.2 Heterogeneity in effects by financial constraints

Aside from industry affiliation, there are other forms of firm-level heterogeneity that also predict the response to the natural experiment, and that are useful in establishing the interpretation of the effects as capturing financial constraints. Table 8 summarizes this heterogeneity: Each column interacts all terms in the core empirical model with an indicator variable, listed at the bottom of the table, that proxies for the presence of financial constraints. Panel (a) displays the results for debt financing, and Panel (b) for R&D expense. For space reasons, the table reports only the original treatment-after interaction coefficient, plus its triple-interaction with the financial-constraints proxy.

In each column, the firms that are presumed to be more financially constrained also exhibit stronger effects of the natural experiment:

In Column 1, which splits firms at $200m of total assets (the median among the treated firms), the difference in debt financing is not statistically significant. This echoes the finding in Column 2 of Table 2 that size had no clear association with patent collateral usage when we did not separately control for patent portfolio size. However, the difference in the R&D effects between the two subsamples is statistically significant in this column.

The remaining columns split firms based on amount of property, plant, and equipment (PP&E); on tangibility (PP&E as a fraction of total assets); on lack of payout; and on lack of profitability. In each table, the firms that are hypothesized to be more financially constrained exhibit stronger effects of the natural experiment, and the difference between the subsamples is statistically significant, for both debt financing and R&D expense. This table provides strong supporting evidence that the effect of the court decisions was to relax financial constraints for patenting companies.

4.3 Heterogeneity in effects by intellectual property portfolio

The natural experiment results so far require only the standard Compustat database. At this point, I re-introduce the patent data to continue investigating heterogeneity in effects:

Table 9 shows that a company's patenting history is informative about the strength of the estimated effect of increasing its patent collateral value on its debt financing. Column 1 restricts the sample to companies that are eventually observed to produce patents, but that have not yet done so as of the beginning of the event window. These companies are a useful placebo sample, as they are known to have no patent collateral.[13] The results reveal that there is no effect on debt financing among this subsample.

Conversely, Column 2 interacts the post/treatment dummy with the number of patents the company has produced as of the beginning of the event window. Here, the sample is restricted to companies that will eventually receive patents, but currently have received no more than six patents (the median number). As in Column 1, companies with zero patents exhibit no treatment effect (see third row), but each marginal patent increases the estimated debt financing effect by 0.6% of total assets.[14] Using the same subsample, Column 3 estimates a much larger effect on debt financing among the 9% of sample firms that had *already* pledged their patents as collateral in the USPTO data before the start of the event window: For these firms, the effect on debt financing is 4.7% of total assets, compared with roughly 1% for the rest of the sample. Again, this supports the interpretation that the

[13] Companies that are never observed to patent may be false negatives, as the company-name match from patent data to Compustat is imperfect and designed to be conservative (see Appendix).

[14] Beyond six patents, the marginal effect of a patent becomes statistically indistinguishable from zero. Presumably, past this point, the simple count of patents becomes a poorer proxy for the overall collateral value of the portfolio, which would be consistent with Table 2.

natural experiment captures the impact of an increase in the collateral value of patents.

Finally, Column 4 retains all firms that had already produced patents as of the beginning of the respective event window, and proxies for the quality of their existing patent portfolios with the well-established metric of citations. Portfolios generating a high number of citations should have many potential buyers, and thus should constitute more-valuable collateral. To capture this idea, I sort firms based on the average number of citations ultimately received by their existing patents, and divide the sample at the median value of 4 citations per patent.[15] Column 4 shows that the debt financing effect is indeed largely concentrated among portfolios with a high average citation count.

The court decisions only concerned patent law, so no other asset class should have been affected. To provide corroborating evidence, in Table 10, I conduct a placebo analysis of the firm's *trademark* portfolio, using trademark registration data from the USPTO.[16] The table builds on Column 2 of Table 9, by modifying that specification in several ways to check if trademark portfolios have similar predictive power. In each case, the answer is negative:

Column 1 repeats the prior specification, but interacts with the firm's number of trademarks. In contrast to the prior result, the triple-interaction term here reflects no moderating effect of the firm's *trademark* portfolio on the natural experiment. Instead, the "Treated, after" coefficient in the third row matches the magnitude of the core results in Table 6.

For consistency, the sample in Column 1 is the same as was used in Column 2 of Table 9, namely patenting companies that had six or fewer patents as of the beginning of the event window. In Column 2, the sample uses the analogous criteria for trademarks: companies that eventually produce trademarks, but had six or fewer trademarks as of the beginning of the event window. Now all interaction terms appear insignificant.

In Column 3, the subsample is the intersection of the previous two: Firms that receive both a patent and a trademark at some point, but had six or fewer of each as of the beginning

[15] As noted in Section 1, I count only citations received within the first five years after the patent is granted, in an attempt to mitigate right-truncation problems.

[16] Section C.4 of the Internet Appendix provides details about the trademarks data.

of the event window. Again, the triple-interaction term remains insignificant.

Finally, Column 4 uses this intersected sample, but interacts with the size of the patent portfolio, thus returning to the specification of Column 2 of Table 9. Contrasting the non-results in the first three columns, the number of patents in this specification again appears as a powerful predictor of the effect of the natural experiment, as in Column 2 of Table 9.

In sum, the effects of the natural experiment seem indeed to be driven by patents, not other intellectual property. This seems natural since patents are the only asset class that should have been affected by the court decisions.

The results in this subsection have used intellectual property data to explore heterogeneity in the effects of the natural experiment, and thereby pin down its mechanism. In the next subsection, I turn to a second and equally important use of the patent data: Demonstrating that corporate innovation responded to the strengthening of creditor rights through output (an increase in patents produced), and not only through input (R&D spending).

4.4 Patenting output

The preceding results showed that a strengthening of creditor rights to patent collateral increased firms' access to finance, and in turn their investment in R&D, which can be thought of as an input to innovation. This section shows that, in turn, firms also increased their *output* of innovation, as measured by patenting volume. This is a natural corollary: When creditor rights to patents strengthened, and patent collateral value thereby increased, firms used newly-available credit to expand research and subsequently produce more patents.

Patenting output is unlikely to respond to a strengthening of creditor rights within the two-year windows employed in the previous analysis, but extending the length of these windows would lose the tight identification employed in the core specifications. Instead, I construct treated and untreated cohorts in 2001 (the year before the first event date) and follow them over time. The drawback of this longer-horizon specification is that more sources of confounding variation can enter. On the other hand, greater variation over the longer time series allows for tigher, firm-level fixed effects. I include these in all subsequent analysis to tighten identification as much as possible. My approach is thus to regress the log of the firm's cumulative patent grants, timed to the end of the application year, on firm fixed effects, year fixed effects, and interactions between year and treatment dummies.[17]

Panel (a) of Figure 7 summarizes this analysis by plotting the estimated yearly interaction coefficients, along with bands two standard errors wide above and below. The dynamics of these coefficients are revealing: In 2002 and 2003, treated companies exhibited no significant departure from their 2001 patenting volumes relative to untreated firms, consistent with the view that the increased R&D spending had not yet translated into the output of new patents. But starting in 2004, the coefficients exhibit a gradually-developing, positive effect on patenting cumulative output, reaching about a 19% differential by 2013. This represents about 6 marginal patents relative to the initial sample mean portfolio size of 32 patents (from Table 5). Panel (b) shows that the estimated patenting effect remains large when separately estimated in California, in the northeast, and in the remainder of the sample, although it is substantially larger in California (30%) than the other subsamples (about 10%-15%).

A potential concern with the above finding is that firms were not actually producing valuable patents in greater volume, but rather labeled existing ideas with patent applications (instead of leaving them as trade secrets, for example). This may account for at least part of the effect, but is unlikely to account for all of it, as the full treatment effect only appeared gradually. If the only consequence of the decisions was an incentive to label ideas with patents, that incentive rose discretely at each event date and should not have required years to manifest. In contrast, a slow process of innovation investment fits both the gradual upward trend in the figure and the R&D effects documented earlier. Also, as suggested by Column 2 of Table 9, beyond some minimal patent portfolio size, lenders likely care less about the sheer number of patents possessed than about the quality of the technology they embody.

Another way to address this issue is to examine the quality of patents produced. The

[17] It is difficult to measure ultimately-*unsuccessful* patent applications at the firm level, as the applications are typically held in the inventor's name until they are granted by the USPTO.

most common proxy for patent quality is the citations received by a company's new patents. Citations are direct evidence of other researchers building on a patent's contribution, which is a reasonable measure of the impact or social value of a patent (and also its potential collateral value, as documented earlier). Figure 8 investigates this proxy. The specification is as in Figure 7, but the dependent variable is the total number of citations received by the firm's existing stock of patent applications.[18] The estimated effect on citations in Figure 8 is similar to Figure 7 until about 2008, at which point it flattens out at a statistically-significant 10% increase in citations. The initial upward trend, mirroring Figure 7, is reassuring evidence that the patents produced in response to the court decisions were of comparable quality to the firm's existing patent stock.

All four figures exhibit a flattening-out of the effects in the later years of the sample, but this pattern should be interpreted cautiously. In Figure 7, the estimated effects are subject to a right-truncation problem arising from the fact that patent applications cannot be matched with firms until they are granted (see previous footnote). In Figure 8, this problem is even more severe, especially in the last five years, since not only the cited but also the citing patent must be published for the citation to be observed (see Hall et al., 2001). The main conclusion from these figures is more simply the departure of treated from untreated firms.

4.5 Use of patents as collateral

The previous sections demonstrated that that patenting firms were able to raise more debt financing, invest the proceeds, and ultimately produce more patents, in response to court decision that increased the collateral value of existing patents. This section demonstrates another corollary: Firms that had not previously pledged their patents as collateral were increasingly likely to do so in response to their increased pledgeability. To demonstrate this, I repeat the specifications of Figures 7 and 8, but employ as the outcome variable an indicator for whether a firm has ever been observed to pledge its patents as collateral. The

[18] As noted in Section 1, I count only citations received within the first five years after the patent is granted, in an attempt to mitigate right-truncation problems.

growth of this variable over time reflects firms "entering" the patent-backed loan market by pledging their patents as collateral for the first time.

Figure 9 plots the estimated coefficients on the interaction terms between treatment and year dummies. The figure reveals a steady upward trend in the coefficients, capturing a faster growth rate in use of patents as collateral among treated compared to untreated firms. By 2013, the probability of having pledged patents as collateral had risen by 4% more among treated than untreated firms. As with the prior figures, Panel (b) shows that this steady increase is shared by firms in California, the northeast, and other regions in the sample, although again the effect is much larger in California (about a 10% cumulative effect) than in the other states (about 2.5%). Again, these findings support the view that treated firms' patents became more valuable collateral, and that they used this new debt capacity.

The results of this and the previous section, taken together, also suggest an intriguing feedback effect between debt capacity and investment: When the collateral value of patents increased, firms used them as collateral to produce more patents. The collateral value itself may have provided a marginal incentive to invest in patents as opposed to other forms of capital. However, identifying and quantifying this mechanism would likely require a different empirical strategy than pursued in this paper, and I do not attempt it here.

4.6 Prior studies of creditor rights and innovation

Previous empirical studies of creditor rights and innovation often reach the opposite conclusions to mine: Acharya and Subramanian (2009) and Seifert and Gonenc (2012) find *lower* rates of patenting and usage of secured debt by innovative industries in countries that have strong creditor rights, or that have recently strengthened creditor rights. Acharya et al. (2011) demonstrate a negative relationship between creditor rights and firms' willingness to take risks. Vig (2013) shows that firms used less secured debt, and invested more conservatively, in response to a strengthening of secured creditor rights in India.

One explanation for our disparate results may be institutional differences between the US and the countries driving their results. For example, India is a country with strong creditor rights but weak enforcement of the law, receiving scores of 4/4 and 4.17/10, respectively, in these areas in La Porta et al. (1997). The nine reforming countries that provide the identifying variation in Acharya and Subramanian (2009) have an average creditor-rights score of 2.9 out of 4, but an average rule-of-law score of 8.35, with Indonesia and Israel both scoring below 5. The US, by contrast, scores only 1 out of 4 for creditor rights – partly due to the strength of its automatic stay – but receives the maximum rule-of-law score of 10.

Thus, the results of our studies may suggest that strengthened creditor rights are effective for fostering financing and innovation when they are relatively weak to begin with, and in legal environments with strict enforcement of contractual contingencies. This is consistent with evidence from Lerner and Schoar (2005), who show that financial contracts exhibit more state contingencies in countries with stronger legal enforcement, as well as Ponticelli and Alencar (2016), who demonstrate that the effectiveness of legal reform depends on the effectiveness of its enforcement.

5 Conclusions

My findings demonstrate that intangible collateral can have significant economic value. Nevertheless, legal uncertainty about the status of patent collateral has evidently been a major issue for participants in this market. Clarification of legal uncertainties led to large responses in financing and innovation output. This suggests a useful focus for policymakers interested in spurring innovation and growth in the economy. As innovative firms increasingly gain access to collateralized financing, this could also decrease their reliance on equity funding, potentially providing a channel for the financing of innovation that is not dependent on fluctuations in the availability of venture capital or the conditions of IPO markets. These findings should be taken into account in debates over the optimal strength of intellectual property rights, without which patents would have no collateral value at all.

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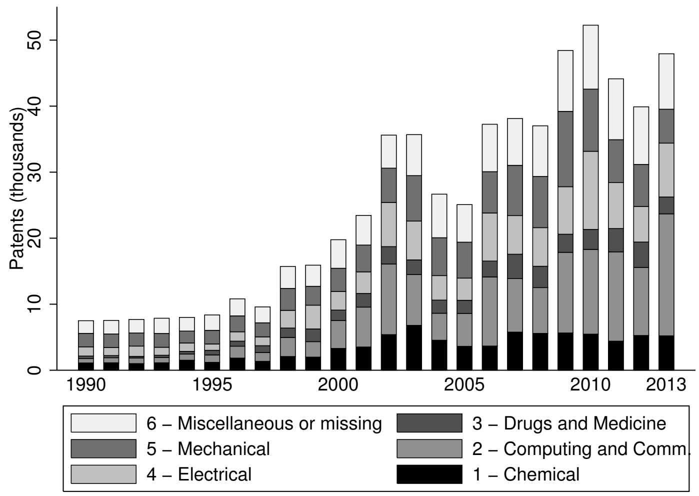
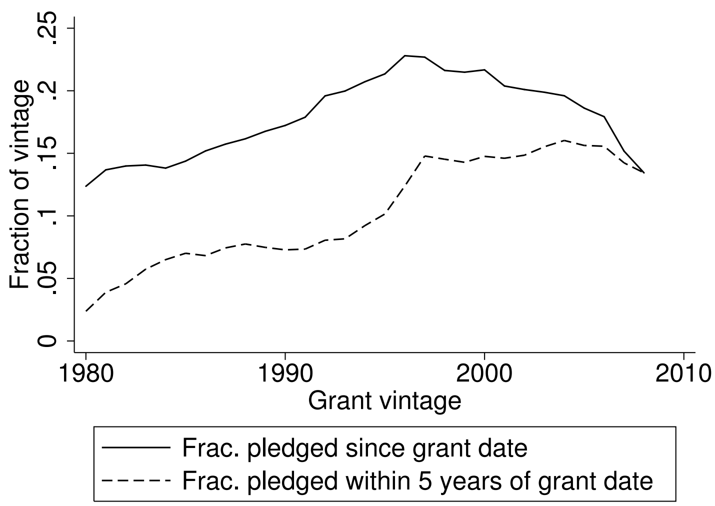
A Tables and figures: Descriptive analysis



(a) Number of patents pledged as collateral per year, 1990-2013. The six patenting categories are taken from Hall et al. (2001).



(b) The solid line shows the fraction of each grant vintage of patents produced by US companies that have been pledged as collateral. The series begins to decrease before 2000, but this is largely an artifact of right-truncation, since the younger vintages have had less time to be pledged as collateral. To illustrate this, the dotted line shows the fraction of patents that are pledged within five years of their grant date.

Figure 1: Growth in the market for patent-backed loans over time.

	(1) Pledge	(2) Pledge	(3) Pledge	(4) Pledge	(5) Pledge
Cited patent	0.0269***				
	(0.00553)				
Ln(citations)		0.0151***		0.0141***	0.00733**
		(0.00315)		(0.00307)	(0.00273)
Generality			0.0124***	0.00867***	0.00587*
			(0.00258)	(0.00256)	(0.00293)
Public company				-0.0672***	-0.0648***
				(0.00699)	(0.00777)
1996 - Grant Year					0.0000391
					(0.000479)
$(1996 - \text{Grant Year})^2$					-0.000336***
					(0.0000315)
Constant	0.155***	0.164***	0.178***	0.204***	0.240***
	(0.00907)	(0.00784)	(0.00699)	(0.00651)	(0.00814)
Obs.	1389533	1193799	1389533	1193799	1193799
R^2	0.000597	0.00141	0.00104	0.00919	0.0155

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1: Predictive regressions of the decision to pledge patents, patent level. The sample is all patents granted to US corporations through 2013 that can be matched to a *pdpass* identifier from the NBER database. The outcome variable is an indicator for the patent ever being pledged as collateral in the USPTO Assignments database. The "Cited patent" indicator and the log citation count are based on the first five years of the patent's term. The generality measure is computed as in Hall et al. (2001), and is demeaned and scaled by its sample standard deviation. "Public company" is an indicator for whether the patent can be matched to a Compustat company. Standard errors are clustered by the 36 two-digit technology subcategories introduced in Hall et al. (2001).

	(1) Pledge	(2) Pledge	(3) Pledge	(4) Pledge
Ln(Patents)	0.0676***		0.0845***	0.0729***
	(0.0123)		(0.0104)	(0.0121)
Ln(Citations/Patents)	0.0521***		0.0564***	0.0532***
	(0.0180)		(0.0177)	(0.0165)
Ln(Patents) $\times$ Ln(Citations/Patents)	-0.0210***		-0.0212***	-0.0173***
	(0.00711)		(0.00530)	(0.00551)
Ln(Assets)		-0.00593	-0.0321***	-0.0322***
		(0.00498)	(0.00530)	(0.00609)
PP&E/Assets		-0.181**	-0.119*	-0.0531
		(0.0788)	(0.0680)	(0.103)
Cash/Assets		-0.524***	-0.585***	-0.656***
		(0.0543)	(0.0603)	(0.0640)
Profits/Assets		0.0398	0.0592	0.0561
		(0.0400)	(0.0404)	(0.0409)
Market/Book		-0.000180	-0.000245	-0.000197
		(0.000248)	(0.000256)	(0.000170)
Dividend payer		-0.0875***	-0.0860***	-0.0762***
		(0.0287)	(0.0277)	(0.0290)
Constant	0.0835***	0.529***	0.430***	0.457***
	(0.0257)	(0.0425)	(0.0445)	(0.0451)
Fixed effect	None	None	None	3-digit SIC
Obs.	1604	1604	1604	1604
R^2	0.0384	0.0529	0.109	0.0913

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2: Predictive regressions of the decision to pledge patents, firm level. The sample is the cross-section of Compustat firms from 2010q1 that had produced at least one patent in the NBER data by that date, and have non-missing data for each of the explanatory variables. The outcome variable is an indicator for whether the firm had pledged patents as collateral in the USPTO Assignments database by 2010q1. Ln(Patents) is the total number of patents the firm had produced by 2010q1, and Ln(Citations/Patent) is the log average number of citations that the firm's patents had received up to that point. Citation counts are based on the first five years of the patent's term. The financial explanatory variables from Compustat are standard, and are averaged over the prior four quarters. "Dividend payer" is an indicator for the firm having a positive dividend payout over the prior four quarters. Standard errors are clustered by 3-digit SIC code.

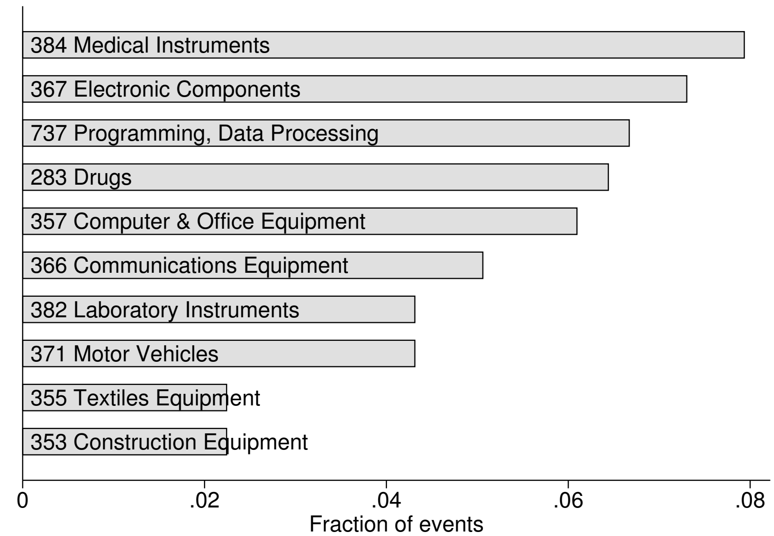


Figure 2: Top ten SIC industries of borrowers against patents collateral, by number of deals in the sample (restricted to Compustat-matched borrowers).

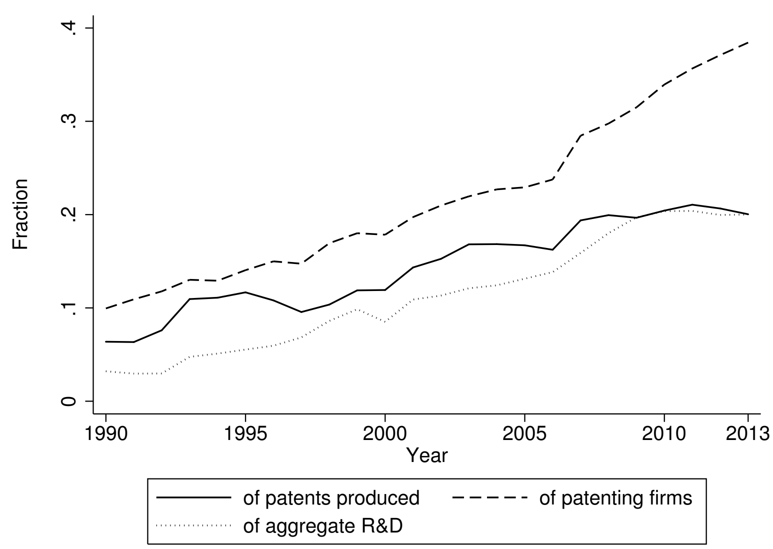


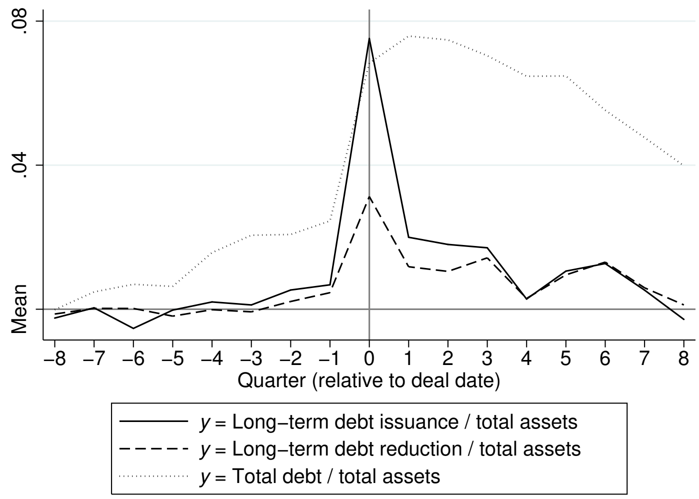
Figure 3: Fraction of Compustat patenting and R&D expense performed by companies that have pledged their patents as collateral at some point in the past.

	(1) Debt/Assets	(2) Debt/Assets	(3) Debt/Assets	(4) R&D/Assets
Pledged patents	0.0637***	0.0452***	0.0436**	0.00380*
	(0.0158)	(0.0169)	(0.0210)	(0.00221)
Ln(Assets)	0.00659*	0.000802	-0.00715	-0.00588***
	(0.00397)	(0.00421)	(0.00481)	(0.00117)
PP&E/Assets	0.306***	0.195***	0.296***	0.0284*
	(0.0429)	(0.0404)	(0.101)	(0.0158)
Market/Book	0.000309	0.000473***	0.000599	0.0000718
	(0.000188)	(0.000169)	(0.000692)	(0.0000858)
Profits/Assets	-0.192***	-0.190***	-0.191***	-0.0942***
	(0.0287)	(0.0307)	(0.0386)	(0.0218)
Cash/Assets		-0.204**	-0.162***	0.0890***
		(0.0826)	(0.0530)	(0.0151)
Dividend payer		0.00172	0.00638	-0.00414*
		(0.0186)	(0.0212)	(0.00240)
Constant	0.0721***	0.184***	0.195***	0.0351***
	(0.0268)	(0.0274)	(0.0429)	(0.00785)
Sample	All	All	High-tech	High-tech
Obs.	1604	1604	861	861
R^2	0.116	0.143	0.148	0.608

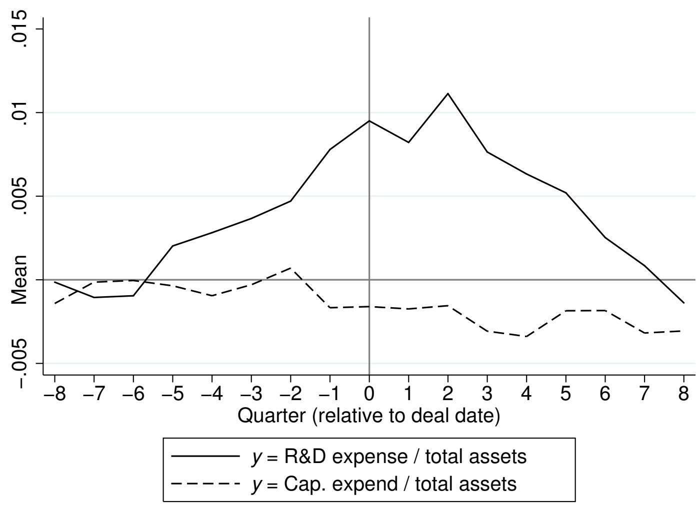
Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Cross-sectional association between the decision to pledge patents, debt usage, and R&D investment. The sample is the same as in Table 2: the cross-section of Compustat firms from 2010q1 that had produced at least one patent in the NBER data by that date, and that have non-missing data for each of the explanatory variables. The outcome variable in the first three columns is the ratio of total debt to total assets. In the last column, it is the ratio of R&D expense to total assets. The first, key explanatory variable is an indicator for whether the firm had pledged patents as collateral in the USPTO Assignments database by 2010q1 (the same as the outcome variable in Table 2). The financial explanatory variables from Compustat are standard, and are averaged over the prior four quarters. "Dividend payer" is an indicator for the firm having a positive dividend payout over the prior four quarters. Standard errors in Columns 1 and 2 are clustered by 3-digit SIC code. Columns 3 and 4 restrict to the 861 sample firms in the ten "high-tech" SIC codes identified in Figure 2. In these columns, standard errors are clustered at the intersection of 3-digit SIC code and state of location.



(a) Financing activity around dates of patents being pledged as collateral. The outcome variables are quarterly debt issuance (Compustat item DLTISQ; solid line), debt reduction (item DLTR; dashed line), and total debt (DLTTQ + DLTQ; dotted line), all divided by total assets (ATQ).



(b) Investment activity. The outcome variables are quarterly R&D (solid line) and capital expenditures (dashed line), both divided by total assets. See Appendix C.3 for details on R&D measurement.

Figure 4: Dynamics of financing and investment around dates of patents being pledged as collateral, restricting to the ten 3-digit SIC industries identified in Figure 2. The sample is quarterly Compustat data, 1990-2013. The figure plots the coefficients β_τ from the specification $y_{iq} = \alpha_i + \sum_{\tau=-8}^{8} \beta_\tau Pledge_{q-\tau} + \epsilon_{iq}$, where i indexes firms.

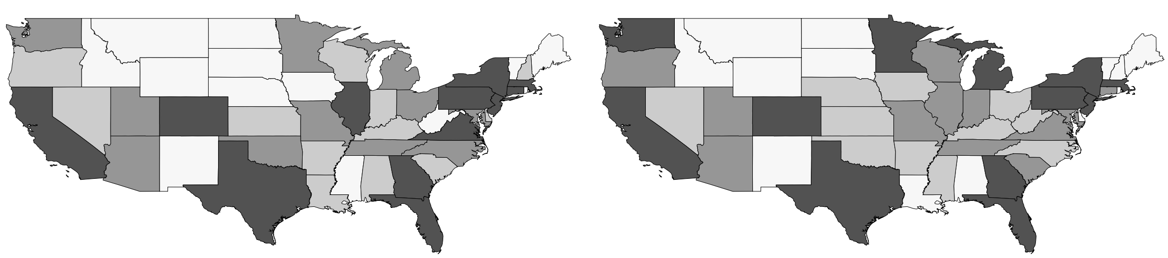
B Tables and figures: Natural experiment

Case	Decision date
Rhone-Poulenc Agro v DeKalb Genetics Corp.	March 26, 2002
Pasteurized Eggs Corporation v Bon Dente Joint Venture	May 30, 2003
Braunstein v Gateway Management Services	May 15, 2007
Sky Technologies LLC v SAP AG and SAP America	August 20, 2009

Table 4: Court decisions used in the natural experiment.

	Untreated	Treated	$p - val$
Assets - Total	2074.21	2592.70	0.17
Ln(Assets)	4.67	4.64	0.56
Property Plant and Equipment - Total (Net)	400.06	376.89	0.61
PP&E / Total assets	0.22	0.22	0.66
Num. patents	40.33	31.97	0.37
Has pledged patents	0.03	0.04	0.02
Total Debt / Total assets	0.24	0.27	0.00
R&D / Total assets	0.01	0.03	0.00
Number of firms	3903	7309	

Table 5: Comparison of sample means of several observables for treated and untreated companies included in the natural experiment. Data are from quarterly Compustat as of 2000Q1, or as of the firm's first sample quarter, whichever is later. Treatment status is defined in Section 3.3. The last column is the p-value from a t-test of difference in means.



(a) Treated firms. **(b)** Control firms.

Figure 5: Geographic distribution of sample firms by headquarters state. Shading represents quartiles of the number of firms headquartered in each state.

	(1) Debt/Assets	(2) Debt/Assets	(3) Debt/Assets	(4) Debt/Assets	(5) Debt/Assets
Treated	0.0204**	0.0249***	-0.0125	-0.0113	
	(0.00974)	(0.00669)	(0.0141)	(0.00782)	
After	-0.0131***	-0.0132***	-0.00848*	-0.00887*	-0.00411
	(0.00218)	(0.00230)	(0.00464)	(0.00522)	(0.00471)
Treated, after	0.0102***	0.0100***	0.0145***	0.0148***	0.0103*
	(0.00218)	(0.00264)	(0.00464)	(0.00426)	(0.00559)
Fixed Effect	None	HQ	None	HQ	HQ x Inc.
Sample	All	All	High-tech	High-tech	High-tech
Obs.	412246	412246	124136	124136	124136
R^2	0.00225	0.00273	0.000227	0.000198	0.000121

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

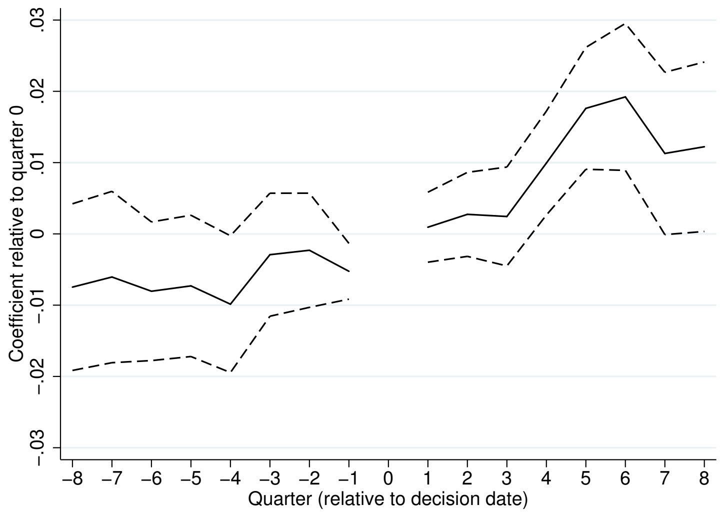
Table 6: Robustness of the debt increase to fixed effects, and stronger effects for high-tech firms. *HQ* is the company's state of headquarters, and *Inc.* is its state of incorporation, both taken from Compustat. All fixed effects are interacted with dummies for the individual decisions. Standard errors are clustered at the level of the fixed effect included in the regression, or by state of incorporation when no fixed effect is incorporated.

	(1) R&D/Assets	(2) R&D/Assets	(3) R&D/Assets	(4) R&D/Assets	(5) R&D/Assets
Treated	0.00948***	0.00859***	0.0140***	0.0107***	
	(0.00210)	(0.00105)	(0.00392)	(0.00257)	
After	-0.00115***	-0.000924**	-0.00171*	-0.00152	-0.000620
	(0.000378)	(0.000400)	(0.000851)	(0.00120)	(0.00115)
Treated, after	0.00169***	0.00157***	0.00394***	0.00371***	0.00285*
	(0.000378)	(0.000478)	(0.000851)	(0.00120)	(0.00151)
Fixed Effect	None	HQ	None	HQ	HQ x Inc.
Sample	All	All	High-tech	High-tech	High-tech
Obs.	412246	412246	124136	124136	124136
R^2	0.00580	0.00433	0.00580	0.00322	0.000114

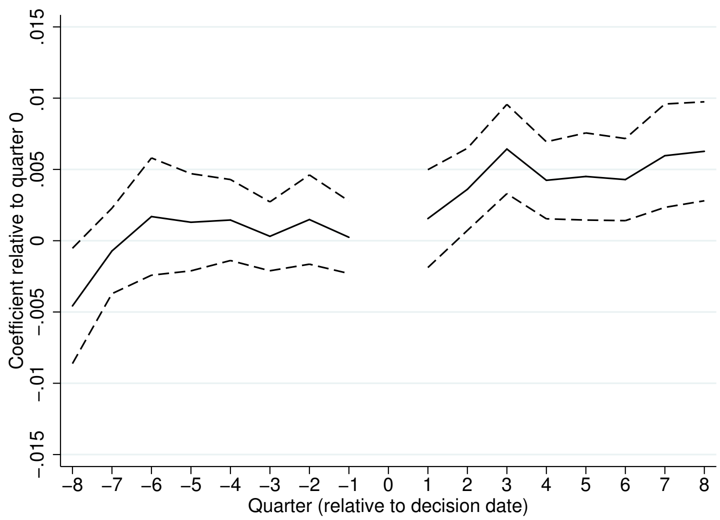
Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 7: Robustness of the R&D increase to fixed effects, and stronger effects for high-tech firms. *HQ* is the company's state of headquarters, and *Inc.* is its state of incorporation, both taken from Compustat. All fixed effects are interacted with dummies for the individual decisions. Standard errors are clustered at the level of the fixed effect included in the regression, or by state of incorporation when no fixed effect is incorporated.



(a) The dependent variable is total debt (short-term plus long-term) as a fraction of total assets.



(b) The dependent variable is quarterly R&D as a fraction of total assets.

Figure 6: Regression coefficients in event time. The analysis is identical to Column 3 of Tables 6 and 7, except that the *After* indicator is replaced with a full set of event-quarter dummies (with $q = 0$ representing the court decision date). As in those tables, the sample is restricted to the "high-tech" industries that exhibit the strongest effects, and standard errors are clustered by the firm's state of incorporation. Dashed lines are two standard errors above and below the estimated coefficients.

	(1) Debt/AT	(2) Debt/AT	(3) Debt/AT	(4) Debt/AT	(5) Debt/AT
Treated, after	0.00699***	0.00663***	0.00544	0.00218	0.00160
	(0.00191)	(0.00240)	(0.00332)	(0.00174)	(0.00197)
Treated, after, proxy	0.00409	0.00702**	0.00788*	0.00720*	0.0187**
	(0.00372)	(0.00310)	(0.00425)	(0.00367)	(0.00738)
Constraint proxy	AT < $200m	PP&E < $20m	Tang < 15%	No dividend	Unprofitable
Obs.	412246	398550	398550	412246	412246
R^2	0.0131	0.0252	0.0466	0.00320	0.00678

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

(a) The outcome variable is total debt over total assets.

	(1) R&D/AT	(2) R&D/AT	(3) R&D/AT	(4) R&D/AT	(5) R&D/AT
Treated, after	0.000116**	0.0000738	0.000238	0.0000529	0.000439***
	(0.0000449)	(0.0000894)	(0.000632)	(0.000174)	(0.000105)
Treated, after, proxy	0.00292***	0.00363***	0.00289***	0.00146**	0.00717***
	(0.000844)	(0.000770)	(0.000823)	(0.000671)	(0.00149)
Constraint proxy	AT < $200m	PP&E < $20m	Tang < 15%	No dividend	Unprofitable
Obs.	412246	398550	398550	412246	412246
R^2	0.0540	0.0484	0.0153	0.0257	0.0839

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

(b) The outcome variable is quarterly R&D expense over total assets.

Table 8: Effects of the natural experiment, separated by proxies for financial constraints. The specification is as in Column 1 of Tables 11 and 12, but adding interactions of all explanatory variables with a binary proxy for greater financial constraints. For reasons of space, only the interaction terms of interest are reported. The financial-constraints proxy used in each column is listed at the bottom of the table.

	(1) Debt/Assets	(2) Debt/Assets	(3) Debt/Assets	(4) Debt/Assets
Treated	-0.0517	-0.0362	-0.0256	0.0284***
	(0.0366)	(0.0278)	(0.0153)	(0.00948)
After	0.000359	-0.00668	-0.0113**	-0.0176***
	(0.0136)	(0.00779)	(0.00459)	(0.00392)
Treated, after	-0.00141	0.0000861	0.0100**	0.00445
	(0.0136)	(0.00779)	(0.00459)	(0.00392)
# patents		-0.00624		
		(0.00671)		
Treated × # patents		0.00843		
		(0.00671)		
After × # patents		-0.00409		
		(0.00298)		
Treated × After × # patents		0.00604*		
		(0.00298)		
Has pledged			0.0667	
			(0.0427)	
Treated, has pledged			0.0885**	
			(0.0427)	
After, has pledged			-0.0625***	
			(0.0183)	
Treated, after, has pledged			0.0373**	
			(0.0183)	
High cites				-0.0685***
				(0.0137)
Treated, high cites				-0.0320**
				(0.0137)
After, high cites				0.00669
				(0.00543)
Treated, after, high cites				0.0113**
				(0.00543)
Constant	0.239***	0.241***	0.225***	0.238***
	(0.0366)	(0.0278)	(0.0153)	(0.00948)
Sample	No patents yet	0-6 patents	0-6 patents	1+ patents
Obs.	22667	74589	74589	124652
R^2	0.00686	0.00224	0.0144	0.0308

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

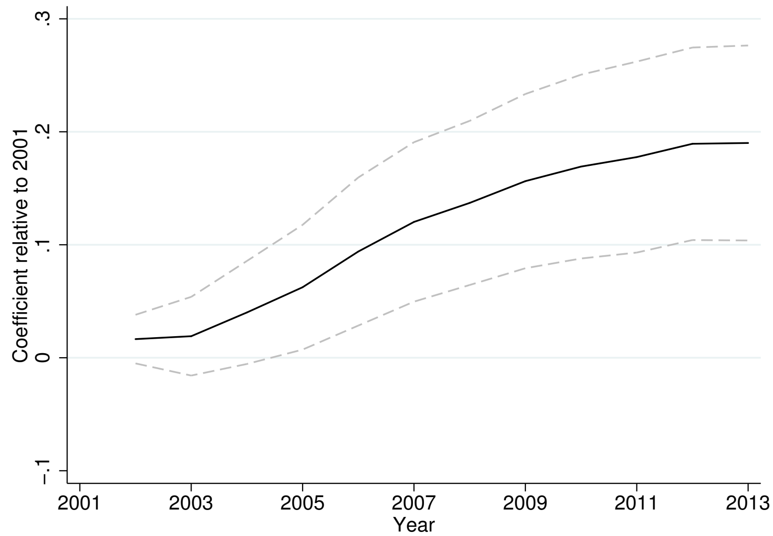
Table 9: Moderating effects of a company's patenting history. In Column 1, the sample is companies that are eventually observed to produce patents, but have not as of the beginning of the event window. In Column 2, the sample is companies that are eventually observed to produce patents, and have produced zero to six patents as of the beginning of the event window. In Column 3, the sample is the same, and *has pledged* is an indicator for whether the company has been observed to pledge patents as collateral at any point prior to the beginning of the event window. In Column 4, the sample is all companies that have produced at least one patent as of the beginning of the event window, and *high cites* is an indicator for whether the average number of citations received by the company's patents in the first five years after being granted is at least four.

	(1) Debt/Assets	(2) Debt/Assets	(3) Debt/Assets	(4) Debt/Assets
Treated	-0.0139	-0.0129	-0.118*	-0.0676
	(0.0166)	(0.0124)	(0.0587)	(0.0422)
After	-0.0150***	-0.00946	-0.0336	0.00986
	(0.00474)	(0.00676)	(0.0202)	(0.0163)
Treated, after	0.0110**	0.00397	0.0214	-0.0250
	(0.00474)	(0.00676)	(0.0202)	(0.0163)
# trademarks	0.000501	-0.00806*	-0.0386**	
	(0.000303)	(0.00430)	(0.0145)	
Treated × # trademarks	-0.000599*	-0.00125	0.0305**	
	(0.000303)	(0.00430)	(0.0145)	
After × # trademarks	0.0000234	-0.000533	0.00909	
	(0.000107)	(0.00251)	(0.00544)	
Treated × After × # trademarks	0.0000290	0.00132	-0.00748	
	(0.000107)	(0.00251)	(0.00544)	
# patents				-0.0129
				(0.0117)
Treated × #patents				0.0176
				(0.0117)
After × # patents				-0.0113**
				(0.00481)
Treated × After × #patents				0.0173***
				(0.00481)
Constant	0.224***	0.262***	0.326***	0.246***
	(0.0166)	(0.0124)	(0.0587)	(0.0422)
Sample	0-6 patents	0-6 trademarks	0-6 both	0-6 both
Obs.	74589	90401	22364	22364
R^2	0.00172	0.00384	0.0186	0.00971

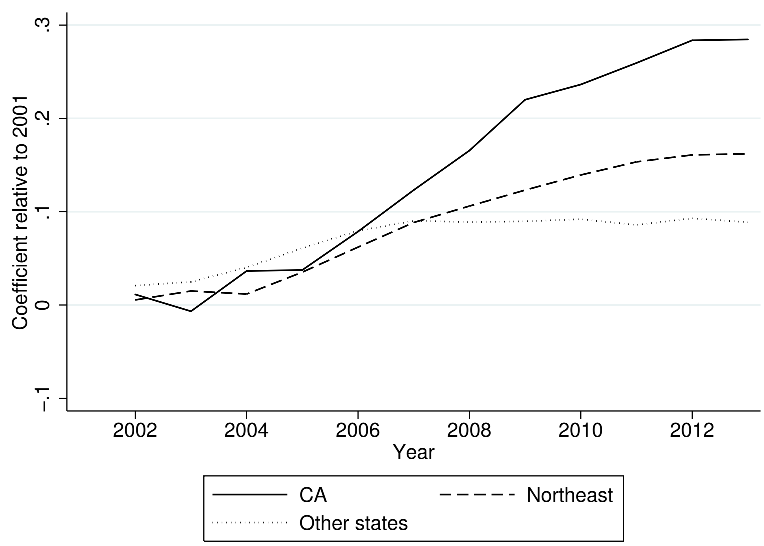
Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 10: The regressors "# trademarks" and "# patents" are the number of trademarks registered and the number of patents granted as of the beginning of the event window. The specifications in the table are analogous to those in Column 2 of Table 9. In Column 1, the sample is patenting companies that had six or fewer patents as of the beginning of the event window. In Column 2, it is trademark-producing companies that had six or fewer trademarks. In Columns 3 and 4, it is the intersection of these two. Standard errors are clustered by state of incorporation.

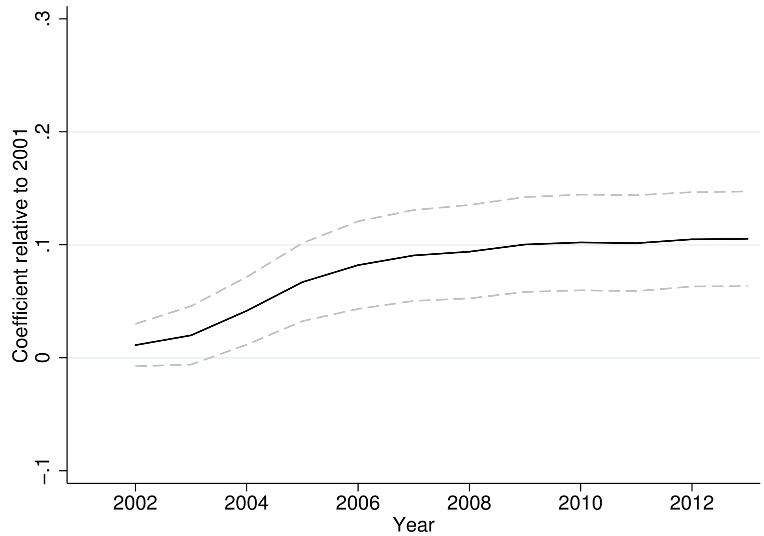


(a) Effect on cumulative patent production for treated and untreated firms. Dashed lines are two standard errors above and below the estimated effects. Standard errors are clustered by state of incorporation.

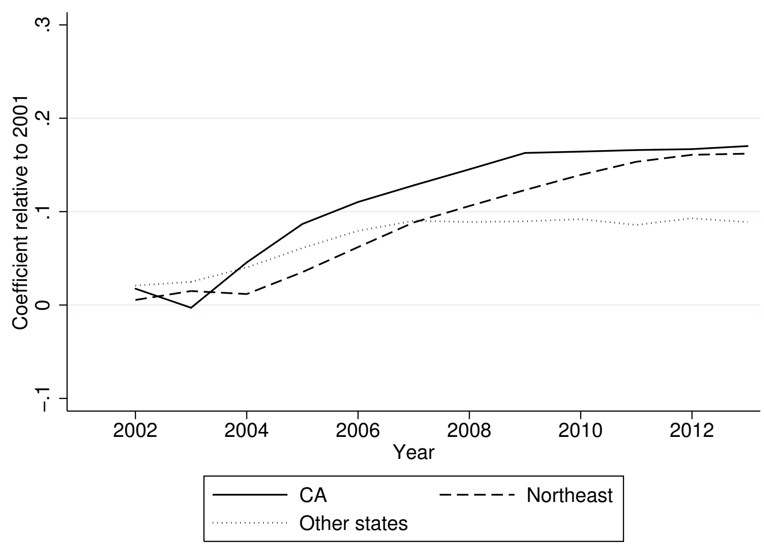


(b) Separate effects for firms in California, in the Northeast region (NY, MA, CT, RI, NJ, PA), and in the rest of the country.

Figure 7: Cumulative effects on patent production. I regress the log of the firm's cumulative successful patent applications filed from 2001-2013 on firm dummies, year dummies, and interactions between the year dummies and treatment status. The figures plot the estimated coefficients on the interaction terms.

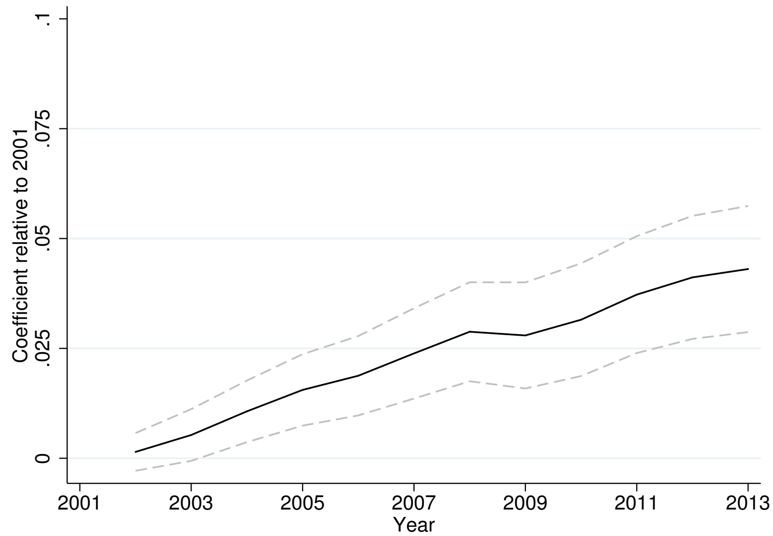


(a) Effect on cumulative citation-weighted patenting output for treated and untreated firms. Citations are measured in the first five years of the patent's term. Dashed lines are two standard errors above and below the estimated effects. Standard errors are clustered by state of incorporation.

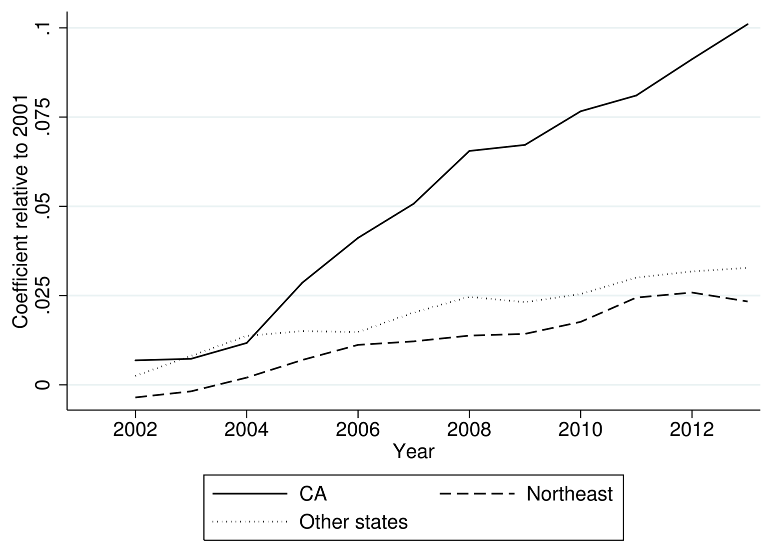


(b) Separate effects for firms in California, in the Northeast region (NY, MA, CT, RI, NJ, PA), and in the rest of the country.

Figure 8: This figure repeats the analysis of Figure 7, but weights the patent counts by the number of citations each patent received in the first five years of its life, or through 2013, whichever is sooner.



(a) Effect on probability of having pledged patents in the past. Dashed lines are two standard errors above and below the estimated effects. Standard errors are clustered by state of incorporation.



(b) Separate effects for firms in California, in the Northeast region (NY, MA, CT, RI, NJ, PA), and in the rest of the country.

Figure 9: Cumulative effect on the usage of patent collateral. I regress an indicator for having pledged patents as collateral in the past on firm dummies, year dummies, and interactions between the year dummies and treatment status. The figures plot the estimated coefficients on the interaction terms.

Internet Appendix for
"Creditor Rights and Innovation: Evidence from Patent Collateral"

C Data appendix

C.1 Patent collateral data

The Assignments dataset provides a patent number and contract execution date for each pledge of patents as collateral that is recorded with the Patent Office. Although this recordation is not mandatory, lenders have a strong incentive to file these documents to maintain a clear chain of title, so any missing contracts are likely to be simply those for which lenders attach little value to the patent collateral. Thanks to the execution date, I can observe the exact timing of the loans, even in cases where there is a delay between the signing of a contract and its recordation with the Patent Office. The dataset also includes the names of the secured party and the pledging firm. I match the latter to NBER firm identifiers, then use the NBER linking file to match these identifiers to Compustat, carrying forward the most recent set of matches to years since 2006.

Assigning the pledging companies to NBER identifiers is a complex task, as the name of the pledging company may be different from that of the original patenting company due to M&A activity, name changes, holding patents in subsidiaries or holding companies, or sales or other transfers of the patents. My approach is as follows: The Assignments data includes the entire chain of title to each patent, from its initial grant to the inventor, then its assignment to the patenting company, and subsequently through any future assignments and collateral pledges. At each point in this chain, I attempt to match the name of the current or new owner with a name listed in the NBER data. My name matching algorithm is described in Appendix C.2 below. Having performed this matching, for any pledge events that could not be matched manually, I refer to the most recent assignment or grant in that patent's chain and fill in the acquiring company from that event, if it was matched. If the previous assignment was not matched, I leave the identifier missing.

Even with this approach, problems can arise when a small company includes in its collateral portfolio limited rights to a patent that it is merely licensing from a larger corporation. For example, in August 2002, Matrics Inc pledged nine patents that it owned to Comerica Bank, but also included patent number 6198937, owned by Motorola. To avoid coding Motorola as a patent pledger in this case, I require a firm to pledge at least 2% of its existing patent stock in order to record a pledge event (patents are usually pledged in bundles, and most often as entire portfolios). This procedure leaves me with 2,208 firm-events of Compustat firms pledging patents as collateral since 1990. This approach yields a high degree of accuracy, as verified by extensive hand-checking, but at the cost of screening out over half of firm-months that appear to pledge patents, resulting in conservative estimates of the prevalence of patenting activity.

Figure 10 on the next page reports a histogram of the most common lenders in the patent collateral data. (This figure was reported as Figure 3 in previous versions of the paper.)

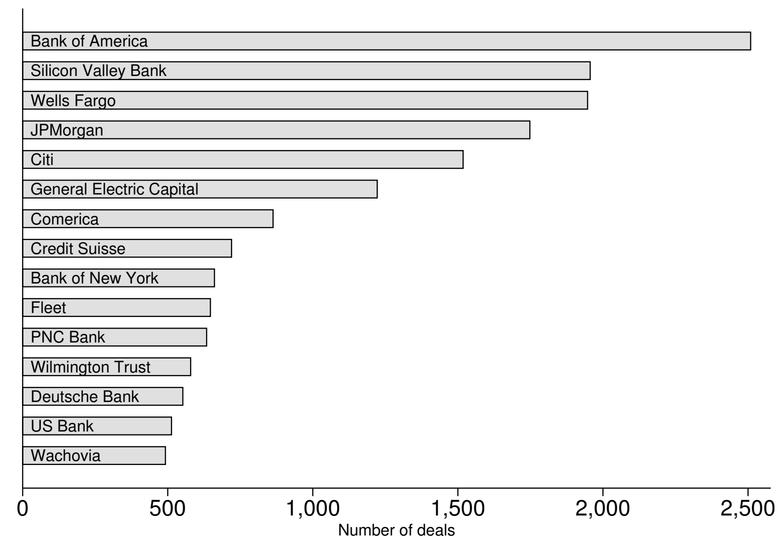


Figure 10: Top fifteen lenders against patent collateral, by number of deals in the sample (not restricted to Compustat-matched borrowers). Previous versions of the paper reported these results as Figure 3.

C.2 Patenting by firms since 2006

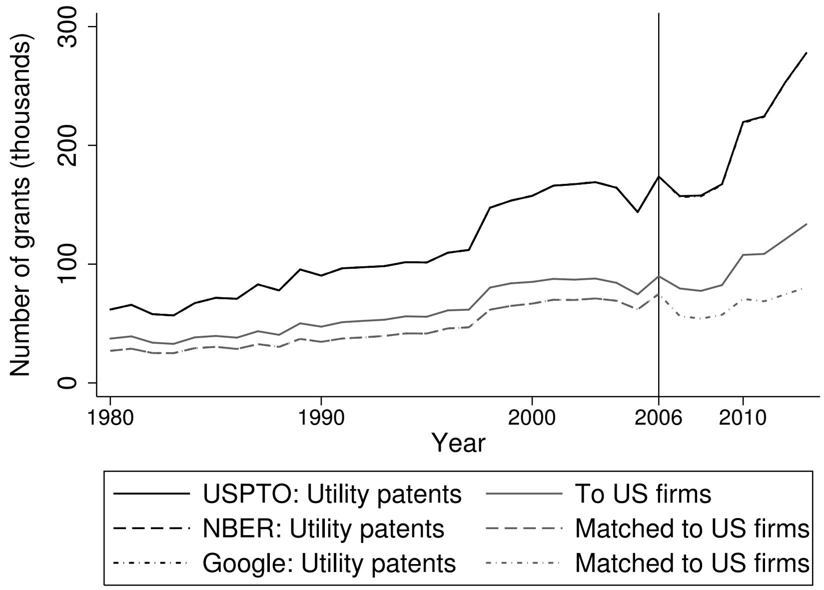
The Google Patents data contain both granted patents and pending applications, but the company producing the patent cannot be observed until the patent is granted, at which point the inventor transfers it to the corporation. Thus, I restrict to patents that have actually been granted by the Patent Office, and I manually match the company name to a list of all NBER firm identifiers for US-headquartered companies that have been assigned at least five patents as of year-end 2006 (this restriction is necessary to keep the processing time manageable).

To match the company names, I take the first 30 characters of each name, eliminate some of the most common corporate words, and then calculate a "distance" between pairs of names, matching any pairs with a sufficiently low distance. My algorithm to calculate this distance is as follows: First, I sequentially calculate the commonly-used Levenshtein distance between each pair of words in the two names (that is, between the first two words, the second two words, and so on). Next, I divide each word-pair distance by the length of the longer of the two words, so that the penalty is per fraction, not number, of mismatched letters. Finally, I divide again by the square of the word's position in the company name, so that the algorithm overweights the first few words of a company's name. I add up the resulting value from each word pair to get the overall distance between the two names.

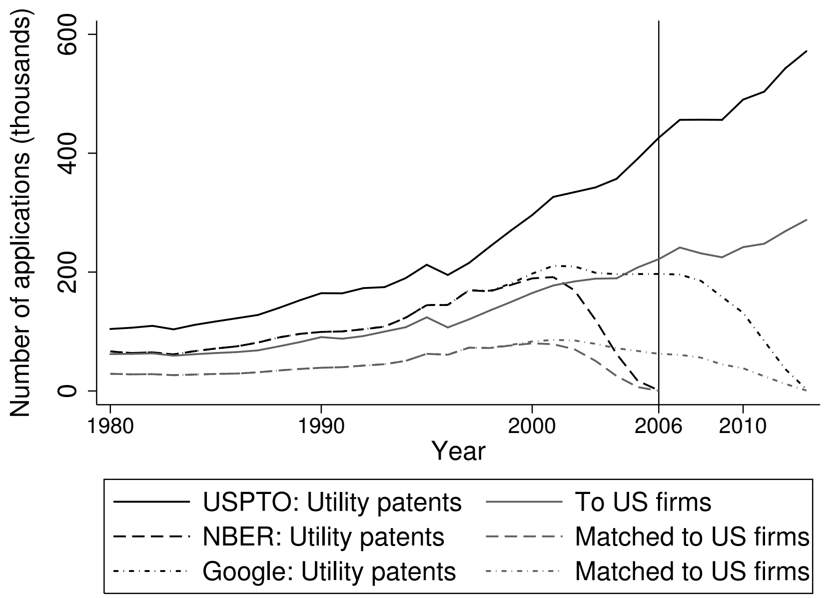
Using this algorithm, I am able to match 59% of domestic corporate patents granted since 2007 with firm identifiers (for comparison, 76% of domestic corporate patents granted prior to 2007 are assigned firm identifiers in the NBER data), Extensive hand checking has turned up no mistaken matches. Figure 11 displays the fraction of patents reported by the USPTO that are accounted for in the NBER and in my data, dated by grant year in Panel (a) and by application year in Panel (b). Citation pairs are also available for all granted patents through the present, allowing me to construct the citation counts received by a patent within any horizon of its grant date. Throughout the paper, I set this horizon to five years in an attempt to mitigate right-truncation problems.

C.3 Compustat definitions

- Total assets = **atq**. I drop any observations with zero, negative, or missing assets.
- Total debt = **dlttq** + **dlcq**.
- R&D expense = **xrdq** + **rdipq**. Variable **rdipq** is coded in Compustat as a negative number, so adding it to **xrdq** removes the acquired in-process component of R&D expenditure. For firms that report R&D only in the fourth quarter, I divide the reported figure evenly over all four quarters in the fiscal year.
- Whenever scaling a financial variable by total assets, I winsorize the resulting ratio between zero and one, and replace it with zero when missing.
- State of headquarters = **state**; State of incorporation = **incorp**.



(a) Timed by grant year.



(b) Timed by application year.

Figure 11: Summary of aggregate and firm-matched patenting data. Solid black lines are the total utility patent grants (Panel (a)) or applications (Panel (b)) reported by the USPTO. Lighter solid lines are the number from US companies only. Dashed lines are the aggregate figures from NBER data, which end in 2006, and dotted lines are the figures from the combined NBER and Google Patents data.

C.4 Trademarks data

Table 10 in section 4.2 of the paper presents a placebo analysis of the natural experiment using USPTO data on trademarks granted to US firms. This dataset is described in a working paper published by the USPTO (Graham et al., 2013), but to my knowledge it has not been used in previous empirical finance research. Therefore, this subsection provides some details about the dataset that are relevant to this study. All information is taken from Graham et al. (2013).

Trademarks are uniquely identified by a serial number, which is assigned by the USPTO when the trademark application is received. The final step in the process of approving the application is *registration*, and the trademark is effective beginning on the registration date. Figure 7 in Graham et al. (2013) suggests that approximately two-thirds of trademark applications in a given vintage are ultimately registered. The entire application and approval process is detailed on the USPTO website, at https://www.uspto.gov/trademarks-getting-started/trademark-basics/trademarks-what-happens-next.

In some ways, a trademark registration is not exactly equivalent to a patent grant. Trademarks may have certain rights under common law even if they are not registered, while the extra rights conferred by registration lapse if the trademark is not in continual use, in contrast to a patent which expires on a preset date. However, these facts are not important for the placebo analysis in my paper.

To implement my analysis, I first download the *owner* dataset from the USPTO website.[19] This dataset contains a unique row for each trademark-owner combination. I select from this dataset all rows with owners that match the names of Compustat firms in my sample. (About 1.5% of trademarks in my sample are jointly owned by more than one sample firm. I retain all these matches, so one trademark can count towards the portfolio of more than one firm.)

Next, I match this dataset by serial number to the *case_file* dataset, which contains one row for each trademark application, along with other information including registration dates. I retain only trademarks that have been registered, and use the registration date to determine the size of each sample firm's trademark portfolio as of each court decision date.

[19] The trademarks data download website is https://www.uspto.gov/learning-and-resources/electronic-data-products/trademark-case-files-dataset-0

D Additional results from the natural experiment

Robustness across geographic regions

	(1) Debt/Assets	(2) Debt/Assets	(3) Debt/Assets	(4) Debt/Assets
Constant	0.225***	0.220***	0.306***	0.231***
	(0.00869)	(0.0437)	(0.0185)	(0.00958)
Treated	0.0276***	-0.0350	-0.0307	0.0738***
	(0.00869)	(0.0437)	(0.0185)	(0.00958)
After	-0.0155***	-0.0198***	-0.0259***	-0.00249
	(0.00213)	(0.00493)	(0.00725)	(0.00624)
Treated, after	0.0135***	0.0195***	0.0249***	0.0206***
	(0.00213)	(0.00493)	(0.00725)	(0.00624)
Sample	Northeast	CA	TX/CO	FL/GA
Obs.	113618	71520	43326	34447
R^2	0.00390	0.00191	0.00112	0.0213

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 11: Effects of the court decision on total debt, in the full sample and across regions. *Northeast* states are NY, MA, CT, RI, NJ, and PA. Standard errors in all columns are clustered by state of incorporation.

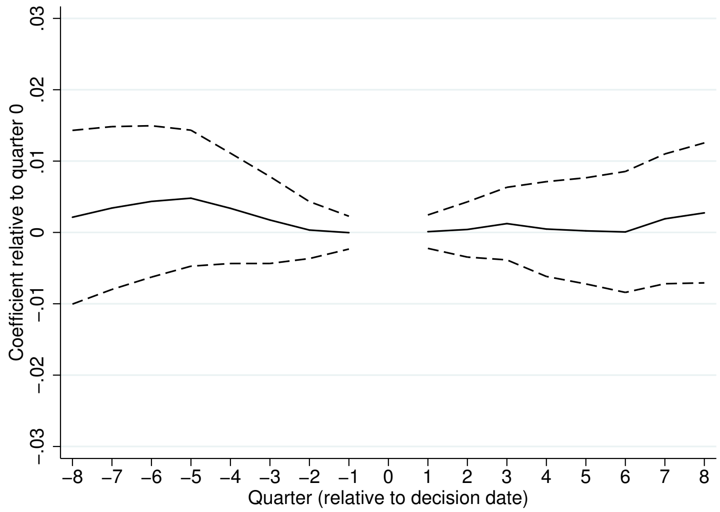
	(1) R&D/Assets	(2) R&D/Assets	(3) R&D/Assets	(4) R&D/Assets
Constant	0.0105***	0.0234***	0.0153***	0.0121***
	(0.00316)	(0.00362)	(0.00393)	(0.00210)
Treated	0.0146***	0.0136***	-0.00422	0.00236
	(0.00316)	(0.00362)	(0.00393)	(0.00210)
After	-0.00105	0.000279	-0.00744**	0.000629
	(0.000624)	(0.000872)	(0.00268)	(0.000778)
Treated, after	0.00110*	0.00194**	0.00786***	0.00202**
	(0.000624)	(0.000872)	(0.00268)	(0.000778)
Sample	Northeast	CA	TX/CO	FL/GA
Obs.	113618	71520	43326	34447
R^2	0.0116	0.00614	0.000913	0.000827

Standard errors in parentheses

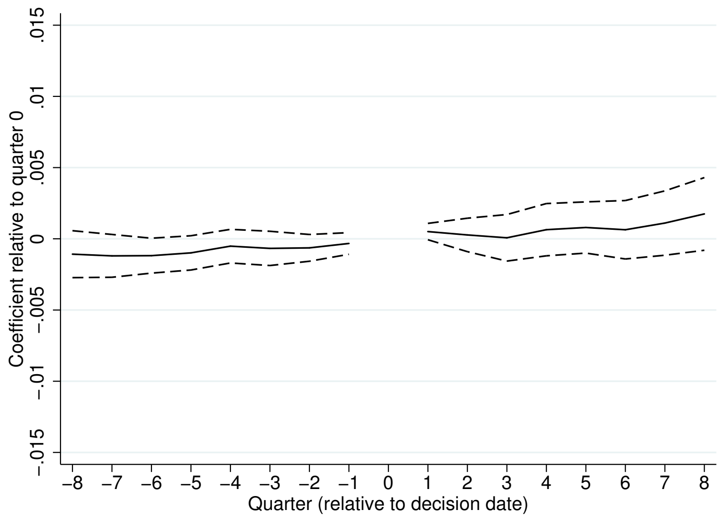
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 12: Effects of the court decision on R&D spending, in the full sample and across regions. *Northeast* states are NY, MA, CT, RI, NJ, and PA. Standard errors in all columns are clustered by state of incorporation.

Placebo results on non-event dates



(a) The dependent variable is total debt (short-term plus long-term) as a fraction of total assets.



(b) The dependent variable is quarterly R&D as a fraction of firm total assets.

Figure 12: Placebo analysis. The figures repeat the analysis of Figure 6, but using different event timing: Instead of being centered around the true court decision dates, the windows sampled from Compustat are centered on all quarters from 2000-2011 that are *not* within one year of a true decision date. This yields a total of 16 placebo "events."

Placebo results for capital expenditures

	(1) Capex/Assets	(2) Capex/Assets	(3) Capex/Assets	(4) Capex/Assets	(5) Capex/Assets
Treated	0.00231***			-0.000235	
	(0.000125)			(0.000317)	
After	-0.00124***	-0.00122***	-0.00205***	-0.00204***	-0.00211***
	(0.000135)	(0.000144)	(0.000328)	(0.000393)	(0.000367)
Treated, after	-0.000488***	-0.000500***	0.000159	0.000135	0.000203
	(0.000171)	(0.000144)	(0.000328)	(0.000398)	(0.000586)
Fixed Effect	None	Treatment	Treatment	HQ	HQ x Inc.
Sample	All	All	High-tech	High-tech	High-tech
Obs.	412246	412246	124136	124136	124136
R^2	0.00229	0.000861	0.00199	0.00204	0.00209

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

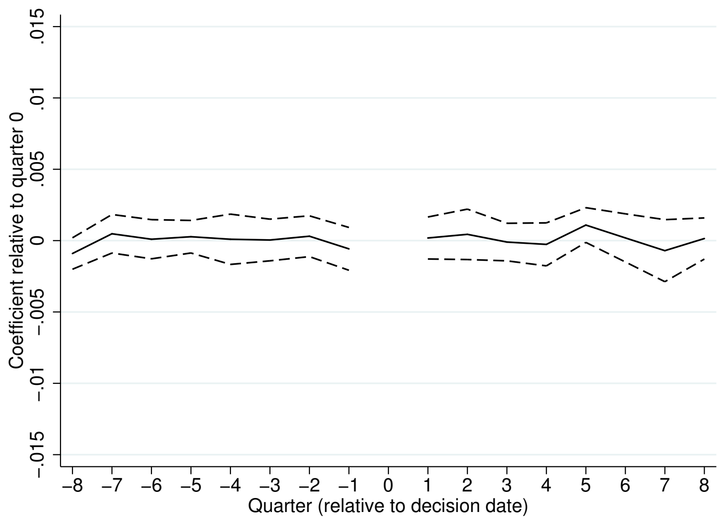
Table 13: Placebo analysis of the effect of the natural experiment on capital expenditures.

	(1) Capex+R&D	(2) Capex+R&D	(3) Capex+R&D	(4) Capex+R&D	(5) Capex+R&D
Treated	0.0118***			0.0105***	
	(0.000312)			(0.00253)	
After	-0.00239***	-0.00235***	-0.00377***	-0.00356***	-0.00273**
	(0.000325)	(0.000442)	(0.000991)	(0.00121)	(0.00122)
Treated, after	0.00121***	0.00115**	0.00406***	0.00385***	0.00306*
	(0.000438)	(0.000442)	(0.000991)	(0.00131)	(0.00157)
Fixed Effect	None	Treatment	Treatment	HQ	HQ x Inc.
Sample	All	All	High-tech	High-tech	High-tech
Obs.	412246	412246	124136	124136	124136
R^2	0.00725	0.000148	0.000105	0.00293	0.0000578

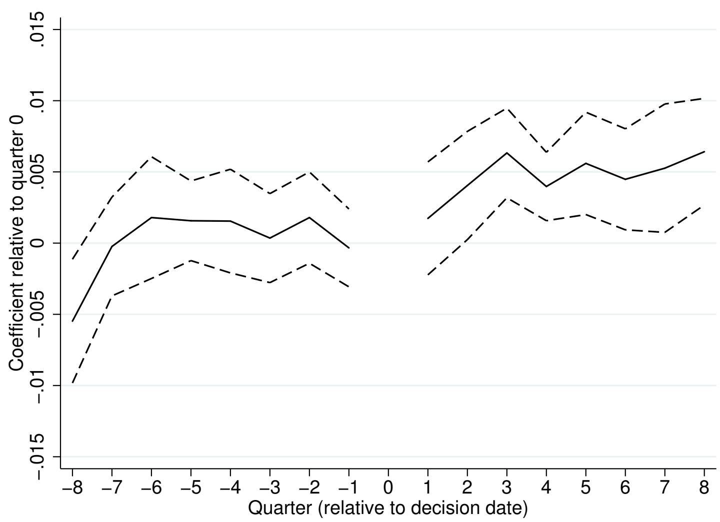
Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 14: Placebo analysis of the effect of the natural experiment on combined R&D and capital expenditures. The outcome variable in each column is capital expenditures plus R&D expense, divided by total assets.



(a) Placebo analysis of capital expenditures: The dependent variable is quarterly capital expenditures as a fraction of total assets.



(b) Total investment: The dependent variable is quarterly combined R&D and capital expenditures as a fraction of firm total assets.

Figure 13: Regression coefficients, in event time, for the effect of the natural experiment on capital expenditures (Panel (a)), and on combined R&D and capital expenditures (Panel (b)). Except for the outcome variables, the figures are constructed as in Figures 6a and 6b.

Robustness of core results to control variables

	(1) Debt/Assets	(2) Debt/Assets	(3) R&D/Assets	(4) R&D/Assets
Treated, after	0.0119***	0.0118***	0.00148***	0.00147**
	(0.00190)	(0.00271)	(0.000423)	(0.000586)
Treated	0.0328***	0.0332***	0.00299**	0.00317***
	(0.00707)	(0.00487)	(0.00135)	(0.000624)
After	-0.0110***	-0.0109***	-0.000778*	-0.000721
	(0.00190)	(0.00230)	(0.000433)	(0.000504)
Ln(assets)	-0.00558**	-0.00524***	-0.00207***	-0.00219***
	(0.00215)	(0.000993)	(0.000179)	(0.000225)
Tangibility	0.200***	0.197***	-0.000539	0.00174
	(0.00841)	(0.00816)	(0.000910)	(0.00125)
Market/book	-0.000670***	-0.000719***	0.000172***	0.000177***
	(0.0000929)	(0.000197)	(0.0000453)	(0.0000596)
Profitability	-0.0804***	-0.0803***	-0.0199***	-0.0199***
	(0.00327)	(0.00407)	(0.00193)	(0.00181)
Cash/Assets	-0.252***	-0.246***	0.0584***	0.0538***
	(0.00540)	(0.00710)	(0.00557)	(0.00372)
Fixed effect	None	HQ	None	HQ
Obs.	340228	340228	340228	340228
R^2	0.149	0.137	0.160	0.144

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 15: Leverage and investment regressions with firm-level controls. The specification in Column 1 is the same as Column 1 of Table 11, except for the additional controls starting in the fourth row. Similarly, Column 2 of this table corresponds to Column 1 of Table 6; Column 3 of this table with Column 1 of Table 12; and Column 4 of this table with Column 1 of Table 7. The controls are fixed at the beginning of each event window. Missing values of the control variables are responsible for the decrease in observation counts relative to Tables 11 through 7.

Testing for heterogeneous effects of the decisions

	(1) Debt/Assets	(2) R&D/Assets
ABSF × after	0.0136**	0.00138**
	(0.00514)	(0.000666)
ABSF × after × Decision 1	-0.00700	0.000535
	(0.00459)	(0.000797)
ABSF × after × Decision 3	-0.00868	0.000938
	(0.00674)	(0.00117)
ABSF × after × Decision 4	0.00189	-0.000508
	(0.00555)	(0.00105)
F-test p-value	0.0798	0.6905
Obs.	412246	412246
R^2	0.00409	0.00629

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 16: This table repeats the specification of Column 1 of Tables 11 and 12, but with additional interaction terms that separate the treatment effect for court decisions 1, 3, and 4 from the effect of decision 2, which is left as the baseline.

E Delaware Asset-Backed Securities Facilitation Act

Downloaded from the State of Delaware official website.

§ 2701A Title.

This chapter may be referred to as the "Asset-Backed Securities Facilitation Act."

§ 2702A Intent.

It is intended by the General Assembly that the term "securitization transaction" shall be construed broadly.

§ 2703A Securitization transaction.

(a) Notwithstanding any other provision of law, including, but not limited to, §9-506 of this title, "Debtor's right to redeem collateral," as said section existed prior to July 1, 2001, and §9-623 of the title, "Right to redeem collateral," which became effective July 1, 2001, to the extent set forth in the transaction documents relating to a securitization transaction:

(1) Any property, assets or rights purported to be transferred, in whole or in part, in the securitization transaction shall be deemed to be no longer be the property, assets or rights of the transferor;

(2) A transferor in the securitization transaction, its creditors or, in any insolvency proceeding with respect to the transferor or the transferor's property, a bankruptcy trustee, receiver, debtor, debtor in possession or similar person, to the extent the issue is governed by Delaware law, shall have no rights, legal or equitable, whatsoever to reacquire, reclaim, recover, repudiate, disaffirm, redeem or recharacterize as property of the transferor any property, assets or rights purported to be transferred, in whole or in part, by the transferor; and

(3) In the event of a bankruptcy, receivership or other insolvency proceeding with respect to the transferor or the transferor's property, to the extent the issue is governed by Delaware law, such property, assets and rights shall not be deemed to be part of the transferor's property, assets, rights or estate.

(b) Nothing contained in this chapter shall be deemed to require any securitization transaction to be treated as a sale for federal or state tax purposes or to preclude the treatment of any securitization transaction as debt for federal or state tax purposes or to change any applicable laws relating to the perfection and priority of security or ownership interests of persons other than the transferor, hypothetical lien creditor or, in the event of a bankruptcy, receivership or other insolvency proceeding with respect to the transferor or its property, a bankruptcy trustee, receiver, debtor, debtor in possession or similar person.

It is not the purpose of this chapter to change the tax treatment of securitizations that take place pursuant to this chapter.

F Court decisions

F.1 *Rhone-Poulenc Agro v DeKalb Genetics Corp.*

The first decision ("Rhone Poulenc II") was a reversal of two previous decisions ("Heidelberg Harris" and "Rhone Poulenc I"). The specific issue in all three was the bona fide purchaser defense, which for most assets is codified by state law (the Uniform Commercial Code, or UCC). An example of the bona fide purchaser defense is the following: Suppose party A owns a car, promises it to B, but then sells it to C, who is unaware of that promise. B cannot reclaim the car from C, because C is a "bona fide purchaser," unaware of any wrongdoing by A at the time of purchase.

In Heidelberg Harris, the defendant claimed that as a *licensee*, it was protected from a claim of patent infringement by the bona fide purchaser defense (arguing that it had "purchased" the right to use the patent, unaware that the licensor was acting fraudulently). The plaintiff never disputed that a licensee could make this argument, so the court allowed it. However, the argument was inconsistent with the approach of the UCC, which requires transfer of title for the bona fide purchaser defense to apply.

Rhone Poulenc I revisited this issue: The defendant here made the same argument and cited Heidelberg Harris. This time, the plaintiff challenged the idea that a mere licensee could claim the bona fide purchaser defense, but the court found itself bound by the Heidelberg Harris precedent, and thus forced to rule against the plaintiff. This court also articulated more clearly the implication of that precedent: the creation of a distinct, federal bona fide purchaser defense for patents, with an approach very different from that of the UCC. The decision quickly attracted commentary as an example of a growing trend towards federal law preempting state law (see Ziff (2002), especially footnote 28). However, the plaintiff filed an en banc appeal, allowing the court to set aside the precedent and reconsider the issue.

The appeals decision (the event that I employ) was delivered in Rhone Poulenc II. This time, the court rejected the argument that a patent licensee can claim the bona fide purchaser defense. It reasoned from the approach of the UCC, which traditionally requires a transfer of title for this defense to apply, and emphasized repeatedly that North Carolina law makes this requirement explicit. (In contrast, the 2001 decision stated that "it was irrelevant if [North Carolina law] did not recognize such a defense.") The previous outcome was reversed and the plaintiff was able to sue for infringement.

In the bigger picture, the court signaled that patents should, as much as possible, be subject to the same property and contract law as other asset classes. The decision is often cited for the phrase "the interpretation of contracts for rights under patents is generally governed by state law" (see, for example, Gibbons (2004), Rosenstock (2005), and Young (2008)).

Citation: *Rhone-Poulenc Agro v Dekalb Genetics*, United States Court of Appeals for the Federal Circuit, March 26, 2002 (en banc rehearing of Rhone-Poulenc I). Available online at http://caselaw.findlaw.com/us-federal-circuit/1440442.html

F.2 *Pasteurized Eggs Corporation v Bon Dente Joint Venture*

This decision was made on May 30, 2003 by the Bankruptcy Court for the District of New Hampshire. It first held that patent law does not recharacterize limited or conditional transfers of patents as licenses instead of sales. It then held that state filing procedures are sufficient to perfect a security interest in a patent, and that the Patent Act was not intended to supersede these procedures.

The first of the two holdings arose because of a patent sale in which the seller retained the right to infringe prosecutors and file followup applications. After the buyer filed for bankruptcy, the seller argued that this transaction should be characterized as a patent license, not a sale, and therefore that the seller should be able to reclaim the patent from the bankruptcy estate. The closest available precedent was a case involving standing to sue for patent infringement. Building on that case, the court held that patent law did not relabel this transaction as a sale.

This issue is particularly relevant for patent-backed lending, because transfers of collateral to an SPV also involve the retention of such patent rights, so that the original owner of the patents can continue to manage them. The retention of these rights increases the probability that the transfer of the collateral is not ultimately respected as a sale. The pro-creditor laws in certain states provide blanket protection against this outcome, and they are more likely to be effective for patents in light of this decision that patent law does not overrule them.[20]

The second ruling, concerning perfection of security interests in patents, adopted the position of the Ninth Circuit Court in a 1999 case. The two cases are generally cited alongside each other (see, for example, Menell, 2007 and Baker et al., 2013). Both cases contrast their conclusion with copyrights, for which the Copyright Act explicitly overrides state filing systems for perfection of security interests, and mention similar issues concerning railroads and airplanes.

Citation: *In re Pasteurized Eggs Corporation*, United States Bankruptcy Court for the District of New Hampshire, May 30, 2003. Available online at http://www.nhb.uscourts.gov/Opinions/1999-present/2003BNH013-PasteurizedEggs.pdf

[20] Kieff and Paredes (2004): "The more control the originator exerts over the IP SPE [...] the greater is the risk that a bankruptcy court will determine that the IP SPE is not bankruptcy remote and that the transferred IP assets are in fact part of the debtor's bankruptcy estate." And later: "Notably, Delaware has attempted to mitigate the legal risk surrounding securitizations by adopting the 'Asset-Backed Securities Facilitation Act,' which, by characterizing what constitutes a true sale under Delaware law, attempts to shore up an SPE's standing as a bankruptcy-remote entity."

F.3 *Joseph Braunstein v Gateway Management Services (In Re: Coldwave Systems, LLC)*

This decision was made on May 15, 2007 by the Bankruptcy Court for the District of Massachusetts. It first disallowed an attempt to foreclose on patent collateral via the Patent Office registry due to noncompliance with California UCC. It next held that a state filing is necessary for perfection of a security interest, even if the Patent Office is notified of that interest. It finally held that a security interest cannot be perfected simply by possession of Patent Office transfer statements, which would preclude the need for filing the interest at all.

The chain of events began when the borrower pledged its patents as collateral, then defaulted on its loan payments. The lender notified the borrower and the USPTO that it was confiscating the patents, and believed thereafter that the patents were its property. The borrower never responded, and subsequently it filed for bankruptcy. The court ruled that the lender did not legally own the patents, because the borrower had never explicitly agreed to settle the debt by relinquishing them, as is required under California UCC.

Next, the lender claimed that it had, at least, a perfected security interest in the patents, which should allow it to reclaim them even after the bankruptcy filing. As evidence, the lender showed that it had notified the Patent Office of its security interest at the time of the loan (but it did not file with the state). The court recognized that this case was a novel situation, the reverse of existing precedents in which the lenders had filed with the state but not with the USPTO. It followed the same logic to conclude that the Patent Office filing was ineffective for perfection: Patent law was not intended to replace state law with respect to commercial lending.

Finally, the lender argued that filing was unnecessary, because it had perfected its security interest by taking possession of the patents, as evidenced by the transfer statement it had filed with the Patent Office. The judge disallowed this too, reasoning that actual possession is impossible for an intangible asset, leaving state filing as the only method of perfection, and further elevating the perceived importance of state law for transactions involving patents.

McJohn (2010):

> Patent law is federal law. Commercial law is generally state law, governed by various states adoption of the Uniform Commercial Code. Where the two bodies of law overlap, there can be uncertainty as to which governs [...] The creditor [in this case] creatively, if vainly, argued that it need not file, because it had possession of the patent certificate, just as a pawnshop perfects by possession of the jewelry in its safe. *Coldwave* reflects a great uncertainty in the intersection between commercial law and intellectual property.

Citation: *In re Coldwave Systems LLC*, United States Bankruptcy Court for the District of Massachusetts, May 15, 2007. Available online at http://chapter11cases.com/2012/07/01/in-re-coldwave-systems-llc-368-br-91-bankr-court-d-massachusetts-2007/

F.4 *Sky Technologies, LLC, v SAP AG and SAP America*

This decision was announced on August 20, 2009 by the Court of Appeals for the Third Circuit. The issue was whether a foreclosure on patents following UCC procedures was sufficient to transfer ownership, even though the transfer was not filed with the USPTO. A district court had assumed that this was the case, but acknowledged that there was room for disagreement on the issue, and certified an appeal, resulting in this decision.

The patents were originally granted to TradeAccess, a Massachusetts company which later changed its name to Ozro. Ozro pledged them to Silicon Valley Bank and to a venture fund. Ozro subsequently defaulted and the lenders foreclosed on the patents, following the procedures outlined by the Massachusetts Uniform Commercial Code (staging and bidding at a public auction), but they did not file transfer documents with the USPTO. Subsequently, the founder of TradeAccess started a new company, Sky Technologies, which acquired the patents from the lenders once more.

When Sky sued SAP for patent infringement, SAP alleged that the lenders had never truly acquired the patents in foreclosure because they had not notified the Patent Office. The Patent Act requires a filing with the Patent Office for any assignment of a patent. However, the court distinguished the "assignments" of the Patent Act from foreclosure in bankruptcy, holding that both were valid means of transfering title. Thus, patent law should imply no exceptions to the typical legal regime for secured lending.

This is perhaps the most direct of the four cases in articulating a state-law regime for secured lending involving patents. The court specifically mentioned the policy implications:

> First, if foreclosure on security interests secured by patent collateral could not transfer ownership to the secured creditor, a large number of patent titles presently subject to security interests may be invalidated. Any secured creditor who maintained an interest in patent collateral would be in danger of losing its rights in such collateral. Second, by restricting transfer of patent ownership only to assignments, the value of patents could significantly diminish because patent owners would be limited in their ability to use patents as collateral or pledged security.

See also McJohn (2010):

> Intellectual property is the subject of many finance transactions, from loans to joint ventures to securitization and beyond. The simple and clear approach taken by Sky Technologies (treating intellectual property like any collateral) will facilitate those transactions.

Citation: *Sky Technologies, LLC v SAP AG and SAP America*, United States Court of Appeals for the Federal Circuit (on appeal from the United States District Court for the Eastern District of Texas), August 20, 2009. Available online at http://caselaw.findlaw.com/us-federal-circuit/1385430.html

F.5 Other notes about the court decisions.

All four decisions were from federal courts, not state courts (two by bankruptcy courts and two by the Federal Circuit court of appeals).

Such decisions are often the result of appeals, and are themselves appealed. I focus on the date of the final decision in the case, consistent with the dates used when these decisions are cited. That is, I ignore appeals that were not heard by a higher court.

The delays between the triggering events and the ultimate decision ranged from three to five years, making it unlikely that the timing of the court decisions correlates with time-varying conditions affecting treated firms differently from untreated firms.

The state of incorporation, not the state of headquarters, determines treatment status:

– Under Revised Article 9 of the Uniform Commercial Code (effective as of July 2001), the state law defining ownership of collateral in bankruptcy is the state in which the debtor is located, and the location of a corporate debtor is defined as its state of corporation.

– Under old Article 9, the relevant state was the state in which the collateral was located. Even then, courts typically located of intangible assets with the debtor's location, so the two regimes generally should have had the same effect for patent collateral.

Loan parties can choose the state law governing some aspects of their contracts, but cannot choose the state law that governs ownership of collateral in bankruptcy. This was established in 1998 in *In re Eagle Enterprises* by the United States Bankruptcy Court for the Eastern District of Pennsylvania, and upheld on appeal by the district court the next year. The loan parties in this case had agreed to use German law to govern a loan secured by trucks located in Pennsylvania. (Germany has no automatic stay protection in bankruptcy, among other creditor-friendly provisions.) The court had no problem with the international aspect of the contract, but held that the ability to select governing laws could not extend to issues of ownership in bankruptcy. It reasoned that the bankruptcy estate, a separate entity from the loan parties themselves, would have been adversely affected by such an ability (which was of course the point), and it had not had the opportunity to object when the contract was signed (as it did not yet exist). Thus, loan parties are not allowed to contract around the provisions of their states' laws for the division of assets in bankruptcy. (I thank Steven Weise of Proskauer Rose LLP for referring me to this decision.)