Property Rights and Debt Financing

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Abstract

I examine how increasing firms' ownership of employee patents affects debt financing. I exploit a Court of Appeals Federal Circuit ruling that increased firms' property rights to employee patents. I find that firm ownership of patents increases firms' total debt-to-assets ratio by 18%, which is equivalent to a $62 million increase in total debt. I also provide evidence that the firm ownership of patents improves innovation productivity and patent pledgeability, which further ease firms' access to secured and longer-maturity debt. Finally, I show that firms' increased property rights to employee patents help reduce holdup problems in innovation processes.

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1. Introduction

Patents are important assets to knowledge-intensive firms in part because of the growing use of patents as collateral to access to debt financing.[1] Mann (2018) further shows that strengthening creditor rights in default allows firms to use more debt ex ante. This paper differentiates from the early study by focusing on the property rights allocation between firm and employees governed by invention assignment agreement. These contracts are inherently incomplete given highly complex, uncertain, and long innovation processes, and, as a result, induce holdup problems and inefficiencies in how patents are managed and utilized depending on who owns the patents. Therefore, focusing on the allocation of patent ownership between firm and employees contributes to understanding how property rights affect firms' relationship with external market even when a firm is solvent.

The goal of this paper is to empirically examine how property rights allocation between firms and employees affect knowledge-intensive firms' debt financing. Patent ownership is important for debt financing not only because firms need to be in possession of the patents to pledge them as collateral but also because the patent ownership structure changes the productivity of underlying innovation processes, which in turn affect the pledgeability of patents as collateral. However, an empirical identification of the effect of property rights on debt financing poses a key challenge. Potential endogeneity issues arise because property rights are not randomly assigned, and any unobserved variables that drive property rights allocation may also be correlated with firms' debt financing decisions. For example, managers with agency problem may prefer more control through greater property rights, which would result in the use of debt as an optimal form of financing (Aghion and Bolton, 1992).

To overcome this identification challenge, I exploit a Court of Appeals for the Federal Circuit (CAFC) ruling in 2008, which, *de facto*, shifted patent property rights from inventor-

[1] Loumioti (2013) documents that the percentage of secured syndicated loans collateralized by intangible assets grew from 11% to 24% over the 1997-2005. Mann (2018) reports that 38% of US patenting firms had pledged their patents as collateral at some point in 2013.

employees to firms in eight pro-employee invention assignment states.[2] The main regression relies on state-level variation in property rights enforcement through invention assignment agreements in a difference-in-difference setting. The regression estimate captures an increase in the leverage ratio of the treated firms in the eight states relative to the control firms after the court ruling. Both the timing of and context in which the decision was made were relatively free from the influence of lobbying, political pressure, and local economic conditions, and thus provide a plausible causal interpretation of the regression estimates.

I first estimate the effect of increasing property rights on firms' debt financing, as measured by the total debt-to-assets ratio. I find that firms in the eight treatment states affected by the CAFC decision increase total debt-to-assets ratio by 2.5 percentage points relative to the firms located in control states. The economic magnitude of the difference-in-difference coefficient is equivalent to an additional \$62 million in total debt for firms in the treated states following the property rights shift. For a pre-treatment average total debt-to-assets ratio of 0.14 for the treated firms, it is an 18% increase in the ratio. This key result stands up to a range of robustness checks. To show that the actual *flow* of debt increases, I re-estimate the regressions using a new long-term debt issuance as the dependent variable and find about a 23% increase in the new issuance. In a falsification test, I verify that the debt financing results are not found in non-patenting firms, which should not be affected by the court ruling for their lack of use of invention assignment agreements. To ensure that the coefficient estimates are not confounded by any concurrent creditor rights related court cases described in Mann (2018), I control for the interaction of Delaware-incorporation dummy and creditor rights court case year dummies and find that the results stand robust.

It is important to note that the increase in debt financing is unlikely driven by concurrent increases in firms' investment opportunities or cash flows. Both the theory (Barclay, Smith Jr., and Watts 1992; Barclay, Smith, and Watts 1995; Barclay, Smith Jr., and Morellec 2006) and empirical findings (Rajan and Zingales, 1995) suggest that incremental investment

[2]The eight states include CA, DE, IL, KS, MN, NC, UT, WA. The background under which these eight states became pro-employee state is explained in Section 2.

opportunities decrease firms' debt as the marginal underinvestment cost of debt grows relative to the declining marginal free cash flow benefit of debt. Similarly, an increase in cash flows should also *decrease* debt as the cash flow would be a cheaper source of financing particularly for knowledge-intensive firms compared to any external financing options.

Therefore, I next show corroborating evidence of improving the pledeability of patents as an underlying channel that facilitates firms' access to secured debt financing. First, I find that the number of pleadgeable patents as measured by the number of granted patents rises by 8%, and also the number of granted patents per million dollars of R&D spending increases by as much as 65%. More importantly, the number of actually pledged patents as collateral also increases by 2.4% for treatment firms relative to control firms post-treatment. Second, I examine the types of debt issued after patent ownership shifts to firms. Firms would have easier access to secured debt as pledged patents increase liquidation values by shifting control rights on collateralized patents to lenders when a borrower firm defaults. Consistently, I find that treated firms' use of bank debt, which is often secured by assets, increases. Also, pledged patents allow lenders to extend credit on more favorable terms. I find that while the level of long-term debt increases substantially, the level of short-term debt hardly changes. In addition, firms that are relatively ex-ante financially constrained benefit more from the increase in patent property rights, reinforcing the role of property rights in alleviating the financing friction between firms and financiers by strengthening pledgeability of patents.

Finally, consistent with Hart (1995), which shows that when assets are complementary some form of integration is better than separate ownership, I find that firm ownership of patents improves innovation productivity and patent pledgeability by reducing holdup problems. I first show a cross-sectional heterogeneity where firms with a relatively larger *ex-ante* holdup marginally benefit more from the integration of patent ownership. I also show results indicating that firms in general benefit from a reduction of holdup *ex-post.* As such, I find that asset complementarity as measured by the number of self-citations and the average number of inventors assigned per patent, a proxy for inventor collaboration, increase by

12.5% and 5%, respectively. Moreover, in addition to the increase in the number of pledgeable patents shown in the earlier result, I find that the quality of patents as measured by the number of citations, increase by 40%.

Despite the growing use of patents as collateral, which improve knowledge-intensive firms' access to debt financing, an important friction arising from the underlying allocation of property rights to patents between firms and their employees has been unexplored. This is a timely question of interest as U.S. firms have moved toward technologies that rely on the generation of knowledge assets and away from those that rely on brick-and-mortar assets. To this end, the results in this paper collectively showcase an important contribution toward understanding of how property rights affect firms' financial decisions, financial frictions, and innovation productivity.

This paper contributes to a few strands of related literature. First, this paper builds on the growing literature in patent collateral and debt financing. Loumioti (2013) and Mann (2018) document an increase in the use of patents as collateral. Mann (2018) further empirically shows that, everything else constant, stronger creditor rights on patents as collateral lead to greater access to debt financing and R&D investments. Chava, Nanda, and Xiao (2017) provides complementary results on how the value of patent collateral is priced in bank loans. Hochberg, Serrano, and Ziedonis (2017) provides evidence on how venture lending to startup firms increases with the redeployability of patents when the liquidity of secondary markets for patents rises. This paper focuses on the incomplete contract as friction instead and shows how property rights improves firms' access to debt financing by increasing firms' ability to pledge patents while a firm is solvent and hence captures proportionately more important incremental property rights effects.

Recent studies on property rights and firm innovations focus on showing the protection of returns from innovation as an important determinant of firm boundary decisions. Using a sample of UK firms, Acemoglu, Aghion, Griffith, and Zilibotti (2004) shows that incentives for integration are high(low) for firms in a relatively lower(higher) ex ante R&D investment intensity, as R&D investments are easily expropriated. Similarly, Fresard, Hoberg, and Phillips (2017) shows that vertical integration is less likely in industries where innovation is in early stages and R&D spending is high, since returns are best protected under separate ownership when investments by technology developers are more important. By relying on a natural experiment, this paper is able to examine *ex-post* effects of a shift in property rights that alters firm boundaries. Given that corporate inventor-employees are accountable for about 90% of all patentable inventions in the U.S. (Pisegna-Cook 1994; Gruner 2006), studying the allocation of property rights between firms and employees and subsequent financing decisions made by firms is important for understanding the corporate innovation dynamics.

Finally, this paper is related to inventors and innovation and highlights inventors' human capital as important input in innovation processes (Liu, Mao, and Tian 2017; Islam and Zein 2018). In a closely related paper by Hvide and Jones (2016), the authors find that a shift in patent property rights from researchers to universities in Norway led to a large decline in the rate of startups, the quantity, and the quality of innovation by university researchers. These contrasting results may be attributable to institutional differences between universities and corporations. The firm-employee relationship-specific innovation processes, corporate inventor employment contracts, and employee compensation schemes may limit the deterioration of employee incentives when property rights shift in a corporate setting. Therefore, although inventors provide key human capital an institutional setting also plays an important role in the outcome of innovation.

The remainder of the paper is structured as follows. Section 2 explains the empirical strategy of this paper using pre-invention assignment agreements. Section 3 outlines the hypotheses. Section 4 describes the data in detail. Section 5 presents main results, Section 6 provides a discussion of the results, and Section 7 presents additional robustness tests. Section 8 concludes the paper.

2. Empirical Strategy

2.1. Pre-invention Assignment Agreements

In this paper, I focus on property rights to patents arranged by a contract written between corporate inventor-employees and their employer firms. A pre-invention assignment agreement is an employment contract that obligates an employee to assign to the employer all interest in any future inventions conceived during the employment term. This contract is prevalently used and required to be signed by technical employees, engineers, and researchers (Cherensky 1993; Pisegna-Cook 1994; Mattioli 2011). The scope of pre-invention assignment agreements may be broad enough to cover categories of inventions beyond employer-specified inventions and extend past the terms of employment for a reasonable period after employment has ended. As part of employment contracts, pre-invention assignment agreements are governed by state laws, and when present, supersede common laws (the default rule in the absence of such agreement). Generally, courts honor these agreements.[3]

In the early 1990s, eight states enacted state legislation[4] to protect inventor-employees from employers' abuse of their superior negotiation positions, limit the scope of employers' claims on employee inventions, and help clarify conditions under which a pre-invention assignment agreement is considered effective (Pisegna-Cook 1994; Howell 2012). Inventions that fall under the protection of state legislation most likely arise from "general inventive employees[5] (e.g. software engineers)," who perform general research or design work and are subject to specific inventive employment, but no specific inventions or end results are

[3]Absent invention assignment agreements, the court considers the nature of the employment, the subject matter of the invention, and the resource contribution of the employer to determine the extent of firms' ownership or the shop-rights according to the common law principles (Pisegna-Cook, 1994).

[4]California, Cal. Lab. Code §§2870-72 (1994); Delaware, Del. Code Ann. tit. 19, §805 (1993); Illinois, Ill. Ann. Stat. ch. 765, para. 1060/2 (1994); Kansas, Kan. Stat. Ann. §44-130 (1993); Minnesota, Minn. Stat. Ann. §181.78 (1994); North Carolina, N.C. Gen. Stat. §§66-57.1-2 (1994); Utah, Utah Code Ann. §§43-39-1 to -3 (1994); Washington, Wash. Rev. Code §§49.44.140-.150 (1994)

[5]The other ends of the employment type spectrum are "specific inventive" or "employed-to-invent" employees and "non-inventive" employees (Gullette, 1980). Since specific inventive employees' work serves specific purpose of inventing defined process or product, once the goal is achieved, the employer is entitled to the invention. On the other hand, the work of non-inventive employees, such as shop or manufacturing as well as non-technical employees, does not involve any expectation of inventive activity.

contemplated. Inventions from general inventive employees tend to be a grey area because these employees may be encouraged by their employers to pursue creative projects that may diverge from assigned work.[6]

There are several advantages of using pre-invention assignment agreements in this paper. First, although property rights to any asset used for production nevertheless have implications for debt financing as highlighted in the introduction, the growing role of knowledge assets as collateral for securing financing suggests patents as a timely and appropriate venue for this study. Second, pre-invention assignment agreements are prevalently used in knowledge-intensive firms and somewhat pre-define the division of ownership of patents. Lastly, the existence of a plausibly exogenous shock on the interpretation of pre-invention assignment contracts helps establish causal inference and quantify the effect of strengthening firm ownership of employee patents. Empirically showing the equilibrium outcome and establishing a causal relationship between property rights and firms' debt financing are challenging tasks because property rights allocation is endogenous. Before I explain the quasi-experiment setting in Section 2.2 and 2.3, I provide an example of how pre-invention assignment agreements are used.

Recently, pre-invention assignment agreements have become widely used throughout an organization regardless of an employee's likelihood of inventing (Mattioli, 2011). For example, Ford has initiated a companywide innovation challenge and encouraged its employees from any part of the business to participate by submitting invention ideas on new products or changes to the company's existing offerings.[7] The contest rules require a submission of an invention disclosure form (a pre-invention assignment agreement), which says "Each entrant will assign and Sponsor will hold exclusive right, title and interest in all inventions or other materials submitted and, in all revenue, profits and Net Proceeds generated as a result of commercialization of a Submission[...]."[8] The company claims that, from the start of the

[6]For example, Google is known for encouraging its engineers 20 percent of their paid time to work on pet projects.

[7]*The Washington Post*, Dec. 14, 2016. "How Ford turned thousands of employees into inventors"

[8]The Ford innovation contest rules are available on http://henryfordinnovation.com/challenge/contestrules/

first challenge in January 2015, more than 4,500 Ford employees have submitted invention ideas and nearly 3,500 *first-time* inventors have participated in the event. This example illustrates two important aspects of pre-invention assignment agreements. The first is the broad use of the agreement across *all* employment types, and the second is the important role of the law's interpretation of such agreements when disputes over property rights on aforementioned general inventive employee inventions arise. Recently, however, the overwhelming employer claims on employee inventions[9] have raised concerns. Firms increasingly take advantage of the protection provided by pre-invention assignment agreements, and thus invention assignment agreements highlight the significant value of knowledge assets to firms.

2.2. Institutional Setting

In 2008, the Court of Appeals Federal Circuit (CAFC) made a decision in *DDB Technologies LLC v. MLB Advanced Media, LP*[10] on a pre-invention assignment agreement case that shifted employee invention property rights from employees to firms, resulting in more pro-employer trends toward invention assignment agreements. CAFC cases are heard by a panel comprised of three judges who are selected randomly, which minimizes potential political influences. In addition, CAFC case sessions are generally held in Washington, D.C., which further limits the possible impact of local state economies on the court's ruling.

The CAFC decision on *DDB Technologies LLC v. MLB Advanced Media, LP* had three main parts (Hedvat, 2011). First and foremost, despite the fact that employment contracts are governed by state laws, the court ruled that provisions regarding patent assignment will be regulated under federal law. The significance of this statement is that this would preempt pro-employee state legislation in the eight states and create uniform standards on patent assignment provisions. Second, employers are granted authority over patents when "express" language is provided in employment contracts. This means that making claims over em-

[9] *The Economist*, Dec. 14, 2013. "Ties that bind"; *The New York Times*, Apr. 13, 2014. "My Ideas, My Boss's Property"

[10] 517 F.3d 1284, 1290 Fed. Cir. 2008

ployee inventions would become easier by the presence of expressive terms, such as "agrees to and does hereby grant and assign...," in invention assignment agreements. The expressive terms put the assignment in the present tense, and thus even without any inventions in the present, future inventions are automatically and immediately assigned to firms, under the court's interpretation (Baniak and Dawson, 2009). The practical influence of the court's ruling can easily be found in law firms' advice to corporate clients at the time. Law firms recommended that their corporate clients include express phrases in invention assignment agreements.[11] For example,

> "The *DDB Technologies* decision should provide comfort to employers that the effect of language assigning patents in employment agreements will be interpreted uniformly pursuant to federal law and will not be subject to differing interpretation under varying state law. The decision creates a roadmap to which employers can be reasonably certain that if their employment agreements contain language that expressly assigns rights in existing and future inventions, this assignment language will be interpreted under federal law to vest automatically ownership of the inventions with the employer, regardless of the state law governing the agreement or the domicile of the employee."[12]

Third, since CAFC has nationwide jurisdiction and the court categorized *DDB Technologies LLC v. MLB Advanced Media, LLP* as a precedential case, the decision impacts future pre-invention assignment agreement cases.[13]

[11] For additional discussions written by law firms, see "Employer and employee ownership of intellectual property. Not as easy as you think" available at http://legalsolutions.thomsonreuters.com/law-products/news-views/corporate-counsel/employer-and-employee-ownership-of-intellectual-property-not-as-easy-as-you-think; "Ruling Will Guide Employers' Rights to Inventions" available at https://www.law360.com/articles/48989/rulingwillguideemployersrightstoinventions

[12] The full discussion is available at http://www.kramerlevin.com/Federal-Circuit-Supplants-State-Law-to-Interpret-Patent-Assignments-in-Employment-Agreements-11-06-2008/

[13] Hedvat (2011) shows (in the footnote 63) that the subsequent courts have adopted the reasoning and holding of DDB Technologies LLC v. MLB Advanced Media, LLP case in Board of Trustees of the Leland Stanford Junior Univ. v. Roche Molecular Sys., Inc. (Fed. Cir. 2009), Rothschild v. Cree, Inc. (D. Mass. 2010), EMD Crop Bioscience, Inc. v. Becker Underwood, Inc. (W.D. Wis. 2010), and STMicroelectronics, Inc. v. Harari (N.D. Cal. Aug. 2008).

In summary, the CAFC decision effectively increased firms' property rights to employee inventions in the eight formerly pro-employee states by preempting existing state legislation on protecting inventor-employee interests regarding invention assignment agreements.[14] In the following section, I explain the implementation of this empirical setting in regression analyses.

2.3. Methodology

I exploit the CAFC decision in 2008 as an exogenous shock in a difference-in-difference framework. I define firms with headquarters located in the eight states affected by the court's decision as treated firms. I use the state of headquarter as a definition of state because "Generally, the state where the employer is located or where the job duties are performed will be a reasonable choice of law and likely be honored" (American Bar Association, 2014). I present a few example cases in the Appendix C to verify that the headquarter state is indeed a reasonable indicator for treatment state. For all firms, post-treatment period is defined as years after 2008.

The main regression specification is as follows.

$$Total\ debt/Assets_{i,s,t} = \alpha + \beta\, treat_{i,s} \times post_t + \delta_i + \gamma_t + \epsilon_{i,s,t} \tag{1}$$

The main regression dependent variable is total debt-to-assets ratio as a measure of a firm's level of debt financing. The total debt is a sum of long-term debt and short-term debt. An important identification assumption for the difference-in-difference estimate, β, to be consistent is that, absent treatment, the change in the total debt-to-assets ratio for firms in the treatment states would not have been different than the change in the same ratio for firms in the control states. To provide some evidence of this parallel trend assumption, I

[14] It is a possibility that the ruling discourages invention disclosures by inventor-employees for the fear of losing ownership after the ruling. However, if there is any intention to profit from an invention, the incentives to hide inventions and not patent them would be low given the required protection on property and cashflow rights under the patent system.

present a visual inspection of the parallel trends in Section 4.

Under the identification assumption, the difference-in-difference coefficient captures the additional changes for firms in the treatment states, relative to firms in the control states, following the shift in property rights after the 2008 CAFC decision. In addition to firm and year fixed effects presented in the above specification, I use firm and industry-year fixed effects to rule out potential unobserved heterogeneity across industries over time and get a more precise estimate. Lastly, I include error clustering at state level to correct for potential error correlation within the same state and account for serial correlations in the dependent variable.

3. Hypothesis Development

3.1. Debt Financing

The importance of patents has grown over time as knowledge-intensive firms use patents as collateral to access debt financing. However, property rights to patents raise another underlying financial friction for knowledge-intensive firms' access to debt financing. Property rights to patents are notably important as the ownership boundary of knowledge assets is not as clear as physical assets. An invention assignment agreement dictates some of property rights allocation between a firm and its inventor-employees, but the ill-defined nature of innovation ex-ante makes it too costly for firms to write a complete contract that specifies delivery of exact innovation or usage in every state of the world (Aghion and Tirole, 1994).

The incomplete invention assignment agreements expose firms' innovation processes to holdup and underinvestment problems and thus also affect innovation productivity and access to debt financing secured by patents. In the context of the empirical setting of this paper, a transfer of patent ownership encourages investments by firms but discourages investments by inventor-employees, resulting in a trade-off that makes it difficult to have strong prior on the net effect. However, as firms have comparative advantages in collectively managing, commercializing, and providing resources for innovation (Gruner, 2006), I predict that shifting property rights to firms facilitates firms' access to debt financing by enhancing the pledgeability of patents as collateral, as holdup problems that stem from incomplete contract subside.

3.2. Patent Pledgeability and Holdup Problems

Under modern complex innovation processes, the integration of many components makes the individual ownership of patents more costly (Merges, 2009). Particularly when assets are complementary, such as patents that make up different parts of the same end-product, some form of integration is better than separate ownership (Hart, 1995). That is, integration induces a reduction in holdup, which emerges along with the synergies created by relationship-specific innovation processes, improving firms' patent pledgeability. The patents produced from this economic relationship between a firm and its employees cannot be substituted by market transactions, which lack such synergies.

Hence, I first predict that improved innovation productivity under firm ownership of patents post-treatment increases patent pledgeability as measured by the number of pledgeable patents and the number of actually pledged patents. Further, firms with an ex-ante larger degree of holdup problems would benefit more from the integration of patent ownership and thus have more pronounced debt financing effect. Second, a reduction in holdup problem substantiate an increase in complementarity among firms' patents and inventor-employees as integration promotes greater knowledge transfer and information sharing within firms. Lastly, under firm ownership of patents, synergies in innovation processes would improve the quality of patents, as measured by the number of citations, and would attract greater interest in patented technology and commercializing patents. These effects would further reinforce easier access to debt financing provided that lenders are able to use patent citations to assess patent values(Chava et al., 2017).

4. Data

Although pre-invention assignment agreements are commonly required as a part of employment contract, whether or not a firm uses the contract is only partially observable.[15] However, it is widely accepted that the pre-invention assignment agreements are typically presented to engineers and almost all technical employees. To ensure that sample firm employees are bound by such contract, I restrict my sample to *patenting* US public firms (country of incorporation is United States) in Compustat. I also exclude financial firms (SIC code 6000-6999) and utilities (SIC codes 4900-4999) for the reasons that these firms may be affected by capital requirements or regulatory supervision. I drop observations with missing total assets and replace missing values of debt with zero.

Next, I collect patent grant data from United States Patent and Trademark Office (USPTO). USPTO provides US patent grant documents from 1926 to present. I download each document between 2003-2016. Each document contains information about the patent, application and grant dates, names and locations of inventors and assignees, and citations. I keep only the utility patents[16] assigned to US domicile corporations so that I can make sure the empirical setting applies to the sample firms. Then, I name-match the collected data to the Compustat sample firms. A detailed description of how the data was collected is in the Appendix B.

The final sample consists of 1,959 unique patenting firms during the sample period of 2003-2013.[17] Table 1 describes the sample firms. The main dependent variable is total debt-to-assets ratio, computed by dividing the sum of short-term and long-term debt by total assets. Panel A describes financial characteristics of all sample firms during the entire sample period from 2003-2013. Notice that the total debt-to-assets ratio is slightly smaller than

[15]Sometimes firms disclose the use of pre-invention assignment agreement on annual financial statements such as 10-K, but firms are not required to do so.

[16]Utility patents are inventions of a new and useful process, machine, manufacture, or composition of matter, or a new and useful improvement thereof.

[17]The sample period is 2003-2013. However, a few important outcome variables, such as count of citations, suffer from truncation problem. To avoid such issue, I collect patent data up to 2016.

the average of all Compustat firms. This is consistent with the stylized fact that knowledge-intensive firms tend to carry less debt. The ratio is even smaller for firms in the treatment states presented in Panel B. Panel B compares the firm characteristics by treatment and control state firms only during the pre-treatment period between 2003-2007. The treatment state firms are smaller in size and have relatively lower total debt-to-assets ratio. The treatment state firms also in general seem to be involved in slightly greater patent activities. The p-values in the last column show that both the financial and patent activity characteristics are statistically different between the treatment state firms and control state firms.

The fact that the treated and control firms are different on observable dimensions may raise concerns for endogeneity issues in the empirical analyses given the possibility that they may also differ on unobservable dimensions in a way that violates parallel trends assumption. These concerns are mitigated in the following ways. First, the difference-in-difference setting fully accounts for any observable level difference between the treatment and control groups using the *treat* indicator. Second, although there is no way to formally test the parallel trends assumption, I show in Figure 1 that the treatment and control groups during the pre-treatment period seem to have parallel trends. In addition, I include firm fixed effects in all of my regression specifications to mitigate potential confounding effects of the time-invariant unobservables. Lastly, I do a robustness check with matching on observable regressions at the end of the results section.

5. Results

5.1. Increasing Debt Financing

Table 2 reports the main baseline regression results on debt financing. In Panel A, the dependent variable is the total debt-to-assets ratio. Column (1) is a plain OLS without fixed effects and clustering errors. Column (2) includes firm and year fixed effects to remove firm-specific time-invariant effects and aggregate time trends. Column (3) uses a more stringent specification by additionally including the industry-year fixed effects to absorb potential industry-year-specific shocks. The difference-in-difference estimates are similar. Following the court's ruling that shifted employee patent property rights to firms, treatment firms' total debt increases by about 2.5 percentage points, relative to control firms. This is an 18% increase in the total debt-to-assets ratio for an average treated firm with $2.46 billion in pre-treatment total assets and a total debt-to-assets ratio of 0.14. The estimate translates into about a $62 million increase in total debt. In Panel B, I re-run the baseline specifications in Panel A using the actual *flow* variable of debt as measured by the long-term debt issuances (dltis) scaled by total assets at the beginning of the year and confirm that the issuance of new long-term debt also increases by 1.5 percentage points (a 23% increase in the ratio), or by $37 million.

The impact of the ruling was immediate and easily perceptible to firms. Following the CAFC decision, law firms promptly informed and advised their corporate clients about the ruling's new interpretation of existing invention assignment agreements favoring firms' rights (see Section 2.2.). The ipmlications and significance of who owns employee inventions are evident in media discussions.[18] Furthermore, the economic magnitude of debt changes caused by the shift in property rights to patents is comparable to those in the creditor rights literature, which finds a sizable impact of law on debt financing. Bae and Goyal (2009) finds that better enforceability of loan contracts in 49 countries over 1994-2003 increases loan amounts by $57 million. Loumioti (2013) documents that firms using patents as collateral between 1996 and 2005 increase secured syndicated loan amounts by $51 million. Mann (2018), by exploiting exogenous changes in creditor rights, finds that strengthening creditor rights increases total debt by $26 million per quarter.

Figure 1 traces the difference-in-difference coefficient (vertical axis) estimates over the years to treatment (horizontal axis). The key observation is that, prior to the treatment in $t = 0$, the coefficients are statistically indistinguishable from zero and only become positive

[18] *The Economist*, Dec. 14, 2013. "Ties that bind"; *The New York Times*, Apr. 13, 2014. "My Ideas, My Boss's Property"

and statistically significant following the court's ruling in 2008. This graphical illustration provides some visual inspection of parallel trends, where the flat line prior to the treatment is suggestive of no pre-existing differential trends in the total debt-to-assets ratio. The graph also emphasizes the sharp change in debt financing for treatment firms relative to control firms. There is no reversion of the effects, though there seems to be a slight additional increase in later years, which is likely to reflect some lagged effects and will be further investigated at the end of Section 5.3.

The falsification test in Table 3 further helps establish the internal validity of the empirical setting. To ensure that the court ruling is only relevant for patenting firms' debt financing through its impact on invention assignment agreements, I re-run the baseline regressions in Table 2 using only *non-patenting* firms. The coefficients on the total debt-to-assets ratio are statistically insignificant. The magnitudes are also substantially smaller for non-patenting firms, which have a higher average pre-treatment total debt-to-assets ratio of 0.28. The coefficients on long-term debt issuance are even slightly negative and statistically indistinguishable from zero. Therefore, the falsification test reassures that the observed increase in the level of debt financing found in the main regressions is most likely caused by the treatment effect of increasing the property rights of patenting firms after the CAFC ruling.

Lastly, I ensure that the main results do not capture spurious changes in debt. If firms have geographic subsidiaries located in multiple states and thus are exposed to multiple state laws governing invention assignment agreements, the effect of the treatment may be less clear. Table A1 in the Appendix shows that this is not the case. Each column presents baseline regression estimates for zero, fewer than or equal to one, or fewer than or equal to two geographic subsidiaries, respectively.[19] The results still hold, but the statistical significance weakens, possibly attributable to the significant reduction in the number of observations. In Table A2, I run the baseline regression by each treatment state against control states and

[19] The data is provided on Professor Scott Dyreng through https://sites.google.com/site/scottdyreng/Home/data-and-code

show that the main result in Table 2 is also not driven solely by California.[20]

A failure to control for the series of strengthening creditor rights occurring in my sample period may raise omitted variable bias problem. Following Karpoff and Wittry (2018), I eliminate such concern by separately controlling for the interaction between a Delaware-incorporation dummy and a post dummy based on the dates of the creditor rights court cases used in Mann (2018).[21] In Table A3, the baseline results are essentially unaffected, which ensures that the baseline results correctly identify and captures the incremental impact of strengthening firm ownership of employee patents above and beyond the concurrent changes in creditor rights.

I avoid including time-varying control variables that may be affected by the treatment and give inconsistent estimates of the treatment effect. However, Table A4 in the Appendix shows similar results when I include common controls in the leverage literature in columns (1) through (6) and also when I include measures of innovation stock and R&D expenditures in columns (7) and (8), respectively. In an unreported table, I confirm that the new long-term debt issuance results also stay robust with the inclusion of these controls.

5.2. Pledgeability and Types of Debt

One may be concerned that the treatment concurrently increases firms' investment opportunities or cash flows, which in turn increase firms' debt capacity. Since investment opportunities are unobservable and likely affected by the treatment, I cannot empirically disentangle this possibility. However, both the established theory (Barclay et al. 1992;Barclay et al. 1995; Barclay et al. 2006) and empirical findings (Rajan and Zingales, 1995) show

[20]Lerner and Seru (2017) warns of regional biases of patent data, particularly with California and Massachusetts, where innovative activities are concentrated.

[21]The empirical setting in (Mann, 2018) relies on heterogeneous effect of firms incorporated in creditor-friendly Delaware and on a series of time variations from four federal court decisions (2002, 2003, 2007, and 2009) that strengthened state property laws for the ownership of patents. The original paper stacks the four creditor rights court cases as they occur in a very tight time frame making it difficult to distinguish "pre" and "post" with overlapping time windows around each court case. Hence, I individually control for the last three court cases that occur within my sample period between 2003 and 2013. However, controlling for all three creditor rights interaction terms in one regression does not change the result.

that incremental investment opportunities decrease firms' debt because they increase the marginal underinvestment cost of debt while decreasing the marginal benefit of reducing free cash flow problems with debt.[22] Furthermore, for financially constrained firms, cash flow should be a cheaper source of financing and thus should decrease debt.[23] Knowledge-intensive firms are likely concerned more about the lack of collateral, which makes debt very difficult and expensive to obtain in the first place. In this vein, I provide channel evidence that improving patent pledgeability facilitates secured debt financing and relaxes the terms of borrowing.

The firm ownership of patents creates relationship-specific innovation processes, which improve innovation productivity and patent pledgeability. Therefore, I further show that the treatment enhances the production of pledgeable patents and allows firms to pledge more patents as collateral. The results are reported in Table 4. In columns (1) and (2), I measure the number of granted patents at $t + 2$ to account for the patent system process, which takes an average of two years (Hall, Jaffe, and Trajtenberg, 2001). Following the court's ruling, treatment firms' number of granted patents increases by 7.7% ($= e^{0.074} - 1$), relative to control firms. Also, in columns (3) and (4), I scale the number of granted patents by R&D expenditure, which also increases by 65% for treatment firms (the unconditional pre-treatment ratio is 0.38).[24]

I further examine how the number of patents *pledged* as collateral changes after the treatment, providing direct evidence of how the enhanced pledgeability of patents under firm ownership results in the actual growth in the number of pledged patents. I collect patent assignment data from USPTO and identify patent reassignments marked as "secu-

[22] In a dynamic setting, unless the adjustment cost is zero and/or there is high correlation between investment opportunities and future cash flow, the correlation is less likely be a positive one.

[23] Lian and Ma (2018) documents that a cashflow-based borrowing is less common among small firms given their low profits. The median share of cashflow-based borrowing for small public firms is 7% compared to asset-based borrowing of 61%.

[24] To provide some comparison, the National Science Foundation National Center for Science and Engineering Statistics reported a national aggregate patent propensity of 0.42 in 2008 (see https://www.nsf.gov/statistics/infbrief/nsf13307/)

rity interest."[25] The dependent variable is computed as the logarithm of total number of collateralized patents per firm-year. Columns (5) and (6) show that, under firm ownership of patents, treated firms are able to pledge about 2.4% more patents as collateral relative to control firms.

Next, I explore how the types of debt financing changes following the improved patent pledgeability. Table 5 presents the results on changes in different types of debt. Column (1) shows that bank debt, which is likely to be secured by assets, increases by 16%, where the pre-treatment average ratio is 0.037. However, convertible debt, reported in column (2), continues to be a viable means of accessing debt financing for high R&D-intensive firms (Stein 1992; Julio, Kim, and Weisbach 2007). This suggests that patent-secured debt financing may not completely substitute for all other types of debt financing.

I further explore how debt maturity is affected. The transfer of control rights on pledged assets upon default enables greater bargaining power for lenders, who in turn may extend credit on more favorable terms, such as lower interest rates or longer maturities. Earlier studies find that firms subject to stronger secured creditor rights and enforcement have longer maturity loans (Giannetti 2003; Diamond 2004; Qian and Strahan 2007). I also find consistent results, where an increased capacity to pledge assets makes collateral more effective and increases loan availability. Whereas long-term debt increases significantly in column (3), the estimates on short-term debt in columns (4)-(6) are statistically indistinguishable from zero.

To further strengthen the argument that the shift in property rights facilitates financing for innovation by improving patent pledgeability, I test the heterogeneous effects of ex-ante financial constraints on debt financing. In Table 6, I use several widely used proxies of financial constraints measured during the pre-treatment period to split the sample firms and then interact each proxy with $treat \times post$ to estimate the marginal effect of ex-ante financial constraint on firms' debt financing. All coefficients on triple-difference terms are positive.

[25] USPTO assignment files show which patents are reassigned under security interests, but since the amount of borrowings associated with each security interest is not reported, I cannot explicitly identify patent secured debt in the previous section.

However, only columns (2) and (4) are particularly statistically significant, indicating that firms that are younger and have low cash level prior to the treatment in general benefit more from the shift of property rights to firms. Overall, the results collectively provide support toward property rights' role on facilitating access to debt for knowledge-intensive firms.

5.3. Holdup Problems

This subsection further considers how the increase in firms' property rights reduces the degree of holdup problems. This is also the key insight that differentiates this paper from earlier studies of patent collateral and debt financing (Loumioti 2013; Mann 2018), in which the changes in creditor rights drive the results but will not affect firms' underlying innovation processes.

I first test whether the debt financing benefit of patent ownership integration is larger for firms that experienced ex-ante greater holdup problems. In Table 7, I use two different proxies for ex-ante degree of holdup during the pre-treatment period to examine how firms with greater ex-ante holdup have a marginally larger increase in debt financing. The first proxy, the average number of inventors assigned per patent, attempts to capture how the integration of many components makes individual ownership more costly (Merges, 2009). Thus, a larger of number of inventors assigned to each patent would have caused a greater potential holdup problem (or coordination problem). Columns (1) and (2) of Table 7 show that firms with larger ex-ante holdup also raise more debt, suggesting that these firms benefit more from the integration of ownership after the court's ruling.

The second proxy is the non-compete agreement enforcement index (Garmaise, 2011), for which a higher index score indicates stronger enforcement of a non-compete agreement and thus lower mobility. The idea is that the shift of patent ownership from employees to firms may decrease inventor-employees' incentives to innovate, and the benefit of the reduced holdup from integration is diminished when weaker non-compete agreement enforcement allows inventor-employees to leave the firm. Therefore, the treatment effect should be marginally larger when non-compete agreement enforcement is stronger, indicating the ex-post synergies between the firm and inventor-employees remain inside the firm. Columns (3) and (4) of Table 7 show that the marginal increase in debt financing is larger for firms with greater non-compete agreement enforcement (or lower employee mobility).

Next, I show that shifting property rights to firms reduces *ex-post* holdup problems in general and, in turn, substantiates an increase in complementarity among firms' patents and inventor-employees as integration promotes greater knowledge transfer and information sharing within firms. In Table 8, the first two columns show that the number of self-citations increases by about 12.5%. Self-citation captures the degree of change in firms' complementary use of patents, and the positive and statistically significant coefficient is suggestive of an increase in firms' knowledge accumulation and incentives to internalize knowledge spillovers created by their own developments (Hall et al., 2001). Similarly, there is greater collaboration among inventor-employees, proxied by the average number of inventor-employees assigned per patent, around the court ruling. Columns (2) shows that inventor collaboration increases by 5%, where the pre-treatment mean is 2.78 inventors per patents. Since there may be some variation across industries in how R&D is conducted, column (4) includes industry-year fixed effects to remove such variation, and the result strengthens even more.

In addition, Table 9 shows that the significant increase in the number of pledgeable patents (columns (1) and (2) in Table 4) is accompanied by improvements in the quality of existing and new patents. Chava et al. (2017) shows that lenders are able to differentiate the quality of pledged patents, based on the number of citations per patent.[26] Therefore, improvement in the patents quality would also reinforce the pledgeability of patents as collateral. The dependent variable in columns (1) and (2) of Table 9 is the number of average citations per patent. The regression estimates show that the number of citations increases by 40%. This is surprising given that patent citations tend to decrease over time. It is important

[26]Patent citation is a well-established and widely-used measure of patent quality for its conveyance of both technological and economically significant information that signify the economic value of the cited patents (Trajtenberg 1990; Hall et al. 2001; Hall, Jaffe, and Trajtenberg 2005).

to note that patent citations measure not only the quality of patents but also redeployability of patents as they indicate interests from external users of the technology (Hochberg et al., 2017). This is strong evidence that firm ownership of patents fosters external interests in firms' innovation and that the increase in redeployability of patents helps attract lenders. Similarly, columns (3) and (4) show that firm ownership of patents also increases citations received by patents that are granted *after* the treatment by 6.5% compared to those that are granted *prior* to the treatment.[27]

Finally, I briefly revisit Figure 1 to examine some lagged effects of shifting property rights. By the nature of the R&D process, there may be some time lags for the treated firms to integrate assets to their full capacity. In Appendix Table A5, I trace the treatment effects over time.[28] The treatment effects on self-citation appear immediately, though they are not as sharp as the debt results. Citations do not require time and thus picks up immediate changes. However, changes in patent grants and inventor collaboration appear with some time lag. The change in the number of granted patents already accounts for the average 2-3 year patent review process, and thus seeing the effects starting at year $t + 0$ can be interpreted as capturing lagged effects. Likewise, firms may need some time to reorganize inventor groups and corporate-level innovation processes, and thus the effects on inventor collaboration materialize within two years of the treatment. These lagged effects likely explain why some of the main debt financing effect increases further in the later years of the post-treatment period.

[27] I use only the first 3-year count of citations to do a fair comparison of citations counts between existing and new patents as citations accrue over time. Therefore, the measure is fuzzy for patents that are granted around the treatment, where the three years include both years prior and post treatment. However, this would work against finding strong increase in citations post treatment.

[28] All time interaction terms are included in the regression, but for simplicity, I report only years close to the treatment.

6. Discussion

The findings of this paper speak narrowly to firms' debt financing and thus are limited in conveying the optimal financing[29] or patent ownership structure that lead to a normative conclusion. The implications for firm value would depend on changes in the level of ex-post innovation investments and inventor-employee incentives. However, addressing these questions in detail has limitations as to obtaining inventor-employee-level data. Instead, I attempt to address some of these questions as corollaries using the best available firm-level and state-level data.

I first examine how firms' innovation investments change ex-post. Table A6 shows how the absolute amount of R&D investment changes after the treatment. The dependent variable is the logarithm of R&D expense. Column (1) shows that post-treatment R&D spending increases marginally more for the treatment firms and also cross-sectionally more for firms that were ex-ante relatively financially constrained as measured by younger age (column (3)) and no record of dividend payments (column (4)). Also, firms with a *higher* ex-ante degree of holdup (column (6)) spend marginally *more* on R&D expenses. The results become statistically weaker when the dependent variable is replaced with the R&D expense to assets ratio. However, what may be a more relevant interest following the property rights shift pertains to changes in human capital investments as inventor-employee incentives would be directly affected.

Shifting property rights of employee inventions would naturally have an adverse impact on inventor-employee innovation incentives unless they are additionally compensated otherwise. Surprisingly, despite this shift, in an untabulated result I find a statistically indistinguishable change in inventor-level productivity following the court ruling.[30] This result

[29] It is difficult to draw implications on the *optimal* form of financial contracts or capital structure, as invention assignment agreements are restricted to the relationship between firms and employees, not between firms and financiers (Hart and Moore, 1998). Nonetheless, it may still be of some interest to explore whether the increase in property rights also affects the issuance of new equity. In an unreported analysis, I find that there is no sizable statistically significant effect on seasoned equity offerings of the sample firms.

[30] In unreported result, I find that a rough estimate of changes in the number of granted patents *per inventor* is positive but statistically insignificant. However, absent more detailed employee-level data, it is

is somewhat consistent with the theory that complementarity should yield benefits but no costs because marginal returns from owning part of the asset are the same as marginal returns from not owning any assets (Hart, 1995). This also helps explain why this paper finds contrasting results from Hvide and Jones (2016), which finds negative innovation incentives when researchers affiliated with universities lose their property rights to universities. In the university setting, both relationship-specific innovation and asset complementarity are absent.

However, beyond complementarity, there still may be another reason for the lack of change in inventor innovation incentives. Specifically, firms may offer increased employee compensation or remuneration for their inventions, financed by the new debt, and thus allow the inventor-employee incentives to remain the same. A direct test of this hypothesis requires detailed inventor-employee-level wages and inventor labor market data, which is not available.[31] However, there are anecdotal stories about how firms are gradually increasing invention-related compensations to incentivize employees. For example, since Ford Motor Company initiated the firm's internal innovation competition by encouraging employees from any division or rank to invent, the company has also introduced new compensation measures for Ford inventors. Employees with promising ideas can receive a three-month membership to TechShop, which provides equipment and space for inventors to turn ideas into prototypes. More importantly, employees are given financial incentives, including a share of license income, and other monetary compensations in addition.

Although a detailed test is not possible due to lack of firm-level inventor-employee compensation data, I attempt to provide a rough state-level analysis on wages of innovation-related occupations. Table A7 shows a suggestive correlation that the wages of occupations

difficult to clearly confirm that employee incentives changed one way or the other. The interpretation of this result is limited by a few shortcomings. First, USPTO patent grant database reports only eventually granted patents, which can result in a upward bias. Second, despite of shift in property rights to firms, firms may have increased compensation for patenting activities instead, which is also unobserved. However, even with disincentivized inventor-employees post-treatment, the comparative advantage of firms in managing innovation processes may still result in positive changes in innovation productivity.

[31] Even the US Census Longitudinal Employer–Household Dynamics (LEHD) does not classify occupations in detail nor have separate occupation for "inventors."

related to R&D and invention increases post-treatment. I obtain occupation-level wages by state over the sample period from the Bureau of Labor Statistics Occupational Employment Statistics (OES). OES provides a break-down of occupations and definitions on its website. In columns (1) and (2), I use a coarse classification of occupation codes that are potentially closely related to R&D and inventions. In columns (3) and (4), I further narrow-down to sub-classifications of occupation codes that explicitly mention "research and development" or "invention." The list comes down to 26 sub-classifications of occupation codes. For all specifications, I include state, occupation, and year fixed effects. Using both coarse and fine classifications, I find that the state-level mean and median annual wages of employees in occupations that deals with research and development and invention increase post-treatment.[32]

Finally, I explore state-level *aggregate* innovation effects of shifting patent ownership to firms to assess the generalizability of the court's ruling. I use the universe of patents on USPTO patent grant records, which include public and private firms, government, and individuals. In Table A8 columns (1) and (2), I confirm that there is a general property rights effect on state-level aggregate innovation, which is also illustrated in Figure A2. In columns (3), (4), and (5), I further divide the state-level sample into different assignee groups. Columns (3) and (4) capture state-level aggregate innovation effect only on firms and government, where the tension from shifting the property rights to employee patents exists. Although there seems to be some degree of group-specific effects, the estimates are positive and statistically significant, showing that the increase in property rights to patents boosts both firm and government innovation. In contrast, the estimate in column (5) is small and statistically insignificance, where individual inventors should not have been affected by the court's ruling. Subgroup analyses again confirm the validity of the empirical setting on an aggregate-level, where the court ruling is only relevant for subgroups that experience tension

[32] It is important to note important caveats and caution readers of some limitations in the interpretations. Since this is state-level wage, it includes both private and public firms, of which my sample firms would make a small fraction. Also, wages are determined by labor market conditions, which are not thoroughly reflected in the regression specifications. Since occupation codes are defined by tasks and not by rank or title, the wages include employees of all rank and title not limited to corporate inventor-employees.

in patent ownership structure through invention assignment agreements.

7. Robustness

7.1. Matching Regressions

To ease concerns for the time-varying differences in observable firm characteristics, I re-run the main regressions using matched samples on observable firm characteristics. Table 10 reports the propensity score matching diagnostics. As with any endogeneity problems, a matching regression itself does not fully resolve identification concerns, but, used in conjunction with the difference-in-difference setting, can provide a useful robustness test for earlier regression results. There are observable and statistically significant differences between the treatment and control groups during the pre-treatment period shown in the pre-match columns. It is important to note that the difference in average leverage growth rates between the treatment and control groups is statistically insignificant, reinforcing the parallel trends assumptions. The next three columns compare the same variables after propensity score matching. The p-values reported on the pairwise mean differences between treatment and control groups become all statistically insignificant, assuring that the matching process has removed meaningful differences on observable dimensions. In sum, the main results of this paper remain robust to matching away observable differences.

Table 11 presents the matching regression results using the matched sample. I use propensity score matching using observable differences between the treated and control firms reported in Table 1. In addition, to ensure that the matching process embodies the parallel trends assumption of the difference-in-difference framework, I include the annual growth rate of the total debt-to-assets ratio in the propensity score (Lemmon and Roberts, 2010). The matching regression coefficients decrease slightly but remain robust to both nearest neighbor matching with $n = 1$ and $n = 2$ with replacement.

7.2. *Alternative Explanations*

In this section, I evaluate potential alternative explanations stemming from the fact that firms can choose the state of their corporate headquarters, which is used to assign a treatment indicator in the empirical setting. Then, I address a possible concurrent effects of the 2008 financial crisis around the CAFC decision.

7.2.1. *Non-random Selection of Headquarter State*

Since firms choose in which state to locate their headquarters, I cannot completely rule out the possibility that the results may be affected by unobserved factors that are correlated with both the headquarter state decision and the financing decision. However, for the non-random treatment to be consistent with the results, it would need an omitted variable that not only relates to firm's headquarter choice but also explains why the level of debt financing for the firms in the eight treatment states responds differently from that of firms in the control states, specifically around 2008.

The major determinants of a firm's choice of headquarter state are natural resources, unionization levels, input-output relationships, state taxes, founder's home location, energy costs, and environmental regulation (Garmaise, 2011). One likely confounding factor is the state corporate tax rates. That is, firms choose to locate in one of the eight treated states for corporate tax reasons, particularly with regard to debt tax shields. If this is so, then the differential debt financing responses between treated firms and control firms may be found, even in the absence of a shock to the property rights. In Table A9, I verify that the pre-treatment trends assumption holds for state corporate tax rates, and that the year-by-year changes in corporate tax rates during the entire sample period are not statistically different between treatment and control states.[33] The regression results reported in columns (3) of Table A4 are robust to controlling for state-level corporate tax rates and, thus, rule out the

[33] The corporate tax rates are collected from Tax Foundation. The data is available at https://taxfoundation.org/state-corporate-income-tax-rates/

state tax story.

Second, following the enactment of employee protection state legislations in the early 1990s, innovative firms that are more protective of their legal rights over patents may have selected *out* of the eight treatment states, leaving only firms with a relatively higher fraction of tangible assets, such as plants and equipment, that are easily pledged as collateral. This may cause the differential access to debt financing over time. To eliminate the possibility that a difference in pre-treatment level of tangible assets drives the aforementioned results, I augment the baseline specification by including pre-treatment level of tangible assets, measured by pre-treatment average level of plants and equipment scaled by total assets, interacted by the *post* indicator. In column (4) of Table A4, I verify that the results remain robust.

Lastly, I check to see if firms with relatively high future innovation investment opportunities selected into the eight treatment states to take advantage of the employee rights protection, thereby increasing their debt financing to materialize these opportunities. In Figure A1 Panel (b) in Appendix A, I show that the distribution of intellectual property-intensive firms (or employments) are not all concentrated in the eight treated states. Therefore, if the main results were driven by ex ante investment opportunity differences, I should find similar results in firms in untreated states, as well. This is not so. In an untabulated regression without year fixed effects, I find that the coefficient on *post* is very close to zero and statistically insignificant, showing that the level of debt financing for firms in control states remained about the same over time. In addition, I include the pre-treatment level of innovation to control for ex ante innovation opportunities and again find robust results.

7.2.2. Financial Crisis

I address potential concerns with the overlapping period of the CAFC ruling and the financial crisis in 2008, such that the main results may be driven by some unobservable state-specific factor that causes treatment states to react differently to the financial crisis. Ideally, I would repeat my analysis by including state-year fixed effects to control for a state-year specific shock that would account for the differential effect of the financial crisis. However, the treatment variable is state-level, and the state-year fixed effects would absorb the *treat*$\times$*post* effect. I handle this problem in two ways. First, in Appendix Table A9, I report the differences in means of important state-level economic variable growth rates between treated and untreated states. I show that GDP growth, GDP per capita growth, and unemployment rate growth are all statistically indifferent from zero for all years during the sample period, ensuring that the trends in state economic conditions of treatment and control states are similar both before and after 2008.

Second, I mitigate the above concerns by additionally including states with industry-year fixed effects or state-industry with year fixed effects. The former specification captures variation among same-state firms, whereas the latter is a more stringent model that captures variation only among the same industry firms in the same states. Tables A10, A11, A12, and A13 report these additional results. The main regression results in Table A10 remain similar, both economically and statistically significant, for the total debt-to-assets ratio, and strengthen for the long-term debt issuance after including state fixed effects. In Table A11, the number of granted patents, patent propensity, and the number of pledged patents are robust both in magnitude and statistical significance. The estimate for the average number of citations per patent in Table A12 strengthens from about 0.4 to 0.5, while the first 3-year citation measure becomes statistically insignificant. Lastly, in Table A13, self-citation strengthens in magnitude, whereas the inventor collaboration measure weakens but is still statistically significant. Overall, the inclusion of state-level fixed effects changes a few estimates, but the results remain qualitatively consistent.

8. Conclusion

In this paper, I consider how property rights allocation alters firms' economic relationship with inventor-employees in innovation processes and affects knowledge-intensive firms' debt financing. The results collectively showcase an important contribution toward our understanding of how property rights affect financial frictions and firms' productivity and financial decisions.

I empirically investigate the effects of firms' increasing property rights to employee patents on debt financing. To mitigate endogeneity concerns, I exploit the Court of Appeals Federal Circuit ruling on invention assignment agreements that exogenously increased firms' property rights to inventor-employee patents. I find that the pro-*employer* interpretation of invention assignment agreements increases total debt by an average of $62 million. I further provide plausible evidence that firm ownership of patents improves patent pledgeability and innovation productivity and reduces holdup problems as shown by increases in asset complementarity and quality of patents.

Overall, this paper highlights the importance of the patent ownership structure under incomplete contracting from the firm's perspective. Recognizing that corporate inventor-employees are accountable for about 90% of all patentable inventions in the US (Pisegna-Cook 1994; Gruner 2006), whether firm ownership of employee patents is most efficient and optimal for the social level of innovation in the economy is an interesting question but beyond the scope of this paper. Detailed data on inventor-employee employment, moves, and wages would provide the opportunity to expand the current research to find implications of property rights on firms' investments in human capital to address more comprehensive impact of property rights allocation on innovation.

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Figure 1. Difference in Leverage Ratio by Years to Treatment

This figure presents the coefficient estimates from the regression equation below over the years to treatment. The horizontal axis indicates event time. The negative numbers are pre-treatment years; zero is year 2008 in which CAFC ruling was made; and positive numbers are post-treatment years. The vertical axis shows the coefficient estimates, β_k's from the baseline specification with firm and year fixed effects. The gray dots show statistically insignificant coefficients, whereas the yellow dots show statistically significant at 1%-level coefficients. The dotted lines are confidence intervals at 10%-level. All standard errors are clustered by state.

$$Total\ debt/Assets_{ist} = \alpha + \sum_k \beta_k\, treat_{is} \times event_k + \delta_i + \gamma_t + \epsilon_{ist}$$

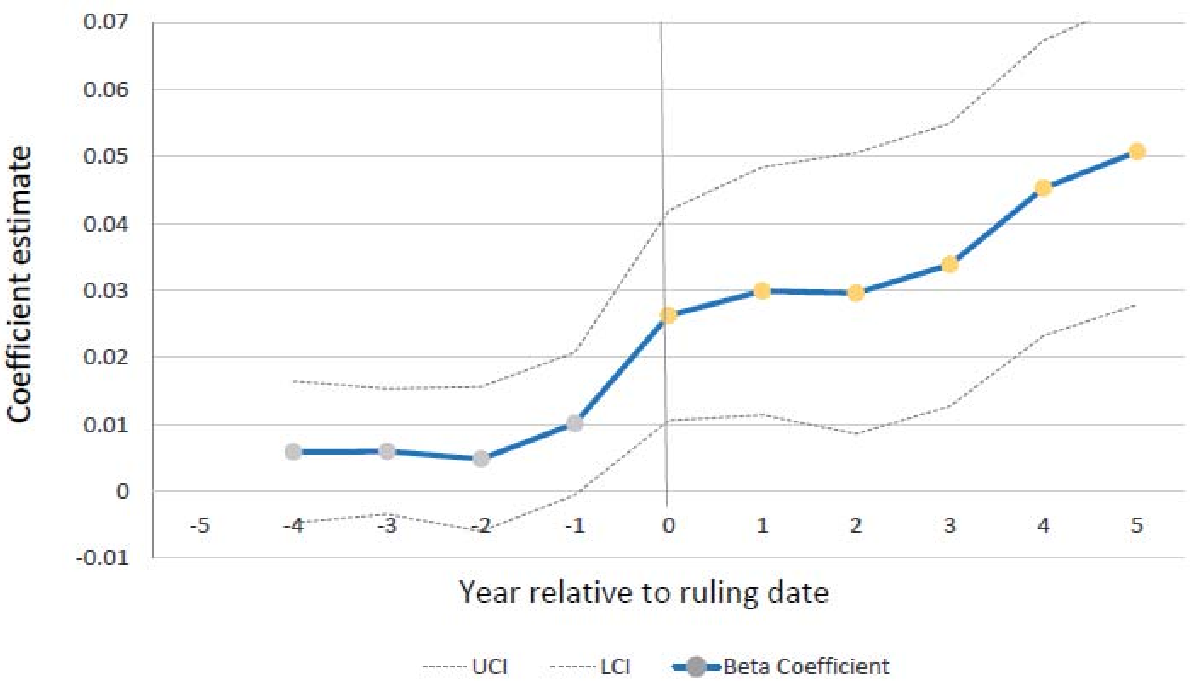


Table 1: Summary Statistics

This table reports summary statistics for firms in my sample, which comprises of actively patenting firms during the sample period of 2003-2013. I also exclude firms in financials and regulated industries. Panel A provides descriptive characteristics of all sample firms between 2003-2013. Panel B summarizes key variables used in the empirical analyses by treatment and control group firms during the pre-treatment period between 2003-2007. The last column shows p-value of difference in means. The outcome variable in the main regression is *Total debt/Assets*, which is winsorized between zero and one. For variable definitions and details of their construction, see Appendix D. Observations with missing asset are dropped.

Panel A: All Sample Firms Summary (2003-2013)

	Mean	Std. dev.	p25	p50	p75	N
Assets ($ mil)	5,046	26,667	70	333	1,850	16,540
Ln(Assets)	5.92	2.32	4.25	5.81	7.52	16,540
Total debt/Assets	0.18	0.22	0.00	0.11	0.28	16,540
R&D exp/Assets	0.12	0.19	0.01	0.06	0.15	16,540
Ppent/Assets	0.17	0.16	0.06	0.12	0.24	16,540

Panel B: Pre-treatment Comparisons (2003-2007)

	Treatment State*		Control State		
Variables	Mean	N	Mean	N	p-value
Assets ($ mil)	2,460	3,630	5,062	5,123	0.000
Ln(Assets)	5.48	3,630	5.88	5,123	0.000
Total Debt/Assets	0.14	3,630	0.20	5,123	0.000
LTD issuance	0.07	3,630	0.10	5,123	0.000
R&D exp/Assets	0.15	3,630	0.10	5,123	0.000
Ppent/Assets	0.14	3,630	0.20	5,123	0.000
Granted patents	18.26	3,630	16.74	5,123	0.551
Avg. First 3-yrs citations	2.43	2,413	1.98	2,988	0.000
Avg. citations per patent	0.84	3,630	0.71	5,123	0.000
Number of patent collateral	0.13	3630	0.12	3960	0.766
Number of inventors	2.78	3,017	2.70	3,960	0.027
Number of self-citation	38.40	3,475	43.52	4,818	0.558

*Treatment states include CA, DE, IL, KS, MN, NC, UT, and WA.

Table 2: Increasing Debt Financing

This table reports the results of estimating the main difference-in-difference regressions to examine how shifting property rights to patents from employees to firms affects firms' debt financing. The dependent variable in Panel A is *Total debt/Assets*. The dependent variable in Panel B is *LTD issuance*, which is computed as the long-term debt issuance scaled by total assets in the beginning of the year. The actual issuance of debt captures the increase in the *flow* of debt. The first columns show results from an OLS regression without fixed effects. The second columns include firm and year fixed effects, and the third columns use more stringent specification by including firm and industry-year fixed effects. Industry is 2-digit SIC. For variable definitions and details of their construction, see Appendix D. Standard errors are clustered by state in columns (2) and (3).

Panel A: Changes in Level

	Total debt/Assets (1)	Total debt/Assets (2)	Total debt/Assets (3)
treat×post	0.026***	0.026***	0.024**
	(0.005)	(0.009)	(0.009)
Firm FE	N	Y	Y
Year FE	N	Y	N
Industry-year FE	N	N	Y
Observations	16,540	16,540	16,540
R^2 (within)	0.003	0.007	0.004

Panel B: Changes in Flow

	LTD issuance (1)	LTD issuance (2)	LTD issuance (3)
treat×post	0.019***	0.018***	0.015**
	(0.006)	(0.006)	(0.007)
Firm FE	N	Y	Y
Year FE	N	Y	N
Industry-year FE	N	N	Y
Observations	16,540	16,540	16,540
R^2 (within)	0.001	0.004	0.012

Standard errors in parentheses
***p<0.01, **p<0.05, *p<0.1

Table 3: Falsification Test Using Non-patenting Firms

This table presents baseline results in Table 2 for *non-patenting* firms. The dependent variable in Panel A is *Total debt/Assets*. The dependent variable in Panel B is *LTD issuance*, which is computed as the long-term debt issuance scaled by total assets in the beginning of the year. The actual issuance of debt captures the increase in the *flow* of debt. The first columns show results from an OLS regression without fixed effects. The second columns include firm and year fixed effects, and the third columns use more stringent specification by including firm and industry-year fixed effects. Industry is 2-digit SIC. For variable definitions and details of their construction, see Appendix D. Standard errors are clustered by state in columns (2) and (3).

Panel A: Changes in level

	Total debt/Assets (1)	Total debt/Assets (2)	Total debt/Assets (3)
treat×post	-0.009	0.010	0.012
	(0.008)	(0.012)	(0.011)
Firm FE	N	Y	Y
Year FE	N	Y	N
Industry-year FE	N	N	Y
Observations	35,619	35,619	35,619
R^2 (within)	0.002	0.006	0.017

Panel B: Changes in new issuance

	LTD issuance (1)	LTD issuance (2)	LTD issuance (3)
treat×post	-0.001	-0.001	-0.005
	(0.006)	(0.005)	(0.007)
Firm FE	N	Y	Y
Year FE	N	Y	N
Industry-year FE	N	N	Y
Observations	35,619	35,619	35,619
R^2 (within)	0.001	0.003	0.006

Standard errors in parentheses
***p<0.01, **p<0.05, *p<0.1

Table 4: Increase in the Number of Pledgeable and Pledged Patents

The table reports the results of estimating the difference-in-difference regressions to examine the changes in the number of pledgeable patents and patents actually pledged as collateral. The dependent variable in columns (1) and (2) is logarithm of one plus the total number of granted patents. The dependent variable in columsn (3) and (4) is the number of granted patents scaled by R&D expenses. The number of granted patents is measured at time $t + 2$ to account for the processing time in the patent system. The dependent variable in columns (5) and (6) is the number of actually collateralized patents. The data comes from USPTO Patent Assignment files. The patents pledged as collateral is identified using assignment transactions marked as "security interest." Regressions in columns (5) and (6) control for the pre-treatment level of R&D spending and size of patent stock and their interaction terms with *Post*. The odd columns include firm and year fixed effects, and the even columns include firm and industry-year fixed effects. Industry is 2-digit SIC. For variable definitions and details of their construction, see Appendix D. All standard errors are clustered by state.

	Log(1+grant) (1)	Log(1+grant) (2)	Grant/R&D exp (3)	Grant/R&D exp (4)	Pledged (5)	Pledged (6)
treat×post	0.074**	0.074**	0.257***	0.280***	0.024*	0.021*
	(0.036)	(0.034)	(0.063)	(0.098)	(0.012)	(0.012)
Firm FE	Y	Y	Y	Y	Y	Y
Year FE	Y	N	Y	N	Y	N
Industry-year FE	N	Y	N	Y	N	Y
Control	–	–	–	–	R&D exp Patent stock	R&D exp Patent stock
Observations	11,893	11,893	10,584	10,584	16,540	16,540
R^2 (within)	0.060	0.076	0.001	-0.023	0.016	0.019

Standard errors in parentheses
***p<0.01, **p<0.05, *p<0.1

Table 5: Types of Debt

The table reports the results of estimating the difference-in-difference regressions to examine how shifting property rights to patents from employees to firms affects the use of different types of debt. The data on bank debt, convertible debt, and long-term debt are collected from Capital IQ. *Bank Debt/AT*, *Conv. Debt/AT*, *LTD/AT*, and *Short-term Debt* correspond to bank debt, convertible debt, long-term debt, and short-term debt (dlc), each scaled by total assets, respectively. *Mature in 1 yr* (dd1) and *Mature in 1 or 2 yrs* (dd2) are current portion of the long-term debt due in one or two years. The result on long-term debt using Compustat (dltt) instead is similar. For variable definitions and details of their construction, see Appendix D. All standard errors are clustered by state.

				Short-term debt		
	Bank Debt (1)	Conv. Debt (2)	Long-term Debt (3)	Short-term Debt (4)	Mature in 1 yr (5)	Mature in 1 or 2 yrs (6)
treat×post	0.006*	0.008**	0.024***	0.001	0.001	0.004
	(0.003)	(0.004)	(0.004)	(0.005)	(0.002)	(0.003)
Firm FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Observations	16,540	16,540	16,540	16,540	16,540	16,540
R^2 (within)	0.010	0.001	0.005	0.002	0.001	0.001

Standard errors in parentheses
***p<0.01, **p<0.05, *p<0.1

Table 6: Ex-ante Financial Constraints

The table reports the results of estimating the triple-difference regressions to examine the cross-sectional heterogeneity of ex-ante financial constriant by interacting *treat×post* by proxies for financial constraints. In each regression specification, firms are split at the pre-treatment median value of the proxies among the treated firms. *Small* is based on total asset, and *cash flow* and *cash level* are based on operating cash flows (oancf) and cash and short-term investments (che), repectively. The specification is as in column (1) of Table 2.

	Total debt/Assets (1)	Total debt/Assets (2)	Total debt/Assets (3)	Total debt/Assets (4)	Total debt/Assets (5)
treat×post×proxy	0.012	0.022*	0.016	0.034***	0.006
	(0.011)	(0.013)	(0.010)	(0.009)	(0.010)
treatXpost	0.018*	0.017***	0.020***	0.009	0.022***
	(0.010)	(0.006)	(0.006)	(0.007)	(0.009)
Constraint proxy	Small	Young	Low cash flow	Low cash level	No dividend
Observations	16,540	16,540	16,482	16,540	16,540
R^2 (within)	0.004	0.003	0.003	0.004	0.003

Standard errors in parentheses
***p<0.01, **p<0.05, *p<0.1

Table 7: Cross-sectional Heterogeneity by Ex-ante Holdup

The table reports the results of estimating the triple difference regressions to examine cross-sectional heterogeneity of ex-ante holdup proxied by the average number of inventors assigned on a patent and strength of state-level non-compete agreement enforcement. The dependent variable is total debt to assets ratio. The state-level Non-compete agreement enforcement index is referenced from Garmaise (2011). A higher index score is associated with stronger non-compete agreement enforcement. The odd columns include firm and year fixed effects, and the even columns include firm and industry-year fixed effects. Industry is 2-digit SIC. All standard errors are clustered by state.

	Avg. number of inventors		Non-compete agreement	
	Total debt/Assets (1)	Total debt/Assets (2)	Total debt/Assets (3)	Total debt/Assets (4)
treat×post×holdup	0.030**	0.037**	0.026	0.034**
	(0.014)	(0.015)	(0.016)	(0.015)
treatXpost	0.012	0.007	0.010	0.003
	(0.009)	(0.009)	(0.014)	(0.014)
Firm FE	Y	Y	Y	Y
Year FE	Y	N	Y	N
Industry-year FE	N	Y	N	Y
Observations	16,540	16,540	16,540	16,540
R^2 (within)	0.009	0.036	0.007	0.005

Standard errors in parentheses
***p<0.01, **p<0.05, *p<0.1

Table 8: Asset Complementarity and Inventor Collaboration

The table reports the results of estimating the difference-in-difference regressions to examine the asset complementarity and inventor collaboration as underlying channel of the main debt financing results. Columns (1) and (2) examine changes in the asset complementarity measured by self-citation, which counts the number of citations made on previous inventions patented by firms. Columns (3) and (4) examine inventor collaboration. The odd columns include firm and year fixed effects, and the even columns use more stringent specification including firm and industry-year fixed effects. Industry is 2-digit SIC. For variable definitions and details of their construction, see Appendix D. All standard errors are clustered by state.

	Log(1+self citation)		Avg. inventors	
	(1)	(2)	(3)	(4)
treat×post	0.117*	0.138**	0.127***	0.140***
	(0.069)	(0.066)	(0.035)	(0.032)
Firm FE	Y	Y	Y	Y
Year FE	Y	N	Y	N
Industry-year FE	N	Y	N	Y
Observations	15,489	15,489	13,095	13,095
R^2 (within)	0.003	0.001	0.006	0.030

Standard errors in parentheses
***p<0.01, **p<0.05, *p<0.1

Table 9: Improvement in Citations and Quality of Patents

The table reports the results of estimating the difference-in-difference regressions to examine the improvement in the quality of new and existing patents. In columns (1) and (2), I measure the average number of citations received *per* existing patent to compare citations received by the same portfolio of patents over time. In columns (3) and (4), I measure the average number of citations received *per* patent in the first 3-years after the grant-year to compare across patents of different age. In all specifications, I allow for differential trends by the average age of patents in pre-treatment patent portfolio. For the patent portfolio age control, I include the variables alone (absorbed by firm fixed effects) and their interaction term with the *post* dummy. The odd-numbered columns include firm and year fixed effects, and the even-numbered columns use more stringent specification including firm and industry-year fixed effects. Industry is 2-digit SIC. For variable definitions and further details of their construction, see Appendix D. All standard errors are clustered by state.

	Avg. citations		Avg. first 3-yr citations	
	(1)	(2)	(3)	(4)
treat×post	0.336**	0.434***	0.158*	0.205**
	(0.135)	(0.129)	(0.080)	(0.079)
Firm FE	Y	Y	Y	Y
Year FE	Y	N	Y	N
Industry-year FE	N	Y	N	Y
Application year FE	Y	Y	Y	Y
Controls	Portfolio Age	Portfolio Age	Portfolio Age	Portfolio Age
Observations	10,695	10,695	10,695	10,695
R^2 (within)	0.252	0.283	0.026	0.024

Standard errors in parentheses
***p<0.01, **p<0.05, *p<0.1

Table 10: Propensity Score Matching Diagnostics

This table presents pairwise comparisons of the variables on which the nearest neighbor matching (n=2) with replacement is performed. The summarized variable are mean values in the pre-treatment periods. Leverage growth is included to ensure the pre-treatment trend in the main outcome variable, total debt-to-assets ratio, is matched. Each of the last columns in Pre-Match and Post-Match are p-value of difference in means between Control and Treatment. The table shows that the post-matched variables are statistically indifferent from zero. For variable definitions and further details of their construction, see Appendix D.

	Pre-Match			Post-Match		
Variable	Control	Treatment	p-value	Control	Treatment	p-value
Leverage growth	10.988	5.378	0.478	1.874	5.403	0.182
LTD issuance growth	22.728	41.658	0.563	33.956	41.848	0.839
Size growth	0.323	0.490	0.188	0.433	0.362	0.369
Assets	4,808	2,527	0.066	2,733	2,533	0.707
R&D exp.	0.119	0.162	0.000	0.169	0.161	0.458
Ppent	0.181	0.133	0.000	0.137	0.133	0.581
Log(1+ grant)	1.247	1.522	0.000	1.514	1.514	0.996
Log(1+ application)	1.588	1.920	0.000	1.945	1.913	0.694
% successful application	0.696	0.709	0.383	0.701	0.708	0.608

Table 11: Increasing Debt Financing - Propensity Score Matching Regressions

The table reports the results of the difference-in-difference estimation using the propensity score matched sample to ensure the results reported in Table 2 are not driven by observable differences between the treated and control firms. The dependent variable is *Total debt/Assets*. Columns (1) and (2) uses nearest neighbor matching with n=1, and columns (3) and (4) uses nearest neighbor matching with n=2 with replacement. The odd-numbered columns include firm and year fixed effects, and the even-numbered columns use more stringent specification including firm and industry-year fixed effects. All standard errors are clustered by state.

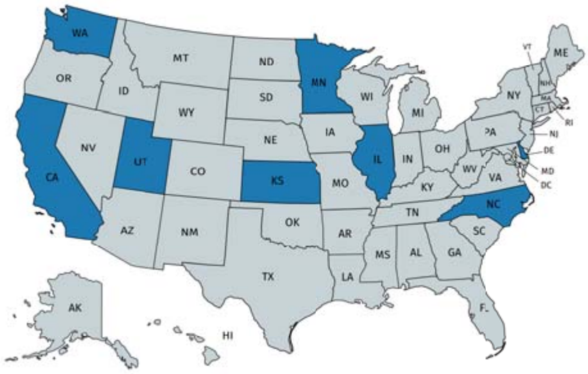
	NN=1		NN=2	
	Total debt/Assets (1)	LTD issuance (2)	Total debt/Assets (3)	LTD issuance (4)
treat×post	0.024** (0.011)	0.015* (0.009)	0.021** (0.010)	0.016** (0.007)
Firm FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Observations	9,340	9,340	10,798	10,798
R^2	0.582	0.232	0.588	0.236

Standard errors in parentheses
***p<0.01, **p<0.05, *p<0.1

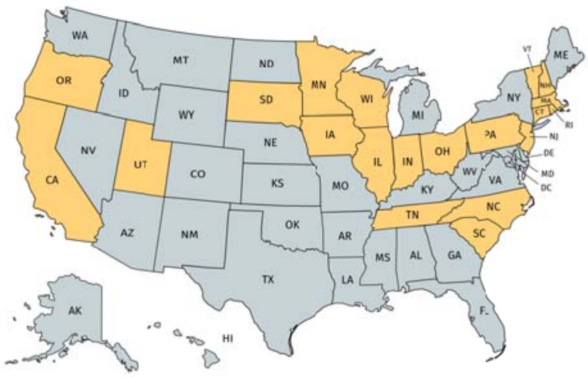
Appendix A. Robustness

Figure A1. Geographic Distribution of Treated States and Patent-intensive States

The figures present the distribution of the treated states in Figure (a) and states in which the fraction of employment from patent-intensive industry is above the national average (USPTO Intellectual Property and the U.S. Economy Report, 2012) as of 2010 in Figure (b). The comparison shows that the results are not driven only by the firms with relatively greater innovation investment opportunities sorting into the treated states. If the results were driven only by such firms sorting into high-innovation states, then the changes in the outcome variables for treated firms relative to control firms would not have been as profound, as control states also include many high-innovation states.



(a) Treated States



(b) IP-intensive Employment States

Figure A2. State-level Aggregate Innovation

The table presents state-level aggregate innovation output by treated and control states. I use all granted patents in USPTO Patent Grant data that are assigned to entities in the US, which subsumes the Compustat sample firms used in the main regression analyses. The horizontal axis indicates time, where the vertical line is drawn on year 2008 when CAFC ruling was given. The vertical axis shows the 2-year lead log number of patent grants to account for the time it takes for firms' underlying innovation changes to take effects. There seems to be a parallel trend in the log number of patent grants between the treated and control states prior to 2008. However, subsequent to the CAFC ruling, whereas the control state patent grant levels off, treated states show larger growth in patent grants. The graph corresponds to regression result in column (1) of Table A8. For variable definitions and further details of their construction, see Appendix D.

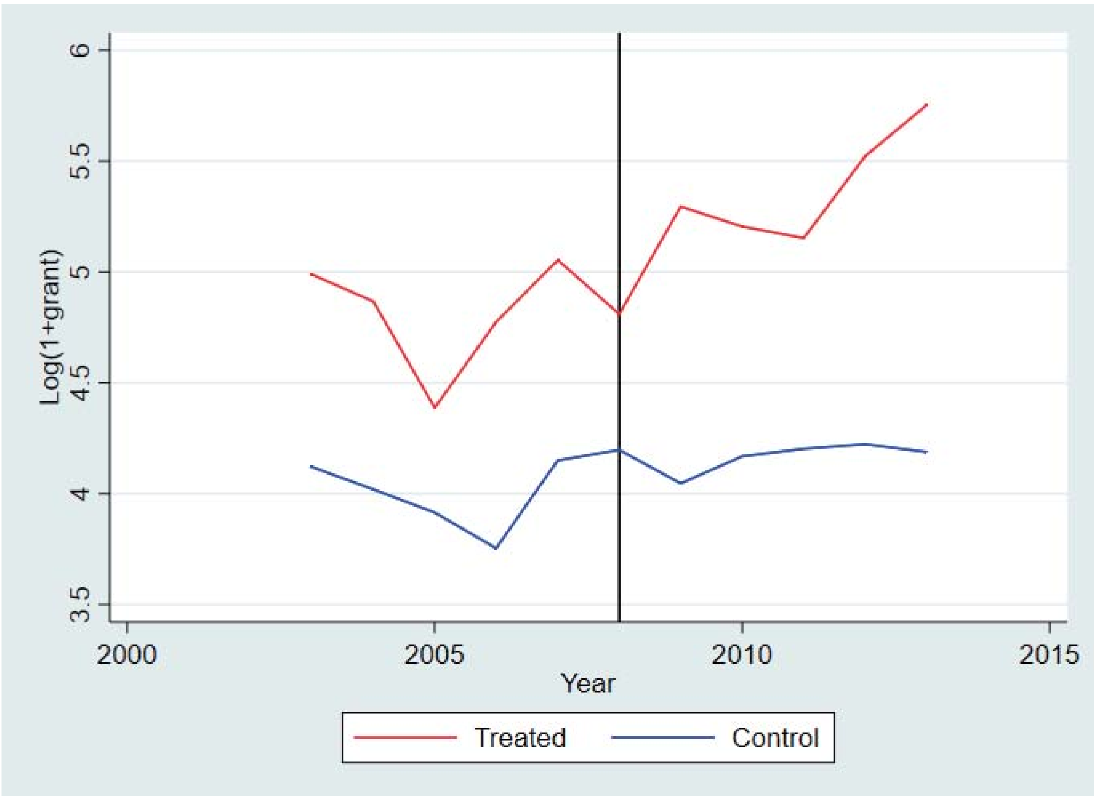


Table A1: Subsample with Single Subsidiary Location

The table reports the main regression results for subsample of firms operating in single or relatively limited number of states. The dependent variable is *Total debt/Assets*. The geographic subsidiary data is from Dyreng, Lindsey, and Thornock (2013). The data provides the count of geographic subsidiaries and the corresponding states. Column (1) reports the result for firms with single operating location in the headquarter state. Columns (2) and (3) report the results for firms with one or two geographic subsidiaries that may be located outside the headquarter state, but certainly limited in geographic presence of the firm. All specifications include firm and year fixed effects. All standard errors are clustered by state.

	Dependent variable: Total debt/Assets		
	Zero sub (1)	One sub (2)	Two subs (3)
treat×post	0.036*	0.033*	0.022*
	(0.019)	(0.018)	(0.012)
Firm FE	Y	Y	Y
Year FE	Y	Y	Y
Observations	3,084	6,001	8,799
R^2 (within)	0.005	0.010	0.004

Standard errors in parentheses
***p<0.01, **p<0.05, *p<0.1

Table A2: Baseline Regression by Treatment States

The table presents baseline results by treatment states. The dependent variable is *Total debt/Assets*. Column(1) is the baseline result from Table 2 column (1). Each of columns (2)-(9) runs the baseline regression using one treatment state against the rest of control states. Notice that the number of observations changes accordingly. For variable definitions and further details of their construction, see Appendix D. All standard errors are clustered by state.

	Dependent variable: Total debt/Assets				
	Baseline	CA	DE	IL	NC
	(1)	(2)	(3)	(4)	(5)
treat×post	0.026***	0.032***	0.002	0.028***	0.003
	(0.009)	(0.007)	(0.007)	(0.007)	(0.007)
Firm FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
Observations	16,540	14,167	9,781	10,519	10,062
R^2 (within)	0.007	0.007	0.002	0.003	0.003

	Dependent variable: Total debt/Assets			
	KS	MN	UT	WA
	(6)	(7)	(8)	(9)
treat×post	0.069***	0.013*	0.033***	-0.024***
	(0.007)	(0.007)	(0.007)	(0.007)
Firm FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Observations	9,796	10,297	9,870	10,130
R^2 (within)	0.003	0.003	0.003	0.003

Standard errors in parentheses
***p<0.01, **p<0.05, *p<0.1

Table A3: Baseline Regression with Controlling for Creditor Rights Effect

The table presents baseline regressions (Table 2) including controls for court rulings identified in Mann (2018) that strengthened creditor rights over the similar period. This table shows dependent variable as *Total debt/Assets*, but the results are robust using long-term debt issuance. Only the last three creditor rights court cases overlap with the sample period. *DE* is an indicator for being incorporated in Delaware, and *After CR Decision* is an indicator for being after the relevant creditor rights court decision year. Columns (1) through (3) include the control for the second case; columns (4) through (6) control for the third case; and columns (7) through (9) controls for the last case. Columns (1), (4), and (7) do not include any fixed effects; columns (2), (5), and (8) include firm and year fixed effects; and columns (3), (6), and (9) include firm and industry-year fixed effects. All standard errors are clustered by state.

	Total debt/Assets								
	Decision 2 (2003)			Decision 3 (2007)			Decision 4 (2009)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
treat×post	0.026***	0.025***	0.024**	0.025***	0.025***	0.024**	0.026***	0.026***	0.025**
	(0.005)	(0.009)	(0.009)	(0.005)	(0.009)	(0.010)	(0.005)	(0.009)	(0.010)
treat	-0.054***			-0.054***			-0.055***		
	(0.009)			(0.009)			(0.009)		
post	-0.003			-0.026***			-0.005		
	(0.003)			(0.004)			(0.004)		
DE	0.010			0.011			0.014		
	(0.011)			(0.009)			(0.009)		
DE×After CR Decision	0.005	0.006	0.003	0.006	0.006	0.003	-0.003	-0.003	-0.009
	(0.008)	(0.009)	(0.010)	(0.005)	(0.009)	(0.010)	(0.005)	(0.009)	(0.009)
After CR Decision	-0.006			0.023***			0.004		
	(0.007)			(0.005)			(0.006)		
Firm FE	N	Y	Y	N	Y	Y	N	Y	Y
Year FE	N	Y	N	N	Y	N	N	Y	N
Industry-year FE	N	N	Y	N	N	Y	N	N	Y
Observations	16,540	16,540	16,540	16,540	16,540	16,540	16,540	16,540	16,540
R^2 (within)	0.005	0.007	0.004	0.006	0.007	0.004	0.005	0.007	0.004

Standard errors in parentheses
***p<0.01, **p<0.05, *p<0.1

Table A4: Baseline Regression with Control Variables

The table presents baseline results with control variables. The dependent variable is *Total debt/Assets*. Each control variable dummy is equal to one if the pre-treatment average is greater than median, otherwise zero. The control variables are included as dummy variables interacted with *post* indicator. The stand-alone control variables are also included but absorbed by the firm fixed effects. Only the interaction terms are reported below. Each control variable is static and computed from the pre-treatment period median. For variable definitions and further details of their construction, see Appendix D. All standard errors are clustered by state.

	Dependent variable: Total debt/Assets								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
treat×post	0.025***	0.028***	0.026***	0.025***	0.026***	0.026**	0.025***	0.024**	0.027***
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.010)	(0.009)	(0.009)	(0.009)
Size	-0.011								-0.007
	(0.011)								(0.010)
Age		0.004							0.008
		(0.008)							(0.009)
State-tax			0.008						0.007
			(0.010)						(0.011)
Tangibility				-0.004					-0.004
				(0.008)					(0.009)
Market-to-book					0.010				0.012
					(0.008)				(0.008)
Profitability						-0.008			-0.005
						(0.009)			(0.009)
Pre-patent stock							-0.000		0.000
							(0.010)		(0.000)
R&D expenditure								-0.000	-0.009
								(0.010)	(0.008)
Firm FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	16,540	15,027	16,540	16,540	16,540	16,540	16,540	16,540	15,027
R^2 (within)	0.007	0.007	0.007	0.007	0.007	0.007	0.007	0.007	0.007

Standard errors in parentheses
***p<0.01, **p<0.05, *p<0.1

Table A5: Immediate and Lagged Changes

The table reports progression of changes over time in patent pledgeability and asset complementarity. For simplicity, I only report coefficients of two years prior to the treatment year in 2008, $t+0$, and four years following the treatment to show some lagged effects in columns (2) and (3). The dependent variable in Column (1) is the a total number of self citations as a percentage of all citations made by new applications each year. Columns (2) and (3) show lagged effects. The dependent variable in column (2) is the average number of inventors assigned per patent. The dependent variable in column (3) is the log of number of granted patents. For variable definitions and further details of their construction, see Appendix D. All standard errors are clustered by state.

	Immediate	Lagged	
	Log(1+self citation) (1)	$Log(1+grant)_{t+2}$ (2)	Avg. inventors (3)
treat $\times$ $t-2$	0.021	0.057	0.015
	(0.046)	(0.058)	(0.080)
treat $\times$ $t-1$	0.175**	0.104	0.112
	(0.066)	(0.074)	(0.070)
treat $\times$ $t+0$	0.136*	0.142**	0.012
	(0.077)	(0.065)	(0.075)
treat $\times$ $t+1$	0.137*	0.134**	0.148
	(0.073)	(0.056)	(0.102)
treat $\times$ $t+2$	0.127*	0.188**	0.188***
	(0.074)	(0.070)	(0.056)
treat $\times$ $t+3$	0.219*	0.212**	0.129
	(0.110)	(0.081)	(0.084)
treat $\times$ $t+4$	0.223	0.205**	0.345**
	(0.141)	(0.077)	(0.128)
Firm FE	Y	Y	Y
Year FE	Y	Y	Y
Application year FE	N	N	Y
Observations	15,489	9,737	13,095
R^2 (within)	0.004	0.055	0.006

Standard errors in parentheses
***p<0.01, **p<0.05, *p<0.1

Table A6: R&D Expenses

The table reports the results on increasing R&D expenses associated with the increase in debt financing due to the treatment using triple-difference regression specifications used in Tables 6 and 7. The dependent variable is logarithm of R&D expense. Column (1) estimates difference-in-difference regression. Columns (2) through (6) interact treat×post with each proxies of ex-ante financial constraint and holdup used in Tables 6 and 7.

		Financial constraint			Holdup	
	R&D Exp (1)	R&D Exp (2)	R&D Exp (3)	R&D Exp (4)	R&D Exp (5)	R&D Exp (6)
treatXpostXproxy		-0.003	0.093^{**}	0.123^{***}	-0.037	0.072^{*}
		(0.048)	(0.042)	(0.045)	(0.041)	(0.043)
treatXpost	0.068^{***}	0.072^{*}	0.014	-0.019	0.084^{***}	0.019
	(0.021)	(0.042)	(0.027)	(0.038)	(0.028)	(0.034)
Proxy	–	Small	Young	No dividend	Holdup	NCA
Observations	16,540	16,540	16,540	16,540	16,540	16,540
R^2 (within)	0.025	0.025	0.029	0.026	0.026	0.026

Standard errors in parentheses
$^{***}p<0.01$, $^{**}p<0.05$, $^{*}p<0.1$

Table A7: State-level Increase in Wages of Occupations Associated with Innovation

The table presents the increase in wages of occupation categories associated with research and development around the treatment. The occupation-level wages by state are obtained from the Bureau of Labor Statistics Occupational Employment Statistics. The coarse classification include wages from broad occupation profiles related to R&D and innovation, e.g. computer and mathematical occupations (15-0000), life, physical, and social science occupations (19-0000). The fine classification uses sub-occupation profiles with Standard Occupational Classification definitions specifically mentioning invention and research and development, e.g. computer and information scientists, Research (15-1011), computer software engineers, systems software (15-1032), medical scientists (19-1042). The entire occupational classification and definitions are available on the BLS website. The dependent variables are mean annual wage in the odd columns and median annual wages in the even columns. All regression specifications include year, state, and occupation-level fixed effects.

	Coarse classification		Fine classification	
	Mean wage (1)	Median wage (2)	Mean wage (3)	Median wage (4)
treat×post	280.519**	390.294***	1164.967**	1215.255**
	(109.133)	(116.472)	(582.213)	(577.572)
State FE	Y	Y	Y	Y
Occupation FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Observations	118,234	118,234	9,778	9,778
R^2 (within)	0.252	0.283	0.026	0.024

Standard errors in parentheses
***p<0.01, **p<0.05, *p<0.1

Table A8: State-level Aggregate Innovation

The table presents state-level aggregate innovation output. The dependent variable is 2-year lead log number of patent grants to account for the time it takes for firms' underlying innovation changes to take effects. I use all granted patents in USPTO Patent Grant data that are assigned to entities in the US with role code 2 (US company or corporation), 4 (US individual), 6 (US Federal government), 8 (US county government), and 9 (US state government). Columns (1) and (2) use all observations. Column (1) include only state and year fixed effects. Column (2) additionally includes group fixed effects (group equals to zero for patents granted to individuals and one otherwise). Columns (3) and (4) use subset of non-individual patents granted to US company or corporation (role code=2) and all US government (role code=6, 8 and 9), which likely have employer-employee tension in ownership. Column (3) includes state and year fixed effects, and column (4) additionally include role fixed effects. Column (5) uses only patents granted to individuals (role code=4) and includes state and year fixed effects. For variable definitions and further details of their construction, see Appendix D. All standard errors are clustered by state.

	Dependent variable: $Log(1+grant)_{t+2}$				
	All		Firms and Government		Individuals
	(1)	(2)	(3)	(4)	(5)
treat×post	0.303**	0.394**	0.644***	0.200*	0.101
	(0.147)	(0.148)	(0.195)	(0.112)	(0.108)
State FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
Group FE	N	Y	N	N	N
Role FE	N	N	N	Y	N
Observations	1,115	1,115	613	613	502
R^2 (within)	-0.005	0.660	0.012	0.731	0.067

Standard errors in parentheses
***p<0.01, **p<0.05, *p<0.1

Table A9: Pre-treatment State Economic Conditions

The table reports difference in means of state-level economic variable growth rates between treated states and control states by year. For simplicity, I report two years before and after the 2008 CAFC court ruling, but the differences in means are statistically insignificant for all sample years. The second and third columns report means of corresponding economic variable, and the last column reports p-values on the difference in means. The objective of this table is to show that the difference-in-difference results are not driven by differential trends in state-level economics variables. The differences in state-level economic variables are small and are not statistically different from zero throughout my sample period.

	Mean Comparison		
Year	Treated States	Untreated States	p-value
GDP growth, percent			
2006	5.313	4.742	0.576
2007	1.450	2.969	0.331
2008	-1.487	−2.013	0.671
2009	2.862	4.273	0.107
2010	4.275	4.100	0.879
GDP per capita growth, percent			
2006	2.425	1.691	0.384
2007	0.988	0.498	0.558
2008	-1.613	−0.495	0.316
2009	-3.650	−2.965	0.534
2010	0.400	1.477	0.183
Unemployment rate growth, percent			
2006	-0.122	−0.083	0.188
2007	0.013	−0.016	0.332
2008	0.284	0.241	0.517
2009	0.637	0.569	0.306
2010	0.023	0.016	0.783
State corporate tax rate growth, percent			
2006	0	−0.004	0.669
2007	0	−0.002	0.671
2008	0	0.034	0.706
2009	-0.005	0.002	0.555
2010	0	−0.002	0.613

Table A10: Increasing Debt Financing - State Robustness

The table reports the results of estimating the main difference-in-difference regressions with state or state-industry fixed effects. The dependent variable in columns (1) and (2) is *Total debt/Assets*. The dependent variable in columns (3) and (4) is *LTD issuance*. The odd-numbered columns include state and industry-year fixed effects, and the even-numbered columns use more stringent specification additionally including year and state-industry fixed effects. Industry is 2-digit SIC. For variable definitions and further details of their construction, see Appendix D. All standard errors are clustered by state.

	(1) Total debt/Assets	(2) Total debt/Assets	(3) LTD issuance	(4) LTD issuance
treat×post	0.023**	0.027***	0.018***	0.020***
	(0.009)	(0.008)	(0.006)	(0.006)
Year FE	N	Y	N	Y
State FE	Y	N	Y	N
Industry-year FE	Y	N	Y	N
State-industry FE	N	Y	N	Y
Observations	16,540	16,540	16,540	16,540
R^2 (within)	0.098	0.235	0.036	0.116

Standard errors in parentheses
***p<0.01, **p<0.05, *p<0.1

Table A11: Increase in Pledgeability of Patents - State Robustness

The table reports the results of underlying patent pledgeability channel with state-industry fixed effects. In columns (1) and (2), the dependent variable is 2-year lead log number of patent grants to account for the time it takes for firms' underlying innovation changes to take effects. In columns (3) and (4), the dependent variable is the 2-year lead number of granted patents scaled by current R&D expense to measure the changes in underlying innovation productivity. In columns (5) and (6), the dependent variable is logarithm of one plus the total number of patents pledged as collateral. The data comes from USPTO Patent Assignment files. The patents pledged as collateral is identified using assignment transactions marked as "security interest." Columns (5) and (6) additionally control for the pre-treatment level of R&D spending and size of patent stock and their interaction with *Post*. The odd-numbered columns include state and industry-year fixed effects, and the even-numbered columns use more stringent specification including year and state-industry fixed effects. Industry is 2-digit SIC. For variable definitions and details of their construction, see Appendix D. All standard errors are clustered by state.

	$Log(1+grant)_{t+2}$		$Grant_{t+2}$/R&D Exp		Log(1+pledged patents)	
	(1)	(2)	(3)	(4)	(5)	(6)
treat×post	0.085*	0.103**	0.293**	0.295***	0.023*	0.024*
	(0.044)	(0.043)	(0.120)	(0.085)	(0.013)	(0.012)
Year FE	N	Y	N	Y	N	Y
State FE	Y	N	Y	N	Y	N
Industry-year FE	Y	N	Y	N	Y	N
State-industry FE	N	Y	N	Y	N	Y
Control	–	–	–	–	Y	Y
Observations	11,893	11,893	10,584	10,584	16,540	16,540
R^2 (within)	0.091	0.233	-0.026	-0.015	0.101	0.137

Standard errors in parentheses
***p<0.01, **p<0.05, *p<0.1

Table A12: Citations on New and Old Patents - State Robustness

The table reports the results of improvement in quality of new and existing patents with state-industry fixed effects. In columns (1) and (2), I measure the average citations received *per* patent in the first 3-years after the grant year to compare across patents of different age. In columns (3) and (4), I measure the average citations received *per* existing patent to compare citations received on the same portfolio of patents over time. In all specifications, I allow for differential trends by average age of pre-treatment patent portfolio. For the patent portfolio age control, I include the variables alone and their interaction term with the *post* dummy. Industry is 2-digit SIC. For variable definitions and further details of their construction, see Appendix D. All standard errors are clustered by state.

	Avg. citations		First 3-yr citations	
	(1)	(2)	(3)	(4)
treat×post	0.590***	0.527***	0.175	0.136
	(0.197)	(0.186)	(0.109)	(0.085)
Year FE	N	Y	N	Y
State FE	Y	N	Y	N
Industry-year FE	Y	N	Y	N
State-industry FE	N	Y	N	Y
Application year FE	Y	Y	Y	Y
Controls	Portfolio Age	Portfolio Age	Portfolio Age	Portfolio Age
Observations	10,695	10,695	10,695	10,695
R^2 (within)	0.203	0.215	0.066	0.090

Standard errors in parentheses
***p<0.01, **p<0.05, *p<0.1

Table A13: Asset Complementarity and Inventor Collaboration - State Robustness

The table reports the results of asset complementarity and inventor collaboration channel with state or state-industry fixed effects. Columns (1) and (2) examine changes in the asset complementarity measured by self-citation, which counts the number of citations made on previous inventions patented by firms. Columns (3) and (4) examine inventor collaboration. Columns (1) and (3) include state and industry-year fixed effects, and columns (2) and (4) include year and state-industry fixed effects. Industry is 2-digit SIC. For variable definitions and details of their construction, see Appendix D. All standard errors are clustered by state.

	Log(1+self citation)		Avg. inventors	
	(1)	(2)	(3)	(4)
treat×post	0.194**	0.163*	0.078**	0.081***
	(0.075)	(0.096)	(0.036)	(0.029)
Year FE	N	Y	N	Y
State FE	Y	N	Y	N
Industry-year FE	Y	N	Y	N
State-industry FE	N	Y	N	Y
Observations	15,489	15,489	13,095	13,095
R^2 (within)	0.052	0.639	0.080	0.122

Standard errors in parentheses
***p<0.01, **p<0.05, *p<0.1

Appendix B. USPTO Patent Data Collection

US patent data is important part of the empirical analyses because I use the firm-level patent characteristics, such as percentage of successful applications and patent citations, to provide evidence for the increasing patent pledgeability mechanism. In this section, I briefly describe the data collection process, and how I validate the data across different publicly available patent data sources.

USPTO publicly provides a bulk data download through Reed Tech. The database keeps patent application and patent grant data separately, and USPTO releases the data weekly in XML data format. I first download these weekly files and parse each XML files to obtain relevant information. The key information contained in each document is the assignee names, assignee state, assignee country, and assignee role code. For each set of patent application and patent grant data, I keep only documents with role code "02," which represents US corporation assignee. Then using assignee state and country, I limit my sample to only ones that are issued to US domicile US corporations. Next, I name-match the assignees to my patenting sample firms from Compustat. I use multiple ways for name-matching, and also verify with spot-checking with eyes given the number of sample firms is not too large.

Additionally, I performed validation of my data using existing sources. First, going through the universe of US patents allows me to verify the claim that about 80-90% of patentable inventions are created by the employee inventors (Cherensky 1993; Pisegna-Cook 1994; Gruner 2006). Consistent with these papers, using the role code categories, I verify that the composition of patent assignees in my data is also mainly composed of corporations, followed by individual and government. Second, I cross-checked with KPSS data (Kogan, Papanikolaou, Seru, and Stoffman (2017)). KPSS is an excellent data source for patents. However, there are few limitations with KPSS. One is that KPSS covers US patent data from 1926 to November of 2010, whereas my sample period is over 2003-2013. The other is that to measure the success rate of patent applications in the application year, I need to be able to verify whether the applied patents are eventually granted. This requires for me to see grant data unto 2016, given it takes on average about two to three years for a patent to go through the grant process. By using USPTO data, I can extend the patent data unto 2016 for computation purpose, and also use KPSS to cross-check my patent grant and citations data for the overlapping period between 2003-2009. I first verified raw number of grant documents parsed in each of KPSS and my dataset. For the period between 2003-2009, the counts match about 99.9% for most of the years. In 2010, KPSS data stops in November 2nd, 2010. The count gap between KPSS and my data is about 15,000 granted patents, which is plausible given the average number of patents granted each month. Lastly, I also validated that the number of citations on overlapping firms during the common time period is almost identical.

Appendix C. Statutory Laws and Firm Headquarters

This section provides a few examples of cases to verify that pre-invention assignment agreements are governed by state law where a firm's headquarter is located. In the empirical analysis, the a sample firm's headquarter state is used to define the treatment indicator.

1. **DDB Technologies, LLC v. MLB Advanced Media**
    - Case no. 04-CV-352, 2006.
    - The initial case was heard in Western District of Texas.
    - The involved inventions by David Barstow assigned to Schlumberger Technology Corporation, whose headquarter is located in Texas.
2. **Evan Brown v. Alcatel USA, Inc (F/N/A DSC Communications Corporation)**
    - Case no. 05-02-01678-CV, 2004.
    - The case was heard in 199th Judicial District Court. Collin County, Texas.
    - DSC Communications was a Texas-based phone equipment maker.
3. **Banks v. Unisys Corporation and Burroughs Corporation**
    - Case no. 228 F.3d 1357, 2000.
    - The case was initially heard in District Court for the Eastern District of Michigan.
    - Gerald Banks and Kelly Banks were employed with Burroughs Corporation, now wholly-owned by Unisys Corporation.
    - Burroughs Corporation headquarter is located in Michigan.

Appendix D. Variable Description

- **Assets** = Total assets. Observations with missing assets are dropped.
- **Total debt/Assets** = (Long-term debt (dltt) + Short-term debt (dlc))/ Total Assets. The missing observations were replaced with zero, then the ratio is winsorized between zero and one following Lemmon, Roberts, and Zender (2008).
- **LTD issuance** = Long-term debt issuance (dltis)/($Total\ assets_{t-1}$). The missing observations were replaced with zero, then the ratio is winsorized between zero and one following Lemmon et al. (2008).
- **R&D exp/Assets** = R&D expenditure (xrd) scaled by total assets.
- **Ppent/Assets** = Plant, property and equipment (ppent) scaled by total assets.
- **Bank and Convertible Debt**= The data is obtained from Capital IQ Capital Structure. The missing bank debt and convertible debt observations are replaced with zero, then the ratio is winsoraized between zero and one.
- **Firm age**= Firm age is counted since date of incorporation obtained from Datastream.
- $Log(1 + grant)_{t+2}$= Logarithm of one plus the number of granted patents in year $t+2$. The lead grant number is used to account for the average of two-years it takes for the patent grant process.
- $Grant_{t+2}$**/R&D Exp**= The number of granted patents in year $t+2$ scaled by the total R&D expenditure in year t. The lead grant number is used to account for the average of two-years it takes for the patent grant process.
- **Avg. citations**= Average annual number of citations received per existing patents granted prior to treatment year.
- **Avg. First 3-yr citations**= Average number of total citations received during the first 3-years post-grant per patent.
- **Portfolio Age**= Average age of all existing patents in firm's patent portfolio in the year prior to the CAFC ruling.
- **Log(1+number of patent collateral)**= Logarithm of one plus the total number of patents pledged as collateral, measured from the USPTO Patent Assignment data classified as security interests.
- **Patent Stock**= The total number of all existing patents in firm's patent portfolio in the year prior to the CAFC ruling.
- **Log(1+self citation)**= Logarithm of one plus the total number of citations on a firm's own patents granted in the last 10 years by new applications. The median backward citation lag is around 10 years (Hall et al. (2005)). I limit the age of pool of cited patents to ten years so that the relationship is not merely picking up the size of existing pool of patents, particularly for older firms.
- **Avg. inventors**= The average number of inventors listed on a given patent document.
- **Leverage growth** = Average of annual growth of debt-to-assets ratio.
- **Size growth** = Average of annual growth of total assets.