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BY

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I dedicate this work to my wonderful wife and greatest inspiration, Camelia; without her unwavering love and encouragement this would not have been possible.

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Abstract

This dissertation is a collection of three essays that investigate the value implications of product market competition, the impact of trade secrets protection on capital structure decision-making, and the long-term value relevancy of the right to adopt a poison pill.

Chapter 1 explores the value impact of product market competition (PMC) on long-term firm value. Using exogenous state adoption of anti-plug-mold statutes, and their subsequent invalidation, I causally show that decreased PMC generates economically and statistically significant long-term firm value, especially for firms with greater innovative ability. My findings are robust to different specifications, including matching and portfolio analysis, and provide support for the view that a reduction in PMC leads to higher firm value by increasing investments in new and existing production technologies.

Chapter 2 isolates the causal effect of an increase in trade secrets protection on financial leverage by examining the staggered implementation of state-level trade secrets laws – the Uniform Trade Secrets Act. First, we show the adoption of these statutes is plausibly exogenous to corporate debt policies. Next, we document that large firms located in states that have passed these laws increase their debt ratios relative to firms in states without such legislation. We also find that better protected large firms experience a reduction in operating leverage, probability of default, and cash flow volatility. Further, our evidence suggests that companies with higher likelihoods of default adjust their debt levels upward after their headquartering state adopts a statute. Overall, the results are consistent with stronger trade secrets protection leading to increases in financial

leverage via decreasing bankruptcy costs. Lastly, we show large firms benefit from this trade-off and experience positive long-term value effects.

Chapter 3 analyzes the value impact of *the right to* adopt a poison pill – or "shadow pill" – on long-term firm value, exploiting the natural experiment provided by the staggered adoption of poison pill laws that validated the use of the pill in 35 U.S. states over the period 1986 to 2009. We document that the availability of a shadow pill results in an economically and statistically significant increase in firm value, especially for firms more engaged in innovation or with stronger stakeholder relationships. Our findings are robust to different specifications, including matching and portfolio analysis, and provide support to the bonding hypothesis of takeover defenses.

Chapter 1: Product Market Competition and Long-Term Firm Value: Evidence from Reverse Engineering Protections

1. Introduction

Empirical measures that proxy for product market concentration have been shown to be positively correlated with firm value (Lindenberg and Ross, 1981; Blundell, Griffith, and Van Reenen, 1999; Giroud and Mueller, 2011; Grullon, Larkin, and Michaely, 2017). However, industry concentration is endogenously determined with corporate valuation making it difficult to establish causality from reduced form correlations. As a result, the extant empirical literature's attempt to investigate whether product market competition is beneficial or detrimental to shareholder interests is difficult to interpret.

Further obscurity is created by the long-standing theoretical literature, which establishes conflicting effects of inhibiting product market competition on shareholder value. On the one hand, shielding incumbent firms from the disciplinary pressure of competition could decrease their incentives to innovate (Arrow, 1962; Loury, 1979; Dasgupta and Stiglitz, 1980; Aghion et al., 2001; and Repullo, 2004), consequently, enabling self-interested managers to slack (Hart, 1983)[1] and thereby enjoy the "quiet life" (Hicks, 1935) at the detriment of shareholder value. On the other hand, safeguarding corporations from the intensity of competition could raise firm value by increasing both the flow and expected duration of economic rents that incentivize

[1] Scharfstein (1988), Rotemberg and Scharfstein (1990), Hermalin (1992), Schmidt (1997), and Raith (2003) suggest the relationship between competition and slack is not so straight forward.

innovation (Gilbert and Newberry, 1982; Romer, 1990; Grossman and Helpman, 1991; Aghion and Howitt, 1992; Davidson and Segerstrom, 1998; and Zeng, 2001).

In this paper, I contribute to the literature on the relationship between product market concentration and firm value by shifting the focus from endogenous proxy variables to a tandem of unique exogenous events that directly influence the intensity of competition in product markets. In particular, I investigate the value implications of state-level anti-plug-mold (APM) laws that were enacted in 12 U.S. states over the period 1978 to 1987, and subsequently reversed by a U.S. Supreme Court ruling in 1989. The APM laws prohibited a certain type of reverse engineering[2], substantially weakening the intensity of product market competition by inhibiting competitors' ability to copy innovative manufacturing products. The present paper, as far as I know, is the first to consider this quasi-natural experiment to study the effect of reduced product market competition – brought about by increased reverse engineering protections – on long-term firm value, as proxied by both Tobin's Q and stock returns.

My main finding is that the passage of APM laws results in a statistically and economically significant increase in the Tobin's Q of the affected firms. This finding is robust to various methodologies, including the incorporation of possible selection effects through the creation of a matched sample, and a long-run stock portfolio return approach. I also find that the increase in Tobin's Q is more pronounced for firms with higher levels of innovative ability.

Overall, my results support what I term the "innovation incentives" hypothesis, which posits that a reduction in the intensity of competition in product markets through

[2] The standard legal definition of reverse engineering was established in Kewanee Oil Co. v. Bicron Corp., where the U.S. Supreme Court described the process as "starting with the known product and working backward to divine the process which aided in its development or manufacture."

the prohibition of quick and relatively costless reverse engineering practices, increases the flow and duration of economic rents that incentivize investments in new and existing production technologies and, in the long-term, increases shareholder value (Aghion and Howitt, 1992; Davidson and Segerstrom, 1998).

I begin the analysis by addressing the primary concern that specific state-level conditions can explain a state's propensity to pass an APM law, by investigating the likelihood that the enactment of these laws followed from state-level institutional and economic characteristics. Using a linear probability model with state and year fixed effects, I find no significant predictors for the adoption of AMP laws. This suggests that these laws' adoption was plausibly exogenous to the then-prevailing market and economic environments, consistent with my central identification assumption. Moreover, I consider the concern of a possible anticipation effect of the U.S. Supreme Court ruling by examining the market's reaction to the Court's decision in a short-term stock event study. I find that for firms located in APM adopting states, capital markets responded significantly and negatively to the invalidation of the laws, consistent with my assumption that the ruling was indeed an exogenous event.

I then move to the heart of the analysis, estimating the effect of APM laws on the long-term value of firms headquartered in the enacting states over the period 1975 to 1992 using pooled panel Tobin's Q regressions that include firm and industry-year fixed effects. I find that the passage of these laws results in a positive and statistically significant increase in firm value, with an economic significance of 5.8% in the baseline specification. Furthermore, when the laws are invalidated by the U.S. Supreme Court I find that the positive value impact of the legislation dissipates.

Next, to refine the main result further, I show affected firms with pre-existing patent portfolios benefit less from the APM statutes than protected firms less reliant on the patent system, as their patented products are already protected from reverse engineering. However, positive increases in Tobin's Q are still present for these firms as their unpatented and future products gain protection against copying by rivals. Indeed, consistent with a substitution effect of competition, I find that corporations headquartered in APM adopting states decrease their level of patenting activity when the laws are effective, and subsequently increase their reliance on patents when the reverse engineering protections are lost.

I then turn to examine the possible economic channels through which a reduction in product market competition, as engendered by the passage of APM laws, may contribute to firm value. Consistent with the traditional focus of the literature on the implications of reduced product market competition, I begin by considering, what I refer to as, the "rents-in-and-of-itself" hypothesis (consistent with the analytical prediction of Lindenberg and Ross, 1981). This hypothesis suggests that increasing protection from reverse engineering creates market power for the affected firms which allows them to extract Ricardian or economic rents at the benefit of its shareholders. Under this hypothesis, it is the standalone extraction of economic rents that creates value – that is, for example, through increased profitability and financial health – and not the use of those rents for operational activities.

I then move to examine an additional hypothesis which emphasizes the importance of how the economic rents are utilized. The "innovation incentives" hypothesis posits that a reduction in product market competition through an increase in reverse engineering protection incentivizes protected firms to invest their newfound economic rents in new and existing production technologies (Aghion and Howitt, 1992; Davidson and Segerstrom, 1998).

I find no evidence supporting the rents-in-and-of-itself hypothesis. Conversely, and consistent with the innovation incentives hypothesis, I find that protected firms increase their investments in new and existing production technologies, and that those firms with the highest levels of innovative ability experience larger increases in Tobin's Q.

Overall, this paper provides the first causal evidence that a decrease in product market competition increases value for shareholders of innovative firms. These findings suggest a potential "bright side" in the recent U.S. trend of increased industry concentration (Grullon, Larkin, and Michaely, 2017; and Kahle and Stulz, 2017), and are especially informative about increased barriers to entry through innovation accumulation (Bena and Li, 2014; Chen, Gao, and Ma, 2017; and Grullon, Larkin, and Michaely, 2017), and the detrimental incentive effects of reverse engineering methods that become excessively efficient (Samuelson and Scotchmer, 2002).

My paper is most closely related to the literature on the intersection of competition and innovation, and its relationship with market value. Blundell, Griffith and Van Reenen (1999) analyze a panel of British manufacturing firms and find that the interaction of market share and innovation is positively associated with firm value. Gu (2016) investigates the joint effect of competition and R&D investment on equity returns. Using a double-sorting portfolio approach, she documents that the positive relation between competition (R&D) and returns only exists in R&D-intensive (competitive) industries.

My study differs from the above two papers in several ways, with the most important being the following. Instead of studying the interaction or sorting of ex-post endogenous proxies, my empirical setting allows me to capture exogenous variation stemming directly from changes in the intensity of competition in product markets. This enables me to improve upon the existing literature by testing how innovative firms respond to changes in their competitive landscape, and how those responses are valued by the market.[3]

2. Institutional Background

2.1 Reverse Engineering

There are two methods for engineering manufacturing products: forward engineering and reverse engineering. Forward engineering is the conventional process of progressing from high-level ideas to the material implementation of those concepts and designs. This usually includes the preparation of engineering drawings from which models can be constructed and then sprayed with a hardening substance to create an original mold for mass production. In contrast, reverse engineering is the process of recreating a finished product or part without the original plans, drawings, models or

[3] My paper also broadly contributes to the extant literatures on: (i) the value relevancy of product market competition (e.g., Lindenberg and Ross, 1981; Nickell, 1996; Sundaram, John, and John, 1996; Hou and Robinson, 2006; Aguerrevere, 2009; Giroud and Mueller, 2011; Bustamante and Donangelo, 2017), (ii) innovation and firm value (e.g., Griliches, 1981; Megna and Klock, 1993; Hall, Jaffe and Trajtenberg, 2005; Simeth and Cincera, 2016), (iii) product market competition and innovation (e.g., Blundell, Griffith, and Van Reenen, 1995; Aghion et al., 2005; Aghion, Van Reenen, and Zingales, 2013; Aghion et al., 2014; Aghion, Akcigit and Howitt, 2015), and (iv) quasi-natural experiments in competition (e.g., Aghion et al., 2005; Frésard, 2010; Lileeva and Trefler, 2010; Valta, 2012; Xu, 2012; Frésard and Valta, 2016; Bloom, Draca, and Van Reenen, 2016).

molds (Raja and Fernandes, 2007). Rather, the reverse engineer analyzes the design and components of the existing product to discover and extract the “know-how” used in its formation. In general, reverse engineering is widely accepted as a useful tool leading to significant advances in innovation. However, the incentives of forward engineers can be substantially compromised when reverse engineering becomes a relatively costless and quick way to make a competing product (Samuelson and Scotchmer, 2002).

2.1.1 Direct Molding Process Reverse Engineering

The “direct molding” process is a specific form of reverse engineering which provides an efficient and inexpensive way to duplicate manufacturing products (Brown, 1986). Direct molding involves using an existing product itself as a “plug” to form a mold, upon which duplicates of the original product can be manufactured. The typical process would involve a competitor firm purchasing an existing product in the open market, spraying or coating it with a mold forming substance (which sets quickly and hardens), and then removing it from the original product and using it as the mold to produce and commercialize replicas (Sganga, 1989; and Shipley, 1990). This benefited competitor companies as they circumvented the research, development, design and manufacturing costs incurred by the originating firm (Devience, 1990). Thus, direct molding process reverse engineering transformed competition in the manufacturing industry by making it ever more difficult for innovative companies to recoup the initial expenses involved in creating original products.

2.2 Anti-Plug-Mold (APM) Statutes

On October 1, 1978, the California legislature passed the first APM statute, prohibiting the duplication and sale of *all* products manufactured by the direct molding process. The California law defined direct molding as “any process in which the original manufactured item was itself used as a plug for the making of the mold which is used to manufacture the duplicate item” (Cal. Bus. & Prof. Code § 17300(c)). The legislative history behind the California APM statute notes that “unscrupulous” manufacturers’ use of the direct molding process enables them to avoid the costs involved with the development of a mold and with new product promotion (Devience, 1990). Moreover, they argued that allowing this form of reverse engineering to continue would “destroy any incentive of manufacturers to develop new and improved designs” (Cal. Bus. & Prof. Code § 17300), which could prove detrimental to consumer welfare (Shipley, 1990). Therefore, to remedy the situation, the California law authorized injunctions against those found guilty of its violation. Furthermore, actual damages, and mandatory attorney fees and costs for prevailing plaintiffs were provided by the statute (Cal. Bus. & Prof. Code § 17301).

Eleven other states followed, enacting similar statutes of their own to protect local consumers and manufacturers from plug molding reverse engineering (in any state). However, only Michigan, in March of 1983, and Tennessee, in July of 1983, adopted APM laws like California’s, which provided protection to *all* manufactured products from the direct molding process (Carstens, 1990). The other nine states, Florida, Indiana, Kansas, Louisiana, Maryland, Mississippi, Missouri, North Carolina and Wisconsin, passed legislation only prohibiting plug molding duplication of originally manufactured hulls and components of boats (Crockett, 1990). Figure B1 shows a U.S. map with the states that have passed these laws, while Panel A of Table B1 provides institutional detail about the twelve APM law adopting states.

Since the plug molding statutes are a matter of state jurisdiction, a conflict of laws may arise when the manufacturer of the originating product and the direct molding process duplicator are located in different states. Although, noteworthy court cases related to the APM laws (listed in the following subsection), seem to suggest that the relevant jurisdiction for firms filing lawsuits to protect their products from plug molding reverse engineering is typically the state where the originating manufacturer (plaintiff) is headquartered (Althauser, 1989; Carstens, 1990; and Heald, 1990).[4] As a result, APM statutes administer reverse engineering protection for a corporation even when the accused direct molding process duplicator is located in a different state whose legislatures have not enacted plug molding laws.[5]

2.2.1 Interpart v. Imos Italia

In July of 1984, almost six years since its adoption, the constitutionality of the California APM law was brought into question when *Interpart Corp.* (Interpart) sued *Imos Italia* (Italia), Vitaloni, S.p.A. (Vitaloni) and Torino Industries, Ltd. (Torino) in the Central District of California (Shipley, 1990). Interpart was seeking a determination of their rights to copy unpatented products first developed and sold by the defendants. The two firms, Interpart and Italia, competed in the Southern California aftermarket for automobile rearview mirrors. Interpart admitted to copying the mirrors sold by Italia and, co-party, Torino, which were first developed by Vitaloni for notable clientele like Ferrari and Lamborghini (Devience, 1990).

[4] The originating or duplicating firm's state of incorporation does not impact the jurisdiction of the APM statutes.
[5] These laws also provide protection against products duplicated via the direct molding process by foreign entities, if the duplicated product is then exported into the U.S. for domestic sale.

Interpart filed suit against the defendants claiming that its manufacture and sale of automobile rearview mirrors was not in breach of the California Business and Professional Code. In response, Vitaloni applied and was granted a design patent for their rearview mirrors and subsequently counter-sued Interpart for patent infringement and copying its mirrors with a direct molding process (Shipley, 1990).

On July 30, 1984, the Central District Court of California ruled that Vitaloni's design patent was invalid since it had been granted more than one year since its initial sale to the public, and that the plug molding claim was unsubstantiated. Further, the district court held that the California APM statute was preempted by federal patent law (Wong, 1990).[6] Vitaloni appealed the direct molding claim and the preemption judgement of the California plug law (Murphy, 1990). The appeal was transferred to the Court of Appeals for the Federal Circuit, which has exclusive federal appellate jurisdiction in cases arising under patent laws (Shipley, 1990).

In November of 1985, the Federal Circuit upheld the constitutionality of the California APM statute, reversing the district court's decision. Further, the court found Interpart guilty of copying Vitaloni's products by way of a direct molding process (Devience, 1990). The Federal Circuit reasoned that the California law was not "an obstacle to the accomplishment and execution of the full purposes and objectives of Congress" (*Interpart Corp.*, 777 F.2d at 684) and therefore not preempted by federal patent law. Moreover, the court stated that: "It is clear from the face of the statute that it does not give the creator of the product the right to exclude others from making, using, or selling, the product as does the patent law…The statute prevents…competitors from

[6] Federal patent law provides protection against reverse engineering but requires disclosure of "know-how" for the protection. Meanwhile, firms located in APM adopting states were granted similar protection without any disclosure. Hence, the district court's ruling of preemption.

obtaining a product and using it as the "plug" for making a mold. The statute does not prohibit copying the design of the product in any other way; the latter, if in the public domain, is free for anyone to make, use, or sell (Interpart, 777 F.2d at 684, 685)."

In addition to *Interpart Corp. v. Imos Italia* (product: rearview mirrors), there were at least seven other court cases involving the use of molds to duplicate a wide variety of manufacturing products (Althauser, 1989; Carstens, 1990; and Heald, 1990): *Metro Kane Imports, Ltd. v. Rowoco, Inc.* (product: orange juicer); *Gemveto Jewelry Co. v. Jeff Cooper, Inc.* (product: jewelry); *Summerford Racing, Inc. v. Shadow Boat, Inc.* (product: boat parts); *Power Controls Corp. v. Hybrinetics, Inc.* (product: electrical part packaging); *Brahma, Inc. v. Joe Yeargain, Inc.* (product: truck camper shells); *Gladstone v. Hillel* (product: jewelry); *Bonito Boats, Inc. v. Thunder Craft Boats, Inc.* (product: boat hulls). Moreover, some legal scholars believe that there would have been many more reported cases if not for the strong pro-plaintiff bias of the statutes (Sganga, 1989). However, of those reported above, the most notable and significant took place in Florida, where two boat manufacturers battled in the courts over the constitutionality of the state's APM law.

2.2.2 Bonito Boats v. Thunder Craft Boats

In September of 1976, *Bonito Boats, Inc.* (Bonito) began the development, design and manufacture of an original recreational boat hull (Murphy, 1990) which, upon completion, was marketed under the trade name Bonito Boat Model 5VBR (Heald, 1990). In its creation, Bonito prepared a complete set of engineering drawings, which were then used to construct a hardwood model. This model was sprayed with fiberglass to craft a mold, which was then used to manufacture hulls for the finished product. The Model 5VBR was mass produced and sold to a broad interstate market, however, no patent applications were filed by Bonito to protect either the utilitarian or design aspects of the boat hull (Carstens, 1990).[7]

Thunder Craft Boats, Inc. (Thunder Craft) was a company located in Tennessee which observed the success of the Model 5VBR in the open market, and consequently, copied the hull for its own commercial purposes by way of a direct molding process. In so making the duplicate product, Thunder Craft purchased a Bonito hull and used it as the model, which in turn was splashed with fiberglass to create a mold. The company then used this mold to manufacture exact replicas of the 5VBR and distribute as its own creation under the trade name “Capri” (Carstens, 1990). Hence, Thunder Craft completely bypassed the need to prepare engineering drawings and make its own model hull from those specifications. Instead, relying solely on the R&D investment and promotional campaign already made by Bonito, Thunder Craft effectively slashed its development costs to practically nothing (Shipley, 1990).

On May 3, 1983, The Florida legislature adopted an APM statute prohibiting the use of a direct molding process to duplicate boat hulls or other components for the ends of selling the copied products. Soon thereafter, on December 21, 1984, Bonito brought suit against Thunder Craft for violating the Florida law (Carstens, 1990). However, the Orange County Circuit Court in charge of the case dismissed Bonito’s suit, ruling that the state’s APM statute was preempted by federal patent law (Heald, 1990). Bonito appealed to Florida’s Fifth District Court of Appeal, and on April 24, 1986, the district court affirmed the earlier ruling in a 2-to-1 decision (Shipley, 1990). Unsatisfied with

[7] It is unlikely either a utility or design patent would have been granted as the resulting boat hull would likely have failed the demanding standards of novelty and nonobviousness required by the U.S. Patent Office (Lichtman, 1996).

the outcome, Bonito then appealed to the Florida Supreme Court. On November 12, 1987, in a 4-to-3 ruling, the Florida Supreme Court affirmed the lower courts' invalidation of the plug molding statute (Wong, 1990). The majority four judges reiterated that "when an article is introduced into the public domain, only a patent can eliminate the inherent risk of competition and then but for a limited time" (*Bonito Boats*, 515 So. 2d 220 at 222).

With no other alternatives, on February 9, 1988, Bonito petitioned for certiorari from the U.S. Supreme Court, requesting a resolution in the conflicting judgements between the Florida Supreme Court and the Federal Circuit in *Interpart Corp. v. Imos Italia*. On May 16, 1988, the U.S. Supreme Court granted Bonito's petition (Shipley, 1990).

2.2.3 U.S. Supreme Court's Ruling in Bonito

On December 8, 1988, the U.S. Supreme Court heard Bonito's appeal. The Florida headquartered boat manufacturer presented a twofold argument as to why the Florida APM statute was constitutional, and not preempted by federal patent law. The first argument centered on the assertion that the plug molding law did not afford the same level of protection provided by patents. The second argument asserted that the Florida statute was a legitimate exercise of Florida's authority to protect local business interests by regulating and discouraging unfair and "unscrupulous" competition (Carstens, 1990). In contrast, the briefs filed in support of the Florida Supreme Court ruling maintained that the state's APM statute granted patent-like protection to boat hulls and was therefore unconstitutional. Further, the defendants argued that the Federal

Circuit made a wrong decision in *Interpart*, again asserting its inconsistency with United States patent law (Shipley, 1990).

On February 21, 1989, the U.S. Supreme Court unanimously affirmed the ruling of the Florida Supreme Court and rejected the Federal Circuit's decision in *Interpart*. The Court stated Florida's statute granted substantially similar rights to originating boat hull manufacturers as to those conferred to a patentee, by excluding competitors from making and selling duplicates procured by the direct molding process (Heald, 1990). The Court noted: "the duplication of boat hulls and their component parts may be an essential part of innovation in the field of aquadynamic design", and that "the competitive reality of reverse engineering may act as a spur to the inventor, creating an incentive to develop inventions that meet the rigorous requirements of patentability" (*Bonito Boats*, 489 U.S. at 160). As outcome of the Supreme Court's decision in *Bonito Boats, Inc. v. Thunder Craft Boats, Inc.*, every states' APM statute was effectively invalidated (Carstens, 1990).[8] Panel B of Table B1 details the significant court cases outlined above.

3. Data and Summary Statistics

3.1 Data

I use several sources of data to construct two main data samples covering the period 1975 to 1992. The first main data sample consists exclusively of manufacturing firms (SIC codes 2000-3999) in the Compustat database, with publicly traded stock

[8] In 1998, Congress enacted the Vessel Hull Design Protection Act (VHDPA) as part of the Digital Millennium Copyright Act to protect boat hulls and its component parts from the direct molding process (Samuelson and Scotchmer, 2002). However, the VHDPA was too late for Bonito Boats, as the company went out of business on July 16, 1991 (http://articles.orlandosentinel.com/1992-05-25/business/9205231057_1_boat-makers-regal-marine-boating-industry).

price observations in the Center for Research in Security Prices (CRSP) database, headquartered in the United States, and without missing data for the main variables of interest. This selection criterion yields 32,808 firm-year observations, of which 5,781 are affected by an APM statute. I denote this dataset as the "Manufacturing" sample. The other main data sample is a subset of the "Manufacturing" sample, in which I exclude two-digit SIC code industries (within the 2000-3999 SIC code range) without tangible, direct molding process "copyable" products (e.g., SIC code 20-food and kindred products). Table B2 provides a description of this second main dataset, which I refer to as the "Products" sample. From this selection, I obtain 21,791 firm-year observations, where 4,265 are affected by the APM laws.

My samples begin three years before California adopts the first APM statute, and end three years after the U.S. Supreme Court's ruling in *Bonito Boats v. Thunder Craft Boats* effectively invalidated all 12 plug molding laws.

My main independent variables, *APM Law*, *All Item APM Law*, and *Boat Hull APM Law*, capture whether a firm is headquartered in a state that has passed any type of APM legislation, or specific to whether the adopted statute covers all manufacturing items or only those specific to boat hulls and its components. I obtain information on each enacting states' statute name from Sganga (1989), Carstens (1990), Crockett (1990), and Heald (1990). With these statute names, I then use the *LexisNexis Academic* "State Statutes and Regulations Search" option to verify the details of each law, and to establish the month and year in which it was adopted. Further, I confirm which statutes protected all manufacturing items relative to those that stipulated protection for hulls and component parts of boats. Figure B1 provides a U.S. map depicting the dispersion of enacting states by type of product coverage. The 12 adopting states' respective statute names, adoption dates, product coverage, and the number of unique firms over the sample period are reported in Panel A of Table B1.

To ensure that I use historically accurate accounts of firms' headquartering states when defining my main independent variables, I supplement the current headquarter data provided by Compustat with historical location information from the CRSP Historical U.S. stock database that is available from the University of Chicago directly (rather than through WRDS). This historical CRSP dataset spas the period 1990 to 2015. I approximate the state of location for the years 1975 to 1989 by backfilling firm-year headquarter data using the oldest data point of historical headquarter information available. Using this procedure, I successfully match 58.2% of the 32,808 firm-year observations. For the remaining missing headquartering state values, I supplement my sample with Compustat's data on current states of location. The fact that the data is current and not historical should not be a major concern, however, as empirical evidence suggests firms likely do not switch headquartering states very often. For example, Pirinsky and Wang (2006) find less than 2.4% of firms changed their state of location over a 15-year period. Applying this finding to my dataset suggests roughly 1% (=0.024 × 0.418) of the firm-years might be incorrectly assigned (or not assigned) protection from the direct molding process.

From the above information, I create the *APM Law*, *All Item APM Law*, and *Boat Hull APM Law* indicator variables, which are set equal to one for firms affected by any, all item, or boat hull specific APM legislation in the year of and after the respective adoption date, and zero in the years prior to the adoption date, or always zero for corporations in states that never enact an APM law. In addition, I permit the adjustment of the indicator variables for the states of California and Florida dependent on important court decisions validating or invalidating the statutes in their respective jurisdictions (depicted in Figure B2).

As shown in Panel B of Table B1, these two states have significant court cases addressing the constitutionality of their laws, and in 1984, both have their protection from the direct molding process judiciously stripped away. However, roughly one and a half years later the Federal Circuit re-administers protection to California companies finding that the state's APM legislation is constitutionally valid. On the other hand, after two additional court rulings within state, Florida firms never regain protection.

My main interaction variables involve the multiplication of *APM Law*, *All Item APM Law*, and *Boat Hull APM Law* with a time indicator variable, *Post 88*, which is set equal to one in the year after 1988, and zero before. These interactions allow me to capture the change in legal and competitive environment engendered by the U.S. Supreme Court ruling on February 21, 1989, which effectively eliminated the enacting states protection from direct molding process reverse engineering.

My primary focus is studying the value relevance of product market competition, and as such the main dependent variable in my analysis is long-term firm value. Consistent with prior empirical studies investigating the value implications of competition (Lindenberg and Ross, 1981; Blundell, Griffith, and Van Reenen, 1995, 1999; Giroud and Mueller, 2011), I measure firm value using Tobin's Q (*Tobin's Q*). Following Fama and French (1992), *Tobin's Q* is defined as the market value of assets divided by the book value of assets. Additionally, in a separate robustness test, I employ an alternative measure of firm value by performing long-run stock return event studies around the adoption of the APM statutes. Using monthly stock returns *(Monthly Stock Returns)*, I estimate the portfolios' abnormal returns (*Alpha*), where the stock return data for this analysis comes from the CRSP database.

The models I estimate also include a number of control variables which other product market competition studies have shown are important when investigating a policy's influence on *Tobin's Q*. My default specification includes: *Size*, *Ln(Age)*, *Debt-to-Equity*, *ROA*, *Operating Cash-Flow*, *HHI*, *Sales Growth*, *Loss*, *Firm Liquidity*, *R&D/Sales*, *CAPX/Assets*, and *Industry-Year Tobin's Q*. In particular, since I am investigating the value impact of state-level legislation, and since industries often cluster by geography, I specify *Industry-Year Q* to capture time-varying three-digit SIC code industry shocks (following Giroud and Mueller, 2010). Data for all of the controls come from Compustat.

Lastly, to mitigate the influence of extreme outliers, I winsorize all continuous dependent and independent variables in my samples at the 1% level in both tails, and, additionally, I adjust dollar values for inflation using 2015 dollars.

3.2 Descriptive Statistics

I present summary statistics in Table B3 for all of the variables used in the main regression analyses over the period 1975 to 1992. Specifically, Panel A of Table B3 provides the mean, standard deviation, median, and 25th and 75 percentiles for the main dependent, independent, and interacted variables for the "Manufacturing" sample, whereas Panel B shows the analogous variable summaries for the "Products" dataset. From these two panels, it is observed that the mean *Tobin's Q* is 1.54 for each of the respective samples. Further, the average proportion of firm-years in which a company is protected by an *APM Law* ranges from 17.6% to 19.6% across the datasets. Specific to the “Manufacturing” (“Product”) sample the percentage of firms affected by the *All Item APM Law* is 13.2% (15.3%), while those protected under the *Boat Hull APM Law* is 4.5% (4.2%). Furthermore, the remaining independent variables appear to be equally comparable across both the “Manufacturing” and “Products” samples.

Figure B2 depicts the percentage of firms affected by the APM statutes for each of the respective samples. From this graph, it is evident that both the “Manufacturing”, and “Products” samples behave similarly over the period. Specifically, it is observed that the number of affected firms increases to roughly 8% across the two samples when California passes an APM statute in 1978. The percentage of protected firms continues to rise as more states adopt the legislation and by 1983 the proportion of affected companies surrounds the 22% threshold. Then, in 1984, both Californian and Floridian businesses lose plug molding protection when their respective states’ courts find them preempted by federal patent law dropping the percentage to around 10%. The number of affected corporations’ increases again by 1985 as additional states adopt laws and the Federal District Court rules that California’s APM statue is enforceable. The final state, Indiana, enacts an APM statute in 1987 bringing the percentage of affected firms to roughly 29%. However, this protection comes to an abrupt halt in 1989 as a U.S. Supreme Court ruling leaves all APM laws unenforceable.

4. Identification Strategy and Methodology

4.1 Identification Strategy

The main working assumption of my identification strategy is that absent the adoption of APM statutes and their subsequent reversal by the U.S. Supreme Court, long-term firm value of companies located in states that enacted and did not enact these laws would have evolved in a similar fashion. In other words, I contend that the group of firms headquartered in plug molding passing states and the group of firms headquartered in states that do not adopt such statutes would have had parallel trends in value if no such legislative action occurred. Therefore, to preserve the validity of my strategy and maintain causal interpretations, it is imperative that I rule out two important concerns that would call into question the parallel trends assumption. The first concern is that states adopted APM laws with the intention of achieving certain firm value implications. The second concern is that companies located in plug molding passing states might have anticipated either the adoption of the statutes or the U.S. Supreme Court decision. In the following subsections, I provide discussion and evidence which attempts to demonstrate that neither of the above two concerns are in fact a problem.

4.1.1 Determining the Adoption of the APM Statutes

There is a possibility states with APM statutes passed such legislation specifically to alter the long-term firm value of businesses in their state (i.e., reverse causality). This would be detrimental for my identification strategy as the instrument I use to study the effect of product market competition, the *APM Law*, *All Item APM Law*, and *Boat Hull APM Law* indicator variables, would no longer satisfy the exclusion restriction. As a first pass, I review the extant law literature detailing the state-level adoption of the statutes and document that none of the legislative histories seem to suggest the laws were passed with any sort of corporate value intents. Rather, it appears the lawmakers stated aim was to protect local consumer welfare (Shipley, 1990), providing suggestive evidence consistent with the validity of the exclusion restriction.

To test this formally, I follow Cremers et al. (2018) and estimate a linear probability model with state-level covariates, and state and year fixed effects, where the dependent variable denotes the enactment of an APM law.[9] The results of these tests are presented in Table B4. I exclude firm-year observations from the sample once their headquartering states pass plug molding legislation (i.e., a "failure event" occurs). The sample period covers 1975 to 1992, where all of the independent variable are lagged one period (t-1), and all of the continuous predictor variables are standardized to have a mean of zero and unit variance. I estimate robust standard errors independently double clustered by state of location and year.

Columns (1) – (2) of Table B4 show results specific to the "Manufacturing" sample, with the first column including annual averages of headquartering state-year firm characteristics, while the second column appends controls for other macro and legal factors at the state level. From these two columns, it's clear that none of the specified predictor variables significantly determine the adoption of state APM laws in the "Manufacturing" sample. In particular, consistent with my exogeneity assumption, the average annual *SY Tobin's Q*, *SY Δ Tobin's Q*, and *SY Industry-Year Tobin's Q* cannot predict the adoption of an APM statute. Similar results hold for columns (3) –

[9] Cremers et al. (2018) conduct a similar analysis, but specific to a different legal experiment, where the failure event represents incorporating state adoption of poison pill statutes.

(4) in the "Products" dataset.[10] Overall, I conclude that there is no evidence for reverse causality.[11]

4.1.2 *Anticipation of the Supreme Court's Ruling*

In the above subsection, I provided suggestive evidence that states did not pass APM statutes with the specific intent of altering firm value. Since I am also using the reversal of the APM statutes in a 1989 U.S. Supreme Court ruling, it is important that I show the previously mentioned concerns are also not a problem for this judicial event. First, I argue that it is highly improbable the U.S. Supreme Court would reverse or uphold lower court rulings in a specific state with the intentions of altering headquartering state firm value. Thus, this first concern does not apply in this context. Second, while it is also unlikely that firms would be able to anticipate the U.S. Supreme Court's decision, I put statistical formality to this argument and test this concern using an event-study approach.[12]

Following Serfling (2016) and Klasa et al. (2018), I conduct a short-run event study of abnormal stock returns around the Supreme Court's decision date, February 21, 1989, for firms located in adopting states. I estimate cumulative abnormal returns (CARs) using either the four-factor Carhart (1997) model or the three-factor Fama and

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[10] Table G1 of the supplementary appendix reports the results specific to the adoption of all item APM statutes. The findings are qualitatively similar to Table B4.

[11] In section 7, I provide additional evidence for the validity of my identification strategy by testing the timing of the change in firm value relative to the timing of the passage of the APM statutes. Organizationally, I choose to present these results after first documenting that the statutes are indeed value relevant. However, for the purpose of this section, I briefly note the suggestive evidence from Table B14 that the effect of APM statutes on *Tobin's Q* transpires *after* the passage of the laws and not before. This offers some reassuring evidence that both the affected and unaffected firms' value would have evolved in a similar fashion absent the adoption of this legislation (i.e., the parallel trends assumption likely holds).

[12] I contend it is especially unlikely firms would be able to anticipate the Supreme Court's ruling given the Florida Supreme Court had just upheld the invalidation of Florida's plug molding law on a closely split 4-to-3 decision, and that the Federal Circuit reached the opposite conclusion in *Interpart Corp. v. Imos Italia*.

French (1993) model, with both an equally- and value-weighted CRSP market index. The regression parameters are estimated over the trading window [-271,-21], relative to the Supreme Court's ruling date. However, one important adjustment is required. Since all firms in plug molding adopting states will be affected by the same event on the same announcement day, the Supreme Court ruling is not independent across firms and correspondingly the standard errors in these regressions will be contaminated by a cross-sectional correlation bias. To deal with this issue, I correct the standard errors following the technique outlined in Kolari and Pynnönen (2010).

The results of these tests are presented in Table B5. Specifically, I show in the [-17,-3] and [-12,-3] (prior two and three week trading day) periods before the U.S. Supreme Court's ruling, the CARs are statistically insignificant for all model specification and in both the "Manufacturing" and "Products" samples. In contrast, and consistent with the Court's ruling being a surprise to capital markets, I find negative and statistically significant CARs for all models and in both respective samples over the five-day period surrounding the announcement date, [-2,+2]. These estimates vary from -0.33% to -0.39% in the "Manufacturing" sample, and -0.37% to -0.44% within the "Products" dataset.[13]

In sum, it appears that investors expected the loss of protection from reverse engineering (and, consequently, an increase in product market competition) to be detrimental to value. Importantly, for my identification strategy, the findings provide suggestive evidence that firms did not anticipate the U.S. Court ruling preempting APM statutes, but rather it was an exogenous event.

[13] Table G2 of the supplementary appendix reports the results specific to affected firms in all item APM adopting states. The findings are qualitatively similar.

4.2 Methodology

I employ a difference-in-differences methodology to investigate how the passage of APM statutes affects the long-term value of businesses in adopting states. Since these laws are passed in a staggered fashion by 12 states with varying adoption dates the research design I use follows the approach outlined in Bertrand, Duflo and Mullainathan (2004), where firms headquartered in eventual plug molding law enacting states are considered as part of the unaffected group until their legislatures adopt a statute, upon which they enter the affected group in the analysis. For example, companies located in Michigan will have their *APM Law* (and *All Item APM Law*) indicator variable set equal to zero in the period prior to March of 1983, whereas after the law is passed the variable switches to one for the remaining nine years in the panel. Further, firms headquartered in states that never receive legislated protection from the direct molding process are always coded as unaffected. In equation form, I estimate the following panel regression model:

$$Tobin's\ Q_{ijst} = \gamma_i + \omega_{jt} + \beta_1 APM\ Law_{st} + \alpha X_{ijst} + \varepsilon_{ijst}, \qquad (1)$$

where $Tobin's\ Q_{ijst}$ measures firm value for firm i, in industry j, located in state s, during year t, and $APM\ Law_{st}$ is an indicator variable for whether the state in which a company is located has adopted an APM law as of year t. Further, some regression models also include a set of control variables X_{ijst}, detailed in the above subsection 3.1, to account for other firm characteristics the extant literature has deemed important when examining a policy's influence on firm value. Some of my models, however, exclude all controls, because some of these controls are also likely impacted by APM laws and could thus bias my coefficient estimates (as discussed in Roberts and Whited, 2013).

Moreover, following the work of Gormley and Matsa (2014), I control for time invariant unobservable heterogeneity within different firms using firm fixed effects γ_i. Additionally, I include an industry-year interacted fixed effect ω_{jt} to control for unobserved, time-varying differences across industries, where the industry grouping is defined at the two-digit SIC code level.[14] Including such high-dimensional fixed effects provides additional robustness to my methodology, allowing me to effectively control for common sources of industry or time-dependent unobserved heterogeneous variation (Gormley and Matsa, 2014, 2016; Karpoff and Wittry, 2018).[15] Lastly, I estimate robust standard errors clustered by state of location since my main independent variables are defined at the state-level (Serfling, 2016; Klasa et al., 2018).[16]

Additionally, I break apart the *APM Law* indicator variable into *All Item APM Law* and *Boat Hull APM Law* variables to test for the differential value implications from the varying product coverage of the statutes.

In addition to equation (1), I estimate a supplementary panel regression model to capture the change in legal and competitive environment after the U.S. Supreme Court effectively invalidated all states' plug molding laws in February of 1989:

$$Tobin's\ Q_{ijst} = \gamma_i + \omega_{jt} + \beta_1 APM\ Law_{st} + \beta_2 Post\ 1988_t \times APM\ Law_{st} + \alpha X_{ijst} + \varepsilon_{ijst}, \tag{2}$$

where $Post\ 1988_t \times APM\ Law_{st}$ measures the value relevance of the plug molding laws after 1988, with i indexing for firms, j indexing for industry, s indexing states of

[14] The two samples are restricted to manufacturing firms with SIC codes ranging from 2000 – 3999, and, moreover, in the product specific sample, the number of SIC code defined industries is further truncated. Hence, measuring industry fixed effects at a more refined level than two-digits greatly reduces the amount of available variation.

[15] Results are similar in models with firm and year fixed effects only.

[16] Results are qualitatively similar if I cluster by firm, or independently double cluster by firm-year, state-year, or firm-state.

location, and t indexing years, and all other variables as before. The indicator variable $Post\ 1988_t$ is set equal to one beginning in 1989 and afterwards, and otherwise equal to zero, but is excluded from the regression due to its multicollinearity with year fixed effects. The estimated standard errors are robust to heteroscedasticity and autocorrelation, with clustering performed at the state of location level. Again, I supplement the model in equation (2) by decomposing the *APM Law* indicator variable in each of the coefficient terms into *All Item APM Law* and *Boat Hull APM Law*.

5. Main Results

5.1 APM Laws and Firm Value

Table B6 begins my investigation of the value relevance of product market competition by presenting coefficient estimates from difference-in-differences (DID) regressions of *Tobin's Q* on an *APM Law* indicator variable over the period 1975 to 1988. In this first set of tests, I exclude firm-year observations after 1988, since protection from reverse engineering is eliminated by the U.S. Supreme Court in February of 1989. In all specifications I include firm and industry-year fixed effects, where the industry grouping is defined at the two-digit SIC code level. The reported *t*-statistics are based on robust standard errors clustered at the state of location level. In columns (1) – (3), I report results specific to the "Manufacturing" sample, while columns (4) – (6) correspond to the "Products" dataset. Furthermore, columns (1) – (2) and (4) – (5) employ regression model (1), outlined in subsection 4.2, while columns (3) and (6) decomposes the *APM Law* indicator into *All Item APM Law* and *Boat Hull APM Law* indicator variables.

Moving to my main results, in columns (1) and (2) of Table B6, I find a positive and significant relation between *APM Law* and *Tobin's Q*. In particular, with the full set of controls specified, column (2) documents that *Tobin's Q* increases by an economically significant 5.3% (=0.082/1.542), relative to its sample mean. Further, in column (3), I investigate the differential effect of APM laws that protect all manufacturing items relative to those that only cover boat hulls and its components and find that the entirety of the value gains come from *All Item APM Laws*. Specifically, when the statutes are enforceable, firms headquartered in California, Michigan and Tennessee experience sample mean relative increases in *Tobin's Q* of 5.8% (=0.090/1.542). In comparison, *Boat Hull APM Law* affected corporations have a positive but insignificant point estimate.[17]

I further examine the extent to which APM statutes increased long-term firm value in the "Products" sample. In this dataset, I carefully select firms in two-digit SIC code industries that are the most likely to have tangible products that could be copied via the direct molding process, and exclude the remaining manufacturing corporations for which the APM statutes do not apply.[18] Columns (4) – (6) provide suggestive evidence that the sample selection process reduces estimation noise as all significant point estimates are larger in magnitude than their "Manufacturing" analogues. For example, in the second column with the full set of controls included, I find *APM Law* affected firms display positive increases in *Tobin's Q* of 5.6% (=0.086/1.542) relative to

[17] It makes intuitive sense I find a positive point estimate, since there are a portion of affected (boat manufacturing) firms within the sample. However, the value effect for this select number of companies is not large enough to override the statistical insignificance of the other manufacturing firms for which the law does not provide protection.
[18] For example, companies in the food and kindred products industry (SIC code 20) manufacture items (e.g., meat, dairy and bakery products) which a rival firm would never copy using a mold.

the sample mean. This represents a positive 4.9% (=[0.086-0.082]/0.082) difference relative to the corresponding coefficient estimate in column (2). Furthermore, consistent with the earlier results, I provide evidence in column (6) that the total value effect stems from *All Item APM Law* firms and not the *Boat Hull APM Law* companies.

Overall, I conclude that the decreased product market competition experienced by corporations headquartered in states that adopt all manufacturing item APM statutes increases firm value. Furthermore, given these findings, I choose to focus the remainder of my analysis on the *All Item APM Law* variable in the "Manufacturing" and "Products" samples. I choose this research design for the following two reasons. First, focusing on all manufacturing item protection should provide the cleanest identification as it is apparent from Table B6 the average proportion of firms in the boat hull specific APM statute states are not significantly affected by the legislation, and just add noise to the estimates. Second, concentrating on the companies headquartered in the three states with all item protection enhances the external validity of my findings, as my conclusions are not limited to boat hull and its component businesses.

5.1.1 APM Laws, Supreme Court's Ruling and Firm Value

In this subsection, I make use of opposing exogenous variation derived from the U.S. Supreme Court decision in *Bonito*, which effectively made all APM statutes unenforceable. This judicial event provides a superb testing ground for whether the positive value implications documented in Table B6 actually derive from the plug molding laws, or if some other unobservable factor is creating a spurious correlation. Table B7 reports the triple differences (DDD) estimates over the sample period 1975 to 1992, capturing the value impact of both the protection granted by adopting states prior to 1988, and then its subsequent removal afterwards. As before, all specifications include firm and industry-year fixed effects, where the industry grouping is defined at the two-digit SIC code level, and the estimated *t*-statistics are based on robust standard errors clustered at the state of location level. The first two columns of the table pertain to the "Manufacturing" sample, while the last two are specific to the "Products" dataset. In columns (1) – (2), I find the *Post 1988* × *All Item APM Law* interaction is always negative and insignificant. In contrast, the *All Item APM Law* coefficients are always positive and statistically significant, and qualitatively similar in magnitude to those documented in Table B6. Further, in testing the joint significance of APM statute relation with *Tobin's Q* across the two periods I find an insignificant value effect. This is clear evidence, that any of the positive value implications affected firms derived in the period when the statutes were enforceable are entirely wiped out after the protection is lost. The "Products" columns, (3) – (4), shows similar findings. In particular, the point estimate on the interaction term in the fully specified regression model of column (4) is negative but insignificant, while the *All Item APM Law* coefficient suggests an increase in *Tobin's Q* in the pre-1988 period of 5.3% (=0.081/1.542), relative to the sample mean. Finally, in a test of the total value effect across the period 1975 to 1992, I document that any of the value gains experienced by corporations with "copyable" products and located in the all item APM statute states prior to the Supreme Court's ruling are completely removed along with the reverse engineering protections.

5.2 APM Laws, Patent Activity and Firm Value

Another interesting test that can be performed to supplement the findings above, is to see whether affected firms with pre-existing patent portfolios are impacted by the law to the same extent as companies using a different form of intellectual property protection. Intuitively, businesses that are already protected from direct molding process reverse engineering by patents should not experience the same benefit from the APM statutes as non-patenting firms. However, given a forward-looking measure like *Tobin's Q*, which captures long-term anticipated value, this is not to say that the law will not also be valuable for these patenting companies too, as their current unpatented and future products gain protection. Table B8 tests this intuition.

To measure firm-level patenting activity I use the following three measures: *Ln(Patent)*, *Ln(CW Patent)*, and *Ln(SM Patent)*. These variable definitions can be found in Table A1, but, described briefly here, the first two continuous variables measure patent count and citation-weighted patents (Hall, Jaffe and Trajtenberg, 2001), while the third is constructed by weighting patents based on stock market reactions to their grants (Kogan et al., 2017). Columns (1) – (3) provides the regression estimates for the "Manufacturing" sample, where each of the columns specify the complete set of controls with firm and industry-year fixed effects. Further, the reported *t*-statistics are estimated using robust standard errors with state of location level clustering.

Column (1) documents a negative and significant differential value effect for "Manufacturing" firms protected by APM statutes and with higher levels of patent counts, while an *All Item APM Law* company without any patenting activity experiences increases in *Tobin's Q* of 7.4% (=0.114/1.542), relative to the sample average. However, it is important to be careful of the interpretation here, as the negative and significant point estimate on the interaction term does not suggest that firms with patents were hurt by the plug molding laws, but rather their gains in value are smaller in magnitude since they already had some form of reverse engineering protection. This is demonstrated formally in column (1) in a test of joint significance for a corporation headquartered in an *All Item APM Law* with an average level of *Ln(Patent)*. Using this approach, I find the affected company with an average portfolio of patenting activity still experiences an increase in firm value of 5.6% (=0.086/1.542), relative to the sample mean *Tobin's Q*. Columns (2) – (3) presents similar findings using the *Ln(CW Patent)* and *Ln(SM Patent)* measures of patenting activity, respectively.

In the subsequent three columns of Table B8, I investigate the value relevancy of the APM statutes for firms with existing patenting activity in the "Products" sample. Columns (4) – (6) finds qualitatively similar results using the products-based dataset. Specifically, the DDD estimate in the sixth column suggests that firms with an average level of stock market-weighted patents experience significantly smaller increases in *Tobin's Q*, a 6.7% (=01.04/11.542) gain, relative to a 9.4% (=0.145/1.542) increase for a similarly protected firm with an *Ln(SM Patent)* measure equal to zero. Furthermore, it appears the products-based sample estimates are less confounded by noise, as the magnitudes of these point estimates are larger than those from the general manufacturing regressions. Finally, I find some evidence that patent counts and citation-weighted patents, in general, are not value relevant in the 1975 to 1988 period, consistent with the descriptive findings documented in Hall (1993), where she shows the stock market's valuation of the intangible capital created by manufacturing firm R&D decreased substantially in the mid-1980s.[19]

[19] Table G3 of the supplementary appendix regresses *Tobin's Q* on the triple interaction term: *Post 88* × *All Item APM Law* × *Patent Activity*. I do not find evidence that corporations with pre-existing patent portfolios are impacted differentially by the loss in reverse engineering protections.

5.2.1 *APM Laws, Supreme Court's Ruling and Patent Activity*

Next, before I explore possible economic channels for the positive value effect stemming from a reduction in product market competition, I investigate whether patenting activity decreases (increases) when firms headquartered in APM adopting states gain (lose) protection from reverse engineering. As discussed before, the APM laws provided similar protection to those granted by patents but without requiring the necessary disclosure of "know-how," while the Supreme Court ruling in *Bonito Boats v. Thunder Craft Boats* effectively invalidated this state conferred protection. Therefore, economic intuition suggests that companies wanting to preserve an operational (informational) advantage on their rivals would decrease their use of patents when the laws were valid and increase their level of patenting activity when the reverse engineering protections are lost. Table B9 tests this hypothesis.

In these regressions, I specify the dependent variable as either *Ln(Patent)*, *Ln(CW Patent)* or *Ln(SM Patent)*. Furthermore, consistent with existing work (Atanassov, 2013; Fang, Tian and Tice, 2014; Mukherjee, Singh and Žaldokas, 2017; and Chemmanur and Tian, 2017) I lead these three measures by two years because the Supreme Court ruling likely affects patenting activity with a lag.[20] Another concern with patent data is the inherent truncation bias. That is, depending on when the sample period ends there will be patents that have yet to be granted but already applied for and thus missing from the data. Moreover, patent citations will also display a bias since earlier granted patents have more time to accumulate citations relative to more recently granted patents (Hall, Jaffee, and Trajtenberg, 2001). However, these truncation biases

[20] Table G4 of the supplementary appendix reports the results for one-year leaded (*t*+1) and three-year leaded (*t*+3) patenting activity measures as well.

are likely not a concern for my study since the most recent dependent variables in my regressions are from 1994 (1992 + 2-year lead), while the patent dataset I employ has observations from 1926 to 2010.

Columns (1) – (3) provides the regression estimates for the “Manufacturing” sample, where each of the columns specify the complete set of controls (including *Tobin’s Q*) with firm and industry-year fixed effects. Further, the reported *t*-statistics are estimated using robust standard errors with state of location level clustering. In column (1), I document, relative to the sample mean, after corporations gain protection from plug molding, *Ln(Patent)* decreases by 5.2% (=0.008/0.155) but increases by 18.7% (=0.029/0.155) after the laws are struck down. Additionally, *Ln(CW Patent)* and *Ln(SM Patent)*, which measure patent quality and importance, also decrease (increase) before (after) the Supreme Court ruling.

Similar results hold in columns (4) – (6) for the “Products” dataset, as all three measures of patenting activity differentially decrease in the pre-1989 period. For example, relative to its sample mean, *Ln(CW Patent)* decreases by 3.2% (=0.033/1.026) for businesses that gain reverse engineering protections, but increase by 9.9% (=0.102/1.026) when competition intensifies in the post-1988 period. Overall, these findings are consistent with a substitution effect of product market competition in intellectual property protection.

6. Hypothesized Sources of Value

Having established empirical evidence that the adoption of all item APM laws are positively related to firm value, I now turn to examining two possible explanations for how decreased product market competition, as brought about by the passage of these laws, may contribute to firm value. First, consistent with the traditional focus of the industrial organization literature on the implications of reduced competition, I begin by considering, what I refer to as, the "rents-in-and-of-itself" hypothesis (consistent with the analytical prediction of Lindenberg and Ross, 1981). This hypothesis suggests that increasing a firm's protection from reverse engineering erects a technological barrier to entry which creates a source of Ricardian or economic rents at the benefit of the protected firm's shareholders. Under this hypothesis then, the increase in value is solely attributed to the rents from market power (e.g., from increased profitability and financial health) and not the use of those rents for operational activities.

Additionally, I further consider what I term an "innovation incentives" hypothesis, which posits that increased protection from reverse engineering, and thereby a decrease in the intensity of a firm's product market competition, can increase the investment incentives of protected firms and improve long-term firm value (Aghion and Howitt, 1992 and Davidson and Segerstrom, 1998). Therefore, under this hypothesis, value is created from the use of Ricardian or economic rents towards investments in new and existing production technologies.

6.1 Rents-in-and-Of-Itself

In this subsection, I consider if the positive relation I document between firm value and APM laws might be explained by an increase in the economic "rents-in-and-of-itself," that protected firms earn through a reduction in product market competition (Lindenberg and Ross, 1981). The all item APM laws prohibited an efficient method to reverse engineer manufacturing products, creating a technological barrier to entry that increased the relative market power of protected firms. In order to test this potential economic rents-in-and-of-itself channel of value, I analyze the impact of the APM laws on protected firms' profitability and financial health.

I first test the rents-in-and-of-itself hypothesis by examining the impact of APM laws on the profitability of protected firms over the period 1975 to 1988. In order to proxy for firm-level profitability, I employ three separate empirical measures (following Giroud and Mueller, 2010). The first proxy variable for profitability is return on assets (*ROA*), which is measured as income before extraordinary items (*ib*) plus depreciation and amortization (*dp*) divided by the book value of total assets (*at*). The second profitability proxy is net profit margin (*NPM*). I measure *NPM* as operating income before depreciation and amortization (*oibdp*) divided by sales (*sale*). The last proxy for profitability is operating profit margin (*OPM*), where this variable is defined as total revenue (*sale*) minus the cost of goods sold (*cogs*) minus selling, general, and administrative expenses (*xsga*) over total revenue. Furthermore, I lead all three dependent variables one period (t+1) since the APM laws likely affect profitability with a lag. Each specification includes firm and industry-year fixed effects and I estimate robust standard errors clustered by state of location.

Panel A of Table B10 presents the results from the regressions of *Profitability* on an *All Item APM Law* indicator variable. Interpreting the results for both the "Manufacturing" and "Products" datasets, I find, in columns (1) and (4), that next year's *ROA* does not increase for affected companies relative to firms in non-APM adopting states. Similarly, in columns (2) and (5), and columns (3) and (6), the respective profitability measures of *NPM* and *OPM* do not increase in the next fiscal year either.

This provides some suggestive evidence that economic rents firms might be earning in a weakened product market competition environment are not simply being allocated to the corporation's bottom line.

As an additional test of the rents-in-and-of-itself hypothesis, I explore the effect of the plug molding protection statutes on affected firms' financial soundness. The idea behind this test is that if competition is reduced and rents are created for protected firms, then, all else equal (including corporate policies), their financial soundness should improve after the adoption of the laws. I proxy for financial soundness using the following three variables. First, I use Altman's Z-score (*Z-score*) which indicates the likelihood of a company going bankrupt or having significant financial distress (for an exact definition see Table A1). The second proxy for financial soundness is an operating cash flow ratio (*OCF Ratio*) measured as operating cash flow (*ocf*) divided by current liabilities (*lct*). The third proxy measure is *Loss*, which is an indicator variable set equal to one if a firm has negative net income (*ni*) during a fiscal year, and zero otherwise (Cain, McKeon, and Solomon, 2017). Similar as above, I lead all three of the dependent variables by one year (t+1), and always specify firm and industry-year fixed effects.

Panel B of Table B10 indicates that the APM laws do not significantly determine next year's financial soundness for protected firms. For example, in column (3), an *All Item APM Law* does not yield an economically (point estimate=-0.015) or statistically significant (t-stat=-1.01) reduction in the likelihood an affected firm will experience a negative net income in the following fiscal year vis-à-vis unaffected firms. Similar results hold in all other specification and in both the "Manufacturing" and

"Products" samples. In sum, the evidence I document in Table B10 suggests that it is unlikely that APM laws create value by increasing the profitability and financial soundness of a protected firm, thereby rejecting the rents-in-and-of-itself hypothesis.

6.2 Innovation Incentives

In the following two subsections, I investigate whether the "innovation incentives" hypothesis of reduced product market competition might explain the positive relation between *Q* and all item APM laws (Aghion and Howitt, 1992 and Davidson and Segerstrom, 1998).

6.2.1 Investment Activity

Under the innovation incentives hypothesis, the weakening of product market competition intensity created by the enactment of reverse engineering protections encourages investments in new and existing production technologies by enhancing the flow and duration of economic rents firms earn as compensation for these operational strategies. To test if this applies to the competitive implications of APM statutes, I consider the effect of these laws on the investment behavior of protected firms.

First, I consider changes in investment in new production technologies for corporations covered by the APM laws using the following three proxies. The first proxy variable measures firm-level investments in R&D *(R&D)* by scaling research and development expenditure *(xrd)* with sales revenue *(sale)* (Chan, Lakonishok and Sougiannis, 2001; Eberhart, Maxwell and Siddique, 2004; and Phillips and Zhdanov, 2012). The second proxy I specify, *CAPX*, is employed to capture the amount of expenditure firm's allocate to undertake new projects and increase their scope of operations (Rauh, 2006). Specifically, I divide capital expenditures *(capx)* with the book value of total assets *(at)*. The last proxy variable used for investment in new production technologies is a firm's investment rate (*Invest Rate*) measured as capital expenditures (*capx*) plus acquisitions (*aqc*) minus the sales of property (*sppe*) over the book value of assets (*at*) (Sanati, 2017).

Panel A of Table B11 reports the DID estimates over the sample period 1975 to 1988, capturing the impact of the APM laws on own-firm investment activity in new production technologies. Consistent with previous specifications I include firm and industry-year fixed effects, where the industry grouping is defined at the two-digit SIC code level, and the estimated *t*-statistics are based on robust standard errors clustered by state of location. Moreover, in each of these models the dependent variables are led one year (t+1) since the effect of the statutes likely won't impact corporate investment policy until the following year (Sapra, Subramanian, and Subramanian, 2014). The first three columns of the table pertain to the "Manufacturing" sample, while the last three are specific to the "Products" dataset.

Column's (1) and (4) of Table B11 provides evidence that when corporations located in plug molding law states become better protected from reverse engineering they respond with increases in *R&D* investments. Specifically, affected firm's *R&D* increases by 6.2% (=0.004/0.065), relative to the "Manufacturing" sample mean, and it rises by an even higher 7.8% (=0.005/0.064) in the "Products" sample. In addition, columns (2) and (5), and (3) and (6) indicate that APM protected businesses also increase their investment activity in *CAPX* and *Invest Rate*, respectively. For instance, the plug molding statute protection yields increases in *CAPX* of 7.7% (=0.005/0.065) and 9.5% (=0.006/0.063), relative to the respective sample means in the two separate datasets. Therefore, these above findings are consistent with the hypothesis that APM laws increased the incentives of companies to allocate capital to long-term investment projects in new production technologies.

Next, I investigate whether changes in the competitive landscape of firms protected by APM statutes brought about changes in investments in existing production technologies. I use three proxy variables for this second batch of tests. The first variable employed to capture changes in firm-level investment behavior in existing production technologies is changes in advertising (*Advertise*) measured as advertising expenditure (*xad*) divided by sales (*sale*) (Bizjack, Brickley, and Coles, 1993; Coles, Lemmon, and Meschke, 2012). The second proxy variable is a measure of organizational capital (*Organize*) defined at the ratio of selling, general, and administrative expenses (*xsga*) to total assets (*at*) (following Eisfeldt and Papanikolaou, 2013). The third proxy measure I employ is labor intensity (*Labor*), which captures changes in a firm's human capital and is measured at the number of employees (*emp*) divided by real total assets (*at*) (Dewenter and Malatesta, 2001), where assets are adjusted using (inflation-adjusted) 2015 dollars.

Panel B of Table B11 presents the pooled panel regressions of *Q* on each of these three proxies for investments in existing production technologies over the period 1975 to 1988, where all of the dependent variables are led by one year. In each of these models (1) – (6), I include the full set of controls, firm and industry-year fixed effects, and estimate robust standard errors with clustering by state of location. Consistent with my conjecture under the innovation incentives hypothesis, I find in four out of the six specifications that firms located in states with an APM law experience a statistically significant increase in investments in existing production technologies. For example, in columns (3) and (6) companies better protected from reverse engineering have increases in next year's labor capital of 6.3% (=0.001/0.016) and 5.9% (=0.001/0.017) relative to the respective "Manufacturing" and "Products" sample means. I thus conclude that Table B11 provides evidence consistent with the innovation incentives hypothesis of product market competition.

6.2.2 Innovative Ability

In this subsection, I continue my evaluation of the sources of value of all item APM laws by considering their heterogeneous effects on companies that have greater innovative ability. According to Aghion and Howitt (1992) and Davidson and Segerstrom (1998), these firms can arguably be expected to experience the greatest amount of increase in their investment incentives with the decrease in product market competition engendered by the plug molding legislation. I therefore conjecture that if the innovation incentives hypothesis can explain the value added by the introduction of APM statutes, this value should be more prominent for this subset of firms. I employ the research quotient (*RQ*) measure proposed by Knott (2008), and provided on WRDS for the period 1971 to 2015, to capture the innovative ability of the manufacturing businesses in my sample. As described in Table A1, *RQ* estimates the output elasticity of R&D (i.e., how successful are corporations at converting R&D into sales revenue). In additional robustness checks, I create two indicator variables from the continuous measure *RQ*: *RQ Median* and *RQ High*, which are set equal to one if the company's research quotient is above the sample-year median or 66th percentile, respectively, and zero otherwise. Moreover, in each specification I include firm and industry-year fixed effects, the full set of control variables, and estimate robust standard errors with clustering by state of location.

In the first four columns of Table B12, I find suggestive evidence that the positive value relevancy of APM statutes is attributable to “Manufacturing” firms with higher levels of innovative ability. For example, in columns (2) and (3), using the indicator variables *RQ Median* and *RQ High*, I find strong statistical evidence the companies with the greatest ability to convert R&D into sales are the sole beneficiaries of the increases in *Tobin’s Q*. Furthermore, as expected, the point estimate magnitudes increase monotonically from columns (2) to (3), consistent with the hypothesized mechanism. Lastly, in column (4), I specify the full regression model, interacting *All Item APM Law* with *RQ High* and *RQ Low*, respectively; *All Item APM Law* × *RQ Medium* and *RQ Medium* are omitted to avoid perfect multicollinearity. Relative to the sample mean, *Tobin’s Q* increases by 7% (=0.108/1.542) for firms with high levels of innovative ability in APM statute adopting states, while companies with low innovative ability do not experience any value gains (point estimate=0.003 and *t*-stat=0.17).

The last four columns of Table B12 pertain to the “Products” sample, and, as anticipated, the interpretations are even stronger in this dataset. In column (5), I document a one standard deviation increase in *RQ* yields a 7.5% (=0.706 × 0.107) rise in *Tobin’s Q* for firms located in APM law states. Meanwhile, decomposing the continuous *RQ* measure into *RQ Median* and *RQ High* indicator variables, in columns (6) and (7), shows that corporations in the respective upper median or upper tercile of innovative ability accrue the entirety of the gains in long-term value. I specify the full tercile split model in column (8), and find that, relative to the sample mean, high *RQ* companies headquartered in adopting states, have increases in *Tobin's Q* of 9.9% (=0.153/1.542), while the low innovative ability firms gain nothing from the laws.

The above analysis is carried forward into Table B13, where I explore how the value of firms with higher levels of innovative ability is impacted by the invalidation of APM statutes by the U.S. Supreme Court. In particular, I repeat the analysis performed in Table B12, except now I expand the panel to 1992, and interact *All Item APM Law* × *Innovative Ability* with an indicator variable equal to one if the year is 1989 and afterwards (*Post 88*). Columns (1) – (8) include the full set of control variables and firm and industry-year fixed effects. Further, the first four columns pertain to the "Manufacturing" dataset, whereas the last four are specific to the "Products" sample. I report estimated *t*-statistics based on robust standard errors with state of location clustering in parentheses.

The findings in columns (1) – (4) suggest companies with higher levels of output elasticity of R&D are adversely affected by the removal of reverse engineering protections. Furthermore, consistent with the innovation incentives hypothesis, the coefficient estimates are greater in absolute magnitude and significance for *RQ High* businesses relative to *RQ Median* firms. Meanwhile, the value of the least innovatively able companies in these states are unaffected by the invalidation of the APM legislation. Qualitatively similar results obtain in the "Products" sample, presented in the last four columns.

I conclude that, overall, the evidence across Tables B11 – B13 suggests that a reduction in product market competition is beneficial to shareholder value as it increases the investment incentives of innovative firms.

7. Robustness Tests

7.1 Firm Value Dynamics

I begin the robustness analysis in Table B14, by studying the timing of changes in long-term firm value relative to the timing of the adoptions of the APM statutes to gauge whether the parallel trends assumption is likely satisfied. In addition, this test also sheds some light on the problem of lobbying contaminating the regression estimates, as it is likely the case these motivating agents would have inside information about the likelihood of the laws' passage and would relay this knowledge to their corporate employers prior to their enactment. In each column I include the full set of controls and firm and industry-year fixed effects. Further, in columns (2) - (3) and (5) - (6), I append a state time trend variable to control for time varying state-level factors that might influence APM statute adoption.

In these tests, I regress *Tobin's Q* on the following: an indicator variable equal to one if a firm is located in a state that will adopt an all item APM statute in one year and equal to zero otherwise, *All Item APM Law*$^{[-1]}$; an indicator variable equal to one if a firm is headquartered in a state that adopts an APM statute in the current year and equal to zero otherwise, *All Item APM Law*$^{[0]}$; an indicator variable equal to one if a firm is located in a state that adopted an APM statute one year ago and equal to zero otherwise, *All Item APM Law*$^{[1]}$; and an indicator variable equal to one if a firm is located in a state that adopted an APM statute two or more years ago and equal to zero otherwise, *All Item APM Law*$^{[2+]}$. Hence, if the point estimate on *All Item APM Law*$^{[-1]}$ (the placebo estimator) is statistically significant, there are serious concerns about the validity of the parallel trends assumption.

The first three columns report results for the "Manufacturing" sample. Reassuringly, in columns (1) - (3) the point estimate on *All Item APM Law*$^{[-1]}$ is statistically and economically insignificant. This provides suggestive evidence, that absent the adoption of the APM statutes, *Tobin's Q* for firms headquartered in enacting states would have evolved in a similar fashion to those located in states without such legislation. Moreover, the positive and significant coefficients on *All Item APM Law*$^{[1]}$ and *All Item APM Law*$^{[2+]}$ indicates increases in firm value transpires *after* protection from reverse engineering is granted. In columns (4) - (6), I find qualitatively similar results in the "Products" dataset. That is, the placebo estimator remains both statistically and economically insignificant while the indicator variables *All Item APM Law*$^{[1]}$ and *All Item APM Law*$^{[2+]}$ suggest increases in *Tobin's Q*.

7.2 Matched Sample

The next robustness check shifts to assessing the reliability of my main finding in a matched sample. Indeed, since I employ a fairly long-panel (17 years) a potential concern is that some other confounding events or differences in observed and unobserved firm characteristics might be correlated with both the adoption of APM laws and firm value, potentially creating a spurious correlation between *Q* and *APM Law*. Additionally, corporations more reliant on intellectual property protection might self-select into states with plug molding legislation, making the control group of firms a poor counterfactual for testing the causal effect of these laws.

In constructing my matched sample, I consider treated and control firms with equidistant pre- and post-estimation windows surrounding the adoption date of the all item APM statutes. In particular, I match all sample firms in each of the states that enact an all item APM law to a control firm in a state that does not have such legislation during the five-year period after the APM legislation is adopted in the treated firms' state of location. This matching procedure is conducted in the year prior to the adoption date of each of the three all item APM laws. I use propensity scores with nearest neighbor matching on *Q*, *Size, Ln(Age), HHI, Sales Growth,* and *Loss*, as well as *Ln(Patent)* to proxy for the importance of intellectual property protection to address the concern of a self-selection effect. In addition, I use exact matching on two-digit SIC codes.

Panel A of Table B15 presents the pre-treatment year summary statistics for the resultant matched sample. Columns (1) and (2) show the means and standard deviations (in parentheses) of the matching variables for the "Manufacturing" sample. I then present the differences between the treated and control group variables and corresponding *t*-statistics (in parentheses) in column (3). Columns (4) , (5), and (6) report the analogue statistics for the "Products" dataset. The panel shows that the treated and control groups are insignificantly different from one another for each of these characteristics. Hence, the matched sample mitigates the two concerns surrounding the relatively long-pooled panel analysis discussed above. Panel B of Table B15 presents the means, standard deviations, and number of observations of the matched variables used in the full matched sample for both the "Manufacturing" and "Products" datasets.

Panel C of Table B15 reports the matched sample difference-in-differences estimates of a $Treat \times Post$ interaction term on *Q*, where *Treat* is always equal to one for firms located in a state with an all item APM law, and zero otherwise, and *Post* is set equal to one in the one year after the enacting states' adoption date, and zero in the one year period before. I include firm and industry-year fixed effects in all four columns but exclude the individual *Treat* and *Post* terms due to their multicollinearity with the respective fixed effects and estimate standard errors with clustering by state of location. Columns (2) and (4) specify the full set of control variables for firm and industry characteristics.

In column (1), without including the control variables, I find that the treated firms experience economically and statistically significant increases in *Q* of 7.4 percentage points relative to the matched controls over a ± one-year estimation window, where the year of adoption is excluded from the panel. This represents a substantial 6.1% (=0.074/1.208) increase in firm value relative to the matched sample mean value of *Q*.[21] Consistently, when I estimate the fully specified model in column (2) I find a positive impact of *Treat* × *Post* on *Q* relative to the control group over the (*t*-1) to (*t*+1) period. Similar findings hold in columns (3) and (4) for the "Products" sample, but with larger coefficient magnitudes, indicating that less noise from "non-product" firms is obfuscating the regression estimates. Overall, I find robust evidence in both the pooled panel and matched sample *Q* regressions that a reduction in product market competition increases firm value.

7.3 Non-Manufacturing Companies

Another interesting robustness test is to repeat the analysis performed in Tables B6 and B7, but, instead of focusing on manufacturing firms, consider all other non-manufacturing companies (firms outside of the 2000 to 3999 SIC code range). This falsification test has the advantage of objectivity, as a corporation either is or isn't

[21] The matched sample average *Q* is much smaller than the average in the pooled panel. This is an artifact of both increasing *Q*s over time, and the three all item APM laws being enacted, and thus matched, earlier in the time series (1978-1983).

located in an adopting state, and either does or doesn't operate in the manufacturing industry.[22] In addition, there is only one sample employed in this test, "Non-Manufacturing", since this dataset also includes non-products-based businesses outside of the manufacturing sector. Table B16 presents the results.

In columns (1) and (2), I do not find a statistically significant effect of *All Item APM Law* on *Tobin's Q* for non-manufacturing firms headquartered in adopting states over the period 1975 to 1988. In particular, column (2) finds that, after controlling for the full set of covariates and firm and industry-year fixed effects, the regression point estimate is economically (-0.012) and statistically (*t*-statistic=-0.14) insignificant. In the next two columns, (3) and (4), I expand the panel out until 1992 to investigate a differential effect after the Supreme Court's ruling. However, both columns indicate the lack of an impact on *Tobin's Q*. Specifically, in column (4), both the *Post 88* × *All Item APM Law* (*t*-statistic=0.85) and *All Item APM Law* (*t*-statistic=-0.27) point estimates are insignificant, as is the joint effect (*t*-statistic=0.24) for the full period 1975 to 1992. These results are reassuring that some state-level unobserved trend in economic conditions is not driving the positive value effects in the main analysis since it should also manifest itself in this sample.

7.4 Portfolio Analysis

As a further robustness check to the pooled panel regressions, I investigate the relation between APM statutes and firm value using a long-run stock return event study. To do so, I construct the following calendar time portfolios. First, I use corporations located in *All Item APM* adopting states and within the "Manufacturing" and "Products"

[22] Furthermore, Villalonga (2004) documents that manufacturing firms are less likely to engage in strategic accounting when classifying their industry affiliation.

samples, respectively, to create two long portfolios. Next, I construct two corresponding short portfolios, by including stocks of firms headquartered in neighboring states that were falsely assigned protection in the "Neighboring State Manufacturing" and "Neighboring State Products" datasets. Figure B3 and Panel C of Table B1 show the neighboring state assignment. Further, I am able to create one additional short portfolio using non-manufacturing businesses' stocks that are located in states with all item APM law protection, coming from the "Non-Manufacturing" sample.

For each of these five portfolios, I either long or short the respective stocks 12-months prior to the adoption of the state's corresponding statute and continue these investments until 36-months after the laws are passed ("12m36"). Finally, I construct long-short portfolios by differencing the portfolio returns of the long and short portfolios for each respective month. In order to estimate the monthly abnormal returns *(Alpha)*, I use the four-factor Carhart (1997) and three-factor Fama-French (1993) models. Moreover, I define the market factor using the CRSP value-weighted index.[23] All *t*-statistics are estimated using robust standard errors and are presented in parentheses below *Alpha*. Table B17 provides the results.

In Panel A of Table B17, the portfolio analysis focuses on stocks bought from all item affected states in the "Manufacturing" sample, and pseudo affected stocks sold short from the "Neighboring State Manufacturing" dataset. In both the four-factor and three-factor models, I find positive and significant *Alpha* for the long and long-short portfolios. In contrast, the short portfolio does not produce statistically significant abnormal returns. Next, in Panel B, the *Monthly Stock Returns* regressions are

[23] Table G5 in the supplementary appendix reports the results using a CRSP equally weighted index. The results are qualitatively similar.

performed on the "Manufacturing" (results repeated from above for ease of comparison) and the "Non-Manufacturing" portfolios. In these results, I document positive and significant abnormal returns, using both models, for the long portfolios and insignificant results for the short portfolios. Moreover, the long-short *Alpha* in the three-factor specification is positive and statistically significant. Lastly, Panel C reports the regression estimates from the long "Products", short "Neighboring State Products", and long-short portfolios. In particular, the long-short *Alpha* estimated with the four-factor model is positive and statistically significant, representing an 8.2% (=0.687×12) annual abnormal return. In sum, I conclude that the documented positive relation between APM laws and firm value is robust to using equity returns.

7.5 Additional Robustness

I provide additional robustness to the main finding of a positive relation between APM laws and firm value with three supplementary tables in the supplementary appendix. The first additional robustness check verifies the validity of my "Products" sample by regressing *Q* on a sample of "Non-Products" corporations. This additional dataset is created by including all firms in the "Manufacturing" sample while excluding those from the "Products" dataset. This leftover cohort of manufacturing firms consists of companies operating in industries without tangible products that could be copied via the direct molding process (e.g., food, tobacco, textiles, and apparel products). Table G6 reports the regression estimates, where columns (1) and (2) correspond to the period 1975 to 1988, and columns (3) and (4) are for the period 1975 to 1992. In each of these four columns, irrespective of the inclusion of controls, I find an absence of statistical evidence that non-product-based firms located in APM adopting states have higher firm values than those of non-products-based companies without such legislation.

The second additional robustness check is a placebo test over the period 1975 to 1988 where I assign firms headquartered in states neighboring actual adopters', protection from reverse engineering and define these indicator variables as *Neighboring State All Item APM Law* and *Neighboring State Boat Hull APM Law*. My assignment scheme is shown in Figure B3 and cataloged in Panel C of Table B1. For instance, Wisconsin formally adopts an APM statute in 1983, while its neighboring state of Minnesota does not. In this test, I pretend Minnesota also enacts plug molding legislation in 1983 and compare how the value of firms located in its borders evolves relative to corporations located in other non-neighboring states without (actual or placebo) APM statute protection. Furthermore, actual adopting states are always excluded from the analysis. Table G7 reports the results.

In columns (1) and (2), I focus the analysis on the "Neighboring State Manufacturing" sample. From these first two columns it's clear that companies located in neighboring states of actual adopters do not experience the same value gains. Specifically, in the second column with the full set of controls included, I document a positive (0.022) but insignificant (*t*-statistic=0.34) point estimate on *Neighboring State All Item APM Law*. Similar conclusions are reached in columns (3) and (4) with the "Neighboring State Products" dataset.

The final additional robustness check confirms that the main findings in the pooled panel analysis are supported using another alternative measure of firm value: Total Tobin's Q (*Total Tobin's Q*), as proposed by Peters and Taylor (2017). This measure of firm value attempts at explicitly accounting for intangible assets (which are neglected by *Tobin's Q*). Accordingly, it seems particularly useful in assessing my results since I document that investments in intangible assets (i.e., R&D, advertising, organizational capital, and labor) are a key driving force behind the value gains from APM laws.

The first two columns of Table G8 present estimates for the "Manufacturing" sample, while the last two columns are specific to the "Products" dataset. Moreover, columns (1) and (3) correspond to the period 1975 to 1988, when the APM laws were constitutionally valid, while columns (2) and (4) extend the sample to 1992 to capture the invalidation of the laws by a 1989 U.S. Supreme Court ruling. I find that, irrespective of sample period and dataset, all item APM laws remain a significant determinant of firm value for protected firms even when value is proxied for with *Total Tobin's Q*.

8. Conclusion

Existing studies document a positive association between empirical proxies for product market concentration and shareholder value. However, this result is difficult to interpret from reduced form correlations, as industry concentration and firm valuation is endogenously determined.

In this paper, I contribute to the literature on the relationship between product market concentration and firm value by shifting the focus from endogenous proxies to a unique tandem of exogenous events that directly influence the intensity of competition in product markets. I do so by exploiting the quasi-natural experiment provided by the staggered adoption of anti-plug-mold (APM) laws that prohibited an efficient method of reverse engineering products, and thus reduced competition in product markets, in 12 U.S. states over the period 1978 to 1987, and their ensuing invalidation by the U.S. Supreme Court in 1989.

I document that the weakened product market competition environment results in an economically and statistically significant increase in long-term value for the firms located in the adopting states, and especially so for firms with greater levels of innovative ability. Overall, my results are most consistent with what I refer to as the "innovation incentives" hypothesis, under which reduced product market competition increases firm value by increasing the flow and duration of economic rents that incentivize investments in new and existing production technologies.

Chapter 2: Are Some Things Best Kept Secret? The Effect of the Uniform Trade Secrets Act on Financial Leverage

1. Introduction

Survey evidence suggests that trade secrets[24] are the *most* important mechanism to protect businesses' intellectual property (IP). The National Science Foundation's National Center for Science and Engineering conducts the annual Business Research and Development and Innovation Survey (BRDIS) which targets responses from for-profit companies with at least five or more paid employees, a minimum of one business establishment in operation during the survey year, and performs some form of R&D activity all within the United States. One of the survey questions asks the respondent to assess "how important to your company were the following types of intellectual property protection?" (Form BRDI-1, 2013, p.45) with answers ranging from "very important", "somewhat important", to "not important." Table D1 reports the most recently published results in which 57.2% of businesses in all industries said trade secrets were a very important form of intellectual property protection, followed by utility patents (51%), trademarks (43.4%), copyrights (27.2%), and design patents (24.3%). The surveyed level of importance of trade secrets for firms in all industries with some R&D expenditure skyrockets to 93.7%[25] for large businesses defined as having 10,000 or more domestic employees.

[24] Examples of trade secrets include food and beverage recipes, marketing strategies, computer algorithms, business plans, customer contact lists and "leads", and other confidential information that may or may not be patentable and which give the holder of the secret an economic advantage.
[25] Measured by combing the "very important" and "somewhat important" percentages.

In addition to this survey evidence, there exists recent empirical work examining the effect of trade secrets protection on innovative activity. Png (2017a) finds a positive association between stronger trade secrets laws and R&D among large firms, and firms operating in high-technology industries. Further, Png (2017b) and Dass, Nanda, and Xiao (2015), in contemporaneous studies, document a negative relation between increased trade secrets protection and patenting activity. Png shows that firms in complex technology industries covered by strengthened trade secrets laws are associated with 18 percent fewer patents. Meanwhile, Dass et al. find that state-level statutes that augment trade secrets protection results in fewer patent applications for the average firm. What remains an open question in the literature, however, is how do firms finance these increases in non-patented, innovative endeavors?

Our study analyzes the impact of trade secrets protection on capital structure decision-making by comparing the debt ratios of firms located in states adopting stronger trade secrets laws with firms headquartered in states without such legislation. In particular, we investigate the effect of a stronger trade secrets environment on large firms' financial leverage, which, given both the survey and empirical evidence, are most likely to be significantly affected by better protection. Moreover, secrecy is a form of informal IP designed to protect appropriation of rewards from invention and innovation (Hall, Helmers, Rogers, and Sena 2014).[26] Thus, big firms generating larger sales revenue should be differentially impacted by laws that increase appropriability (Png 2017a). Further, small firms disproportionately rely on patents (Figueroa and Serrano 2013) rather than on secrecy as it provides IP protection at a lower cost. This motivates

[26] Hall et al. (2014) define the main forms of formal IP as patents, trademarks, designs, and copyright, whereas informal IP can take the form of secrecy, confidentiality agreements, lead time, and complexity.

our study to consider how large firms adjust their financial leverage after becoming covered by trade secrets laws.

There are at least two ways in which trade secrets protection could potentially influence large firms' financial leverage. On one hand, prior work finds that firms in which R&D is an important form of investment, fund this activity almost entirely with cash holdings and/or equity capital (e.g., Nelson 1959, Arrow 1962, Bradley, Jarrell, and Kim 1984, Titman and Wessels 1988, Opler and Titman 1994, Alderson and Betker 1996, Chung and Wright 1998, Hall 2002, Brown, Fazzari, and Petersen 2009, Hall and Lerner 2010, Brown, Martinsson, and Petersen 2013, and Chava, Oettl, Subramanian, and Subramanian 2013). This is consistent with theories suggesting that innovative firms plagued by informational problems (Akerlof 1970, Leland and Pyle 1977, Stiglitz and Weiss 1981, Bhattacharya and Ritter 1983, and Anton and Yao 2002), moral-hazard dilemmas (Jensen and Meckling 1976), and limited collateralizable assets (Williamson 1988, Berger and Udell 1990) are less likely to use debt financing. Thus, we might expect large firms experiencing strengthened trade secrets protection to reduce financial leverage.

On the other hand, large firms treated with greater trade secrets protection are less susceptible to a rival firm misappropriating their economically valuable, confidential information. The use of secrecy as a mechanism to protect IP is inherently risky. Trade secrets can be legally acquired if a competitor firm independently discovers or reverse engineers the same coveted information (Png 2017a). Consequently, the competitor firm could patent the newly acquired secret, if patentable, thus revoking the initial firm's ability to continue to use the secret, as specified by patent law (Jaffe 1986, Friedman, Landes, and Posner 1991, and Hall et al. 2014). This would be legal. However, under strengthened trade secrets laws improper means of misappropriation are illegal, reducing the likelihood of diminished future cash flows generated by the secret. Hence, we might expect that large firms affected by increases in trade secrets protection have reduced financial distress costs – i.e., they are less likely to default since they are less likely to lose out on future cash flows – and therefore, trade-off these lowered costs with the benefits of increasing financial leverage (Miller 1977).

We exploit the staggered state-level adoption of the Uniform Trade Secrets Acts (UTSA) between 1975 and 2003 to isolate the causal effect of trade secrets protection on capital structure decision-making. The UTSA increased the protection of firm's trade secrets by codifying the existing common law, precisely defining a "trade secret", enumerating what constitutes misappropriation, and clarifying the rights and remedies of victimized firms (Uniform Law Commission, 1985). Figure D1 depicts the number of states that have passed these statutes by decade. Further, we proxy for trade secrets protection using a state-level index constructed by Png (2017a) which accounts for pre-existing common law, and represents the change in legal protection resulting from the enacted UTSA.[27] We find that large firms, measured by the natural logarithm of sales, protected by stronger trade secrets laws increase their debt ratios. Specifically, using a difference-in-differences framework, we find that once large firms become covered by UTSA, their book and market leverage ratios are increased by 3.85 (= 0.018 × 2.137) and 2.14 (= 0.010 × 2.137) percentage points, respectively, for every one standard deviation increase in the natural logarithm of sales. The results are robust to alternative

[27] Table C1 in the appendix, which is an exact reproduction of Table A2 from the appendix of Png (2017a), provides a full description of the construction of the measure. In addition, we provide a concise explanation of the protection index in Section 4.2.

definitions of financial leverage, and to alternative proxies for firm size which includes the natural logarithm of total assets and the total number of employees, respectively, and splitting the size proxy into indicator variables based on median and median-year sales. Further, we show that the positive change in the debt ratios transpires *after* the passage of the UTSA law, assuaging concerns of lobbying or anticipatory leverage adjustments. In addition, a Cox proportional hazard analysis shows that firm-level, state-level, and industry-level debt measures do not explain the decision for a state to adopt the UTSA, suggesting that reverse causality does not contaminate the estimates.

In further tests, we investigate if the interaction of the UTSA and firm-specific innovative activity also determines the level of financial leverage. In particular, we analyze firms affected by the UTSA that are characterized as having high R&D intensity, and existing patent portfolios. This added layer of analysis is beneficial in understanding the underlying relationship governing our main finding that large UTSA protected firms increase debt ratios. The only negative relation we document between leverage and increased trade secrets protection is for high R&D intensity firms. Thus, it appears, without differentiating on size, firms with greater levels of pre-existing R&D expenditure decrease debt after the passage of the UTSA, which is consistent with the extant literature on R&D and its financing (e.g., Bradley, Jarrell, and Kim 1984, among others). In contrast, UTSA protected firms with large pre-existing patent portfolios increase debt. This seems on par with recent work documenting a negative relation between the UTSA and patent applications (Png 2017b, and Dass et al. 2015). That is, large innovative firms potentially transition to or increase their usage of secrecy after the passage of these laws, and they do so with debt.

Overall, our results suggest that increased trade secrets protection affects larger firms' debt ratios by decreasing their probability of default. Specifically, we analyze the relationship between the UTSA and the sensitivity of changes in earnings to changes in sales to capture the level of a firms' operating leverage, and find that it is lower following the enactment of the UTSA. Further, we find a significant negative relation between large UTSA protected firms and the inverse of modified Altman's Z-score, and operating cash flow risk, respectively. Next, we investigate the effect of UTSA specific to firms characterized by higher likelihoods of default on debt ratios and find these companies adjust their book and market leverage upward. We conclude that large firms are differentially affected by the UTSA, and as such the inherently risky but rewarding IP protection mechanism of secrecy becomes less dangerous. Accordingly, companies optimally respond by financing increased innovative activity with leverage. Finally, we provide evidence that there exists positive long-term firm value implications for large firms headquartered in these UTSA adopting states.

This paper makes new and important contributions to several strands of the literature. First, we provide new evidence on the impact of the UTSA for large firms and their capital structure decision-making. We are the first to document this specific relationship, but one of two contemporaneous studies to investigate the general effect of an increase in trade secrets protection on leverage. Klasa, Ortiz-Molina, Serfling, and Srinivasan (2018) consider the recognition of the Inevitable Disclosure Doctrine (IDD) by U.S. state courts, which decreases the mobility of workers with trade secrets knowledge from gaining similar employment with a rival firm. They argue that firms in which trade secrets are an important IP mechanism retain unused debt capacity in case a competitor gains access to the secret. Thus, the risk of losing IP to rivals is reduced after rulings in favor of the IDD and as such firms' capital structure decisions are less conservative.

Our study differs from Klasa et al. (2018) in the following seven ways. First, we make use of exogenous variation stemming from the staggered passage of the UTSA, whereas they consider the IDD. These experiments are fundamentally different as the former codifies the "rules of the game", which includes the legal remedies for victimized firms and is implemented via the legislative process, while the latter immobilizes employees with trade secrets knowledge and is recognized by state courts. Consequently, they do not necessarily imply the same effect on capital structure decision-making. Second, we find suggestive evidence using a Cox proportional hazard model that the UTSA is a substitute for the IDD, as states with the doctrine in place are less likely to legislate for the statute. This is important as nearly all U.S. states have adopted the UTSA, while less than half recognize the IDD. Again, confirming that these laws are worth studying in isolation. Third, our evidence shows that both laws have separate and significant impacts on debt ratios. That is, we include an IDD indicator variable as a control in all of our tests, and find that the effect of the UTSA on large firms' financial leverage persists. Thus, both experiments have important implications for a firm's capital structure. Fourth, the two studies are methodologically different. We employ an index which accounts for pre-existing common law, whereas Klasa et al. specify a "0/1" dummy. Fifth, we uniquely investigate the impact of trade secrets protection on innovative firms' debt ratios. The evidence from these tests show that companies located in UTSA states with large and meaningful patent portfolios increase their book and market leverage. Sixth, we find evidence that large firms adjust their levels of debt upward because of a reduction in bankruptcy costs. In contrast, Klasa et al. document results consistent with conservatism in unused debt capacity yielding the positive relation between the doctrine and debt ratios. Moreover, they do not find evidence for the trade-off theory of capital structure, and we confirm their result as the IDD does not predict reductions in bankruptcy costs in our sample. Lastly, we find positive long-term value effects for large firms located in UTSA passing states, whereas the IDD indicator is insignificant. On the other hand, Klasa et al. have well-defined event dates which allows them to document positive and significant short-term abnormal returns for firms headquartered in states that recognize the doctrine. Hence, both studies provide incrementally valuable, novel evidence to this important and relatively unexplored strand of literature.

Specifically, our results add to the existing research that uses the UTSA as a source of exogenous variation for secrecy. Other topics of papers in this area include its effect on R&D expenditure (Png 2017a), internal patenting (Png 2017b, Dass et al 2015), and financial disclosure (Guo, Nanda, and Pevzner 2016). Furthermore, we contribute to the trade secrets protection literature, which thus far has primarily employed the IDD setting. These papers consider the impact of the doctrine on capital structure decision-making and its respective channel (Klasa et al.), short-term value implications (Qui and Wang 2017), employee mobility by level of education (Png and Samila 2015), internal patenting activity (Contigiani, Barankay, and Hsu 2016), M&A activity (Gao and Ma 2016), and operational uncertainty (Lin, Wei, and Wu 2016).

Moreover, we broadly contribute to the literature investigating capital structure and its determinants (Myers 1977, Bradley, Jarrell, and Kim 1984, Titman and Wessels 1988, Rajan and Zingales 1995, Alderson and Betker 1995, Leary and Roberts 2005, Frank and Goyal 2008, and Lemmon, Roberts, and Zender 2008, Kisgen 2009, Hackbarth, Mathews, and Robinson 2014, and DeAngelo and Roll 2015, among others), and specifically to studies finding support for trade-off theory (Danis, Rettl, and Whited 2014, Serfling 2016, Glover 2016, and Reindl, Stoughton, and Zechner 2016, among others). Lastly, we add to the literature on financing and innovation (Stiglitz and Weiss 1981, Bradley, Jarrell, and Kim 1984, Stiglitz 1985, Titman and Wessels 1988, Cornell and Shapiro 1988, Williamson 1988, Blair and Litan 1990, Berger and Udell 1990, Hall 1993, 1994, Opler and Titman 1993, 1994, Alderson and Betker 1996, Chung and Wright 1998, Blass and Yosha 2003, Acharya and Subramanian 2009, Brown, Fazzari, and Petersen 2009, Chava, Oettl, Subramanian, and Subramanian 2013, Acharya, Baghai, and Subramanian 2014, and Sapra, Subramanian, and Subramanian 2014).

2. Hypothesis Development

It is unclear how an exogenous increase in trade secrets protection will affect financial leverage for large firms. On the one hand, stronger secrecy protection yielding increases in R&D expenditure (Png 2017a) might bring about a decrease in debt ratios. Inventive firms choose cash holdings and/or equity capital to avoid debt overhang problems and high borrowing costs (Bradley, Jarrell, and Kim 1984, Titman and Wessels 1988, Opler and Titman 1994, Alderson and Betker 1996, Chung and Wright 1998, Hall 2002, Brown, Fazzari, and Petersen 2009, and Hall and Lerner 2010). These findings suggest that the financing decision for R&D dependent firms is predicted by the challenges they face with information asymmetry (Leland and Pyle 1977, Stiglitz and Weiss 1981, Bhattacharya and Ritter 1983, and Anton and Yao 2002), moral-hazard or hidden action (Jensen and Meckling 1976), and reliance on intangible assets which cannot be used as collateral (Berger and Udell 1990). This leads to the hypothesis that large firms protected by the UTSA will reduce their levels of outstanding debt.

On the other hand, an increase in trade secrets protection for large businesses could relate positively with book and market leverage. The use of secrecy as a mechanism to protect IP is optimal if the confidential information is non-patentable and/or the potential returns from the indefinite future cash flows generated by the secret is greater than the in-flow of legally protected finite rewards granted to successful patent applicants (Friedman et al. 1991, and Hall et al. 2014). However, when comparing the potential infinite streams of future returns garnered by the use of secrecy with finite appropriations from patenting, the former should be probability-weighted (Almeida and Philippon 2007) to account for the likelihood that the confidential information is discovered or misappropriated by a rival firm. If the UTSA decreases the likelihood that secrets will be discovered through improper means, this increases the odds that a firm will be able to capitalize indefinitely on their confidential information and correspondingly reduces the probability of default (Andrade and Kaplan 1998), all else equal. Thus, based on this argument an alternative hypothesis is that large firms significantly affected by the UTSA will increase their financial leverage, taking advantage of the benefits of debt (Miller 1977).

3. Institutional Background

3.1 The Uniform Trade Secrets Act (UTSA)

To assist in the improved protection and codification of trade secrets laws, the Uniform Law Commissioners designed and proposed the Uniform Trade Secrets Act (UTSA) in 1979 for state-level enactment. The UTSA was later amended in 1985 and provided the following three major improvements above the previously established common law procedures.[28] First, it more comprehensively defined a trade secret as meaning "information, including a formula, pattern, compilation, program device, method, technique, or process that derives independent economic value, actual or potential, from not being generally known to, and not being readily ascertainable by proper means by, other persons who can obtain economic value from its disclosure or use, and is the subject of efforts that are reasonable under the circumstances to maintain its secrecy" (Section 1.4, p. 5, 1985). The Commissioners further commented on the definition to specify certain refinements. These comments detailed that negative information about failed ideas was valuable and also covered under the act. In addition, works-in-progress, such as ongoing R&D activity, constituted a protected trade secret.

The second major improvement of the UTSA over the general common law of the time, was that it outlined what it meant for a secret to be misappropriated. Section 1.2 of the UTSA prescribes misappropriation of a secret to mean the "acquisition of a trade secret of another by a person who knows or has reason to know that the trade

[28] Prior to the UTSA, the primary governing code for trade secrets protection was established in the Restatement (First) of Torts, which is a treaty specific to this subject matter providing guidance to judges and lawyers in a common law system. Under this code a trade secret was defined to "consist of any formula, pattern, device or compilation of information which is used in one's business, and which gives him an opportunity to obtain an advantage over competitors who do not know or use it" (Section 757, Comment b (1939)). However, although an important historical event in trade secret protection, this formalization was not legally binding and produced conflicting court decisions across states.

secret was acquired by improper means, or disclosure or use of a trade secret of another without express or implied consent by a person who used improper means to acquire knowledge of the trade secret" (pp. 4-5. 1985). The misappropriation of a trade secret through improper means can include bribery, theft, misrepresentation, breach of duty to maintain secrecy, or espionage. This would be considered a form of unfair competition. However, trade secrets can be legally acquired if the covered company involuntarily disclosed the secret, or a competitor firm independently discovered or reverse engineered the prized, clandestine information. Moreover, as specified by existing patent law, the competitor firm could attempt to patent its newly discovered information, disallowing the use of the secret by the originating firm.

Finally, the third major improvement was that the UTSA clarified rights and remedies for businesses which had secrets wrongly appropriated and used. Remedies for infringement include injunctive relief, damages, reasonable royalties, and, in certain circumstances, attorney fees.[29] The UTSA established a statute of limitations upon which any action under the act must be brought forth within three years after the discovery of the misappropriation. Moreover, the UTSA outlines that courts deciding cases should take reasonable precautions to preserve the secrecy of the contested information, and if the UTSA is enacted it supersedes existing state-specific common laws.

[29] Anecdotal evidence suggests that protected firms do prosecute suspected perpetrators and earn sizeable awards for their victimization. For instance, Best Buy, the world's largest consumer electronics retailer, was found liable of stealing corporate secrets from an electronics recycling start-up, TechForward, and forced to pay $27 million (see, Kopelman 2012 for details). Further, a back-of the envelope calculation, in Hall, Helmers, Rogers, and Sena (2014), based on a 2011 federal court ruling in *Kolon Industries Inc. v. Dupont Co.*, suggests an average value of $6.3 million per trade secret.

3.2 The Inevitable Disclosure Doctrine (IDD)

Another important form of state-level trade secrets protection stems from the Inevitable Disclosure Doctrine (IDD). Under this doctrine, firms have the legal ability to obtain an injunction to prevent current or former employees from gaining employment at another company without having to show that the individual actually applied, disclosed or intended to use any of the plaintiffs' trade secrets. Instead, the IDD only requires firms prove that the defendant's new position is one in which trade secrets would inevitably be disclosed (Png and Samila 2015). This key legal distinction, contrasted above, is that of "threatened misappropriation" (Klasa et al. 2018). Thus, if a firm perceives there is a risk of threat of misappropriation by an individual with trade secrets knowledge whom finds work in a similar position at a rival firm, the IDD can be invoked.

By the doctrine, in order for a firm to file suit and obtain an injunction against the individual it must establish the following: (i) the employee worked in some capacity which granted him or her access to the firm's trade secrets, (ii) the role and responsibilities of the employee in their new position is so similar to that which they had at the plaintiff firm, that it would not be difficult to use or disclose the trade secrets, and (iii) the employee and new employer cannot be trusted not to use the trade secrets, and this would cause the former employing firm irreparable economic harm. Again, however, this three-part test does not require the firm to prove any actual wrongdoing.

3.3 Comparing UTSA and IDD

Trade secrecy in the United States is largely governed by state rather than federal law (Pooley 1997-), and the two most important state-level legal precedents, outlined above and considered in the finance and economics literature, are the UTSA and the IDD. The UTSA is passed in the form of a state statute via a legislative process, whereas the IDD is recognized by state courts. Although, the IDD was adopted in some states prior to the UTSA (New York in 1919, Florida in 1960, Delaware in 1964, Michigan in 1966, and North Carolina in 1976), the codification of the UTSA in 1979 strengthened the protective capacity and applicability of the IDD. That is, due to the non-uniformity of general trade secrets common law, prior to the creation of the UTSA, the IDD was subject to state-varying definitions of secrecy and misappropriation which made the doctrine more difficult to cite in judicial proceedings. Hence, IDD adoptions after 1979 follow the same guiding principles specified in the UTSA (Lin, Wei and Wu 2016).

The number of states that have passed the UTSA more than doubles those that recognize the IDD. Figure D2 shows the number of states that have adopted the UTSA and the IDD by year. In total, 46 states have such legislation, whereas the remaining four states without it have either passed their own trade secrets law (North Carolina in 1981, and Wisconsin in 1986) or currently have introduced bills (Massachusetts and New York in their respective 2017 sessions) to adopt this statute. In contrast, 21 states have experienced precedent-setting cases in which their courts recognize the IDD and three instances (Florida in 2001, Michigan in 2002, and Texas in 2003) where judges later reject the doctrine (Klasa et al. 2018). There are no such examples of states later abolishing their UTSA laws. We hypothesize, but do not test, that the reason behind this difference in permanent acceptability is likely due to the controversial nature of the IDD. That is, the UTSA defines secrecy and misappropriability, and, most importantly, the rights and remedies of victimized firms, whereas the IDD reduces employment mobility. There can be a much stronger case made against the equitability and justifiability of the latter than the former, and this likely influenced the three reversals of judicial attitude.

3.4 Evidence on the Exogeneity of the UTSA

We use the UTSA as an instrument to study the effect of an unobservable predictor, namely, trade secrets protection, on capital structure decision-making. The validity of this identification strategy hinges on two crucial components. First, it is important to rule out any anticipatory effects of the passage of the law. That is, we need to test whether or not firms begin adjusting their leverage ratios prior to the adoption of the statute – a violation of the parallel trends assumption. This could be the case as the legislative process requires at a minimum the introduction of the bill, passage at the House and Senate level, before finally obtaining approval by the Governor. In addition, if lobbying is a concern, then firms with motivating agents might observe private information about the likelihood of the UTSA being passed before actual adoption. We attempt to rule this out in Section 5.3, where we construct falsification tests to analyze the dynamics of the effect.[30] In short, we find that leverage ratios for large firms, increases one year or more after becoming better protected, thus mitigating concerns about preemptive capital structure changes.

The second concern is that states enacted the law for reasons specifically related to corporate debt policy (i.e., reverse causality). While less plausible than the above concern, we attempt to address this possibility in the following two ways. First, we

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[30] We include this test later in the paper, because we think it makes the most sense organizationally to explore the dynamics of the effect, after first establishing its existence.

summarize what the literature has found with respect to UTSA adoption and firm-level R&D policy. Lastly, we conduct our own analysis to verify that firm-level, state-level, and industry-level measures of leverage do not explain the passage of the statute.

Png (2017a) provides supplemental analyses addressing the concern of reverse causality, but as it relates to R&D expenditure. First, he follows Romanosky, Telang, and Acquisti (2011) and constructs a scatterplot between the lag of UTSA adoption and R&D growth. He finds no apparent relation between the lag in enactment and the growth of R&D. Further, Png estimates a least squares regression of the legislative lag on R&D growth and finds an insignificant relation. Next, he estimates a Cox proportional hazard model to the effective year of the UTSA in the states between 1979 and 1997. His results indicate that the adoption of these trade secrets protection laws are not significantly related to gross state product, population, state industrial structure, R&D, policies to support R&D (such as R&D tax credits), or pro-business orientation (Republican-dominated legislatures). Hence, there is suggestive evidence that the UTSA was exogenous to firms located in states passing these laws, and specifically to R&D.

We follow a similar approach, but focus on the predictive ability of financial leverage. That is, we estimate a Cox proportional hazard model over the period 1975 to 2003 and specify firm-level book leverage, and average state-year book leverage and industry-year book leverage, among other controls, as potential explanatory variables of the state-level adoption of the UTSA. The passage of these laws represents the failure event in the analysis, and therefore firms headquartered in these states are excluded from the sample after they become better protected by trade secrets legislation. The other control variables include average state-year natural logarithm of sales, an indicator variable equal to one for states that recognize the IDD and zero otherwise, R&D expenditure divided by sales, an indicator variable equal to one if a state offers an R&D tax credit and zero otherwise, average state-year natural logarithm of patents, average state-year modified Altman's Z-score, natural logarithm of state GDP per capita, a state's GPD growth rate, the percent of state-level representatives in the U.S. House of Representatives whom belong to the Republican party in a given year, state property crime rate by year, and a state corruption score. Table C2 in the appendix provides detailed account of these measures. Further, for ease of interpretation, we standardize all of the continuous variables to have a mean of zero and unit variance. The independent variables are lagged one-period (*t*-1). We also include year fixed effects in all of the specifications to control for time varying, unobserved heterogeneity, and the continuous variables are winsorized at the 1% level. We present the results in Table D2.

The columns report hazard ratios for varying specifications of the Cox models. These hazard ratios and their corresponding robust standard errors are clustered by state of location. Columns 1 through 4 provide suggestive evidence that firm-level, state-level and industry-level leverage is not significantly correlated with the adoption of the UTSA. This provides some reassuring initial evidence that reverse causality is not a concern for this identification strategy. Further, there is only one independent variable that seems to predict the failure event in our sample, and that is the IDD dummy. Its hazard ratio ranges from 0.105 to 0.116 with 1% to 5% significance in the four separate specifications. These estimates indicate that firms that have already had IDD laws passed at the judicial level are less likely to legislate for the UTSA. Thus, we provide suggestive evidence that the two might be substitutes, further warranting the need to study both laws and their effects on capital structure decision-making. Overall, we have no reason to believe that using the UTSA as an instrument to identify the effect of trade secrets protection on financial leverage is contaminated by endogeneity.

4. Data and Empirical Methodology

4.1 Sample Selection

The main sample is composed of 80,691 firm-year observations based on 9,553 publicly traded industrial firms, excluding utilities and financial companies (SIC codes 4900-4999 and 6000-6999, respectively), headquartered in the U.S.[31], and without missing data for the main variables of interest over the period 1975 to 2003. We combine financial data from Compustat with the UTSA index constructed by Png (2017a) by state of location and year. The year of enactment, strength of pre-existing common laws, and change in trade secrets protection after passage of the UTSA are shown in Table D3.

Our sample period begins five years before the first state, Minnesota, passes the UTSA, and ends five years after Michigan adopts. Figure D2 depicts the number of states that have enacted the UTSA by year through 2016, and contrasts this with the number of IDD recognizing states. There are five states that pass the UTSA after Michigan: Tennessee in 2000, Pennsylvania in 2004, Wyoming in 2006, New Jersey in

[31] We obtain data on a firm's state of location from Compustat. Unfortunately, these sample points are specific to the current headquartering state, and do not provide historical information. This would be a concern if firms relocate, as some observations would be wrongly classified as being either a treated or controlled unit, when in fact they are not. However, it does not appear that firms switch headquartering states often. For example, Pirinsky and Wang (2006) find, over a 15-year period, that less than 2.4% of firms changed their state of location.

2012, and Texas in 2013. However, we truncate the sample at 2003 and exclude treatment-years for firms headquartered in these states for the following reasons. First, the two most recent states to adopt the UTSA, New Jersey and Texas, are not included because we do not have data on the UTSA trade secrets protection index after 2010. Second, Tennessee, Pennsylvania, and Wyoming treatment-year observations are left out of the sample because there is little gained by their inclusion. Namely, the number of additional treatment observations by including these firms is less than 5% of the total treatment sample, and, further, extending the sample to 2010[32] potentially creates noise that interferes with isolating the effect of trade secrets protection on financial leverage.[33] This is especially true in our empirical framework, which specifies a staggered difference-in-differences methodology.

4.2 The Main Explanatory Variables

Trade secrets protection, prior to the UTSA, was derived from common law. Therefore, it would be inaccurate to characterize the level of protection for businesses located in states with and without UTSA laws using a "0/1" indicator variable. This is the case for both treatment and control firms. Namely, there are firms headquartered in states without UTSA, but with pre-existing common law. Therefore, it would be incorrect to specify their level of protection with a "0".[34] Further, most companies covered by the UTSA, similarly, had pre-treatment protection under common law. In

[32] Png (2017a, 2017b) constructs the trade secrets protection index from 1970 until 2010. We thank Ivan Png for making this data available: https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/BFP2IC.

[33] In robustness checks, we find that our main results hold over the sample periods: 1975 to 2005 excluding PA and WY treatment-years, 1975 to 2009 excluding WY treatment-years, and 1975 to 2010 including all treatment-years.

[34] Karpoff and Wittry (2017) investigate the misspecification of regression models analyzing the effect of business combination laws on various corporate outcome variables, and show that not accounting for legal and institutional context can lead to substantial biases that alter interpretations. Specifying trade secrets protection with Png's index mitigates this potential bias.

order to cleanly identify the effect of trade secrets protection on financial leverage it is necessary to account for this state and year variation in strength of secrecy.

We follow Png (2017a, 2017b) and use his state-level index of protection, which represents the *change* in strength of trade secrets protection stemming from enactment of the UTSA. Png constructs the index based on three main dimensions: (1) substantive law, (2) civil procedure, and (3) remedies. Further, within the substantive law and remedies dimensions there are three and two items, respectively, that characterize a state with stronger protection.[35] Png codes four of these items a "0" or "1" dependent on the strength and language of the laws and procedures. The other two are ratios of years allowed in civil procedures or years included in remedy calculations divided by three and six, respectively. Each of these values are summed and then divided by six, yielding a scaled protection index between 0 and 1, with a higher score representing stronger legal protection of trade secrets. The *change* stemming from the UTSA is the difference between the index pre- and post-enactment.[36] This represents half of our main variable of interest.

The other remaining half is size. As noted in the introduction, there is a positive monotonic relation in the BRDIS survey data between the importance of the trade secrets mechanism for IP protection and the number of domestic employees. In addition, Png (2017a) finds that UTSA by itself is not significant in determining R&D expenditure, but only once he differentiates on firm size does the relation become significantly positive. Moreover, large firms are more likely to be impacted by the

[35] Please refer to Table C1 in the appendix, which is a reproduction of Table A2 in the appendix of Png (2017a), for a detailed account of the dimensions and items.
[36] In robustness tests, we append Png's specification to include the pre-enactment *level* of trade secrets protection, in addition to the *change* variable, *UTSA*, and find the results are nearly identical.

increase in trade secrets protection as they tend to have a greater reliance on secrecy than do small firms who disproportionately sell and acquire patents (Figueroa and Serrano 2013). Following, the lead of Png (2017a) we interact the UTSA protection index with the natural logarithm of sales to create the main explanatory variable, $UTSA \times Ln(Sales)$. However, since we are interacting two continuous variables we center $Ln(Sales)$ by differencing firm-year sales with its sample average. This is consistent with Png (2017a) and allows for more meaningfully interpretation of the coefficients of interest. For robustness, we also proxy for size using the continuous measures of natural logarithm of total assets (*Ln(Assets)*) and total employees (*Ln(1+Employees)*), both centered by their sample means, respectively, and with indicator variables that equal one for firms with *Ln(Sales)* greater than the sample median, or the sample-year median, respectively, and zero otherwise.

4.3 The Dependent Variables

In this paper, we measure financial leverage in the following two ways. First, we use *Book Leverage* which is defined as the ratio of total debt to the book value of assets for each firm-year. According to Graham and Harvey (2002), most managers pay particular attention to book leverage as opposed to market leverage when making decisions regarding their firm's capital structure. In addition, Welch (2004) documents that much of the variability in market leverage ratios is derived from changes in market values instead of actual debt policy alterations. However, to provide further robustness to our findings, we also measure *Market Leverage* using the ratio of the book value of total debt divided by the market value of assets for each firm-year. In robustness checks, we also consider the natural logarithm of total debt, net book leverage, and net market leverage as dependent variables, respectively.

4.4 Other Explanatory Variables

The other explanatory variables are those widely accepted and documented by the literature as theoretically and/or empirically showing to significantly associate with leverage (e.g., Harris and Raviv 1991, Rajan and Zingales 1995, Frank and Goyal 2008, Lemmon, Roberts, and Zender 2008, Kisgen 2009, Danis, Rettl, and Whited 2014, Matsa 2010, Agrawal and Matsa 2013, Gormley and Matsa 2014, DeAngelo and Roll 2015, Serfling 2016, and Klasa et al. 2018). We include the log of sales (*Ln(Sales)*), assets (*Ln(Assets)*), or total number of employees (*Ln(1+Employees)*), depending on which variable is interacted with UTSA, to control for firm size. We control for a firm's investment opportunities using its market-to-book ratio (M/B). *Profitability* is specified in the regression model to account for the availability of internal funds. We include *Fixed Assets* to control for firm tangibility. We also specify a dummy variable for whether a firm paid out earnings as a dividend to proxy for the level of financial constraint (*Div Payer*). Modified Altman's Z-score (*Mod Z-score*) is added as a regressor to control for firm-level financial soundness; as noted in Mackie-Mason (1990), Altman's Z-score includes the ratio of market equity to book debt, thus he proposes to exclude this term when studying capital structure, as the debt ratio directly enters the analysis as a dependent variable.

Another important independent variable that we specify in the model is an indicator variable equal to one if a state recognizes the Inevitable Disclosure Doctrine (IDD), and zero otherwise. In a contemporaneous paper, Klasa et al. (2018) find that the

IDD is a positive, significant predictor of financial leverage. Moreover, in Section 3.2 of this paper we document empirical evidence that IDD states are less likely to legislate for adoption of the UTSA. Hence, to avoid omitting a relevant variable we directly specify this dummy in the model as a control. This is further interesting, as it will provide direct evidence if the UTSA has explanatory power for firm-level financial leverage, above and beyond that of the IDD.

Lastly, to control for state, political, and industry conditions, we follow Serfling (2016), and include state-level GDP per capita (*Ln(State GDPPC)*), one-year state-level growth in GDP (*State GDPG*), and the proportion of state-level representatives in the U.S. House of Representatives whom belong to the Republican party (*Republican*), and, following Giroud and Mueller (2010), we include the average industry-year leverage (*IY Leverage*), and state-year leverage (*SY Leverage*), excluding firm *i* from both calculations, where industry is defined at the three-digit SIC level. Table C2 in the appendix provides a more precise account of the variables used in the analyses. All continuous variables, with the exception of the *UTSA*, state-level economic and political variables, are winsorized at the 1^{st} and 99^{th} percentiles to remove the influence of extreme outliers, and the dollar values have been deflated using 2001 dollars.

4.5 Empirical Methodology

Since the UTSA is adopted in a staggered fashion by different states over different times in the sample, we employ a difference-in-differences framework to study the relationship between the large firms protected by the UTSA and leverage at the firm-year level (Bertrand, Duflo, and Mullainathan 2004). We estimate the following panel regression model:

$$Leverage_{isjt} = \gamma_i + \omega_t + \beta_1 UTSA_{st} + \beta_2 Size_{it} + \beta_3 (UTSA \times Size)_{ist} + \alpha X_{isjt} + \varepsilon_{isjt}\,, \tag{1}$$

where i indexes firms, s indexes the state of location, j indexes industry, t indexes time, $Leverage_{ist}$ is the dependent variable, which is either $Book\,Leverage$ or $Market\,Leverage$. $UTSA_{st}$ is a continuous variable, scaled between 0 and 1, which accounts for pre-existing trade secrets protection by measuring the change in strength once the UTSA law is enacted in year t in state s.

The main variable of interest is $(UTSA \times Size)_{ist}$ which interacts the index of trade secrets protection with a proxy for the size of firm i, located in state s, in year t, where $Size_{ist}$ is the natural log of sales deflated using 2001 dollars and centered around its sample mean.[37] X_{isjt} is a vector of control variables detailed in the above Section 4.4. We include firm fixed effects γ_i to control for time invariant unobservable heterogeneity within different firms. Further, we control for time variant heterogeneity that could affect leverage for all firms as well as transitory unobservable factors that could impact the likelihood of state adoption of the UTSA using year fixed effects ω_t. We estimate robust standard errors clustered at the state of location level (Bertrand et al. 2004).

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[37] Without centering *Ln(Sales)*, β_1 would represent the effect of the UTSA for a firm with zero sales on leverage. By subtracting the sample mean from firm-year sales, β_1 becomes the effect of UTSA for a firm with average sales on leverage. There is no need to center *UTSA* since there are instances in which firms in both UTSA passing and non-passing states experience zero change in trade secrets protection. Thus, β_2 represents the relation between the *Size* of a firm without any change in protection and financial leverage. Finally, β_3 represents the effect of UTSA on corporate debt policy as firms get larger. For a more in-depth analysis on specifying regression models with continuous interaction terms please refer to Jaccard, Wan, and Turrisi (1990), Aiken and West (1991), and Jaccard and Turrisi (2003).

5. Empirical Results

5.1 Descriptive Statistics

Panel A of Table D4 reports the summary statistics for the variables used in the main analyses. From this table it is observed that the mean book leverage ratio is 23.4% and the average market leverage ratio is 25.9%. Further, the proportion of treatment-years in our sample is 39.9% where the average *change* in the protection index after the enactment of the UTSA is 0.236. In contrast, the *level* of pre-existing state-level common law offers a substantially lower 0.116 degree of protection. The other control variables means and medians are similar to other studies (e.g., Kisgen 2009, Danis, Rettl, and Whited 2014, Frank and Goyal 2014, and Serfling 2016).

Panel B of Table D4 provides the temporal distribution of total firm-year and treatment-year observations, as well as the percentage of firms affected by the UTSA in a given year. The pre-treatment period begins in 1975, with a total of 2,177 firm-year sample points. Then, in 1980, 55 firms (2.54% of the sample) headquartered in Minnesota enter the treatment sample. As more and more states implement the UTSA, the number of treatment-year to total firm-year observations grows, reaching more than 51% of the sample in 1990. The final treatment state, Michigan, passes the trade secrets legislation in 1998. Overall, the sample includes 32,153 treatment-year observations.

5.2 UTSA, Firm Size and Financial Leverage

We present the results from the main analysis exploring the relation between large firms covered by the UTSA and book leverage in Panel A of Table D5. First, however, we estimate model 1 without the interaction term to assess the effect of coverage by the trade secrets law for the average firm on book debt policy. Although, as seen from column 1, the UTSA coefficient is insignificant using this specification. This result indicates that the UTSA, by itself, does not impact capital structure decision-making. This finding is consistent with Png (2017a), whom finds that the UTSA is an insignificant determinant of R&D expenditure for the average firm, the BRDIS survey evidence which indicates only half of the respondents, who actually perform some form of R&D, found secrecy a "very important" form of IP protection, and Figueroa and Serrano (2013) who show that smaller firms acquire and sell patents disproportionately more than large firms. Meanwhile, the coefficient on the IDD dummy is identical in significance, and nearly in magnitude to that found by Klasa et al. (2018), providing further evidence that our sample is consistent with the extant literature.

Next, we explore the main competing hypotheses of the paper, analyzing the relation between large firms covered by the UTSA and book leverage in columns 2 – 6. Column 2 regresses book leverage on the interaction term, the UTSA index, and natural log of sales, and standard leverage controls (*Size*, *Profitability*, *M/B*, and *Fixed Assets*) along with firm and year fixed effects. The estimated coefficient on the main variable of interest is 0.020 and significant at the 1% level. Next, we sequentially add further leverage determinants, as well as state, political, and industry controls in the remaining columns. Column 3 includes additional firm-characteristic controls (*Div Payer*, and *Mod Z-score*), while column 4 further appends on state and political variables (*IDD*, *Ln(State GDPPC)*, *State GDPG*, and *Republican*). The results are almost identical after including the additional controls, as the coefficient on the UTSA and natural log of sales interaction ranges between 0.019 and 0.020, respectively, and remains significant at the 1% level.

The column 5 regression model drops the state and political controls from the column 4 specification and instead includes the average state-year and industry-year book leverage, where firm *i*'s observation is excluded from the calculations and industry is defined at the three-digit SIC level. Column 6 is the full model specification and includes all controls. The magnitude is reduced to 0.018 in these specifications, but remains significant at the 1% level. Moreover, the estimated coefficients on the control variables are similar to previous studies on financial leverage.[38] Of particular interest, we find that the UTSA has explanatory power for large firms' capital structure, even after controlling for the IDD. Overall, these results suggest an economically significant effect, as an increase in *Ln(Sales)* by one standard deviation is associated with an increase in *Book Leverage* of $0.018 \times 2.137 = 0.0385$, or 3.85 percentage points.

Panel B of Table D5 provides the same analyses as Panel A, except now we measure debt using the market leverage ratio. Column 1 indicates that using the UTSA index as a standalone exogenous regressor does not significantly relate with market leverage. Columns 2 – 6 provides evidence that with varying leverage controls, the interaction term between the UTSA and natural log of sales is positive, ranging from 0.010 to 0.013, and significant at the 1% level. The findings indicate that protection by the UTSA for large firms results in a 2.14 ($=0.010 \times 2.137$) percentage point increase in *Market Leverage* for a one standard deviation increase in *Ln(Sales)*.

In Table D6, we use four alternative proxies for size in place of the continuous and centered natural logarithm of sales measure. The first two variables are also

[38] The coefficient on *Profitability* is significant and negative in column 2 of Panel A, consistent with the empirically documented "profits-leverage puzzle" (e.g., Fama and French 2002, and Frank and Goyal 2015), but becomes positive and insignificant in the *Book Leverage* regressions once *Mod Z-score* is added as a control. This change in sign and significance occurs because the *Mod Z-score* is composed of a measure of profitability, namely the ratio of EBIT/assets (this is noted by Serfling 2016).

continuous and they are: the natural logarithm of assets, and the natural logarithm of the number of firm employees. Thus, we center these measures with their respective sample means before interacting with the UTSA index. In addition, we specify two indicator size proxies: the first, *Median Ln(Sales)*, equals one if a firm's natural logarithm of sales is greater than the entire sample's median and zero otherwise, whereas the other, *Median-Year Ln(Sales)*, equals one for firm's that have *Ln(Sales)* above the by year sample median value and zero otherwise. The findings are consistent with Table D5. In both Panel's A and B, where the dependent variable is book and market leverage, respectively, there is a positive relationship between the size of the protected firm and financial leverage. For example, column 2 of Panel A shows that the largest firms, measured by the number of firm-level employees (as in the BRDIS Survey), covered by UTSA increase their leverage by 3.64 percentage points for every one standard deviation increase in *Ln(1+Employees)* (=0.032 $\times$ 1.136). Hence, it appears the finding is robust to alternative measures of firm size.

5.3 Do Firms Make Anticipatory Leverage Adjustments?

We follow Bertrand and Mullainathan (2003), Giroud and Mueller (2010), Atanassov (2013), Roberts and Whited (2013), Fich, Harford, and Yore (2016), and Serfling (2016) and perform a placebo test in order to address concerns of reverse causality and provide evidence that the primary difference-in-differences identification assumption of parallel trends is satisfied. This analysis is conducted by evaluating the timing of changes in debt ratios relative to the timing of the UTSA, and the interaction of the UTSA with size. Thus, the placebo is administered by specifying the model to include an interaction term of the protection index and the natural logarithm of sales a year before the law is actually enacted.

The main variables of interest are $UTSA \times Size^{(-1)}$, $UTSA \times Size^{(0)}$, and $UTSA \times Size^{(1+)}$. These continuous variables are created by interacting the change in trade secrets protection stemming from the UTSA with the centered size proxy if the firm is headquartered in a state that passes the law in the year before actual adoption, the year of actual adoption, and one year and beyond actual adoption, respectively. Thus, the first interaction term falsely assigns treatment a year before it should be assigned, where the remaining measures accurately indicate that treatment is or has already been dispensed. Therefore, if the coefficient on $UTSA \times Size^{(-1)}$ is statistically significant there are serious concerns about differences in trends pre-treatment, and anticipatory leverage adjustments.

The results of the falsification tests are presented in Table D7. The first two columns correspond to measuring the outcome variable using book leverage and columns 3 and 4 employ market leverage. Further, columns 1 and 3 are without state, political, and industry controls, while the even numbered columns include the full set of controls and a state-time trend. It is reassuring to find that the coefficient on the placebo interaction term is both economically and statistically insignificant. This is also the case for the UTSA index as a standalone regressor, as it is not significantly related to capital structure decisions in the year prior to treatment. In all four columns, the treatment effect is positive and significant for large firms in the first year and beyond the enactment of the law. Furthermore, the magnitude and significance of the $[UTSA \times Ln(Sales)]^{(1+)}$ coefficients are almost identical to the estimates reported in

Table D5, Panel A and B. In summary, the evidence from Table D7 seems to suggest that lobbying and preemptive leverage changes are not concerns, and the parallel trend assumption is likely satisfied. Thus, the evidence is suggestive that the adoption of the UTSA was an exogenous shock, a requirement necessary for causal implications.

5.4 Alternative Leverage Definitions

In this section, we conduct the following robustness check. We test whether or not the relationship we have documented between large protected firms and UTSA coverage is specific to the book and market leverage measures of debt or if the relation persists using alternative definitions of financial leverage. This includes the natural logarithm of one plus total debt, net book leverage, and net market leverage. In columns 1, 3, and 5 of Table D8, we specify the full model regression with each respective alternative debt measure, but without the interaction of *UTSA* and *Ln(Sales)*. As documented previously, there is not a significant effect of UTSA adoption on financial leverage for the average firm. Columns 2, 4, and 6 of Table D8 employ the full model regression for each alternative measure of debt, respectively, but with the variable *UTSA* × *Ln(Sales)* specified. The results show clear and consistent evidence that a positive and 1% statistically significant relationship holds with the alternative measures of financial leverage. For example, a firm that moves from the 25^{th} percentile to the 75^{th} percentile in *Ln(Sales)* adjusts their net market leverage upward 3.68 (=0.013 × [6.545-3.717]) percentage points after the adoption of the UTSA. Hence, the largest firms located in states with enacted UTSA laws differentially increase their financial leverage. Our findings suggests that the relation between the intersection of the UTSA and the natural logarithm of sales is robust to alternative financial leverage measures.

5.5 UTSA, Innovative Activity and Financial Leverage

Having established the positive relation between large firms with strengthened trade secrets protection and financial leverage, we now turn to examining cross-sectional variation in innovative activity and UTSA, and its effect on firms' debt ratios. These tests are carried out to gain a greater understanding of the mechanisms underlying our main findings detailed in Section 5.2.

In columns 1 - 4 of Panel A, Table D9, we explore the relation between UTSA protection, firm-level innovative activity and book leverage. Following Denis and McKeon (2016), we create an indicator variable set to one if a firm has R&D expenditure greater than 0.02. Column 1 indicates that businesses located in UTSA enacting states that have high levels of R&D intensity reduced book leverage by 2.8 percentage points. Thus, it appears, without differentiating on size, firms highly-dependent on R&D do not finance this activity with debt (e.g., Bradley, Jarrell, and Kim 1984, among others), even after trades secrets laws become stronger.

Columns 2 – 4 consider the interaction of covered firms and three patent measures commonly employed in the corporate innovation literature (e.g., Hall, Jaffe, and Trajtenberg 2005, Atanassov 2013, Bena and Li 2014, Chu, Tian and Wang 2015, Bradley, Kim and Tian 2016, and Kogan, Papanikolaou, Seru, and Stoffman 2016). Specifically, we use the natural logarithm of one plus patents, the natural logarithm of one plus citation-weighted patents, and the natural logarithm of one plus stock-market weighted patents.[39] We find positive and significant coefficients of 0.060, 0.016, and 0.021 for firms with increased protection from UTSA and with higher levels of the three

[39] We thank Noah Stoffman for making this data available on his website: https://iu.app.box.com/v/patents.

respective patent variables. This seems consistent with the findings from Png (2017b) and Dass et al. (2015) in that increases in trade secrets protection decreased patent applications. Thus, previously successful patent applicants fund their innovative activity with debt after state-level strengthening of the secrecy mechanism.

In Panel B of Table D9, we find roughly similar results in magnitude and statistical significance using market leverage as the dependent variable. All of the models specified in these tests included the full set of controls, and firm and year fixed effects. We cluster robust standard errors by state of location since treatment is dispensed at this level.

5.6 UTSA, Firm Size and Bankruptcy Costs

Our findings appear to indicate that firms which are larger in size, and have pre-existing patent portfolios increase their use of debt financing after becoming better protected by trade secrets laws. This is suggestive that firms whose innovative risk is reduced by the UTSA are less likely to default and therefore take advantage of the benefits of debt (Miller 1977). We attempt to more explicitly test this hypothesis by considering the effect of UTSA on operating leverage as well as the effect the law had on larger firms' probability of default, and operating cash flow volatility using commonly employed proxies.

First, testing the effect of the UTSA on operating leverage, as defined by the composition of a firm's fixed to variable costs, provides insight into how sensitive a company is to general business conditions. If a company has greater amounts of variable relative to fixed costs, its expenses rise and fall with its level of productivity. In contrast, high fixed costs firms are characterized as having higher operating leverage and are more susceptible to negative cash flow shocks. Thus, if a firm experiences a negative change in sales, and as consequence, suffers an even larger reduction in earnings, than this company has greater operating leverage.

Following Eisfeldt and Papanikolaou (2013) and Serfling (2016), we investigate the relation between operating leverage and increases in trade secrets protection using the following regression specification[40]:

$$\Delta\, Ln(EBIT)_{isjt} = \gamma_i + \omega_t + \beta_1 UTSA_{st} + \beta_2 \Delta\, Ln(Sales_{it}) + \beta_3 \big(UTSA \times \Delta\, Ln(Sales)\big)_{ist} + \alpha X_{isjt} + \varepsilon_{isjt}\,, \qquad (2)$$

where $EBIT_{it}$ is earnings before interest and taxes, γ_i and ω_t are, respectively, firm and year fixed effects, $UTSA_{st}$ is a continuous variable, scaled between 0 and 1, which accounts for pre-existing trade secrets protection by measuring the change in strength once the UTSA law is enacted in year t in state s, $\Delta\, Ln(Sales_{it})$ is the natural logarithm of the change in firm sales centered by its sample mean, and X_{isjt} is the full set of controls from the main leverage regressions. The standard errors are robust and clustered by the state of location.

The main variable of interest is the interaction between the $UTSA_{st}$ protection index and the percentage change in firm sales. Column 1 of Table D10 indicate that earnings sensitivity to changes in sales is significantly less for firms protected by the UTSA. In particular, interpreting the estimated coefficients implies that, prior to state-level enactment of the UTSA, a 1% decrease in sales is associated with a 1.28% decrease in earnings for a firm with an average change in sales. However, UTSA

[40] My motivation for testing the operating leverage of better protected firms stems from the likelihood that fixed expenses on maintaining secrecy (such as attorney fees and security guards, systems, etc.) are plausibly reduced after the passage of the UTSA.

protected firms experiencing a change in sales realize a 0.21% reduction in operating leverage. Column 2 suggests that large firms covered by the UTSA do not experience a differential reduction in operating leverage. Thus, the evidence suggest that the UTSA reduced operating leverage for *all* protected firms, but only the largest companies are able to capitalize (e.g., R&D expenditure (Png 2017a), and debt ratios). Moreover, the coefficient on the IDD indicator variable is insignificant.

Next, we investigate the relation between the probability of default and trade secrets protection. If a firm, in which trade secrets are a very important form of IP protection, experiences an increase in the strength of secrecy laws, this should reduce the misappropriability of future cash flows, and, all else equal, reduce the likelihood of bankruptcy. We use the following regression model to test this prediction:

$$Prob.of\ Default_{isjt+1} = \gamma_i + \omega_t + \beta_1 UTSA_{st} + \beta_2 Size_{it} + \beta_3 (UTSA \times Size)_{ist} + \alpha X_{isjt} + \varepsilon_{isjt}\,, \tag{3}$$

where $Prob.of\ Default_{t+1}$ is the next period probability of default proxied by the inverse modified Altman's Z-score (Mackie-Mason 1990), and the remaining variables are identical to those specified in the full leverage model.

First, however, in column 3 of Table D10, we examine if the UTSA lowers the likelihood of bankruptcy for the average firm in our sample, excluding the interaction term effect, β_3. The coefficient on the protection index is negative but not significantly different from zero. Further, the interaction $UTSA_{st} \times Prob.of\ Default_{it}$ is positive but insignificant. Next, we estimate model 3 in column 4, and show that large firms protected by the UTSA are associated with a 10.5 (=0.049 $\times$ 2.137) percentage point decrease in next year's probability of default for every one standard deviation increase in *Ln(Sales)*. So far, these findings suggest that large firms located in UTSA passing states increase financial leverage as a response to the reduction in their financial distress costs. In contrast, firms located in IDD passing states do not have lower next period *Prob. of Default* in our sample.

The next test examines if a reduction in future cash flow volatility is a channel through which trade secrets protection reduces bankruptcy costs. We employ the following model:

$$Vol_{isjt+1} = \gamma_i + \omega_t + \beta_1 UTSA_{st} + \beta_2 Size_{it} + \beta_3 (UTSA \times Size)_{ist} + \alpha X_{isjt} + \varepsilon_{isjt} \ , \tag{4}$$

where Vol_{it+1} is the rolling standard deviation of firm $i's$ operating cash flows over the past ten years leaded one-year into the future, and the other regressors are identical to the main debt ratio regressions.[41] Columns 5 and 6 in Table D10 indicates that large firms protected by the UTSA associate with a reduction of 1.1 (=0.004 × [6.545-3.717]) percentage points in the volatility of cash flows for a move from the 25th to the 75th percentile in *Ln(Sales)*. We interpret this finding as firms with stronger trade secrets protection are less at risk for rival firms misappropriating secrets and thus more likely to sustain the indefinite stream of future cash flows generated by the economically valuable, confidential information. Overall, the findings in Table D10 are consistent with increased trade secrets protection for larger firms decreasing operating leverage, the probability of default, and the volatility of cash flows. Also of note, consistent with Klasa et al. (2018), we do not find evidence that the positive relation between IDD and financial leverage stems from a reduction in bankruptcy costs, as none of the

[41] In addition, our results are robust to estimating the cash flow volatility measure over the past five years instead.

coefficients of interest predict a significant reduction in any of the three default cost proxy regressions.

5.7 *UTSA, Probability of Default and Financial Leverage*

In this section, we further explore if the trade-off theory of capital structure (Myers 1977) is a potential channel that explains our results by testing if firms characterized as having higher likelihoods of default, that become better protected by the UTSA increase their financial leverage. That is, we center the inverse of modified Altman's Z-Score with its sample mean, and then interact it with the UTSA index. Then, we regress *Book Leverage* and *Market Leverage*, separately, on *UTSA* × *Prob. of Default* plus control variables to determine if in fact there is a positive and significant relation between financial leverage and default risky firms that experience an increase in trade secrets protection. As in the previous tests, we include the full spectrum of control variables, firm and year fixed effects, and cluster robust standard errors by state of location. The results are presented in Table D11.

Column 1 of Table D11 indicates that current period book leverage is adjusted upward for firms that are located in UTSA passing states and concurrently have higher inverse modified Z-scores. For instance, a one standard deviation increase in *Prob. of Default* leads to a 12.8 (=0.025 × 5.117) percentage point increase in *Book Leverage*. Column 2 finds a qualitatively similar coefficient of 0.019, significant at the 5% level, for a predictive regression in which the dependent variable is leaded one period (t+1). Columns 3 and 4 are identical to those described above with the one exception that market leverage is specified on the left-hand side. Again, the results suggest that firms more likely to file for bankruptcy increase their debt ratios after their trade secrets become better protected. These results, in conjunction with those reported in Section 5.2-6, provide suggestive evidence that large firms located in UTSA adopting states adjust their financial leverage upward because their bankruptcy costs are reduced, consistent with trade-off theory.[42]

5.8 UTSA, Firm Size and Long-Term Firm Value

The previous findings indicate that increases in trade secrets protection are met with a proliferation of debt for large firms. To assess the economic significance of these results we explore the firm value implications of the UTSA index interacted with firm size. To the best of our knowledge, we are the first to study the effect of trade secrets protection on long-term firm value. However, previous research has investigated if the IDD was beneficial for shareholders in the short-run using an event study methodology. Klasa et al. (2018) and Qiu and Wang (2017) both find positive and significant abnormal stock returns for firms headquartered in states that announce the adoption of IDD. The latter paper also finds a negative and significant market reaction for firms located in states around the rejection date of the IDD. These findings are not directly comparable to those we present in Table D12 for the following three reasons. First we study an entirely different law which protects trade secrets differently than the IDD. This is evidenced in the findings above as we document that states with IDD are less likely to adopt the UTSA. Further, even after controlling for the doctrine, there is an effect of the statutes for large firms on capital structure decision-making, Second, we are considering the long-term value implications proxied with Tobin's Q and Total Tobin's Q, whereas the other studies focus on short-term stock returns. Lastly, the

[42] We also run regressions of *Book Leverage* and *Market Leverage*, both contemporaneous and leaded one-period, on the IDD indicator variable interacted with *Prob. of Default* and find insignificant coefficients.

channel in which Klasa et al. (2018) identifies – conservatism – is different than what we find – trade-off theory – and argue is the reason for the possible changes in long-term firm value.

In order to study the value implications of the UTSA for large firms we estimate the following model:

$$Q_{isjt} = \gamma_i + \omega_t + \beta_1 UTSA_{st} + \beta_2 Size_{it} + \beta_3 (UTSA \times Size)_{ist} + \alpha X_{isjt} + \varepsilon_{isjt}\ , \quad (5)$$

where Q_{it} is one of two measures for firm i, located in state s, operating within industry j, in period t. The first proxy for firm value is the standard measure of Tobin's Q used frequently by the governance literature (e.g., Straska and Waller 2014, and Cremers, Litov, and Sepe, 2017) and specified in Fama and French (1992), whereas the second is a new measure introduced by Peters and Taylor (2017)[43], defined at Total Tobin's Q or Total Q for short, which is estimated to account for intangible assets. Table C2 in the appendix provides descriptions of each. In addition, equation 5 identically specifies the other regressors as in the main debt ratio regressions, with the one exception of excluding *M/B* and replacing it with *Book Leverage*. Table D12 reports the staggered DID regression estimates from the above model 5.

Columns 1 and 4 in Table D12 explores if there is any effect of UTSA on firm value for the average firm in our sample. We find a positive and insignificant result with Tobin's Q as the dependent variable, and a negative and insignificant coefficient for the Total Tobin's Q regression. Also of note, we specify the IDD indicator variable and find a positive, but insignificant estimate in both specifications. Thus, while IDD has been found to increase financial leverage (Klasa et al. 2018), we don't find any long-term

[43] we thank Ryan Peters and Lucian Taylor for making their Total Q measure available on WRDS: http://www.whartonwrds.com/datasets/included/luke-taylors-total-q/

value implications for the average firm in our sample. Next, in columns 2 and 5, we interact the UTSA index with the centered *Ln(Sales)* measure, include control variables for *Profitability, Book Leverage, Fixed Assets, Div Payer,* and *Mod Z-score,* and find a positive coefficient, significant at the 1% level. Finally, we append the full spectrum of controls, including state, political, and industry variables, in columns 3 and 6, and find that large firms that become better protected experience increases in long-term value. For example, an increase in *Ln(Sales*) from its median to the 75th percentile yields a 27.6 (=0.198 × [6.545 – 5.153]) percentage point rise in Tobin's Q and a 38.4 percentage point improvement in Total Tobin's Q (=0.276 × [6.545 – 5.153]), respectively. The results from Table D12 indicate that better trade secrets protection, which reduces bankruptcy costs and increases financial leverage for large firms, is incredibly valuable.

6. Conclusion

We examine the effect of increased trade secrets protection on financial leverage. In order to deal with endogeneity and isolate causal relationships, our identification strategy exploits the staggered adoption of state-level trade secrets laws. The UTSA increased the protection of trade secrets for firms by precisely defining a trade secret, outlining what constitutes misappropriation, and clarifying the rights and remedies of firms victimized by competitors, hence decreasing the resources required to prevent theft and recover losses. We find suggestive evidence for its exogeneity as an instrument using a Cox proportional hazard model, in which firm-level, state-level, and industry-level financial leverage ratios are unable to explain its adoption, providing support against reverse causality.

Based on survey evidence from the BRDIS and recent empirical work by Png (2017a, 2017b), Dass et al. (2015), and Figueroa and Serrano (2013), we consider the impact of the UTSA on large firm's capital structure decision-making. We employ a difference-in-differences framework in order to contrast the book and market leverage ratios of firms with higher levels of sales located in states covered by legislation with firms headquartered in states without such coverage. We find an economically and statistically significant increase in both measures of debt for large UTSA firms. These results hold even after controlling for another trade secrets law, the IDD. Moreover, we document, using a dynamic regression specification, that the effect transpires one year or more after the adoption of the law. Most importantly, there is no significant relation in the year prior to its passage, assuaging concerns of lobbying and anticipatory effects. In addition to the falsification tests, we further use alternative definitions of leverage and size to interpret the findings causally.

We also explore the effect of R&D intensity, and pre-existing patent portfolios on leverage for firms covered by the UTSA. Our results suggest that firms with higher levels of R&D expenditure and increased protection decrease leverage, consistent with the literature on financing innovation. Further, we show a positive relation with financial leverage and UTSA covered firms with greater amounts of patents, citation-weighted patents, and stock market-weighted patents, consistent with Png (2017b), and Dass et al. (2015). Next, we examine the impact of UTSA on operating leverage, probability of default, and cash flow volatility. Overall, the results from these tests suggest that the UTSA decreases operating leverage, and large firms protected by these laws have lower likelihoods of bankruptcy and reduced risk in future streams of operating cash flows. We show that firms with higher likelihoods of default adjust their debt ratios upward after becoming protected by the state statute. In tandem, this evidence seems to suggest that large firms are increasing their financial leverage in response to a reduction in bankruptcy costs, consistent with the trade-off theory of capital structure. We find that this relation yields positive long-term firm value effects. Hence, some things might be best kept secret.

Chapter 3: Shadow Pills and Long-Term Firm Value

1. Introduction

Law and finance scholars agree that the poison pill (formally known as a "shareholder rights plan") is among the most powerful anti-takeover defenses (Carney, 2000; Coates, 2000; Daines, 2001; Bebchuk, Coates, and Subramanian, 2002; Cremers and Ferrell, 2014). While details vary across different implementations of the pill, the basic defensive mechanism provides existing shareholders, but not a hostile bidder, with stock purchase rights that entitle them to acquire newly issued shares at a substantial discount in the "trigger" event that an hostile bidder obtains more than a specified percentage of the company's outstanding shares (see generally Fleischer & Sussman 2013, §5.01[B][1][2]).[44] As a result, poison pills grants the board of directors the ability to substantially dilute the ownership stake of a hostile bidder, de facto giving the board veto power over any hostile acquisition.

Empirical studies have attempted to investigate whether the adoption of a poison pill is beneficial or detrimental to shareholder interests[45] since the use of the pill was validated by the Delaware Supreme Court in 1985.[46] Although earlier findings were largely inconclusive, over the past decade these studies have consistently found that the adoption of a pill is negatively correlated with firm value (Gompers, Ishii, and Metrick,

[44] This is the "flip-in" poison pill that has become largely majoritarian; the earlier "flip-over" poison pill provided for the same right but only if the hostile bidder, after acquiring the target's stock, effected a merger with an affiliate.

[45] For example, see Ryngaert (1988); Malatesta and Walkling (1988); Karpoff and Malatesta (1989); Ambrose and Megginson (1992); Bhagat and Jefferis (1993); Dowen, Johnson and Jensen (1994); Comment and Schwert (1995); Bizjak and Marquette (1998); Brickley, Coles, and Terry (1998); Carney and Silverstein (2003); Gompers, Ishii, and Metrick (2003); Chi (2005); Danielson and Karpoff (2006); Heron and Lie (2006), (2015); Bebchuk, Cohen and Ferrell (2009); Cremers and Ferrell (2014).

[46] This was the landmark decision in *Moran v. Household*, 500 A.2d 1346 (Del. 1985).

2003; Chi, 2005; Bebchuk, Cohen and Ferrell, 2009; Cremers and Ferrell, 2014). However, this result is difficult to interpret, as the decision to adopt a pill is endogenous. In particular, poison pills can be unilaterally adopted at any time by the board of directors, so that even firms that do not currently have a poison pill in place always have a "shadow pill" (Coates, 2000). The availability of the shadow pill exacerbates endogeneity concerns, as reverse causality or other omitted variables might explain both the board's decision to adopt a pill and the reported negative association between the adoption of a poison pill and firm value (Comment and Schwert, 1995; Bhagat and Jefferis, 2002; Catan, 2017).

In this paper, we contribute to the debate on the association between poison pills and firm value by shifting the focus of attention from "visible" pills to shadow pills – studying the effect of poison pills that arises from the *right to adopt* the pill (which right constitutes the shadow pill) rather than the *actual adoption* of a pill. We do so by investigating the value implications of state-level poison pill laws that were enacted in 35 U.S. states over the period 1986 to 2009, consistent with a large body of studies that exploits the variation from state antitakeover legislation as a natural experiment (see Karpoff and Wittry, 2017 for a description of these studies). Poison pill laws sanctioned the validity of adopting a visible pill, explicitly allowing the board to discriminate against one or more classes of shareholders in issuing rights plans and therefore strengthening the relevance of the shadow pill. In recent papers, Karpoff and Wittry (2017) and Catan & Kahan (2016) argue that poison pill laws provide plausibly exogenous variation in firms' takeover protection and thus constitute a valid natural experiment. The present paper, as far as we know, is the first study to consider the effect of poison pill laws – and thus the relevance of the shadow pill – on long-term firm value, as proxied by both Tobin's Q and stock returns.

Our main finding is that the passage of poison pill laws results in an economically and statistically significant increase in the Tobin's Q of the firms incorporated in the states where these laws were enacted, while also leading to enhanced operational efficiency for such firms. In particular, the increase in Tobin's Q is more pronounced in more innovative firms or firms where stakeholder investments are more relevant (e.g., with a large customer or in a strategic alliance).

Overall, our results are consistent with the "bonding hypothesis" of takeover defenses (Shleifer and Summers, 1988; Laffont and Tirole, 1988). Under this hypothesis, empowering the board to commit the firm to a business strategy that cannot easily be reversed through a takeover promotes the undertaking of long-term projects and stronger stakeholder relationships, increasing firm value. Other recent papers have documented empirical support for the bonding hypothesis, including Johnson, Karpoff and Yi (2015, 2016) for takeover defenses at the IPO stage and Cremers, Litov, and Sepe (2017) for the adoption and removal of staggered boards by mature firms.

We begin our analysis by investigating the likelihood of the passage of a state-level poison pill law conditional on state-level firm, legal and economic characteristics. With the exception of the prior adoption of directors' duties statutes (which allow the board to consider non-shareholder interests), we find no other significant predictors for the adoption of poison pill laws, suggesting that their adoption is largely exogenous to the market and economic environment in which these laws were introduced.

We next show that poison pill laws meaningfully change firms' takeover protection, as we find that firms incorporated in states adopting poison pill laws are more likely to adopt a visible poison pill than firms incorporated in states without this legislation. Low prior firm value is also a statistically significant predictor for the adoption of a poison pill defense, as previously found in Cremers and Ferrell (2014). This finding supports the view that the negative association between the adoption of a poison pill and lower firm value reported in prior studies may be attributable to reverse causality (Cremers and Ferrell, 2014; Catan, 2017). It also implies that while having a "perpetual" visible pill in place might be a reflection of bad governance, the adoption of a poison pill may not directly cause lower firm value, in contrast with the (causal) view that the adoption of a poison pill leads to greater entrenchment of directors and managers (Bebchuk, Cohen, and Ferrell, 2009).

We then move to the heart of the analysis, estimating the effect of poison pill laws on the long-term value of firms incorporated in the enacting states over the period 1983 to 2012 using pooled panel Tobin's Q regressions that include firm and year fixed effects. We find that the passage of poison pill laws results in a positive and statistically significant increase in firm value for our full sample of firms. The increase in Tobin's Q is also economically significant at 5.6% relative to the sample average Tobin's Q. However, when we disentangle the effect of first-wave poison pill laws (passed in 1986 – 1990) and second-wave poison pill laws (passed during 1995 – 2009), we find that only the second-wave laws result in a positive and statistically significant increase in firm value, while the first-wave laws have an insignificant coefficient.

These results are robust to various methodologies, including the incorporation of possible selection effects through the creation of a matched sample, where the “treated” firms that are incorporated in each of the 35 states with poison pill laws are matched to “control” firms with similar observable ex-ante characteristics but incorporated in a state without a poison pill law in the post five-year period around the adoption date of a poison pill law by the treated firms’ state of incorporation. While the difference in the Tobin’s Q between treated and control firms – as well as pre-event trends of other important firm characteristics – is insignificant in the three-year period preceding the law passage in the state of the treated firms, the difference is significantly positive in the three-year period following the law passage. We further show that stock returns give similar results as using Tobin’s Q in a long-term stock return event study surrounding the adoption of poison pill laws that employs long (short) portfolios that buy (sell) treated (control) stocks from the matched sample group around the time their (matched sample counterpart’s) state of incorporation adopts a poison pill law.

We explain our result that the increase in Q is driven by the second-wave poison pill laws by carefully considering the changing legal context between the two waves, especially pertaining to the state of Delaware, where most publicly traded firms are incorporated. Due to the pervasive influence of Delaware case law over other jurisdictions (Cremers and Ferrell, 2014), there are institutional reasons to believe that the validity of the pill even outside Delaware was fairly clear from 1985 until at least 1988, when two Delaware decisions injected novel uncertainty by restricting a board’s ability to maintain the pill.[47] Therefore, during the 1985 to 1988 period that covers most of the first-wave poison pill laws, most firms – whether incorporated in Delaware or elsewhere – already had access to an effective shadow pill and, in many cases, also had adopted a visible pill, which likely reduced the importance of introducing poison pill laws.

By 1995, which marks the beginning of the second wave of poison pill laws, it had plausibly become clearer what states had endorsed a pro-pill policy (namely those who had passed a poison pill law during the first wave) and which had not. As a result, the second-wave laws significantly strengthened the shadow pill for the firms incorporated in the enacting states, especially considering that firms in these states were less likely to have a visible pill in place before the passage of the second-wave poison pill laws.

Next, we examine two possible economic channels through which a shadow pill could contribute to firm value, respectively reflecting the "bargaining power hypothesis" of Stulz (1988) and Harris (1990) and the "bonding hypothesis" of Shleifer and Summers (1988) and Laffont and Tirole (1988). The bargaining power hypothesis suggests that dispersed shareholders are at a disadvantage when faced with the decision to tender their shares in a potential acquisition, so that providing them with the ability to form a collusive response creates value by obtaining the best offer price for their shares. The bonding hypothesis, instead, posits that limiting the short-term ability of

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[47] These decisions are City Capital Assocs. v. Interco Inc., 551 A.2d 787 (Del. Ch. 1988) (requiring redemption of the pill by the board) and Grand Metro., Pub. Ltd. Co. v. Pillsbury Co., 558 A.2d 1049 (1988) (preliminary injunction ordering redemption of the pill).

shareholders to disrupt the firm's long-term strategy can bond other stakeholders more closely to the firm, thereby improving firm value. Consistent with the latter hypothesis, we find that firms incorporated in a state that adopted a poison pill law and in which stakeholder relationships are likely more relevant – such as firms that have a large customer, are in a strategic alliance, where long-term investments are more important or that have more complex operations – experience a higher increase in Q and operational efficiency. Conversely, we do not find evidence supporting the bargaining power hypothesis, as firms incorporated in states with poison pill laws and also being more at risk of a future takeover do not have differentially higher Tobin's Q or takeover premiums than similar companies incorporated in states without such legislation.

While ours is the first study to consider the value implications of poison pill laws (or the shadow pill), we are not the first to exploit the exogenous variation created by these laws. Karpoff and Malatesta (1989) analyze the effect of *all* state antitakeover legislation enacted from 1982 to 1987 (including the passage of poison pill laws in Ohio and Wisconsin) on stock prices, finding that state-level and firm-level takeover defenses are substitutes. Cain, McKeon, and Solomon (2017) study 16 different state-level antitakeover laws (including poison pill laws) and court rulings over the period 1965 through 2014, and find that poison pill laws did not impact hostile takeover activity, but do not consider their specific impact on firm value. Karpoff and Wittry (2017) and Fich, Harford and Yore (2017) also consider the adoption of poison pill laws. However, in comparison with Karpoff and Wittry (2017), we include both first-wave and second-wave poison pill laws spanning the sample period 1983 to 2012, whereas they consider the period 1976 to 1995 that only included first-wave state laws.[48] Further, we focus exclusively on the effect of poison pill laws, whereas Fich, Harford and Yore (2017) use these as a robustness check within their study of the impact of antitakeover protection more generally on the marginal value of cash.

Finally, our results add to the literature examining the relationship between takeover defenses and shareholders wealth. Our study finds no support for the "managerial entrenchment" hypothesis (Manne, 1965; Cary, 1969; Easterbrook and Fischel, 1991; Bebchuk, Coates, and Subramanian, 2002), but rather supports the view that takeover defenses might serve a positive corporate governance function for some subset of firms, consistent with other recent studies of such defenses (Cen, Dasgupta, and Sen, 2015; Johnson, Karpoff, and Yi, 2015, 2016; Fich, Harford, and Yore, 2017; Cremers, Litov, and Sepe, 2017; Catan, 2017).

2. Legal Background

The landmark 1985 decision of the Delaware Supreme Court in *Moran v. Household International* affirmed the validity of the poison pill for Delaware firms and promoted the widespread adoption of the pill both in Delaware and outside Delaware (Helman and Junewicz, 1987; Fleicher, Hazard, and Klipper, 1988). Most law and finance scholars, however, describe the legal status of the pill outside Delaware as uncertain until states adopted poison pill laws that validated the use of the pill in each enacting state (Catan and Kahan, 2016; Cain, McKeon, Solomon, 2017; Karpoff and

[48] The literature typically refers to state antitakeover laws passed after 1982 as "second-generation" laws, where the "first-generation" laws were invalidated by the U.S. Supreme Court in *Edgar v. Mite Corp.* on June 23, 1982 (see Karpoff and Wittry (2017) for a more detailed discussion); other studies further classify the most recent statutes as "third-generation" state takeover laws.

Wittry, 2017). These laws belong to the broader category of antitakeover laws that a large number of states enacted during the takeover era. In particular, the most prevalent forms of other antitakeover laws are business combination statutes, control share acquisition statutes, fair price statute and directors' duties (or corporate constituency) statutes.[49]

The argument usually adduced to defend the uncertain status of the poison pill outside Delaware before the enactment of poison pill laws is that state courts' decisions invalidated the use of this defense in the states of New York, New Jersey, Georgia, Wisconsin, Colorado, Virginia and Indiana[50] between 1986 and 1989 (Karpoff and Wintry, 2017; Catan and Kahan, 2016, p. 636). However, the uncertainty created by these decisions did not last long, as each of these states passed a poison pill law shortly after the related invalidating court decision. For example, while the New York Supreme court invalidated the use of the pill in June 1988 (in *Bank of New York Co. v. Irving Bank Corp*.),[51] the state of New York passed a poison pill law in December of the same year.

More generally, we argue that the "pervasive" authority attributed to Delaware judicial decisions over non-Delaware corporations (see Cremers and Ferrell, 2014) points to the opposite conclusion that the validity of the poison pill was fairly certain in

[49] Like poison pill laws, the first three forms provide for a direct defense against a potential takeover threat, while directors' duties laws only enable directors to act in the interests of all stakeholders rather than just shareholders. Of course, in practice, this further degree of freedom, offer directors more leeway to justify the adoption of antitakeover measures.
[50] Catan and Kahan include the Seventh Circuit's decisions in *Dynamics Corp. of Am. v. CTS Corp*., 637 F. Supp. 406, 409, 416 (N.D. Ill), aff' d 794 F.2d 250 (7th Cir. 1986) concerning Indiana among the decisions that validated the pill (Catan and Kahan, 2016, p. 636). However, while the court in *CTS Corp*. did not hold the pill invalid per se, it still found the pill to be a violation of directors' fiduciary duties under the specific circumstances of the case.
[51] *Bank of New York Co. v. Irving Bank Corp*., 142 Misc.2d 145, 536 N.Y.S.2d 923 (N.Y.Sup.Ct.1988) (New York law).

the aftermath of *Moran* both in Delaware and outside Delaware. Indeed, the widespread adoption of visible poison pills, even in non-Delaware firms, in the years immediately following *Moran* supports the view that *Moran* was understood to apply to non-Delaware firms as well. This interpretation also finds support in the evidence that state courts' decisions frequently referenced *Moran* in poison pill rulings.[52]

Further, in the period 1986-1990 state courts' decisions also intervened to uphold, rather than reject, the validity of the pill under the laws of Maine, Maryland, Michigan, Minnesota, Texas and Wisconsin. This evidence seems to indicate that not only the validity of the pill was possibly not uncertain *before* those decisions, but the pill certainly gained validity in those states *after* approval by a state court's decision.

Still, under the view that Delaware common law shapes corporate law in all other states, Delaware decisions that followed *Moran* could have mattered more for the uncertainty of the pill in other states than earlier state courts' decisions in those very same states. In particular, in the fall of 1988 the Delaware courts issued two decisions – *City Capital Associates v. Interco Inc.* (November 1, 1988)[53] and *Grand Metropolitan PLC v. Pillsbury Co.* (November 1, 1988)[54] – that injected unexpected uncertainty around the use of the poison pill, although mostly affecting the redemption of the pill rather than its validity *per se* (Fleischer & Sussman 2013, §5.08[B][2][A]).[55] In both of these decisions, the Delaware court halted the continued use of a visible poison pill that

[52] For example, in *Amalgamated Sugar Co. v. NL Industries Inc*., the US District Court for the Southern District of New York (New Jersey law) held the pill invalid by reasoning that the factual circumstances of the case were different from *Moran*. See Amalgamated Sugar Co. v. NL Industries Inc., 644 F.Supp. 1229 (S.D.N.Y.1986) (New Jersey law); Asarco Inc. v. Court, 611 F.Supp. 468 (D.N.J.1985).
[53] 551 A.2d 787 (Del. Ch. 1988).
[54] 558 A.2d 1049 (1988).
[55] While the issue of the validity of the pill attains a board's legitimate ability to adopt a pill, pill redemption cases concern the board's ability to keep a pill in place once confronted with an actual takeover threat.

was preventing an unsolicited tender offer, which prompted considerable comment and even induced corporate lawyers to recommend firms to move out of Delaware (Fleischer & Sussman 2013, §5.08[B][2][A]). This could plausibly explain why several states decided to adopt poison pill laws around 1988-1990, as the viability of the poison pill as a strong defense was no longer assured after *Interco* and *Pillsbury*.

The *Interco* and *Pillsbury* decisions were later reversed by the 1990 Delaware court decision in *Paramount Communications, Inc. v. Time Inc.*,[56] which some commentators read as granting the board an unconstrained power "to just say no" to unsolicited tender offers (Bebchuk, Coates, Subramanian, 2002). Several other commentators, however, maintain that the Delaware jurisprudence on pill redemption cases remains in an unsettled state and tend to depend on fact-specific circumstances that have limited general applicability (Fleischer & Sussman 2013, §5.08[B][2][A]). For these reasons and because Delaware never adopted a poison pill law, Delaware represents a rather unique poison pill "case." Outside Delaware, however, after the first-wave of poison pill laws ended in 1990, the sorting between pro-pill and anti- (or no) pill states was completed, with no other passage of a poison pill law until 1995 (when the second wave of poison pill laws began).

3. Data and Descriptive Statistics

3.1 Data

We use several data sources to construct our main data sample, which covers the period 1983 to 2012. We start by gathering comprehensive data on firm-level visible

[56] 571 A.2d 1140, 1152-55 (Del. 1990).

poison pills, covering 4,796 unique firms between 1976 and 2016.[57] In particular, our visible poison pill variable, *Poison Pill Firm-Level*, is a dummy that equals one if the firm has adopted a poison pill, and is derived from combining data from two institutional data providers, four previous academic studies, and our own hand-collected sample.

The institutional data sources are the SDC Corporate Governance and the Institutional Shareholder Services (ISS) Governance databases,[58] which cover the periods 1976 to 2015 and 1990 to 2015, respectively. We supplement these data with the poison pill data from Comment and Schwert (1995), Caton and Goh (2008), Cremers and Ferrell (2014), and Cremers, Litov and Sepe (2017). These studies' datasets range from 1983 to 1995, 1990 to 2004, 1978 to 2006, and 1978 to 2015, respectively. Lastly, using extensive Factiva searches, we add hand-collected data on firm-level poison pill data in the period 1994 to 2008 for firms with unavailable data from any of the sources above. Table E1 provides a brief definition for *Poison Pill Firm-Level* as well as all of the other variables in the study.

Our main independent variable, *Poison Pill Law*, captures whether the firm is incorporated in a state that has passed either a first-wave or second-wave poison pill law. We obtain information on whether states have passed poison pill laws from Barzuza (2009), Cain, McKeon and Solomon (2017) and Karpoff and Wittry (2017). Figure F1 provides a U.S. map depicting the dispersion of adopting states. The adoption month and years provided by Karpoff and Wittry (2017) are reported in Table F1. To

[57] Firms with missing firm-level poison pill data are excluded from the main sample.
[58] The ISS data consists of the current Governance data set which spans the period 2007 to 2016, and the Governance Legacy database, maintained at the time by the Investor Responsibility Research Center (IRRC) and covering the time period 1990 to 2006.

ensure that we use historically accurate accounts of firms' incorporation status, we supplement the current incorporation data provided by Compustat with historical incorporation information from Compact Disclosure for the period 1988 to 2006, and from the CRSP Historical U.S. Stock database from 1990 to 2012.[59] Combining the poison pill adoption dates and historical incorporation data, we then construct the indicator variable, *Poison Pill Law*, set equal to one for all affected firms in the year of and after the respective adoption date, and otherwise equal to zero. Accordingly, all firms incorporated in states without poison pill laws have this indicator variable set to zero.

We further differentiate the coverage of poison pill laws by two distinct periods, or "waves," of adopting states – that is, following a cohort criterion. The first wave period, *Poison Pill Law First Wave*, comprises the 23 states that passed poison pill legislation during the time period 1986 to 1990, and the second wave, *Poison Pill Law Second Wave*, includes the 12 states enacting poison pill laws in the 1995 to 2009 period.

Consistent with prior work examining the corporate value implications of corporate governance arrangements (Demsetz and Lehn, 1985; Morck, Shleifer, and Vishny, 1988; Lang and Stultz, 1994; Yermack, 1996; Daines, 2001; and Gompers, Ishii, and Metrick, 2003), we measure firm value (our main dependent variable) using Tobin's Q (*Q*). Following Fama and French (1992), we measure *Q* as the ratio of market to book value of assets using financial data from Compustat. Additionally, in robustness tests, we also use data from the CRSP database to analyze the evolution of

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[59] We backfill firm-year incorporation data prior to 1988 in our main sample with the oldest (first) data point on historical incorporation from either the Compact Disclosure or historical CRSP databases.

stock returns (*Monthly Stock Returns*) surrounding the adoption of poison pill statutes (see subsection 5.2.3 below).

We also include a number of control variables shown by the corporate governance literature to be related to Tobin's Q. Our default specifications include the following controls: *Ln(Assets), Ln(Age), Sales Growth, Loss, Debt-to-Equity, Firm Liquidity, CAPX/Assets, R&D/Sales, Institutional Ownership, State-Year Q,* and *Industry-Year Q.* Data for most of the controls come from Compustat, with the exception of the institutional ownership variable, which is obtained from Thomson Reuters. In particular, *State-Year Q* and *Industry-Year Q* attempt to capture local time-varying state of location and three-digit SIC code industry shocks (following Giroud and Mueller, 2010). In some additional specifications, we control for other most common forms of state-level takeover laws adopted by the firm's state of incorporation: *Business Combination Law, Control Share Law, Directors' Duties Law,* and *Fair Price Law*, defined as in Karpoff and Wittry (2017).

3.2 Descriptive Statistics

Our main data sample is composed of 33,826 firm-year observations from 3,423 publicly traded industrial firms, excluding utilities and financial companies (SIC codes 4900–4999 and 6000–6999, respectively), incorporated in the U.S. and without missing data for the main variables outlined above over the time period 1983 to 2012.

Our sample period begins three years before the states of Indiana and Ohio adopt the first state poison pill laws, and ends three years after the state of Wyoming enacts the most recent one.[60] Table F2, Panel A, reports the summary statistics for the full sample. The average *Q* for all firm-years is 1.86 with a standard deviation of 1.25. On average, the percentage of firm-year observations in which a company had a visible poison pill in place is 39.1%. Figure F2 provides a more detailed view of the substantial time variation in firm-level visible poison pills over the period 1983 to 2015. Over the period 1983 to 1990, which roughly covers the so-called takeover era of the corporation, there is a precipitous increase in the fraction of firms in the sample with a visible poison pill, with this fraction going from less than 10% in 1985 to more than 70% by 1990. This is followed by a gradual decline, where in 1999 the fraction of firms in the sample with a poison pill in place is roughly 40%. After that, the fraction of firms with a visible poison pill registers only slight variations until 2005, when it starts to decline steadily, with less than 10% of the firms in the sample having a poison pill in place by 2015.

We further refine our investigation into the time series variation of firm-level poison pill adoptions in our sample by considering new pill adoptions. Panel A of Figure F3 depicts the percentage of corporations that adopted a new poison pill provision each year from 1983 to 2015. From this panel, it is evident that the majority of new pills in our sample were adopted from 1985 to 1988, in the apex of the takeover era and when the legal certainty of the provision was fairly clear under the ruling in *Moran*. After 1988, the instances of new adoptions became less frequent, with fewer than 4% of sample firms adopting a new poison pill between 1992 and 2015. This provides some insight that the majority of poison pills in place in the late 1990s through

[60] Beginning the sample period in 1983 also has the advantage of not overlapping firm-year observations with first-generation state antitakeover laws, and their effective 1982 invalidation by the U.S. Supreme Court in *Edgar v. Mite Corp.* (Karpoff and Wittry, 2017).

early 2000s, as shown in Figure F2, are likely existing pills that had yet to expire or were reinstated from earlier initial adoptions. We also decompose the percentage of firms dropping an existing pill in our sample from 1983 to 2015 in Panel B of Figure F3. In this panel, we provide evidence that firms began dropping (either by expiration or early removal) existing poison pills much more commonly from 1997 to 2002 (and also from 2008 to 2015); a stylized fact which is undetectable from inspecting Figure F2 alone.

These firm-level dynamics can plausibly be explained by an increase in the use of the (visible) poison pill after its introduction and during the period in which takeover activity was most intense. After that we observe a natural decline, paralleling the decline in hostile takeovers, while the most dramatic decline of the past decade is plausibly attributable to the increase in shareholder proposals to remove poison pills and the hostility to the pill of proxy advisory firms (Catan, 2017).

Table F2, Panel A, also shows that the average number of firms incorporated in states that adopted a poison pill law in the full sample is 28.4%. Relatedly, Figure F4 shows the average number of affected firms over the period 1983 to 2015. With the passage of the first-wave laws, the percentage of firms in the sample that are covered by the poison pill legislation increases from about 6% in 1986 to nearly 35% by 1990. We then observe a gradual decline in covered firms until the second wave of laws, when the percentage of affected firms in the sample increases from 25% in 1995 to 37% in 2007. By 2015, the average proportion of firms incorporated in states with a poison pill law equals 30%.

We follow-up on Figure F4 by reporting summary statistics for our main sample split by the first and second wave periods, where the former spans firm-year observations from 1983 to 1994, and the latter contains the sample points between 1995 and 2015. In this panel, we present the mean, standard deviation, and number of observations for each time split cohort, as well as the differences across the waves with respective *t*-statistics indicating if those differences are statistically significant. Providing some initial univariate evidence that firm characteristics are substantially different across the first and second wave of poison pill laws, we document that every variable, except *Firm Liquidity*, is different at the 5% significance level or higher. Accordingly, in all of our tests we explore whether the effect of poison pill laws on shareholder value changes based on which year the firm's incorporating state adopted its poison pill law.

Next, in Table F2, Panel C, we split the full sample by treatment status, where a firm is treated if it is incorporated in a state that adopted a poison pill law, and is a control otherwise. As observed in Section 2 above, while Delaware first endorsed the validity of the poison pill in the 1985 landmark decision in *Moran*, it never passed a poison pill law. We also saw that the redemption of the pill remains an unsettled issue in Delaware. We accordingly choose to assign Delaware firms to the group of control firms in the pooled panel regressions, with the *Poison Pill Law* indicator variable being set equal to zero for Delaware firms.[61] We provide the mean, standard deviation and total number of observations for the treated and control groups, and in the last two columns of the panel, report the differences between the groups and a corresponding *t*-

[61] We provide a robustness check for this methodological assumption in the supplementary appendix by excluding firms incorporated in Delaware entirely. Our results are qualitatively similar in these specifications.

statistic testing if those difference are significant. The two groups have statistically insignificant differences in average firm value. This is also the case for *Ln(Assets)* and *Firm Liquidity*. In contrast, all other variables are different at the 10% significance level or higher. Hence, Table F2, Panel C, underlines the importance of controlling for these variables in the pooled panel regressions. In Section 4.4, we explicitly address these differences in several matched samples, including a propensity score matched sample with nearest neighbor matching.

4. Identification Strategy and Empirical Methodology

4.1 Explaining the Adoption of Poison Pill Laws

The main working assumption of our identification strategy is that poison pill laws provided an exogenous shock to the takeover protection of firms incorporated in the enacting states (Karpoff and Wittry, 2017), with this shock affecting firm value. Therefore, a crucial step in providing evidence for the validity of our identification strategy is to investigate whether states were more likely to adopt poison pill laws based on differences in the ex-ante value of the incorporated firms. Indeed, should we find that states were more likely to adopt poison pill laws if the firms incorporated in the state had relatively high (low) value, that could potentially explain an association between the adoption of a poison pill law and firm value (i.e., reverse causality). More generally, if firm- or state- level economic and legal differences can explain the propensity of states to pass a poison pill law, this would undermine our assumption that poison pill laws provided an exogenous shock to takeover protection.

We estimate a linear probability model of the adoption of poison pill legislation on state-level averages of incorporating firm characteristics, state-level legal and macro factors, as well as incorporation state and year fixed effects. Our main sample covers the period 1983 to 2012, where all firm-year observations are excluded from the analysis after the incorporating state passes a poison pill law (i.e., a "failure" event occurs). In all specifications, we include incorporation state and year fixed effects and estimate standard errors using independent double clustering on the incorporating state and year level. We also lag all our predictor variables one period, and for those that are continuous, we standardize them to have a mean of zero and unit variance. The results of these tests are presented in Table F3.

Columns (1) and (2) reports the estimates for the entire sample period. Column (1) includes the annual averages of incorporating state-year firm characteristics and industry-level merger and acquisition activity, while column (2) includes controls for other antitakeover laws and macro factors at the state level. In columns (1) and (2), the only significant predictor of a poison pill law is whether the adopting state has already passed a directors' duties law. In particular, consistent with our exogeneity assumption, the average incorporating state-year *Q* is not a significant determinant of passing a poison pill law.

Columns (3) and (4) repeat the analysis but specific to the period 1983 –1994, which covers the first-wave poison pill laws. We find similar results and, in particular, that the average annual level of the incorporating state's *Q* does not predict the adoption of a first-wave poison pill law. We also find, however, a few significant determinants. For example, column (3) shows that if the average debt-to-equity of all firms incorporated within a state in a given year (*Incorp State-Year Debt-to-Equity*) is higher, it is less likely that a state will adopt a poison pill law. However, this significance does not hold after controlling for other state institutional and macro factors (see the controls for *Business Combination Law, Directors' Duties Law* and *Ln(Incorp State Per Capita GDP)* in column (4)).

Lastly, columns (5) and (6) report the estimated marginal effects in the period 1995 – 2012, which covers the second-wave poison pill laws. In these specification, none of the predictor variables in column (5) are significant, while in column (6), where we add the full set of controls, we find that a state is more likely to enact poison pill legislation if it has already passed a directors' duties law, and it is less likely to adopt this legislation if it has a fair price law or a higher per capita GDP. In both columns (5) and (6), however, the incorporating state-year level of Tobin's Q does not predict the adoption of a second-wave poison pill law. Overall, we conclude that there is no evidence for reverse causality, and that the results are consistent with our main identification assumption.[62]

4.2 Do Poison Pill Laws Matter for Firm-Level Pills?

The next step in our identification strategy is verifying that poison pill laws *did* affect the actual adoption of poison pills by firms incorporated in the enacting states. Specifically, as poison pill laws sanctioned firms' right to adopt a visible poison pill in

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[62] In subsection 8.3, we provide additional evidence for the validity of our identification strategy by testing the timing of the change in firm value relative to the timing of the passage of the relevant poison pill law. Organizationally, we choose to present these results after first documenting that poison pill laws are indeed value relevant. However, for the purpose of this section, we briefly note our suggestive evidence from Table F15 that the effect of poison pill laws on *Q* transpires *after* the passage of the laws and not before. This offers some reassuring evidence that both the affected and unaffected firms' value would have evolved in a similar fashion absent the adoption of this legislation (i.e., the parallel trends assumption likely holds).

the enacting states – thus strengthening those firms' shadow pill – we would expect firms in states with a poison pill law to be more likely to have a poison pill in place. To verify this hypothesis, in Table F4 we regress *Poison Pill Firm-Level* on whether a firm is incorporated in a state with a poison pill law, along with control variables and firm and year fixed effects.

In columns (1) through (3), we examine the marginal effect of a poison pill law on the firm-level decision to adopt a pill provision over the entire period 1983 to 2012. The first two columns indicate that firms incorporated in a state with a poison pill law are 6% to 7.3% more likely to have a visible poison pill in place than companies incorporated in states without such legislation. Column (3) appends controls for the existence of other state antitakeover laws (Karpoff and Wittry, 2017) and still finds a positive and significant relation between poison pill laws and the adoption of firm-level pills, consistent with the assumption that these laws identify valid external shocks to firms' takeover protection.

We next consider whether the documented relationship is "wave" specific, separately considering *Poison Pill Law First Wave* and *Poison Pill Law Second Wave*, which respectively capture whether a company is incorporated in a state that passed a poison pill law in the period 1986 – 1990 or 1995 – 2009. Columns (4) through (6) presents the estimates from these linear probability model specifications, where the final column adds controls for other antitakeover laws.

With or without the additional state laws' controls, we find that the adoption of a visible pill for firms incorporated in the first-wave enacting states are not affected by the passage of poison pill laws, while companies incorporated in second-wave enacting states are 7% to 12.4% more likely to have a visible pill in place after the adoptions of such laws. These findings are consistent with Figure F2, which shows that the majority of firms during the first wave period already had a visible poison pill in place prior to the adoption of the state poison pill law, with the result that the incremental impact of poison pill laws was likely significantly reduced (Karpoff and Malatesta, 1989; Karpoff and Wittry, 2017). Conversely, the average proportion of firms with a visible poison pill decreases significantly in the second-wave period, suggesting that poison pill laws enacted during this period had a greater impact.

Further, given the reverse causality concerns affecting any estimates of the effect of visible poison pills, we also examine the marginal effect of firms' predetermined Q on the firm-level decision to adopt a pill provision. In all our specifications, we find that having a relatively low firm value is a statistically significant predictor for the adoption of a poison pill defense, consistent with Cremers and Ferrell (2014). This finding provides suggestive evidence supporting the view that the negative association between the adoption of a visible poison pill and lower firm value reported in prior studies is indeed likely attributable to reverse causality (Catan, 2017).

We also supplement the above tests for reverse causality between the adoption of a visible pill and firm value by estimating a pooled panel regression of Q on dummy variables indicating the relative year in which a firm adopts a new poison pill, along with year and industry-year fixed effects (following Catan, 2017). The relative year dummies include indicators for up to 10 years before and after pill adoption, and the industry grouping is defined at the three-digit SIC code level. We also estimate robust standard errors with clustering performed by firm. Consistent with the reverse causality hypothesis, Figure F5 provides suggestive evidence that firm value is significantly higher in the two to five years before a firm decides to deploy a poison pill. Meanwhile, the Tobin's Q of companies is insignificantly different in the year before, year of, and up through five years after the pill's adoption.

4.3 Pooled Sample

Our baseline empirical methodology to identify the effect of the staggered adoption of poison pill laws on firm value employs a differences-in-differences research design in a pooled panel over the period 1983 to 2012. This approach closely follows Bertrand, Duflo and Mullainathan (2004), in which companies incorporated in states that eventually enacted a poison pill law are considered as part of the group of unaffected firms until their legislatures pass such a law. Once these previously unaffected firms become covered by poison pill laws, they enter the affected (or treated) group. For example, firms incorporated in Texas have their *Poison Pill Law* indicator variable set equal to zero in the period prior to 2003, whereas after Texas adopts its poison pill law in 2003 the indicator variable switches to one for the remaining ten years in the pooled panel (2003 – 2012). Accordingly, companies incorporated in states that never passed a poison pill law are always coded as an unaffected (or control) firm. Specifically, we estimate the following pooled panel regression model:

$$Q_{ist} = \gamma_i + \omega_t + \beta Poison\ Pill\ Law_{st} + \alpha X_{ist} + \varepsilon_{ist}\ , \tag{1}$$

where Q_{ist} measures firm value for firm i in incorporating state s during year t, and $Poison\ Pill\ Law_{st}$ is an indicator variable for whether the state in which a company is incorporated has adopted a poison pill law as of year t. The set of control variables X_{ist}

includes the dummy for firm-level poison pills as well as other firm and institutional characteristics that the extant literature has shown to correlate with firm value. In addition, we control for time-invariant unobserved heterogeneity within different firms using firm fixed effects γ_i (Gormley and Matsa, 2014), and for time-variant heterogeneity in unobserved factors that could affect all firms with year fixed effects ω_t. Finally, following Petersen (2009), we estimate robust standard errors clustered at the firm level.

Regression model (1) captures the average effect of poison pill laws on Q over the entire period 1983 to 2012. However, given that 23 of the states adopted the statutes prior to 1991 and 12 states enacted this legislation after 1994, we explore whether the value implications estimated using model (1) are time specific, examining whether poison pill laws differentially affected firm value in the two waves of laws. In particular, we estimate the following pooled panel model:

$$Q_{ist} = \gamma_i + \omega_t + \beta_1 Poison\ Pill\ Law\ First\ Law_{st} +$$
$$\beta_2 Poison\ Pill\ Law\ Second\ Wave_{st} + \alpha X_{ist} + \varepsilon_{ist}\ , \quad (2)$$

where $Poison\ Pill\ Law\ First\ Wave_{st}$ captures the poison pill laws for firms incorporated in first-wave adopting states, and $Poison\ Pill\ Law\ Second\ Wave_{st}$ captures the poison pill laws for firms incorporated in second-wave adopting states, with *i* indexing firms, *s* indexing state of incorporation, and *t* indexing years. Controls and estimated standard errors are the same as in model (1).

4.4 Matched Sample

A concern with the pooled panel research design described in Section 4.3. is that any estimation of the value relevance of poison pill laws might be confounded by other events that take place over the long-time period of our sample, 1983 to 2012. Therefore, we additionally employ a differences-in-differences methodology in a matched sample that consists of treated and control firms in the period surrounding the passage of poison pill laws. The use of the matched sample mitigates the possibility that some other unobserved shocks differentially affect the firms in the states adopting and not adopting a poison pill law, where such shocks are unrelated to the poison pill law but happened to occur around the same time. Our working hypothesis here is that such unrelated shocks would arguably affect the treated and control firms similarly, if the control firms are ex-ante similar to the treated firms.

In constructing our matched sample, we match all sample firms in each of the 35 adopting states to a control firm in a state that does not have a poison pill law during the five-year period after the state of incorporation of the treated firm adopts a poison pill law. We use propensity scores with nearest neighbor matching on *Q* and *Ln(Assets)* and exact matching on firm-level poison pill status and two-digit SIC codes in the year prior to the adoption of a poison pill law by the affected firms' incorporating state. With this matched sample, we estimate the following regression model:

$$Q_{ist} = \gamma_i + \omega_t + \beta_1 Post_t + \beta_2 Treat_i \times Post_t + \alpha X_{ist} + \varepsilon_{ist}\ , \qquad (3)$$

where $Post_t$ is an indicator variable equal to one in the year of and the three-year period after a poison pill law is passed for both treated and control firms, and zero otherwise, and $Treat_i \times Post_t$ is an indicator variable equal to one for firms incorporated in a state that adopts a poison pill law in the period when the law is enforceable and otherwise set to zero, for firm i, in incorporating state s, in year t. $Treat_i$ is omitted from model (4) due to multicollinearity with its firm fixed effect. All other control variables are the same as those employed in the pooled panel regressions described in Section 4.3, and so are the estimated standard errors. Lastly, we also investigate the value relevance of poison pill laws in the matched sample for the different waves.

5. Main Results

5.1 Pooled Sample

5.1.1 Poison Pill Laws and Firm Value

Table F5 reports the differences-in-differences estimates of the effect of the adoption of poison pill laws on long-term firm value of firms incorporated in the enacting states over the period 1983 to 2012. In separate specifications, we decompose the effect of first-wave (1986 – 1990) from second-wave (1995 – 2009) laws. Distinguishing by waves matters in light of the different legal contexts in which the first-wave and second-wave laws were introduced (see Section 2).

Preliminary, it is worth observing that, consistent with Cremers and Ferrell (2014) and Catan (2017), we find that the association of *Poison Pill Firm-Level* and *Q* is negative and significant in every specification. However, in light of the results of Table F4, where we find that having a relatively lower *Q* is a statistically significant predictor of the adoption of a visible pill, and Figure F5, where we show that firm value is significantly higher in the two to five years before the adoption of a visible pill, the negative association between visible poison pills and firm value in Table F5 may be endogenous and due to reverse causality.

Moving to our main results, in columns (1) and (2), we find that the adoption of a poison pill law is followed by a positive and statistically significant increase in *Q* for firms incorporated in the enacting states. This result is robust to controlling for other main state antitakeover laws in column (3), following Karpoff and Wittry (2017). Economically, and relative to the sample mean's Tobin's Q of 1.859, our estimates suggest an increase in value of 5.6% (=0.105/1.859) for firms covered by poison pill laws.

Next, in columns (3) through (6), we investigate whether firms protected by first- and second- wave poison pill laws experience differential changes in value.[63] Focusing on column (6), which controls for the other state-level antitakeover statutes, we find that the passage of a poison pill law in the second-wave jurisdictions results in a positive and statistically significant increase in *Q* for firms incorporated in those jurisdictions, with a percentage effect of 10.9% (=20.2/1.859). Conversely, the coefficient for firms incorporated in states that adopted poison pill laws during the first wave is insignificant, suggesting that the positive effect of poison pill laws on firm value is entirely driven by the second-wave laws.

As we argue in Section 2, the results of our *Q* regressions reflect the different legal contexts underlying the enactment of the first-wave and second-wave poison pill laws (Karpoff and Wittry, 2017). Thus results for first-wave poison pill laws are on average insignificant because (i) poison pill laws enacted before 1988 plausibly did not add much protection in light of the then relative certain validity of the pill after the

[63] Table G8 in the supplementary appendix reports the pooled panel regression results split by the time periods 1983 to 1991 and 1994 to 2012, as opposed to Table F5, which considers the entire sample period 1983 to 2012, but splits the waves using indicator variables. While we prefer the specification in Table F5 as it requires that all of the controls have the same coefficients, Table G8 shows that the results are robust to either design.

decision in *Moran*, and (ii) the effects of the poison pill laws enacted between 1988 and 1990 are in any event difficult to capture because many of these laws were introduced either shortly after related state courts' decisions invalidating the poison pill or the 1988 Delaware decisions injecting uncertainty in the use of the pill. Conversely, the second-wave laws added greater incremental protection at a time when the legal uncertainty of poison pills had been clear in these states for some time.

5.1.2 Poison Pill Laws, Firm-Level Pills and Firm Value

Our next test considers whether the passage of poison pill laws (strengthening the shadow poison pill) has different value implications depending on whether a firm has adopted a visible poison pill. Table F6 presents the results for the pooled panel regressions of *Q* on various poison pill law indicator variables interacted with *Poison Pill Firm-Level*. Columns (2) and (4) include the other state antitakeover laws as controls.

In columns (1) and (2), we do not find evidence of value implications for firms incorporated in a state with a poison pill law and a pill in place, as all of the estimates are positive but statistically insignificant. However, the *Poison Pill Law* indicator variable is positive and significant with point estimates ranging from 0.098 to 0.115. *Poison Pill Firm-Level* also continues to be negatively and significantly correlated with *Q*. Thus, these results seem to suggest that the value of a shadow pill is not affected by the actual adoption of a pill, confirming the assumption derived from institutional reasons that all the effect of poison pills arises from the availability of the right to adopt a pill rather than the actual adoption of the pill (Coates, 2000; Catan 2017). At the same time, when combined, again, with the results of Table F4 and Figure F5 on the likely reverse causality of the negative association between the adoption of a visible poison pill and firm value, the results of Table F6 seem to indicate that when a firm does adopt a visible poison pill, it means that things have already gone awry.

In columns (3) and (4), we then separate again the poison pill law indicator variable for the first- and second- waves, finding results similar to those in columns (4) – (6) of Table F5 and columns (1) and (2) of Table F6. Indeed, firms incorporated in states that adopted poison pill laws in the second-wave period (1995 – 2009) experience positive and statistically significant increases in *Q* of 24.1 to 28.1 percentage points, while results for the firms covered by the first-wave laws are insignificant. Furthermore, neither firms covered by the first-wave laws or second-wave laws and with firm-level pills show a statistically significant differential effect on value, adding further support for the view that the power of the pill rests in the availability of the shadow pill.

5.2 Matched Sample

5.2.1 Summary Statistics

As described in Section 4.4, a potential concern affecting the results for our pooled sample is that we might be capturing some spurious correlation between *Poison Pill Law* and some other confounding events that also relates positively with *Q* over the sample period 1983 to 2012. To address this concern, we create a matched sample of treated and control firms with equidistant pre- and post- treatment windows surrounding the 35 poison-pill-law adoption dates and under the additional criteria specified in Section 4.4.

In particular, our matched sample includes treated firms that are incorporated in states with poison pill laws and control firms that are from incorporating states that did not pass a poison pill law at any time up to at least five years after the adoption of a poison pill law by the matched firms' incorporating state. For example, Michigan passed a poison pill law in July of 2001. Therefore, we match all firms incorporated in Michigan in the year prior to adoption (2000) to its nearest neighbor from a pool of control firms incorporated in either one of the 15 states that never passed a poison pill law or to a company incorporated in a state that adopted this law after July of 2006 (Vermont and Wyoming). Consistent with our analysis for the pooled sample, we further break up the matched samples by the first and second wave of poison pill laws.

Panel A of Table F7 provides the summary statistics for the resultant matched samples in the year prior to treatment (*t*-1). Columns (1) – (3) are for the full sample, whereas columns (4) – (6) and (7) – (9) are specific to the first- and second- wave periods, respectively. In the first three columns, we also show full sample variable averages for treatment and control firms, along with the corresponding differences in means. In column (3), we report the estimated *t*-statistics in parentheses below the differences and indicate statistical significance, if necessary.

Results for Panel A of Table F7 show that our treatment and control firms are similar. In particular, *Q*, *Poison Pill Firm-Level*, and *Ln(Assets)* are not significantly different between the two groups. Furthermore, these variables are similar between treatment and control firms within the two separate wave periods. Despite the statistically insignificant differences between the treatment and control firms within the full, first-wave, and second-wave samples, we continue to include all of the control variables in our matched sample regressions for robustness. Panel B of Table F7 presents the summary statistics for all firm-year observations in the full, first wave, and second wave matched samples.

5.2.2 Poison Pill Laws and Firm Value

Table F8 reports the point estimates for the matched sample regressions with pre- and post- treatment windows of three years, consistent with our pooled panel regressions beginning three years before the enactment of poison pill laws by the first adopting states (i.e., Indiana and Ohio) and ending three years after the last passage of a poison pill law. In columns (1) and (2), we regress Q on $Treat \times Post$, where the treat indicator variable always equals one for firms incorporated in poison pill law states and zero for the control firms, and the post indicator variable equals one in the year of the adoption and afterwards for both groups, and zero otherwise. We omit *Treat* from the regression specification due to multicollinearity with its firm fixed effect, but include *Post* and year fixed effects, and the estimated standard errors are robust to heteroscedasticity and autocorrelation, with clustering performed at the firm level.

The results of Table F8 shows that our main result – that firm value increases after the state in which the firm is incorporated passes a poison pill law –continues to hold in our matched sample. In particular, in column (2), where we include controls for the other state antitakeover laws, the estimates suggest that treatment firms experience an increase in Q of 10.3 percentage points.

In columns (3) and (4), we consider the treatment effect of poison pill laws on firm value for the 23 first-wave adopting states. Consistent with the pooled panel regressions, there are no significant value implications stemming from poison pill laws in this earlier period. However, moving to columns (5) and (6) for the second-wave period, we find that firms incorporated in second-wave adopting states have increases in *Q* of 12% (=0.227/1.892) to 12.8% (=0.243/1.892), relative to the sample mean. This provides further support that our findings in the pooled panel regressions are not an artifact of spurious correlation.

In our final analogue to the Table F5 results, we test for differential value implications of first- versus second- wave laws in columns (7) and (8) in the full matched sample. In these specifications, the point estimates provide more evidence that the entirety of the positive value implications takes place in firms incorporated in the 12 second-wave adopting states, while the *Treat* × *Post* × *Poison Pill Law First Wave* triple interaction term is statistically and economically insignificant.

5.2.3 Portfolio Analysis

As a robustness check to the Q regressions, we perform a long-term stock return event study surrounding the adoption of poison pill laws using our matched sample of treatment and control firms. Following previous studies (Gompers, Ishii, and Metrick, 2003; Bebchuk, Cohen, and Ferrell, 2009; Cremers and Ferrell, 2014; Cremers, Litov, and Sepe, 2017), we construct long (short) portfolios of stocks from the matched sample treatment (control) group around the time their (matched sample counterpart's) incorporating state adopts a poison pill law. Table F9 presents the abnormal returns of value weighted portfolios for the long, short, and long-short portfolios, respectively.[64] Consistent with our *Q* analysis, we split the portfolio results by full sample, first- and second- wave periods, in the respective panels.

[64] We provide results pertaining to equally weighted portfolios in Table G9 of the supplementary appendix, where the findings are qualitatively similar to those using the CRSP value weighted market factor.

In Panel A of Table F9, we report the results from the above portfolios for the full matched sample, where we start holding the relevant stocks 6 months before the event date until 24 and 36 months post adoption date, respectively. We consider both the four-factor Carhart (1997) and three-factor Fama-French (1993) models to estimate abnormal returns. For both holding periods and across models, we find that treated firms earn positive and significant abnormal returns, whereas the control group does not. In addition, when we test our investment strategy of longing the treated companies and shorting the control companies, we find positive and significant abnormal returns. These results are consistent with those in our *Q* regressions, in spite of the inherently noisy nature of abnormal returns estimated from a relatively limited number of stocks in each portfolio (on average 62 to 72 stocks, depending on the length of our holding period).

In Panels B and C of Table F9, we separately consider the portfolios in the first- and second-wave periods. Again, consistent with the *Q* regressions and our considerations about the importance of the different legal contexts pertaining to the passage of the first- and second- wave poison pill laws, all of the abnormal returns for the long, short and long-short portfolios in the first-wave sample are statistically insignificant. In contrast, the second-wave long portfolios are positive and statistically significant in the "6m24" holding period portfolios. Meanwhile, the short portfolios are always insignificant, whereas the long-short portfolios are positive and significant in both the four-factor and three-factor models and in both holding periods. Overall, we conclude that the portfolio analysis yields congruent results with those in the *Q* regressions, and provides further robustness to our main finding that poison pill laws have positive corporate value implications.[65]

6. Shadow Pills and the Channels of Value

6.1 Hypotheses

In this section, we investigate possible explanations for our finding of a positive relation between firm value and the adoption of poison pill laws – that is, the strengthening of a firm's shadow pill. In particular, drawing on the existing theoretical literature, we explore two potential hypotheses for the value relevance of a stronger shadow pill: the "bargaining power hypothesis" and the "bonding hypothesis," respectively. The first hypothesis is rooted in the rationale that having the right to halt a takeover increases the ability of a target's board of directors to "bargain" with a potential bidder and, ultimately, extract a higher purchasing price for the benefit of the target's shareholders (Stulz, 1988; Harris, 1990). The second hypothesis posits that shareholders are made better off by takeover deterrents since these mechanisms allow a firm to "bond" itself to operational strategies that otherwise would be at risk of reversal by an acquiring organization (Shleifer and Summers, 1988; Laffont and Tirole, 1988). We test each hypothesis as a source of value of the shadow pill in our matched sample.

6.1.1 The Bargaining Power Hypothesis

Our first empirical test of the bargaining power hypothesis explores whether the right to adopt a poison pill, as sanctioned by the adoption of a poison pill law, alters the likelihood that a treated firm will: (1) receive a bid (*Bid*) and/or (2) be successfully acquired (*Complete*). We obtain data on M&A activity from the SDC M&A and CRSP

[65] We additionally provide results for a second alternative measure of firm value, *Total Tobin's Q* (as proposed by Peters and Taylor, 2017) in Table G10 of the supplementary appendix. Our main pooled panel and matched sample results hold in these specifications.

(delisting code in the 200s) databases. *Bid* (*Complete*) is defined as an indicator variable equal to one if a target firm announces that it has received a bid (has a completed bid) in the SDC M&A database or has a delisting code in the 200s of the CRSP database, and zero otherwise. In order for a bid to be considered in our sample we require that all targets are U.S. firms and that the size of the deal is at least $100 million. Moreover, we only include bids that are for at least a 50% controlling stake in the target. Table F10 presents the results, where we specify year fixed effects in all four columns and three-digit SIC code industry fixed effects in columns (2) and (4) of each respective panel.

In columns (1) and (2) of Table F10, Panel A, we find that treated firms in the full matched sample are equally likely to receive a takeover bid as control companies, as the coefficient on *Treat* × *Post* is statistically insignificant. Similar results obtain in columns (3) and (4), indicating that poison pill laws neither deter nor bring about successful acquisitions. Congruent with our earlier approach in this study, we also consider the differential impact of first- (Panel B) versus second- (Panel C) wave poison pill laws. As in Panel A, we do not find evidence that incorporation in a state that passed a poison pill law in either the first- or second-wave periods alters a firm’s likelihood of receiving a takeover bid or being successfully acquired. Consistent with previous empirical studies (Ambrose and Megginson, 1992; Bhagat and Jefferis, 1993; Comment and Schwert, 1995; Heron and Lie, 2006), we also show that *Firm-Level Poison Pill* does not significantly alter the propensity to receive a bid or to be successfully acquired.

Nevertheless, the standalone evidence from Table F10 is a necessary but insufficient condition to determine the merits of the bargaining power hypothesis as a potential source of value for the positive association between poison pill laws and *Q*.[66] Fully testing this hypothesis also requires an investigation into the ability of the bargaining mechanism to actually create value. We explore this next in Table F11, where we analyze the value implications of poison pill laws for firms at risk of takeover bids.

In particular, Table F11 shows results for two separate sets of tests. The first empirical specification regresses *Q* on *Treat* × *Post* interacted with two proxy variables for M&A activity. The first proxy variable is *Incorp State-Year M&A Volume*, which is measured as the ratio of completed M&A dollar volume to total market capitalization per state of incorporation in a given year. The second proxy variable is *Industry-Year M&A Volume*, defined as the ratio of completed M&A dollar volume to total market capitalization per Fama-French 49 industry grouping in a given year (following Cremers, Litov, and Sepe, 2017).[67] The second set of tests considers the impact of *Treat* × *Post* on takeover premiums for the *1-Day Premium*, *1-Week Premium*, and *4-Week Premium* respectively. These dependent variables (which are all from the SDC M&A database) capture the premium associated with the offer price to the target's respective closing price 1-day, 1-week, and 4-weeks prior to the announcement date.

Panel A of Table F11 presents the results for our first empirical specification. Columns (1) and (2) suggest that poison pill laws do not provide differential value gains for treated firms that are more susceptible to receiving takeover bids in the full matched sample, as the coefficient estimates on the triple interaction terms are negative and

[66] The standalone evidence from Table F10 is, however, inconsistent with the managerial entrenchment hypothesis (Cary, 1969).

[67] We assign *Industry-Year M&A Volume* by Fama-French 49 industry grouping since we exactly match on two-digit SIC codes in the matched sample.

insignificant for both of our M&A activity proxies. The next two columns consider the effect of first-wave poison pill laws on the Tobin's Q of firms that are more likely to experience takeover activity. In particular, column (4) shows that treated firms that experience a one standard deviation increase in *Industry-Year M&A Volume* exhibit a reduction in Q of 3.1% (=0.901× 0.050)/1.458) relative to the sample average. Finally, columns (5) and (6) document the absence of a differential impact of second-wave poison pill laws on treated firms that are more susceptible to a takeover.

Panel B of Table F11 then shows results for the effect of poison pill law treatment status on target firms' takeover premiums in the full matched sample.[68] The first two columns indicate that a stronger shadow pill does not result in a higher one-day takeover premium relative to control firms without access to a correspondingly strong shadow pill. Moving to columns (3) and (4) and then (5) and (6), we find again no evidence suggesting that the shareholders of treated companies benefitted from an enhanced ability to bargain with bidding firms. These results hold with or without controls for the other four antitakeover laws. However, we do find some evidence consistent with Heron and Lie (2006, 2015) that one-day and one-week takeover premiums are positively correlated with the adoption of visible poison pills (see columns (1) and (3)).

Hence, we conclude that, overall, we do not find evidence that poison pill laws increase the treated firms' bargaining power relative to the bargaining power of firms

[68] Given that our pool of matched firms is restricted to companies with non-missing firm-level poison pill data and that we are estimating regressions around tight three-year windows, we only have 129 deals with non-missing premium data. As such, we focus only on the full matched sample since further splitting by waves reduces the sample points even further.

incorporated in states without such legislation, as neither the treated firms' Tobin's Q nor their takeover premiums are significantly affected by the passage of these laws.

6.1.2 The Bonding Hypothesis

As the bargaining power hypothesis seems unable to explain the positive value implications of poison pill laws, we move to investigating the bonding hypothesis as a potential source of value. As mentioned above, this hypothesis posits that companies shielded from the threat of takeover are more apt to commit to specific operational strategies, which would promote increased firm value. To test if this is the case in our sample, we explore whether the ability to bond to given corporate policies through a more certain right to adopt a poison pill results in gains in either operational efficiency or Tobin's Q.

6.1.2.1 Poison Pill Laws and Operational Efficiency

In Table F12, we employ four dependent variables of operational efficiency. The first proxy is return on assets (*ROA*) scaled by the book value of assets. Second, we consider net profit margin (*NPM*) scaled by sales. Third, we specify operating margin (*OM*) measured as operating income after depreciation and amortization over total sales. Fourth, we use sales growth, which is defined as the difference between next-period and current-period sales divided by this period's sales. Lastly, we lead these measures by one-year since the impact of the poison pill laws on corporate policy likely occurs with a lag.

Panel A of Table F12 shows the matched sample regression estimates for our four operational efficiency measures on $Treat \times Post$ in the full sample. We find that firms incorporated in a poison pill law adopting state experience statistically significant increases in three of four of these measures relative to the sample mean. For example, in column (1), we show that the right to adopt a poison pill increases ROA by 6.9% (=0.009/0.130). Similar instances of increases in operational efficiency hold for columns (2) (*NPM*) and (4) (*SG*), respectively.

Further, we test for differential effects of first- versus second-wave poison pill laws in Panels B and C, and find, again, that the entirety of the increases in operational efficiency occurs for firms incorporated in states adopting laws during the second-wave period (1995 to 2009), as all four columns in Panel C suggest positive and significant increases in *ROA*, *NPM*, *OM*, and *SG*. On the other hand, *Treat* × *Post* is insignificant in each of columns (1) – (4) in Panel B. In sum, Table F12 provides some initial evidence supporting the bonding hypothesis, indicating that treated firms, which are arguably better able to commit to corporate strategies via the access to a stronger shadow pill, experience increases in operational efficiency.

6.1.2.2 Poison Pill Laws, Innovative Activity and Firm Value

If shadow poison pills serve as a commitment device that better enables the board to consider the long-term interests of the firm's stakeholders, as implied by the bonding hypothesis, then poison pill laws could matter more for innovation-intense firms. Indeed, innovation often requires firm-specific investments by top employees, suppliers, customers, or strategic alliance partners. As a result, a shadow pill could be useful to prevent the ex-post expropriation of the stakeholders' firm-specific investments in firms more engaged in innovative or informationally complex business projects.

We test this specification of the shadow pill's bonding hypothesis using the following three proxies. The first proxy is *R&D/Sales* for the intensity of corporate expenditures on research and development activities (Bushee, 1998; Chan, Lakonishok, and Sougiannis, 2001; Eberhart, Maxwell, and Siddique, 2004), which we construct using financial data from Compustat. The second proxy, *Intangible Capital/Assets*, is a "catch-all" measure of the complexity of firm operations and asymmetric information (Core, Holthausen, and Larcker, 1999; Duru, Wang, and Zhao, 2013). We build *Intangible Capital/Assets* using the data provided by Peters and Taylor (2017) on WRDS, with this measure being a component of their Total Tobin's Q *(Total Q)* measure. Our third proxy *Knowledge Capital/Assets* is another "catch-all" measure for the complexity of firm operations and asymmetric information, as it is designed to estimate both the significance of knowledge capital like R&D and intellectual property assets, as well as the complex nature behind their use. This measure is again provided by Peters and Taylor (2017) on WRDS, as it constitutes another input in their construction of *Total Q*.

Panel A of Table F13 shows the results for each of these proxies for innovative activity interacted with *Treat* × *Post* in the full period matched sample. Again, consistent with the bonding hypothesis of the shadow pill, columns (1) – (3) indicate that all three of our proxies for innovative actvity measures interacted with the difference-in-differences estimator have a positive and significant relation with *Q*. For example, in column (2), a one standard deviation increase in *Intangible Capital/Assets* results in a differential increase in *Q* of 8.2% (=0.394×0.339/1.638) for firms incorporated in states with a poison pill law relative to matched controls with average intangible assets.

Panel B of Table F13 reports the estimates from splitting the matched samples into the first- and second- wave adoption periods.[69] Columns (1) and (3) show that companies with higher levels of *R&D/Sales* and *Knowledge Capital/Assets* experience an increase in *Q* after the passage of a poison pill law even during the first-wave period. Specifically, firms with *R&D/Sales* that is one standard deviation higher than the mean experience an 8.24% (=3.336×0.036/1.458) higher *Q* if they are incorporated in a state with a first-wave poison pill law relative to firms with average R&D and absent such legislation. In columns (4) – (6), the three interaction coefficients are again positive and statistically significant for firms incorporated in states that adopted a second-wave poison pill law. Hence, while on average the first-wave poison pill laws were not followed by significant changes in firm value, changes in value are similar across the two waves for innovation-intense firms, suggesting that access to a stronger shadow pill has especially important relevance for such firms.

6.1.2.3 Poison Pill Laws, Stakeholder Relationships and Firm Value

Our next set of specifications to test the shadow pill's bonding hypothesis include three different proxies intended to measure more directly the importance of stakeholder relationships. The first, *Large Customer*, is a proxy variable for the significance of customers in generating financial value. *Large Customer* equals one if the firm has a large customer based on the Compustat segment level database (Johnson, Karpoff, and Yi, 2015; Fich, Harford, and Yore, 2017), where we obtain customer sales

[69] Table G11 of the supplementary appendix further splits our results in the full sample by wave using the quadruple interaction term *Treat* × *Post* × *Innovative Activity Proxy* × *Poison Pill Law First Wave*.

data from the historic Compustat Segment tapes. The second proxy, *Strategic Alliance*, is constructed to indicate whether the business has a long-term partnership with another company (Bodnaruk, Massa, and Simonov, 2013). This indicator variable is set equal to one if the firm participates in an active strategic alliance, and zero otherwise (Johnson, Karpoff, and Yi, 2015; Fich, Harford, and Yore, 2017). The data for this measure comes from the Thomson Reuters SDC M&A database. Finally, we capture the level of importance of employees for a corporation using Compustat financial data about the ratio of selling, general and administrative expenses over the book value of total assets, *Labor Capital* (Lev and Radhakrishnan, 2005; Eisfeldt and Papanikolaou, 2013).

Table F14 presents the matched sample regressions of *Q* on our three proxies for stakeholder relationships over the full sample interacted with the dummy variables indicating the passage of poison pill laws. In particular, Panel A of Table F14 considers the full period matched sample, with the full set of control variables including the indicator variables for other state-level antitakeover laws. Consistent with the bonding hypothesis of the shadow pill, we find in column (1) that firms incorporated in states with poison pill laws and with a *Large Customer* experience an increase in *Q* of 6.35% (=0.104/1.638) relative to the sample mean. Similarly, column (2) indicates that affected firms in a strategic alliance also experience a significant rise in firm value. Lastly, column (3) shows that a one standard deviation increase in *Labor Capital* yields an 8.3% (=0.635 $\times$ 0.213/1.638) gain in *Q* for firms covered by poison pill laws.

In Panel B of Table F14, we then disentangle our analysis for the first- and second- wave matched samples.[70] A quick glance at columns (1) – (3) and (4) – (6) suggests that the larger increase in *Q* for firms with stronger stakeholder relationships, as captured by any of our three proxies, is again entirely driven by the firms incorporated in states that adopted a poison pill law during the second wave.

7. Shadow Pills in the Shadow of Common Law

Throughout our analysis, we find that the positive value effect of poison pill laws is driven by the second-wave adoptions that took place over the period 1995 to 2009. In Section 2, we provide a justification for this difference that considers the different legal contexts underlying the enactment of the first-wave and second-wave poison pill laws. In brief, under the pervasive influence of Delaware case law, there are institutional reasons to believe that the validity of the pill even outside Delaware was fairly clear after the 1985 decision in *Moran* and until at least 1988, when subsequent Delaware decisions (*Interco* and *Pillsbury Co.*) re-injected uncertainty into the validity of the pill. Therefore, during the 1985-1988 period in which most of the first-wave poison pill laws were enacted, many firms arguably already had an effective shadow pill in place, which likely reduced the importance of introducing poison pill laws. Conversely, by the start of the second wave of poison pill laws in 1995, states that had not yet adopted a poison pill law had clearly selected an anti (or at least not-openly favorable) poison pill policy, so that second-wave laws significantly strengthened access to the shadow pill for the firms incorporated in the enacting states.

[70] Table G12 of the supplementary appendix further splits our results in the full sample by wave using the quadruple interaction term *Treat* × *Post* × *Stakeholder Relationship Proxy* × *Poison Pill Law First Wave*.

In this section, we offer two formal statistical tests of this legal argument. The first test considers an adjustment to our first- and second-wave cohorts, defining the former to span the period 1986 to 1988 and the latter to consist of laws adopted from 1989 to 2009. Additionally, in this set-up, we either exclude Delaware firms entirely or exclude them from the sample during the first wave of poison pill laws and include them as controls during the second wave. The second test constructs a poison pill validity index (*PPV Index*) that aims to capture the relative certainty in the legality of the shadow pill to test whether it is value relevant for affected firms.[71]

7.1 Poison Pill Laws, Wave Adjustments and Firm Value

In this subsection, we test whether our main results are robust to redefining the first and second wave periods around the 1988 Delaware decisions that injected novel uncertainty on firms' ability to maintain a pill (*Interco* and *Pillsbury Co.*). Indeed, following these decisions, eleven states (or 31.4% of the total affected states) adopted poison pill laws in 1989. Therefore, as a robustness check, we redefine the first-wave period to include all adopting states from 1986 to 1988 and the second wave to include

[71] In addition to our main legal justification for the differential impact of the first-wave and second-wave poison pill laws, we observe that there could also be a complementary economic explanation: that the firms affected by the first-wave and second-wave laws were different in relevant characteristics. As we document in Table G13 of the supplementary appendix, when we test for pre-treatment year (t-1) differences between the first- and second-wave treated firms and then the first- and second-wave control firms, we find significant differences in firm characteristics across the two waves, which is consistent with an economic explanation of the differential effect of poison pill laws by wave. Under this explanation, poison pill laws might entail a tradeoff. As highlighted by the takeover literature (see, e.g., Manne, 1965; Shleifer and Vishny, 2002), takeovers might have emerged as a response to re-evaluate undervalued assets, either due to managerial entrenchment or the existence of inefficient conglomerates. Accordingly, while takeover defenses, including poison pill laws, on the one hand display beneficial commitment effects, on the other they may also reduce the likelihood that undervalued assets might be put to more efficient uses through a takeover. This interpretation could explain why column 4 of Table F11, which shows results for the interaction between first-wave poison pill laws (which were enacted during the apex of the takeover era, unlike the second-wave laws) with *Industry-Year M&A Volume* has a negative and statistically significant effect on Q.

all adopters after 1988 (i.e., from 1989 to 2009). Table F15 reports the results from this robustness test.

Panel A of Table F15 presents both pooled panel and matched sample results where we exclude Delaware firms in the first-wave period reflecting that Delaware does not have a poison pill law yet is informed by *Moran*, and include these companies as control observations in the second-wave period reflecting the uncertainty injected over the use of the pill by the 1988 Delaware courts' decisions. Column (1) indicates that this different approach to first- and second- wave periods as well as to the position of Delaware yields qualitatively similar results to those in column (1) of Table F5. In addition, the specifications in columns (2) and (3) demonstrate that our main pooled panel results are also robust to the redefinition of the wave periods. For example, in column (2) we find that firms incorporated in second-wave adopting states (in this setup, 1989 to 2009) experience positive increases in Q of 10.5% (=0.155/1.471), relative to the sample median. We further obtain similar results in the matched sample regressions (columns (4) – (6)).

Panel B of Table F15 provides additional robustness that our findings are not specific to the inclusion of Delaware firms in the second-wave period, as qualitatively similar results hold in both the pooled panel and matched sample tests when we exclude Delaware firms from both wave periods. For instance, we document increases in Q of 11.7% (=0.214/1.822) for companies incorporated in second-wave adopting states, relative to its year before treatment matched sample mean.

7.2 PPV-Index and Firm Value

The second test in support of our justification for the differential impact of first-wave and second-wave poison pill laws employs a poison pill validity index (*PPV Index*) designed to capture changes across time and states of incorporation in the validity of the shadow pill. Methodologically, we use poison pill laws and poison pill case law information from Cain, McKeon, and Solomon (2017) and build an index that ranges from zero to three, where higher index values capture an enhancement in the strength of the right to adopt a poison pill or its effectiveness as a takeover defense.

Panel A of Table F16 describes the construction of the *PPV-Index*. Under the thesis of the pervasive influence of Delaware case law (Cremers and Ferrell, 2014), we first assume that the Delaware Supreme Court decision in *Moran* increased the validity of poison pills for both Delaware and non-Delaware incorporated firms (see Section 2). However, we also attempt to capture here the view that the validity of the pill remained more uncertain in non-Delaware states before the enactment of poison pill laws (Catan & Kahan, 2016; Karpoff and Wittry, 2017, Cain, McKeon, and Solomon, 2017), assuming that firms incorporated outside of Delaware are less certain of the effectiveness of poison pills. Hence, the *PPV-Index* is set equal to one for Delaware companies after *Moran* and to one-half for all others.

Next, in order to reflect the impact of validating or invalidating state court decisions, we increase the value of the *PPV-Index* to one whenever a state experiences a court case that reinforces the validity of the shadow pill. On the other hand, when a state court case invalidates the use of poison pills we adjust the *PPV-Index* to zero for firms incorporated in that state. New Jersey is an example of such a state as their court system ruled against pill provisions in the same year as *Moran*.

Further, following Cain, McKeon, and Solomon (2017), we hypothesize that the legal status of the poison pill outside Delaware was subsequently clarified by the 1990 *Georgia-Pacific v. Great Northern*[72] decision under Maine law, which ruled the view that the poison pill is invalid not to "represent statements of the current law on the issue" (Cain, McKeon, and Solomon, 2017, p. 471). Indeed, Cain, McKeon, and Solomon (2017) posit that this decision was the last state-level judicial challenge to the validity of the poison pill. On this premise, we then code the *PPV-Index* as equal to one for firms incorporated in Maine (similar to firms incorporated in Delaware after *Moran*) and also update the index value to one for all the firms incorporated in states with neither a poison pill law nor validating or invalidating case law at the time of the *Georgia-Pacific* decision (reflecting the assumption in Cain, McKeon, and Solomon (2017) that the validity of the shadow pill was no longer in doubt after *Georgia-Pacific*).

In our final adjustments to the *PPV-Index* we increase the total value of the measure to two for companies incorporated in states that adopted a poison pill law, as the statutes sanctioned the certainty of the pill validity above and beyond the decisions of state courts. Lastly, we code the index to three if a corporation is incorporated in a state that has either a poison pill law or court case that validates the use of strong poison pills (e.g., a dead-hand or no-hand pill).[73] Finally, we scale this total score by three to have a measure that ranges between zero and one.

[72] Georgia-Pacific Corp. v. Great N. Nekoosa Corp., 728 F. Supp. 807, 811 (D. Me. 1990) (Maine law).
[73] Dead-hand and no-hand pills, which are prohibited under Delaware case law, allow for a board to

In Panel B of Table F16, we then examine the relation between the *PPV-Index* and firm value. The first two columns include companies incorporated in the state of Arizona and code their index value to two after the state adopts a poison pill law (again as in Cain, McKeon, and Solomon, 2017). However, as Karpoff and Wittry (2017) do not list Arizona as adopting pill legislation, and after our own reading of the law we interpret the language as ambiguous. Thus, in the last two columns we exclude Arizona firms from the regressions entirely to make sure our results are robust to this possible measurement error.[74]

In columns (1) and (2) we find that companies incorporated in states with a higher PPV index (i.e., a more effective poison pill) experience significant increases in firm value. For instance, in the second column, which include controls for other state antitakeover laws, *Q* increases by 2.4% (=0.133×0.333/1.859) when a firm is incorporated in a state that goes from the Georgia-Pacific levels of certainty (*PPV-Index*=1/3) to that engendered by a poison pill law (*PPV-Index*=2/3). The point estimates in columns (3) and (4), which exclude Arizona firms, are nearly identical to those that include Arizona firms. Overall, we find that increases in the relative strength of the right to adopt a poison pill or its effectiveness as a takeover defense is positively related to *Q*.

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provide that the pill survives for a certain period even after the adopting directors are voted off the board.

[74] The entirety of our analysis is robust to the inclusion or exclusion of firms incorporated in the state of Arizona.

8. Robustness Analysis

8.1 Higher Dimensional Fixed Effects

To begin our checks of robustness, we evaluate the concern that the positive value relation we document in subsection 5.1.1 between *Q* and *Poison Pill Law* might be the result of an unobserved and time-varying industry characteristic. Following Catan (2017) and Karpoff and Wittry (2017), we re-specify our model from this earlier subsection with higher-dimensional industry-year fixed effects, where the industry grouping is designated by three-digit SIC codes. We also include all of the control variables we have maintained throughout our analysis and estimate robust standard errors with firm-level clustering.

Table F17 presents the pooled panel results.[75] Columns (1) through (3) document that, on average, poison pill laws remain value enhancing for the shareholders of affected firms even after controlling for unobserved time-varying heterogeneity within industry. In particular, considering the specification in column (3), which includes controls for other antitakeover laws (Karpoff and Wittry, 2017), we find that firms incorporated in states that adopt poison pill laws experience a statistically and economically significant increase in value of 8.2% (=0.120/1.471), relative to the sample median Tobin's Q. The last three columns show the familiar evidence that the effect of poison pill laws is entirely driven by the second-wave laws, whereas the first-wave laws have no statistically significant impact.

[75] We are less concerned of an unobserved and time-varying industry factor driving our results in the matched sample since we match firms exactly on industry dummies.

8.2 Without Same Year, Multi-Law Adopters

Our main focus in this study is establishing the causal effect of poison pill laws on long-term firm value. However, a potential concern of our empirical strategy is that many of the states that adopted poison pill laws also adopted other antitakeover legislation in the same year. For example, on July 18, 1989, Massachusetts enacted at once business combination, directors' duties, and poison pill laws. Therefore, to provide additional evidence that our main results are not confounded by these other state antitakeover laws we exclude all firms incorporated in states that adopt business combination, control share, and/or fair price laws in the same year that they enact poison pill legislation.[76]

Table F18 presents the results. In columns (1) and (2), we report the findings from pooled panel regressions of *Q* on *Poison Pill Law*. We find that our main results are robust to the exclusion of same year, multi-law adopting states, with (in column (1)) and without (in column (2)) firms from Delaware as controls. Columns (3) and (4) present the matched sample results. Again, we show that the effect of poison pill laws on firm value is positive and statistically significant, and unlikely to be confounded by the adoption of multiple antitakeover laws in the same year.[77]

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[76] We do not exclude corporations from states that simultaneously adopt poison pill and directors' duties laws, as the latter is fundamentally different from the other four antitakeover laws (business combination, control share, fair price, and poison pill). Indeed, directors' duties laws do not per se provide an antitakeover defense, but rather offer directors more leeway to justify the adoption of antitakeover measures by enabling them to justify the adoption of such measures based on the best interests of all stakeholders rather than just shareholders. Nine states meet this criterion and are excluded from the analysis in Table F18: Georgia, Idaho, Illinois, Indiana, Massachusetts, Pennsylvania, Rhode Island, South Dakota, and Wisconsin.

[77] Further, Table G15 of the supplementary appendix investigates the effect of poison pill laws with heterogenous provisions on firm value. We find no differential effect on *Q*.

8.3 *Timing of Firm Value Implications*

In Section 4.1 we describe our identification strategy and address potential concerns that threaten the causal interpretation of our results. To the best of our knowledge, we provide the first empirical evidence that researchers wanting to use this natural experiment should be sure to specify firm-level pills in their regression models (otherwise OVB is present), and that the adoption of these laws does not suffer from reverse causality with *Q* or other firm characteristics. The final important step in demonstrating the validity of this experiment is to offer suggestive evidence that the parallel trends assumption holds.

Table F19 presents results from these tests. Following the existing literature (e.g., Giroud and Mueller, 2010; Serfling, 2016; Klasa, Ortiz-Molina, Serfling, and Srinivasan, 2018), we investigate the dynamics of the firm value implications stemming from poison pill laws. The idea of this test is that absent the adoption of these laws, the *Q* of the affected firms (incorporated in the actual enacting states) would have evolved in a similar fashion to that of the unaffected firms (incorporated in states without poison pill laws at the time of the analysis). We implement this research design by inaccurately assigning poison pill law status to affected firms a year before (*[-1]*) the actual adoption occurs, and zero otherwise, and name this indicator variable *Poison Pill Law*$^{[-1]}$. In addition, we create the indicator variables *Poison Pill Law*$^{[0]}$ and *Poison Pill Law*$^{[1+]}$, which accurately assign poison pill law status to affected firms in the year of adoption (*[0]*), and one or more years after adoption (*[1+]*), respectively, and otherwise set these variables equal to zero. If there is no effect on the *Poison Pill Law*$^{[-1]}$ coefficient, the trends between these two groups of firms can be assumed to be parallel in *Q* before treatment occurs. Further, if the point estimate on *Poison Pill Law*$^{[1+]}$ is positive and significant, this can be assumed to suggest that the reason why we have detected a statistically significant positive difference between the affected and unaffected firms is due to the passage of the poison pill laws.

In columns (1) and (2), we consider the timing of the value relevance of pills over 1983 to 2012. Specifically, column (2), which adds controls for other state antitakeover laws, displays a positive but insignificant estimate for the placebo variable, *Poison Pill Law*$^{[-1]}$. In contrast, the "true" treatment assignment variable, *Poison Pill Law*$^{[1+]}$, documents a positive and statistically significant increase in Q of 12.4 percentage points. Further, we evaluate the timing of the effect by wave adoption (in columns (3) – (4) and (5) – (6) respectively). Consistent with the findings throughout our study, there is no positive value implications of poison pill laws in the first wave, in either the placebo or actual variables, while the second-wave period shows a positive and statistically significant point estimate of 0.236 to 0.285, and no statistically significant effect on the placebo coefficient. We therefore conclude that we present the first empirical evidence that the poison pill law natural experiment is plausibly exogenous to corporate value, and hence, our findings can be interpreted as providing causal evidence for the shareholder value of the shadow poison pill.

8.4 Shadow Pills and Staggered Boards

Analyzing the function of the shadow pill vis-à-vis other governance provisions is outside the scope of this work. In practice, however, the adoption of a poison pill is frequently accompanied by the adoption of a staggered board (Cohen and Wang, 2013). This is because the combination of these defenses substantially reduces the chances that a potential bidder might be able to have the pill removed (i.e., by replacing a majority of directors) through the ballot box, therefore strengthening the anti-takeover force of a visible poison pill (Bebchuk, Coates, and Subramanian, 2002; Bebchuk and Cohen, 2005). We accordingly investigate here the combined impact of the shadow pill and staggered boards on firm value. Our conjecture is that unlike visible poison pills, shadow pills might act more as substitute than complementary antitakeover measures. We again base our conjecture on the bonding hypothesis of takeover defenses, under which the shadow pill and the staggered board provide effective, and *independent*, commitment devices. Conversely, under the classic view of the visible pill and the staggered board, *both* these measures would be necessary when they are used for entrenchment purposes. Table F20 examines these empirical predictions.

In columns (1) and (2), we explore whether poison pill laws and staggered boards have standalone explanatory power for long-term firm value. In particular, column (2) specifies indicator variables for *Poison Pill Law* and *Staggered Board*, as well as the full set of controls including the other antitakeover law dummies, and firm and year fixed effects. We find that the adoption of a poison pill law remains a positive and significant determinant of *Q*. We also find *Poison Pill Firm-Level* remains negatively associated with Q. In addition, we confirm the prior work of Cremers, Litov, and Sepe (2017), finding that the adoption (dismissal) of a staggered board results in higher (lower) firm value, with an economic impact of 6% (=0.111/1.859).

In columns (3) and (4), we explore the respective heterogeneous effects of having both a stronger right to adopt a pill (via the enactment of a poison pill law) and a staggered board, as well as a visible pill and a staggered board, on firm value, i.e., *Poison Pill Law* × *Staggered Board* and *Poison Pill Firm-Level* × *Staggered Board* (shown in column 4). We document a lack of statistical evidence that firms in jurisdictions which passed poison pill laws experience additional differential gains in value if they have a staggered board or not (point estimate=-0.009 and *t*-stat=-0.16). Furthermore, we do not find any increase in value for firms with both a visible pill and a staggered board. The lack of statistical significance of the interacting terms does not contradict the bonding hypothesis, as this hypothesis posits that the right to adopt a poison pill and the adoption of a staggered board serve a similar purpose (and are hence substitute, rather than complementary measures). Nevertheless, the results of Table F20 suggest that more research is needed to better understand the relationship between shadow pills and staggered boards.

8.5 Additional Robustness

In addition to the three robustness checks detailed above, we include five additional tables in the supplementary appendix (Tables G16 – G20) verifying the strength of our main results. In particular, in Tables G16 – G18 we document that our methodological choice to include firms incorporated in the state of Delaware (*Poison Pill Law* = 0) as control firms does not alter the value relevance of the poison pill laws in the pooled panel regressions and in the matched sample, as our results are robust to the exclusion of Delaware firms. Finally, in Tables G19 – G20 we report the results for a placebo test in the matched sample, where we purposefully move back the actual adoption date by five years. That is, the pseudo adoption date equals the actual adoption date minus five years. We then estimate the matched sample regressions over plus and minus three-year windows around the pseudo adoption date and find insignificant point estimates on the *Treat* × *Post* coefficient, providing further support for the parallel trends assumption in our matched sample.

9. Conclusion

Consistent with the entrenchment hypothesis of takeover defenses, existing poison pill studies document that the adoption of a pill is negatively correlated with firm value. However, this result is difficult to interpret, as the decision to adopt a pill is endogenous. Indeed, because a board of directors can unilaterally adopt a poison pill at any time, even firms that do not currently have a poison pill in place always have a "shadow pill."

In this paper, we contribute to the debate on the association between poison pills and firm value by shifting the focus of attention from visible pills to shadow pills – that is, studying the *right to adopt* the pill (which right constitutes the shadow pill) rather than the *actual adoption* of a pill. We do so by exploiting the natural experiment provided by the staggered adoption of poison pill laws that validated the use of the pill, and thus strengthened the relevance of the shadow pill, in 35 U.S. states over the period 1986 to 2009.

We document that the availability of a stronger shadow pill results in an economically and statistically significant increase in firm value for the firms incorporated in the enacting states, especially for firms more engaged in innovation or with stronger stakeholder relationships. This suggests that a stronger shadow pill benefits shareholders in some subsets of firms, even if the (endogenous) adoption of a visible pill does not. Overall, our results that the shadow pill serves a positive corporate governance function for some subset of firms are most consistent with the "bonding hypothesis" of takeover defenses, under which the right to adopt a pill increases firm value by re-empowering the board against short-term shareholder interference that can be disruptive of a firm's commitment toward more stable stakeholder relationships or longer-term investments projects.

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Appendix A: Chapter 1 Variable Definitions

Table A1: Variable Definitions

This table provides definitions for all the variables used in Chapter 1 and Tables B1 through B17, and G1 through G8.

Dependent Variables	Definition
Tobin's Q	Market value of assets (*at* – book equity + market equity (*prcc_f*csho*)) divided by the book value of assets (*at*). Book equity and this measure, in general, follows Fama and French (1992).
Ln(Patent)	The natural logarithm of one plus a patent count variable, as constructed in Hall, Jaffe, and Trajtenberg (2001), and Kogan et al. (2017). *Source:* https://iu.app.box.com/v/patents (available for the period 1926 to 2010). This variable is also specified as an interaction variable in a separate analysis.
Ln(CW Patent)	The natural logarithm of one plus citation-weighted patents, as constructed in Hall, Jaffe, and Trajtenberg (2001), and Kogan et al. (2017). *Source:* https://iu.app.box.com/v/patents (available for the period 1926 to 2010). This variable is also specified as an interaction variable in a separate analysis.
Ln(SM Patent)	The natural logarithm of one plus stock market-weighted patents, as constructed in Kogan et al. (2017). *Source:* https://iu.app.box.com/v/patents (available for the period 1926 to 2010). This variable is also specified as an interaction variable in a separate analysis.
ROA	Income before extraordinary items (ib) plus depreciation and amortization (dp) divided by book value of assets (at). *ROA* is also included as a control variable when *Profitability* is not specified as a dependent variable.
NPM	Net profit margin defined as operating income before depreciation and amortization (*oibdp*) divided by net sales.
OPM	Operating profit margin defined as total revenue (*sale*) minus the cost of goods sold (*cogs*) minus selling, general, and administrative expenses (*xsga*) all scaled by total revenue (*sale*).
Z–score	*Z*-score is a measure to indicate the likelihood of a company going bankrupt or having significant financial distress defined as 1.2*(*wcap/at)* + 1.4*(*re/at*) + 3.3(*ebit*/*at*) + 0.6(*prcc*csho/lt*) + 1.0(*sale/at*).
OCF Ratio	Operating cash flow ratio defined as operating cash flow (*ocf*) divided by current liabilities (*lct*).

Table A1 – *(Continued)*

Loss	An indicator variable set to one if a firm has negative net income (*ni*) during a fiscal year, and zero otherwise; also a control variable in the *Tobin's Q* regressions.
R&D	Research and development expense (*xrd*) divided by the value of sales (*sale*). *R&D/Sales* is also included as a control variable when not specified as a dependent variable.
CAPX	Capital expenditures (*capx*) divided by the value of total book assets (*at*). *CAPX/Assets* is also included as a control variable when not specified as a dependent variable.
Invest Rate	Capital expenditures (*capx*) plus acquisitions (*aqc*) minus the sale of property (*sppe*), over the book value of assets (*at*).
Advertise	Advertising expense (*xad*) divided by the value of total book assets (*at*).
Organization	Selling, general and administrative expense (*xsga*) divided by the value of total book assets (*at*).
Labor	Number of employees (*emp*) divided by real assets (*at*), where assets are adjusted using 2015 dollars.
Monthly Stock Returns	Monthly stock returns of a portfolio created by either (i) longing the stocks of firms headquartered in APM adopting states, (ii) shorting the stocks of companies from either the neighboring state(s) of APM law adopters, or non-manufacturing corporations located in a state that passes an APM statute, and (iii) combining both (i) and (ii) into a long-short investment strategy. In all three portfolios, I begin the holding period 12 months before the adoption date and continue to hold until 36 months after the laws are enacted ("12m36").
Total Tobin's Q	Total Tobin's Q equals the market value of outstanding equity (prcc_f*csho) plus the book value of debt (dltt + dlc) minus the firm's current assets (act) divided by the sum of physical (ppegt) and intangible capital. Intangible capital is defined as the sum of externally purchased (intan) and internally created intangible capital (knowledge plus organizational capital). This measure (q_tot) is proposed by Peters and Taylor (2017) and is available on WRDS from 1950 to 2015.

Table A1 – *(Continued)*

Main Explanatory Variables	
APM Law	An indicator variable equal to one if a firm is headquartered in a state that has adopted an anti-plug molding (APM) law, and zero otherwise. I use the state specific statute names provided by Sganga (1989) and Carstens (1990) to perform a *LexisNexis Academic* "State Statutes and Regulations Search" to obtain the adoption month/years and confirm stipulated product coverage.
All Item APM Law	An indicator variable equal to one if a firm is headquartered in a state that has adopted an APM law which protects all manufacturing items, and zero otherwise.
Boat Hull APM Law	An indicator variable equal to one if a firm is headquartered in a state that has adopted an APM law which explicitly stipulates protection for boat hulls and components, and zero otherwise.
Alpha	Monthly portfolio abnormal returns, estimated using the four-factor Carhart (1997) and three-factor Fama-French (1993) models, respectively.
Main Interaction Variables	
Post 88	An indicator variable equal to one if the year of observation occurs after 1988, and zero otherwise.
Research Quotient (RQ)	A continuous variable measuring the percentage increase in revenue from a 1% increase in R&D. That is, it measures the output elasticity of R&D (Knott (2008)). This measure (*aggbeta_lxrd*) is available on WRDS from 1971 to 2015.
RQ Median	An indicator variable equal to one if a firm's *Research Quotient* is above the 50^{th} percentile of all companies in the sample, in a given year, and zero otherwise.
RQ High	An indicator variable equal to one if a firm's *Research Quotient* is in the top 33^{rd} percentile of all companies in the sample, in a given year, and zero otherwise.
Control Variables	
Size	The natural logarithm of the value of total book assets (*at*) in millions, where assets are adjusted using 2015 dollars.
Ln(Age)	The natural logarithm of one plus the number of firm-year observations since the firm's first appearance in Compustat.
Debt-to-Equity	Long-term debt (*dltt*) divided by book equity, where book equity is calculated as in Fama and French (1992).

Table A1 – *(Continued)*

Operating Cash-Flow	Operating cash flow equals the summation of income before extra items (*ibc*), extra items and discontinued operation (*xidoc*), depreciation and amortization (*dpc*), deferred taxes (*txdc*), equity in net loss (*esubc*), gains in sale of PPE and investment (*sppiv*), other funds from operation (*fopo*), other sources of funds (*fsrco*) minus the change in working capital (*dWC*), all scaled by last year's book value of assets (*at*), following Chang et al. (2014).
HHI	The Herfindahl-Hirschman Index for a particular industry defined as the sum of squared market shares for all firms in a three-digit SIC industry. The market share of firm *i* is defined as the value of sales (*sale*) of firm *i* divided by the total value of sales (*sale*) in the industry of firm *i*.
Sales Growth	The natural logarithm of the value of sales (*sale*) in millions in year *t* divided by the value of sales (*sale*) in millions in year *t*-1.
Firm Liquidity	Current assets (*act*) minus current liabilities (*lct*) divided by the value of total book assets (*at*).
Industry-Year Tobin's Q	Control for industry shocks, measured as the mean of Tobin's Q in firm *i*'s three-digit SIC industry in a given year, excluding firm *i* from the calculation.
RQ Medium	An indicator variable equal to one if a firm's *Research Quotient* is in between the upper and bottom 33rd percentiles of all companies in the sample, in a given year, and zero otherwise.
RQ Low	An indicator variable equal to one if a firm's *Research Quotient* is in the bottom 33rd percentile of all companies in the sample, in a given year, and zero otherwise.
Predictor Variables	
SY Tobin's Q	The average *Tobin's Q* of all firms headquartered within a state, in a given year.
SY Δ *Tobin's Q*	The average change in *Tobin's Q* of all firms headquartered within a state, in a given year.
SY Industry-Year Tobin's Q	The average *Industry-Year Tobin's Q* of all firms headquartered within a state, in a given year.
SY Size	The average natural logarithm of total assets of all firms headquartered within a state, in a given year, where assets are adjusted using 2015 dollars.
SY Ln(Age)	The average natural logarithm of one plus the number of firm-year observations since the firm's first appearance in Compustat of all firms headquartered within a state, in a given year.

Table A1 – *(Continued)*

SY HHI	The average Herfindahl-Hirschman Index of all firms headquartered within a state, in a given year.
SY Sales Growth	The average sales growth of all firms headquartered within a state, in a given year.
SY Loss	The average percent of all firms headquartered within a state experiencing negative net income, in a given year.
SY Debt-to-Equity	The average debt-to-equity of all firms headquartered within a state, in a given year.
SY Firm Liquidity	The average firm liquidity of all firms headquartered within a state, in a given year.
SY R&D/Sales	The average ratio of research and development expenditure to sales of all firms headquartered within a state, in a given year.
SY CAPX/Assets	The average ratio of capital expenditure to total assets of all firms headquartered within a state, in a given year.
Ln(GDPPC)	The natural logarithm of a headquartering state's GDP (in thousands) divided by its total population. I use data from the U.S. Bureau of Economic Analysis.
State GDPG	The headquartered state-level GDP growth rate over the fiscal year. I use data from the U.S. Bureau of Economic Analysis.
UTSA Index	The change in state-specific trade secrets protection after the enactment of the Uniform Trade Secrets Act (UTSA), following Png (2017a, 2017b).
IDD	Inevitable Disclosure Doctrine indicator variable, which equals one if it is recognized by a state and zero otherwise, following Klasa et al. (2018).
R&D Tax Credit	An indicator variable set to one if a state has adopted a tax credit for research & development expenditure, and zero otherwise, following Wilson (2009).

Appendix B: Chapter 1 Tables and Figures

Table B1. State-Level APM Laws

This table provides relevant institutional details about the APM law experiment. Using exact state statute names listed by Sganga (1989) and Carstens (1990), I obtain the corresponding adoption month/year and confirm the scope of stipulated product coverage from the "State Statutes and Regulations Search" option on *LexisNexis Academic*. Also included, is the number of unique manufacturing firms headquartered in the statute passing states at any time in the panel from 1975 to 1992. Panel B provides the jurisdiction, name, month/year and decision for the most significant court cases that took place over the existing life of the state laws (Samuelson and Scotchmer, 2002). Panel C shows the pseudo assignment of APM law coverage to adopting states' nearest neighboring state(s).

Panel A: APM Law States, Statute Names, and Adopting Years

State	Statute	Month/Year of Adoption	Products Stipulated	# of Unique Firms
California	*CAL. BUS. & PROF. CODE § 17300*	10/1978	All items	562
Florida	*FLA. STAT. § 559.94*	5/1983	Boat hulls	158
Indiana	*IND. CODE §§ 24-4-8-1*	8/1987	Boat hulls	45
Kansas	*KAN. STAT. ANN. § 50-802*	7/1984	Boat hulls	28
Louisiana	*LA. REV. STAT. ANN. § 51: 462.1*	7/1985	Boat hulls	8
Maryland	*MD. COM. LAW CODE ANN. § 11-1001*	4/1986	Boat hulls	55
Michigan	*MICH. COMP. LAWS §§ 445.621*	3/1983	All items	128
Mississippi	*MISS. CODE ANN. § 59-21-41*	3/1985	Boat hulls	7
Missouri	*MO. REV. STAT. § 306.900*	4/1986	Boat hulls	60
North Carolina	*N.C. GEN. STAT. §§ 75A-27*	7/1985	Boat hulls	83
Tennessee	*TENN. CODE ANN. § 47-50-111*	7/1983	All items	38
Wisconsin	*WIS. STAT. ANN. § 134.34*	6/1983	Boat hulls	76

Panel B: Significant Court Cases Determining the Legitimacy of the APM Laws

Jurisdiction	Court	Court Cases	Month/Year of Ruling	Decision
California	District Court	*Interpart Corp. v. Imos Italia*	7/1984	Invalidates CA statute
California	Federal Circuit	*Interpart Corp. v. Imos Italia*	11/1985	Validates CA statute
Florida	District Court	*Bonito Boats, Inc. v. Thunder Craft Boats, Inc.*	12/1984	Invalidates FL statute
Florida	Supreme Court	*Bonito Boats, Inc. v. Thunder Craft Boats, Inc.*	11/1987	Invalidates FL statute
United States	Supreme Court	*Bonito Boats, Inc. v. Thunder Craft Boats, Inc.*	2/1989	Invalidates all statutes

Panel C: Neighboring State Placebo Test Assignment

State	Neighboring State(s)	Pseudo Adoption Month/Year
California	Arizona, Nevada, Oregon	10/1978
Florida	Georgia	5/1983
Indiana	Illinois	8/1987
Kansas	Colorado, Nebraska	7/1984
Louisiana	Oklahoma, Texas	7/1985
Maryland	Delaware, Pennsylvania, West Virginia	4/1986
Michigan	Ohio	3/1983
Mississippi	Alabama	3/1985
Missouri	Arkansas, Iowa	4/1986
North Carolina	South Carolina	7/1985
Tennessee	Kentucky, Virginia	7/1983
Wisconsin	Minnesota	6/1983

Table B2. Describing the "Products" Sample

This table provides a description of the "Products" sample. I detail each of the two-digit SIC codes within the 2000 – 3999 manufacturing industry, their descriptions, and whether they are included in the "Products" sample. I create this separate sample for the following reason. The APM laws protect firms located in those adopting states from "direct molding process" reverse engineering. As such, the direct molding process requires constructing molds of tangible products that can be fitted with a hardening substance for purposes of manufacturing replica items. Therefore, for example, food and kindred products (e.g., meat, dairy, and bakery products) companies are likely unaffected by the statutes and as such I exclude them from the affected group in this sample. *Source*: siccode.com

two-digit SIC Codes	Description	Included in the "Products" Sample
20	Food and Kindred Products	No
21	Tobacco Products	No
22	Textile Mill Products	No
23	Apparel and other Finished Products Made from Fabrics and Similar Materials	No
24	**Lumber and Wood Products, except Furniture**	**Yes**
25	**Furniture and Fixtures**	**Yes**
26	Paper and Allied Products	No
27	Printing, Publishing, and Allied Industries	No
28	Chemicals and Allied Products	No
29	Petroleum Refining and Related Industries	No
30	**Rubber and Miscellaneous Plastics Products**	**Yes**
31	**Leather and Leather Products**	**Yes**
32	**Stone, Clay, Glass, and Concrete Products**	**Yes**
33	Primary Metal Industries	No
34	**Fabricated Metal Products, except Machinery and Transportation Equipment**	**Yes**
35	**Industrial and Commercial Machinery and Computer Equipment**	**Yes**
36	**Electronic and other Electrical Equipment and Components, except Computer Equipment**	**Yes**
37	**Transportation Equipment**	**Yes**
38	**Measuring, Analyzing, and Controlling Instruments; Photographic, Medical and Optical Goods; Watches and Clocks**	**Yes**
39	**Miscellaneous Manufacturing Industries**	**Yes**

Table B3. Summary Statistics

This table reports summary statistics for the main dependent, independent, and interacted variables used in the panel regressions during the period 1975 to 1992. Panel A presents summary statistics for the "Manufacturing" sample, defined as firms operating within the SIC code range: 2000–3999. Panel B shows the summary statistics for the "Products" dataset. Table B2 describes the "Products" samples. All continuous variables are winsorized at the 1st and 99th percentiles and the dollar values are expressed in 2015 dollars. Table A1 provides variable definitions.

Panel A: "Manufacturing" Sample

Dependent Variables:	Mean	St. Dev.	P25	Median	P75	Obs.
Tobin's Q	1.542	1.293	0.920	1.155	1.612	32,808
Ln(Patent)	0.155	0.373	0	0	0.094	32,808
Ln(CW Patent)	1.010	1.498	0	0	1.773	32,808
Ln(SM Patent)	0.686	1.419	0	0	0.504	32,808
ROA	0.103	0.173	0.066	0.135	0.192	32,808
NPM	0.069	0.159	0.047	0.097	0.143	32,808
OPM	-0.068	1.021	0.030	0.086	0.134	24,643
Z-score	2.141	1.887	1.764	2.488	3.104	32,802
OCF Ratio	0.149	0.926	-0.026	0.236	0.525	24,602
Loss	0.240	0.427	0	0	0	32,808
R&D/Sales	0.065	0.228	0	0.011	0.046	32,808
CAPX/Assets	0.065	0.054	0.029	0.052	0.084	32,808
Invest Rate	0.069	0.073	0.025	0.053	0.094	15,771
Advertise	0.014	0.029	0	0	0.016	32,808
Organization	0.325	0.221	0.176	0.282	0.418	32,808
Labor	0.016	0.011	0.009	0.014	0.020	24,344
Total Tobin's Q	0.696	1.581	0.015	0.280	0.752	32,730

Independent Variables:	Mean	St. Dev.	P25	Median	P75	Obs.
APM Law	0.176	0.381	0	0	0	32,808
All Item APM Law	0.132	0.338	0	0	0	32,808
Boat Hull APM Law	0.045	0.206	0	0	0	32,808
Size	5.170	2.016	3.738	5.047	6.450	32,808
Ln(Age)	2.561	0.561	2.197	2.708	2.996	32,808
Debt-to-Equity	0.469	1.107	0.053	0.267	0.577	32,808
Operating Cash-Flow	0.123	0.318	0.059	0.127	0.192	32,808
HHI	0.246	0.155	0.139	0.217	0.284	32,808
Sales Growth	0.089	0.288	-0.023	0.085	0.188	32,808
Firm Liquidity	0.341	0.209	0.215	0.349	0.477	32,808
Industry-Year Tobin's Q	1.683	0.750	1.136	1.468	2.039	32,808

Interacted Variables:	Mean	St. Dev.	P25	Median	P75	Obs.
RQ	0.138	0.108	0.080	0.142	0.198	13,829
RQ High	0.345	0.475	0	0	1	13,829
RQ Median	0.509	0.500	0	1	1	13,829
RQ Medium	0.333	0.471	0	0	1	13,829
RQ Low	0.320	0.466	0	0	1	13,829

Table B3 – *(Continued)*

Panel B: "Products" Sample

Dependent Variable:	Mean	St. Dev.	P25	Median	P75	Obs.
Tobin's Q	1.542	1.266	0.933	1.167	1.619	21,791
Ln(Patent)	0.145	0.347	0	0	0.096	21,791
Ln(CW Patent)	1.026	1.460	0	0	1.824	21,791
Ln(SM Patent)	0.604	1.286	0	0	0.437	21,791
ROA	0.094	0.177	0.056	0.128	0.188	21,791
NPM	0.065	0.157	0.043	0.095	0.140	21,791
OPM	-0.054	0.939	0.025	0.083	0.129	16,830
Z-score	2.031	1.912	1.684	2.425	3.028	21,786
OCF Ratio	0.138	0.854	-0.056	0.201	0.486	16,799
Loss	0.261	0.439	0	0	1	21,791
R&D/Sales	0.064	0.194	0	0.019	0.062	21,791
CAPX/Assets	0.063	0.054	0.028	0.049	0.080	21,791
Invest Rate	0.066	0.071	0.024	0.049	0.088	10,973
Advertise	0.011	0.022	0	0	0.015	21,791
Organization	0.334	0.212	0.198	0.291	0.418	21,791
Labor	0.017	0.010	0.010	0.014	0.021	16,620
Total Tobin's Q	0.684	1.565	-0.000	0.262	0.749	21,740

Independent Variables:	Mean	St. Dev.	P25	Median	P75	Obs.
APM Law	0.196	0.397	0	0	0	21,791
All Item APM Law	0.153	0.360	0	0	0	21,791
Boat Hull APM Law	0.042	0.202	0	0	0	21,791
Size	4.842	1.898	3.518	4.689	6.008	21,791
Ln(Age)	2.513	0.562	2.079	2.639	2.944	21,791
Debt-to-Equity	0.451	1.131	0.041	0.238	0.556	21,791
Operating Cash-Flow	0.122	0.340	0.052	0.124	0.194	21,791
HHI	0.245	0.143	0.146	0.225	0.280	21,791
Sales Growth	0.091	0.299	-0.032	0.088	0.201	21,791
Firm Liquidity	0.365	0.209	0.249	0.378	0.498	21,791
Industry-Year Tobin's Q	1.705	0.666	1.206	1.554	2.072	21,791

Interacted Variables:						
RQ	0.131	0.107	0.073	0.136	0.191	10,108
RQ High	0.318	0.466	0	0	1	10,108
RQ Median	0.479	0.500	0	0	1	10,108
RQ Medium	0.334	0.472	0	0	1	10,108
RQ Low	0.346	0.476	0	0	1	10,108

Table B4. Explaining the Adoption of APM Statutes

This table reports results for regressions of *APM Law* on state-year average firm characteristics and state-level macro and legal factors over the period 1975 to 1992. Panel A, columns (1) – (2) presents estimates from the "Manufacturing" sample, while columns (3) – (4) are specific to the "Products" dataset. Table B2 describes the "Products" samples. I drop all firms located in an APM law passing states after its adoption. All continuous variables have been winsorized at the 1st and 99th percentiles and standardized to have zero mean and unit variance. The predictor variables are lagged one period. Table A1 provides variable definitions. The estimated *t*-statistics are based on robust standard errors independently double clustered by state of location and year (reported in parentheses). The dollar values are expressed in 2015 dollars. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Dep. Variable: $APM\ Law_{[t]}$	1975 – 1992			
Sample:	"Manufacturing"		"Products"	
	(1)	(2)	(3)	(4)
$SY\ Tobin's\ Q_{[t-1]}$	0.014	0.015	0.017	0.016
	(0.52)	(0.57)	(0.61)	(0.59)
$SY\ \Delta\ Tobin's\ Q_{[t-1]}$	-0.009	-0.011	-0.011	-0.013
	(-0.62)	(-0.78)	(-0.80)	(-0.98)
$SY\ Industry\text{-}Year\ Tobin's\ Q_{[t-1]}$	0.002	0.008	0.002	0.010
	(0.08)	(0.32)	(0.10)	(0.41)
$SY\ Size_{[t-1]}$	0.024	0.026	0.027	0.030
	(0.52)	(0.56)	(0.53)	(0.59)
$SY\ Ln(Age)_{[t-1]}$	-0.008	-0.009	-0.008	-0.012
	(-0.17)	(-0.20)	(-0.17)	(-0.24)
$SY\ Debt\text{-}to\text{-}Equity_{[t-1]}$	0.004	0.003	0.002	0.000
	(0.69)	(0.41)	(0.27)	(0.01)
$SY\ ROA_{[t-1]}$	-0.021	-0.027	-0.026	-0.033
	(-0.43)	(-0.50)	(-0.49)	(-0.57)
$SY\ Operating\ Cash\text{-}Flow_{[t-1]}$	0.006	0.005	0.007	0.007
	(0.43)	(0.38)	(0.50)	(0.45)
$SY\ HHI_{[t-1]}$	0.011	0.010	0.012	0.010
	(0.95)	(0.78)	(0.91)	(0.71)
$SY\ Sales\ Growth_{[t-1]}$	-0.004	-0.003	-0.005	-0.004
	(-0.47)	(-0.32)	(-0.51)	(-0.34)
$SY\ Loss_{[t-1]}$	-0.131	-0.135	-0.146	-0.154
	(-0.46)	(-0.46)	(-0.51)	(-0.51)
$SY\ Firm\ Liquidity_{[t-1]}$	0.017	0.016	0.018	0.017
	(0.67)	(0.66)	(0.63)	(0.60)
$SY\ R\&D/Sales_{[t-1]}$	0.000	0.001	-0.003	-0.001
	(-0.03)	(0.09)	(-0.15)	(-0.08)
$SY\ CAPX/Assets_{[t-1]}$	-0.011	-0.009	-0.013	-0.012
	(0.67)	(-0.56)	(-0.72)	(-0.60)
$Ln(State\ GDPPC)_{[t-1]}$		-0.018		-0.023
		(-0.89)		(-1.00)
$State\ GDPG_{[t-1]}$		0.000		0.000
		(0.00)		(-0.02)
$UTSA\ Index_{[t-1]}$		0.007		0.007
		(0.58)		(0.58)
$IDD_{[t-1]}$		0.001		0.007
		(0.03)		(0.18)
$R\&D\ Tax\ Credit_{[t-1]}$		-0.001		0.007
		(-0.02)		(0.11)
State and year fixed effects	Yes	Yes	Yes	Yes
Number of firms	3,434	3,434	2,340	2,340
Firm-year obs.	25,445	25,445	16,359	16,359
Adjusted R^2	0.333	0.335	0.360	0.362

Table B5. Event Study: 1989 U.S. Supreme Court Ruling, Invalidating all APM Laws

This table reports cumulative abnormal returns (CARs) surrounding the U.S. Supreme Court's ruling in *Bonito Boats v. Thunder Craft Boats* for firms located in APM law adopting states. CARs are estimated over the event window [-2,+2] and pre-event windows' [-17,-3] and [-12,-3]. The first four columns provide CARs for the "Manufacturing" sample, while the last four columns show CARs for the "Products" dataset. Table B2 details the "Products" samples. The odd numbered columns specify the four-factor Carhart (1997) model (i.e., momentum, high minus low book-to-market (HML), small minus big (SMB), and market return), while the even numbered columns use the three-factor Fama-French (1993) model (i.e., HML, SMB, and market return). Columns (1) – (2) and (5) – (6) employ the CRSP equal-weighted index as the market factor, and columns (3) – (4) and (7) – (8) use the CRSP value-weighted index. The parameters for all models are estimated over the window [-271, -21] relative to the event day. The estimated *t*-statistics have been corrected for cross-sectional correlation (Kolari and Pynnönen (2010)) and are shown in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Sample:	"Manufacturing"				"Products"			
	Equal-Weighted Index		Value-Weighted Index		Equal-Weighted Index		Value-Weighted Index	
	4-Factor	3-Factor	4-Factor	3-Factor	4-Factor	3-Factor	4-Factor	3-Factor
CAR Window:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
[-17,-3]	-0.19%	-0.25%	-0.52%	-0.60%	-0.63%	-0.71%	-0.95%	-1.06%
	(-0.27)	(-0.20)	(-0.37)	(-0.42)	(-0.38)	(-0.52)	(-0.96)	(-1.08)
[-12,-3]	-0.08%	-0.09%	-0.43%	-0.45%	-0.24%	-0.26%	-0.59%	-0.62%
	(-0.66)	(-0.65)	(-0.23)	(-0.24)	(-0.33)	(0.29)	(-0.52)	(-0.56)
[-2,+2]	-0.38%**	-0.39%**	-0.33%*	-0.35%*	-0.42%**	-0.44%**	-0.37%*	-0.40%*
	(-1.72)	(-1.76)	(-1.47)	(-1.50)	(-1.73)	(-1.79)	(-1.46)	(-1.52)
Number of firms	767	767	767	767	568	568	568	568

Table B6. APM Laws and Firm Value

This table reports the results for panel regressions of *Tobin's Q* on an *APM Law* indicator variable over the sample period 1975 to 1988. Columns (1) – (3) present the estimates in the "Manufacturing" sample, while columns (4) – (6) are specific to the "Products" dataset. Table B2 describes the "Products" samples. Other controls specified but unreported due to economic and statistical insignificance: *ROA, Operating Cash-Flow, HHI,* and *Firm Liquidity*. Table A1 provides variable definitions. The industry fixed effects are defined at the two-digit SIC code level. All continuous variables are winsorized at the 1st and 99th percentiles and the dollar values are expressed in 2015 dollars. The estimated *t*-statistics are based on robust standard errors clustered by state of location (reported in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Dep. Variable: *Tobin's Q*	1975 – 1988					
Sample:	"Manufacturing"			"Products"		
Variables	(1)	(2)	(3)	(4)	(5)	(6)
APM Law	0.082***	0.082**		0.092***	0.086***	
	(2.67)	(2.58)		(3.71)	(3.40)	
All Item APM Law			0.090***			0.097***
			(3.14)			(4.37)
Boat Hull APM Law			0.068			0.062
			(0.92)			(1.07)
Size		-0.373***	-0.374***		-0.404***	-0.404***
		(-9.46)	(-9.44)		(-9.70)	(-9.69)
Ln(Age)		-0.667***	-0.667***		-0.763***	-0.764***
		(-7.29)	(-7.26)		(-7.92)	(-7.93)
Debt- to- Equity		-0.018***	-0.018***		-0.013	-0.013
		(-2.68)	(-2.68)		(-1.48)	(-1.48)
Sales Growth		0.398***	0.398***		0.393***	0.393***
		(6.73)	(6.73)		(7.22)	(7.22)
Loss		-0.052**	-0.052**		-0.074***	-0.074***
		(-2.05)	(-2.05)		(-2.90)	(-2.90)
R&D/Sales		0.929***	0.929***		1.139***	1.139***
		(4.45)	(4.45)		(4.62)	(4.62)
CAPX/Assets		1.434***	1.435***		1.376***	1.376***
		(6.84)	(6.84)		(5.93)	(5.93)
Industry-Year Tobin's Q		0.038**	0.038**		0.053**	0.053**
		(2.10)	(2.09)		(2.50)	(2.50)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of firms	3,296	3,296	3,296	2,276	2,276	2,276
Firm-year obs.	24,906	24,906	24,906	16,409	16,409	16,409
Adjusted R^2	0.701	0.731	0.731	0.675	0.713	0.713

Table B7. APM Laws, Supreme Court's Ruling and Firm Value

This table reports the results for panel regressions of *Tobin's Q* on the interaction of an *All Item APM Law* indicator variable and a time dummy indicating if the year is after 1988. The sample period spans 1975 to 1992. Columns (1) – (2) present the estimates in the "Manufacturing" sample, while columns (3) – (4) are specific to the "Products" dataset. Table B2 describes the "Products" sample. Other controls specified but unreported due to economic and statistical insignificance: *ROA, Operating Cash-Flow, HHI,* and *Firm Liquidity*. Table A1 provides variable definitions. The industry fixed effects are defined at the two-digit SIC code level. All continuous variables are winsorized at the 1st and 99th percentiles and the dollar values are expressed in 2015 dollars. The row "Test for joint significance" shows the results from a test of whether the summation of the value effect before and after 1988 is different from zero. The estimated *t*-statistics are based on robust standard errors clustered by state of location (reported in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Dep. Variable: *Tobin's Q*	1975 – 1992			
Sample:	"Manufacturing"		"Products"	
Variables	(1)	(2)	(3)	(4)
Post 88 × All Item APM Law	-0.126	-0.076	-0.079	-0.032
	(-1.62)	(-1.08)	(-1.38)	(-0.51)
All Item APM Law	0.074***	0.081**	0.078***	0.081***
	(4.95)	(2.66)	(6.53)	(3.45)
Boat Hull APM Law	0.082	0.073	0.096	0.070
	(1.14)	(1.20)	(1.51)	(1.33)
Size		-0.323***		-0.320***
		(-8.77)		(-8.65)
Ln(Age)		-0.642***		-0.674***
		(-8.29)		(-8.03)
Debt-to-Equity		-0.022***		-0.018***
		(-4.67)		(-3.21)
Sales Growth		0.379***		0.401***
		(7.43)		(8.19)
Loss		-0.066***		-0.080***
		(-3.25)		(-3.81)
R&D/Sales		0.754***		1.095***
		(4.78)		(6.45)
CAPX/Assets		1.651***		1.435***
		(11.64)		(6.74)
Industry-Year Tobin's Q		0.047**		0.055***
		(2.28)		(3.15)
Test for joint significance:				
[*Post 88 × All Item APM Law*] +	-0.052	0.006	-0.001	0.049
[*All Item APM Law*]	(-0.73)	(0.11)	(-0.01)	(0.97)
Firm fixed effects	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes
Number of firms	3,837	3,837	2,640	2,640
Firm-year obs.	32,808	32,808	21,791	21,791
Adjusted R^2	0.684	0.711	0.656	0.691

Table B8. APM Laws, Patent Activity and Firm Value

This table reports the results for panel regressions of *Tobin's Q* on an *APM Law* × *Patent Activity* interaction term over the period 1975 to 1988. Columns (1) – (3) presents the estimates for the "Manufacturing" sample, while columns (4) – (6) are specific to the "Products" dataset. Table B2 describes the "Products" sample. The included controls are: *Size, Ln(Age), Debt-to-Equity, ROA, Operating Cash-Flow, HHI, Sales Growth, Loss, Firm Liquidity, R&D/Sales, CAPX/Assets,* and *Industry-Year Tobin's Q*. Table A1 provides variable definitions. The industry fixed effects are defined at the two-digit SIC code level. All continuous variables are winsorized at the 1st and 99th percentiles and the dollar values are expressed in 2015 dollars. The row "Sample mean" provides the average value of the respective *Patent Activity* measure over the period 1975 to 1988. The row "Test for joint significance" shows the results from a test of whether the *APM Law* value effect for a firm with an average level of *Patent Activity* is different from zero. The estimated *t*-statistics are based on robust standard errors clustered by state of location (reported in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Dep. Variable: $Tobin's\ Q$	1975 – 1988					
Sample:	"Manufacturing"			"Products"		
Variables	(1)	(2)	(3)	(4)	(5)	(6)
$All\ Item\ APM\ Law \times Ln(1 + Patent)$	-0.172***			-0.188***		
	(-5.06)			(-6.59)		
$All\ Item\ APM\ Law \times Ln(1 + CW\ Patent)$		-0.038***			-0.044***	
		(-3.49)			(-3.71)	
$All\ Item\ APM\ Law \times Ln(1 + SM\ Patent)$			-0.056***			-0.069***
			(-5.81)			(-8.65)
$All\ Item\ APM\ Law$	0.114***	0.128***	0.130***	0.125***	0.145***	0.145***
	(3.73)	(3.57)	(4.51)	(5.59)	(5.16)	(7.18)
$Boat\ Hull\ APM\ Law$	0.067	0.067	0.070	0.061	0.61	0.063
	(0.92)	(0.91)	(0.95)	(1.05)	(1.04)	(1.06)
$Ln(1 + Patent)$	0.048			0.034		
	(1.20)			(0.73)		
$Ln(1 + CW\ Patent)$		0.005			-0.001	
		(0.53)			(-0.04)	
$Ln(1 + SM\ Patent)$			0.108***			0.128***
			(10.00)			(9.17)
$Patent\ Activity$ sample mean:	0.165	1.043	0.688	0.156	1.063	0.609
Test for joint significance:						
$[All\ Item\ APM\ Law \times Patent\ Activity] + [All\ Item\ APM\ Law]$	0.086***	0.089***	0.092***	0.095***	0.098***	0.104***
	(3.35)	(3.38)	(3.95)	(5.06)	(5.26)	(6.09)
All control variables	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of firms	3,296	3,296	3,296	2,276	2,276	2,276
Firm-year obs.	24,906	24,906	24,906	16,409	16,409	16,409
Adjusted R^2	0.731	0.731	0.732	0.713	0.713	0.715

Table B9. APM Laws, Supreme Court's Ruling and Patent Activity

This table reports results for panel regressions of *Patent Activity* on a *Post 88* × *APM Law* over the period 1975 to 1992. Columns (1) – (3) presents the estimates for the "Manufacturing" sample, while columns (4) – (6) are for the "Products" dataset. Table B2 describes the "Products" sample. The dependent variables are leaded two years (*t*+2). Included controls: *Tobin's Q, Size, Ln(Age), Debt-to-Equity, ROA, Operating Cash-Flow, HHI, Sales Growth, Loss, Firm Liquidity, R&D/Sales, CAPX/Assets,* and *Industry-Year Tobin's Q*. Table A1 provides variable definitions. Industry fixed effects are defined at the two-digit SIC code level. All continuous variables are winsorized at the 1st and 99th percentiles and dollar values are expressed in 2015 dollars. The estimated *t*-statistics are based on robust standard errors clustered by state of location (reported in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Period:	1975 – 1992					
Sample:	"Manufacturing"			"Products"		
Dep. Variables:	$Ln(1+Patent)_{t+2}$	$Ln(1+CW\ Patent)_{t+2}$	$Ln(1+SM\ Patent)_{t+2}$	$Ln(1+Patent)_{t+2}$	$Ln(1+CW\ Patent)_{t+2}$	$Ln(1+SM\ Patent)_{t+2}$
Variables	(1)	(2)	(3)	(4)	(5)	(6)
Post 88 × All Item APM Law	0.029***	0.091**	0.080**	0.034***	0.102**	0.092***
	(5.85)	(2.76)	(2.85)	(5.06)	(2.56)	(3.07)
All Item APM Law	-0.008**	-0.041*	-0.054**	-0.006*	-0.033*	-0.030**
	(-2.25)	(-1.99)	(-2.53)	(-1.72)	(-1.71)	(-2.10)
Boat Hull APM Law	-0.009	-0.015	-0.021	0.001	0.028	-0.008
	(-1.17)	(-0.39)	(-0.51)	(0.14)	(0.67)	(-0.26)
All control variables	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of firms	3,303	3,303	3,303	2,248	2,248	2,248
Firm-year obs.	28,572	28,572	28,572	18,906	18,906	18,906
Adjusted R^2	0.923	0.845	0.915	0.906	0.820	0.909

Table B10. APM Laws and Profitability and Financial Soundness

This table reports results for panel regressions of *Profitability* and *Financial Soundness* on an *APM Law* indicator variable. *Profitability* is proxied for with: *ROA, NPM,* and *OPM*. *Financial Soundness* is proxied for with: *Z-score, OCF Ratio,* and *Loss*. Panel A shows the results for the impact of *APM Law* on *Profitability*. Panel B presents the results for the effect of the *APM Law* on *Financial Soundness*. Columns (1) – (3) present estimates for the "Manufacturing" sample, while columns (4) – (6) are for the "Products" dataset. Table B2 describes the "Products" sample. The dependent variables are leaded one year (*t*+1). Included controls: *Tobin's Q, Size, Ln(Age), Debt-to-Equity, Operating Cash-Flow, HHI, Sales Growth, Loss, Firm Liquidity, R&D/Sales, CAPX/Assets,* and *Industry-Year Tobin's Q*. Table A1 provides variable definitions. Industry fixed effects are defined at the two-digit SIC code level. All continuous variables are winsorized at the 1st and 99th percentiles and dollar values are expressed in 2015 dollars. The estimated *t*-statistics are based on robust standard errors clustered by state of location (reported in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Panel A: Profitability

Period:	1975 – 1988					
Sample:	"Manufacturing"			"Products"		
Dep. Variables	ROA_{t+1}	NPM_{t+1}	OPM_{t+1}	ROA_{t+1}	NPM_{t+1}	OPM_{t+1}
Variables	(1)	(2)	(3)	(4)	(5)	(6)
All Item APM Law	0.002	0.002	-0.003	0.004	0.002	-0.014
	(1.22)	(0.85)	(-0.26)	(1.37)	(0.93)	(-0.98)
Boat Hull APM Law	-0.001	-0.005	0.058	-0.001	-0.001	0.026
	(-0.11)	(-1.17)	(1.22)	(-0.02)	(-0.20)	(0.69)
All control variables	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of firms	3,293	3,293	2,874	2,274	2,274	2,006
Firm-year obs.	24,869	24,869	16,653	16,385	16,385	11,408
Adjusted R^2	0.782	0.802	0.744	0.780	0.790	0.732

Panel B: Financial Soundness

Period:	1975 – 1988					
Sample:	"Manufacturing"			"Products"		
Dep. Variables	$Z\text{-}score_{t+1}$	$OCF\ Ratio_{t+1}$	$Loss_{t+1}$	$Z\text{-}score_{t+1}$	$OCF\ Ratio_{t+1}$	$Loss_{t+1}$
Variables	(1)	(2)	(3)	(4)	(5)	(6)
All Item APM Law	0.007	-0.002	-0.015	0.014	-0.004	-0.010
	(0.15)	(-0.08)	(-1.01)	(0.32)	(-1.18)	(-0.74)
Boat Hull APM Law	-0.030	0.013	-0.008	-0.051	-0.023	-0.001
	(-0.72)	(0.47)	(-0.32)	(-0.69)	(-0.67)	(-0.04)
All control variables	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of firms	3,072	2,742	3,073	2,110	1,909	2,111
Firm-year obs.	23,170	16,902	23,177	15,249	11,487	15,256
Adjusted R^2	0.811	0.598	0.369	0.799	0.549	0.346

Table B11. APM Laws and Investment Activity

This table reports results for panel regressions of *Investment Activity* on an *APM Law* indicator variable. Investments in new technology is proxied with: *R&D, CAPX*, and *Invest Rate*. Investments in existing technology is proxied with: *Advertise, Organization, and Labor*. Panel A shows the results for the impact of *APM Law* on investments in new technology. Panel B presents the results for the effect of the *APM Law* on investments in existing technology. Columns (1)–(3) report estimates for the "Manufacturing" sample, while columns (4)–(6) are for the "Products" dataset. Table B2 describes the "Products" sample. The dependent variables are led one year (*t*+1). Included controls: *Tobin's Q, Size, Ln(Age), Debt-to-Equity, ROA, Operating Cash-Flow, HHI, Sales Growth, Loss, Firm Liquidity, R&D/Sales, CAPX/Assets*, and *Industry-Year Tobin's Q*. Table A1 provides variable definitions. Industry fixed effects are defined at the two-digit SIC code level. All continuous variables are winsorized at the 1st and 99th percentiles and dollar values are expressed in 2015 dollars. Estimated *t*-statistics are based on robust standard errors clustered by state of location (reported in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Panel A: Investments in New Technology

Period:	1975 – 1988					
Sample:	"Manufacturing"			"Products"		
Dep. Variables	$R\&D_{t+1}$	$CAPX_{t+1}$	$Invest\ Rate_{t+1}$	$R\&D_{t+1}$	$CAPX_{t+1}$	$Invest\ Rate_{t+1}$
Variables	(1)	(2)	(3)	(4)	(5)	(6)
All Item APM Law	0.004**	0.005***	0.008*	0.005**	0.006**	0.006*
	(2.65)	(3.60)	(1.78)	(2.56)	(2.47)	(1.84)
Boat Hull APM Law	-0.003	0.000	0.001	0.007	0.005	0.009
	(-1.16)	(0.05)	(0.16)	(0.88)	(1.16)	(1.01)
All control variables	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of firms	2,923	2,923	1,606	2,007	2,007	1,132
Firm-year obs.	22,766	22,766	8,751	14,798	14,798	5,998
Adjusted R^2	0.810	0.459	0.326	0.774	0.430	0.306

Panel B: Investments in Existing Technology

Period:	1975 – 1988					
Sample:	"Manufacturing"			"Products"		
Dep. Variables	$Advertise_{t+1}$	$Organization_{t+1}$	$Labor_{t+1}$	$Advertise_{t+1}$	$Organization_{t+1}$	$Labor_{t+1}$
Variables	(1)	(2)	(3)	(4)	(5)	(6)
All Item APM Law	0.001***	0.003	0.001**	0.000	0.006**	0.001**
	(2.95)	(1.09)	(2.57)	(1.28)	(2.49)	(2.30)
Boat Hull APM Law	0.000	0.005	0.000	-0.001	0.006	0.000
	(0.46)	(0.71)	(0.32)	(-0.95)	(0.96)	(0.17)
All control variables	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of firms	2,923	2,923	2,524	2,007	2,007	1,756
Firm-year obs.	22,766	22,766	14,830	14,798	14,798	10,031
Adjusted R^2	0.881	0.850	0.861	0.809	0.815	0.832

Table B12. APM Laws, Innovative Ability and Firm Value

This table reports results for panel regressions of *Tobin's Q* on an *APM Law* × *Innovative Ability* interaction term over the period 1975 to 1988. *Innovative Ability* is measured by *Research Quotient (RQ)* (Knott, 2008). Columns (1) – (4) presents the estimates for the "Manufacturing" sample, while columns (5) – (8) are specific to the "Products" dataset. Table B2 describes the "Products" sample. Included controls: *Size, Ln(Age), Debt-to-Equity, ROA, Operating Cash-Flow, HHI, Sales Growth, Loss, Firm Liquidity, R&D/Sales, CAPX/Assets,* and *Industry-Year Tobin's Q*. Other covariates specified but unreported to conserve space: *RQ, RQ Median, RQ High,* and *RQ Low*. Table A1 provides variable definitions. Industry fixed effects are defined at the two-digit SIC code level. All continuous variables are winsorized at the 1st and 99th percentiles and dollar values are expressed in 2015 dollars. The estimated *t*-statistics are based on robust standard errors clustered by state of location (reported in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Dep. Variable: *Tobin's Q*	1975 – 1988							
Sample:	"Manufacturing"				"Products"			
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
All Item APM Law × RQ	0.372**				0.706***			
	(2.42)				(4.92)			
All Item APM Law × RQ Median		0.102***				0.126***		
		(5.24)				(6.21)		
All Item APM Law × RQ High			0.109***	0.108***			0.146***	0.153***
			(4.44)	(4.46)			(5.55)	(5.20)
All Item APM Law × RQ Low				0.003				0.022
				(0.17)				(1.18)
All Item APM Law	-0.077**	-0.083***	-0.069***	-0.069***	-0.115***	-0.085**	-0.069**	-0.079**
	(-2.35)	(-3.85)	(-3.64)	(-3.50)	(-2.71)	(-2.52)	(-2.39)	(-2.71)
Boat Hull APM Law	0.063	0.068	0.066	0.066	-0.017	-0.016	-0.015	-0.015
	(0.68)	(0.71)	(0.71)	(0.70)	(-0.39)	(-0.37)	(-0.35)	(-0.35)
All control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of firms	1,416	1,416	1,416	1,416	1,056	1,056	1,056	1,056
Firm-year obs.	10,696	10,696	10,696	10,696	7,705	7,705	7,705	7,705
Adjusted R^2	0.682	0.681	0.682	0.682	0.658	0.658	0.658	0.658

Table B13. APM Laws, Supreme Court's Ruling, Innovative Ability and Firm Value

This table reports results for panel regressions of *Tobin's Q* on *Post 1988 × APM Law × Innovative Ability* over the period 1975 to 1992. *Innovative Ability* is measured by *Research Quotient (RQ)* (Knott, 2008). Columns (1) – (4) presents estimates for the "Manufacturing" sample, while columns (5) – (8) are for the "Products" dataset. Table B2 describes the "Products" sample. Other insignificant interaction variables not reported: *Post 88 × All Item APM Law, Post 88 × RQ, Post 88 × RQ Median, Post 88 × RQ Low, All Item APM Law × RQ Low, RQ, RQ Median, RQ High,* and *RQ Low*. Other controls: *Size, Ln(Age), Debt-to-Equity, ROA, Operating Cash-Flow, HHI, Sales Growth, Loss, Firm Liquidity, R&D/Sales, CAPX/Assets,* and *Industry-Year Tobin's Q*. Table A1 provides variable definitions. Industry fixed effects are defined at the two-digit SIC code level. All continuous variables are winsorized at the 1st and 99th percentiles and dollar values are expressed in 2015 dollars. The estimated *t*-statistics are based on robust standard errors clustered by state of location (reported in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Dep. Variable: *Tobin's Q*	1975 – 1992							
Sample:	"Manufacturing"				"Products"			
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post 88 × All Item APM Law × RQ	-1.120***				-2.118***			
	(-2.76)				(-5.35)			
Post 88 × All Item APM Law × RQ Median		-0.249**				-0.305**		
		(-2.55)				(-2.25)		
Post 88 × All Item APM Law × RQ High			-0.310***	-0.225*			-0.442***	-0.351**
			(-2.70)	(-1.70)			(-4.19)	(-2.54)
Post 88 × All Item APM Law × RQ Low				0.201				0.219
				(1.27)				(1.20)
Post 88 × RQ High			0.152**	0.127**			0.145*	0.130**
			(2.46)	(2.30)			(1.94)	(2.01)
All Item APM Law × RQ	0.291**				0.817***			
	(2.01)				(6.26)			
All Item APM Law × RQ Median		0.094***				0.146***		
		(2.69)				(4.51)		
All Item APM Law × RQ High			0.120***	0.124**			0.184***	0.187***
			(4.32)	(2.19)			(6.62)	(2.96)
All Item APM Law	-0.075***	-0.087***	-0.082***	-0.089*	-0.138***	-0.103***	-0.091***	-0.098*
	(-2.79)	(-5.72)	(-4.65)	(-1.82)	(-4.05)	(-4.81)	(-3.11)	(-1.78)
Boat Hull APM Law	0.051	0.057	0.054	0.054	0.011	0.013	0.014	0.014
	(0.87)	(0.94)	(0.92)	(0.71)	(0.30)	(0.35)	(0.37)	(0.23)
All control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of firms	1,742	1,742	1,742	1,742	1,313	1,313	1,313	1,313
Firm-year obs.	13,560	13,560	13,560	13,560	9,905	9,905	9,905	9,905
Adjusted R^2	0.708	0.708	0.708	0.709	0.671	0.670	0.670	0.671

Table B14. APM Laws and Firm Value Dynamics

This table reports results of a falsification test, regressing *Tobin's Q* on one-year leaded, contemporaneous, one-year lagged and two-year or more lagged *All Item APM Law* indicator variables. *All Item APM Law*$^{[-1]}$ is an indicator variable equal to one if a firm is located in a state that will adopt an APM statute in one year and equal to zero otherwise. *All Item APM Law*$^{[0]}$ is an indicator variable equal to one if a firm is headquartered in a state that adopts an APM statute in the current year and equal to zero otherwise. *All Item APM Law*$^{[1]}$ is an indicator variable equal to one if a firm is located in a state that adopted an APM statute one year ago and equal to zero otherwise. *All Item APM Law*$^{[2+]}$ is an indicator variable equal to one if a firm is located in a state that adopted an APM statute two or more years ago and equal to zero otherwise. Columns (1) – (3) present the estimates in the "Manufacturing" sample, while columns (4) – (6) are specific to the "Products" dataset. Table B2 describes the "Products" sample. The included controls are: *Size, Ln(Age), Debt-to-Equity, ROA, Operating Cash-Flow, HHI, Sales Growth, Loss, Firm Liquidity, R&D/Sales, CAPX/Assets,* and *Industry-Year Tobin's Q*. In addition, columns (2) – (3) and (5) – (6) specify a headquartering state time trend. Table A1 provides variable definitions. Industry fixed effects are defined at the two-digit SIC code level. All continuous variables are winsorized at the 1st and 99th percentiles and dollar values are expressed in 2015 dollars. The estimated *t*-statistics are based on robust standard errors clustered by state of location (reported in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Dep. Variable: *Tobin's Q*	1975 – 1988					
Sample:	"Manufacturing"			"Products"		
Variables	(1)	(2)	(3)	(4)	(5)	(6)
All Item APM Law$^{[-1]}$	-0.001	0.001	0.001	-0.015	-0.013	-0.013
	(-0.09)	(0.04)	(0.06)	(-0.91)	(-0.74)	(-0.76)
All Item APM Law$^{[0]}$	0.035	0.038	0.038	0.026	0.028	0.027
	(0.76)	(0.81)	(0.80)	(0.85)	(0.95)	(0.89)
All Item APM Law$^{[1]}$	0.110***	0.112***	0.112***	0.112***	0.115***	0.115***
	(3.33)	(3.40)	(3.34)	(4.56)	(4.81)	(4.70)
All Item APM Law$^{[2+]}$	0.095**	0.092**	0.091**	0.118***	0.113***	0.114***
	(2.09)	(2.20)	(2.12)	(2.90)	(3.08)	(3.02)
Boat Hull APM Law$^{[-1]}$			0.031			-0.006
			(0.92)			(-0.12)
Boat Hull APM Law$^{[0]}$	0.068	0.070	0.061	0.064	0.067	0.014
	(0.92)	(0.95)	(0.85)	(1.09)	(1.14)	(0.25)
Boat Hull APM Law$^{[1]}$			0.006			0.036
			(0.06)			(0.52)
Boat Hull APM Law$^{[2+]}$			0.030			0.073
			(0.32)			(0.80)
All control variables	Yes	Yes	Yes	Yes	Yes	Yes
State time trend	No	Yes	Yes	No	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of firms	3,493	3,493	3,493	2,406	2,406	2,406
Firm-year obs.	25,104	25,104	25,104	16,539	16,539	16,539
Adjusted R^2	0.734	0.734	0.734	0.715	0.715	0.715

Table B15. APM Laws and Firm Value in a Matched Sample

This table reports summary statistics and regression results for a propensity score matched sample. Treated (*Treat*) firms are defined as companies located in states that adopt all item APM laws, whereas the control firms are located in states without any APM laws in at least the five-year period following the passage of a law for its matched counterpart. I use nearest-neighbor matching with replacement in year *t-1* to create a sample matched on *Q*, *Size*, *Ln(Age)*, *HHI*, *Sales Growth*, *Loss*, and *Ln(Patent)*, and exactly on two-digit SIC industry codes for the three treated states of California, Michigan, and Tennessee. Panel A presents the summary statistics for the year prior to treatment. The column "Difference" provides the difference between the treat and control sample mean and its test statistic in parentheses. The row "N (by group)" provides the number of unique firms for each treatment and control group. Panel B shows the summary statistics for the full matched panel (*t*-1) to (*t*+1), where the year of treatment is excluded from the panel. Panel C reports the regression estimates of *Tobin's Q* on a *Treat* × *Post* interaction term. *Post* is an indicator variable equal to one in the year after the adoption of an APM law, and zero otherwise. *Treat* is omitted in the regression because of collinearity with its firm fixed effect, and *Post* is omitted due to its collinearity with Industry-Year fixed effects. Industry fixed effects are defined at the two-digit SIC code level. Columns (1) – (2) present the estimates in the "Manufacturing" sample, while columns (3) – (4) are specific to the "Products" dataset. Table B2 describes the "Products" sample. Included controls: *Size*, *Ln(Age)*, *HHI*, *Sales Growth*, *Loss*, *Debt-to-Equity*, *Firm Liquidity*, *R&D/Sales*, *CAPX/Assets*, and *Industry-year Tobin's Q*. Table 1 provides variable definitions. All continuous variables are winsorized at the 1st and 99th percentiles and the dollar values are expressed in 2015 dollars. The estimated t-statistics are based on robust standard errors clustered by state of location (reported in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Panel A: Pre-Treatment Year (*t*-1) Summary Statistics

	"Manufacturing"			"Products"		
	(1)	(2)	(3)	(4)	(5)	(6)
Matched Variables:	Treat	Control	Difference	Treat	Control	Difference
Q	1.198	1.143	0.055	1.203	1.158	0.045
	(0.824)	(0.641)	(0.82)	(0.654)	(0.575)	(0.67)
Size	5.393	5.432	-0.039	5.190	5.179	0.011
	(1.825)	(1.809)	(-0.24)	(1.787)	(1.718)	(0.06)
Ln(Age)	2.519	2.531	-0.012	2.486	2.512	-0.026
	(0.439)	(0.432)	(-0.30)	(0.453)	(0.417)	(-0.55)
HHI	0.225	0.223	0.002	0.224	0.224	0.000
	(0.141)	(0.130)	(0.14)	(0.121)	(0.125)	(0.00)
Sales Growth	0.067	0.050	0.017	0.079	0.061	0.019
	(0.216)	(0.219)	(0.87)	(0.234)	(0.214)	(0.76)
Loss	0.154	0.146	0.008	0.166	0.148	0.018
	(0.362)	(0.354)	(0.25)	(0.373)	(0.356)	(0.45)
Ln(Patent)	0.161	0.173	-0.012	0.173	0.190	-0.017
	(0.368)	(0.404)	(-0.34)	(0.362)	(0.414)	(-0.40)
N (by group)	247	247		169	169	

Panel B: Matched Sample Summary Statistics (t-1) to (t+1)

	"Manufacturing"			"Products"		
	(1)	(2)	(3)	(4)	(5)	(6)
Matched Variables:	Mean	Std. Dev.	Obs.	Mean	Std. Dev	Obs.
Q	1.208	0.776	964	1.223	0.678	667
Size	5.453	1.816	964	5.234	1.765	667
Ln(Age)	2.589	0.421	964	2.566	0.420	667
HHI	0.231	0.139	964	0.231	0.129	667
Sales Growth	0.097	0.209	964	0.106	0.218	667
Loss	0.141	0.348	964	0.150	0.357	667
Ln(Patent)	0.165	0.383	964	0.179	0.388	667

Table B15 – *(Continued)*

Panel C: APM Laws in a Matched Sample

Dep. Variable: $Tobin's\ Q$	(*t*-1) to (*t*+1)			
	"Manufacturing"		"Products"	
Variables	(1)	(2)	(3)	(4)
$Treat \times Post$	0.074*** (2.77)	0.083*** (3.35)	0.094*** (2.95)	0.110*** (3.62)
All control variables	No	Yes	No	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes
Number of firms	481	481	328	328
Firm-year obs.	964	964	667	667
Adjusted R^2	0.841	0.882	0.749	0.829

Table B16. APM Laws and Firm Value with Non-Manufacturing Companies

This table reports results for a falsification test regressing *Tobin's Q* on an *APM Law* indicator variable on a sample of non-manufacturing firms located in states adopting APM statutes. Columns (1) – (2) present the estimates corresponding to the period 1975 to 1988 for which the APM statutes are enforceable, while columns (3) – (4) are specific to the entire sample period, 1975 to 1992, where the APM statutes lose their enforceability after the Supreme Court's preemption ruling in February of 1989. All four of the columns pertain to a "Non-Manufacturing" sample, in which all firms operating in an industry with a SIC code ranging from 2000 to 3999 are excluded. The included controls are: *Size, Ln(Age), Debt-to-Equity, ROA, Operating Cash-Flow, HHI, Sales Growth, Loss, Firm Liquidity, R&D/Sales, CAPX/Assets,* and *Industry-Year Tobin's Q*. Table A1 provides variable definitions. Industry fixed effects are defined at the two-digit SIC code level. All continuous variables are winsorized at the 1st and 99th percentiles and dollar values are expressed in 2015 dollars. The row "Test for joint significance" shows the results from a test of whether the summation of the value effect before and after 1988 is different from zero. The estimated *t*-statistics are based on robust standard errors clustered by state of location (reported in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Dep. Variable: $Tobin's\ Q$				
Sample:	"Non-Manufacturing"			
Period:	1975 – 1988		1975 – 1992	
Variables	(1)	(2)	(3)	(4)
$Post\ 88 \times All\ Item\ APM\ Law$			0.069	0.057
			(1.02)	(0.85)
$All\ Item\ APM\ Law$	-0.025	-0.012	-0.036	-0.026
	(-0.25)	(-0.14)	(-0.35)	(-0.27)
$Boat\ Hull\ APM\ Law$	0.050	0.023	0.010	-0.025
	(0.58)	(0.32)	(0.13)	(-0.37)
Test for joint significance:				
$[Post\ 88 \times All\ Item\ APM\ Law] +$			0.033	0.031
$[All\ Item\ APM\ Law]$			(0.21)	(0.24)
All control variables	No	Yes	No	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes
Number of firms	2,458	2,458	3,059	3,059
Firm-year obs.	15,025	15,025	20,959	20,959
Adjusted R^2	0.679	0.701	0.645	0.666

Table B17. Portfolio Analysis: APM Laws and Abnormal Returns

This table reports abnormal returns of value weighted monthly portfolios of firms located in states that adopt APM statutes. The long portfolios are composed in the following manner. Portfolio *12m36* includes all stocks of affected firms located in an *All Item APM Law* adopting state starting 12 months before the fiscal year-end of the year in which the headquartering state adopts an APM law, and holds these stocks for 36 months. The short portfolios are created by including all stocks of falsely affected firms either from the neighboring state(s) or non-manufacturing firm placebo tests starting 12 months before the fiscal year-end of the year in which the corresponding affected headquartering state adopts an APM law, and shorts these stocks for 36 months. The long-short portfolios are then created by differencing the portfolio returns of the long and short portfolios for each respective month. Panel A presents the results for the "Manufacturing" v. "Neighboring State Manufacturing" portfolio. In Panel B, I show estimates for the "Manufacturing" v. "Non-Manufacturing" portfolio. Panel C reports alphas specific to the "Products" v. "Neighboring State Products" portfolio. Table B2 describes the "Products" sample. I employ two models: the four-factor Carhart (1997) model (i.e., momentum, high minus low book-to-market (HML), small minus big (SMB), and market return), and the three-factor Fama-French (1993) model (i.e., HML, SMB, and market return). Further, I calculate the portfolio return with each stock weighted by its market capitalization immediately preceding its inclusion in the portfolio. The estimated *t*-statistics are based on robust standard errors (presented in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively. The "Average # of firms" row displays the mean number of stocks in the long and short portfolios across all months.

Panel A: "Manufacturing" v. "Neighboring State Manufacturing"

Portfolio "12m36"	Four-factor model			Three-factor model		
	Long	Short	Long - Short	Long	Short	Long - Short
Alpha (monthly)	0.655**	-0.138	0.793**	0.872***	-0.167	1.039***
	(2.53)	(-0.52)	(2.09)	(3.02)	(-0.70)	(2.66)
Average # firms	140.39	87.13	-	140.39	87.13	-
Monthly obs.	98	98	98	98	98	98
Number of firms	320	192	-	320	192	-
Adjusted R^2	0.808	0.837	0.203	0.795	0.838	0.169

Panel B: "Manufacturing" v. "Non-Manufacturing"

Portfolio "12m36"	Four-factor model			Three-factor model		
	Long	Short	Long - Short	Long	Short	Long - Short
Alpha (monthly)	0.655**	0.180	0.475	0.872***	0.091	0.781*
	(2.53)	(0.54)	(1.21)	(3.02)	(0.28)	(1.95)
Average # firms	140.39	91.11	-	140.39	91.11	-
Monthly obs.	98	98	98	98	98	98
Number of firms	320	220	-	320	220	-
Adjusted R^2	0.808	0.774	0.109	0.795	0.774	0.048

Panel C: "Products" v. "Neighboring State Products"

Portfolio "12m36"	Four-factor model			Three-factor model		
	Long	Short	Long - Short	Long	Short	Long - Short
Alpha (monthly)	0.627	-0.059	0.687*	0.746*	-0.243	0.990**
	(1.52)	(-0.19)	(1.88)	(1.94)	(-0.81)	(2.59)
Average # firms	102.78	59.76	-	102.78	59.76	-
Monthly obs.	98	98	98	98	98	98
Number of firms	234	133	-	234	133	-
Adjusted R^2	0.729	0.832	0.128	0.728	0.824	0.048

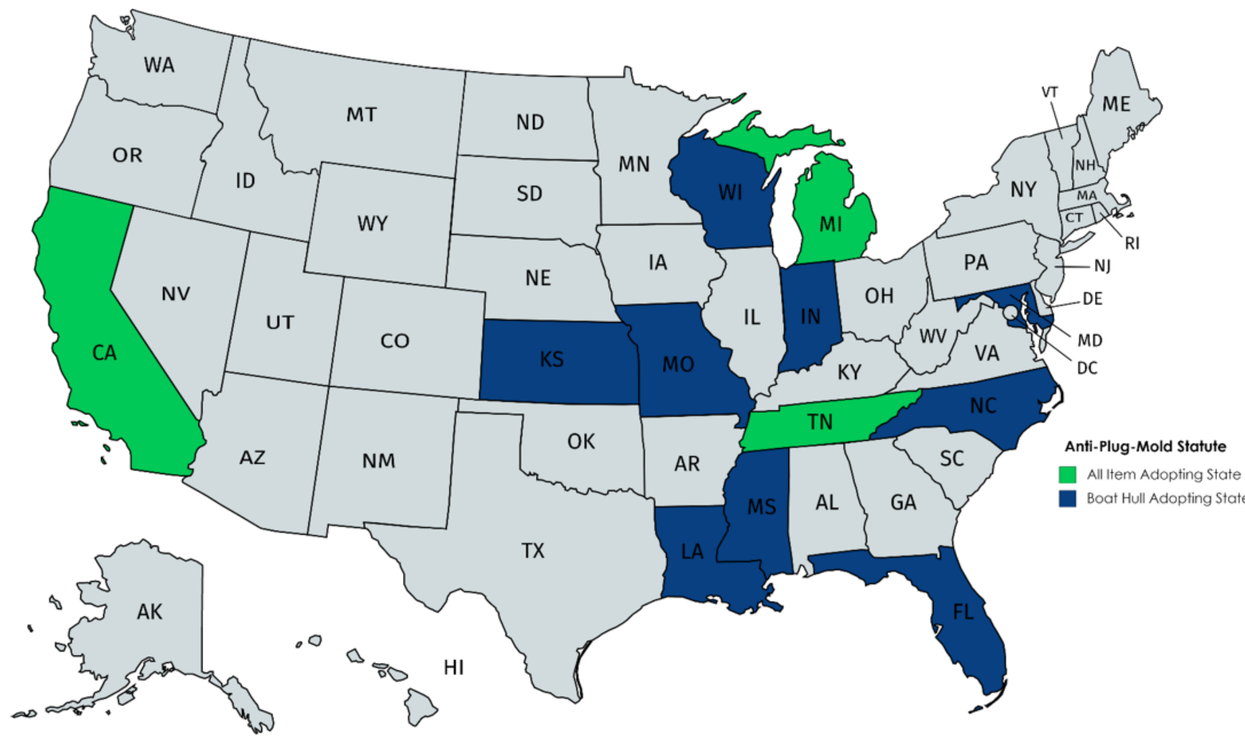


Figure B1. The chart above shows the states that adopted an anti-plug molding (APM) statute during the period 1978 to 1987. States colored with green indicates an adopting state with a plug molding law which protects all manufacturing items. Dark blue colored states denotes the enactment of an APM statute that explicitly stipulates the protection of boat hulls and component parts. The grey colored states are without such legislation. *Created with*: mapchart.net.

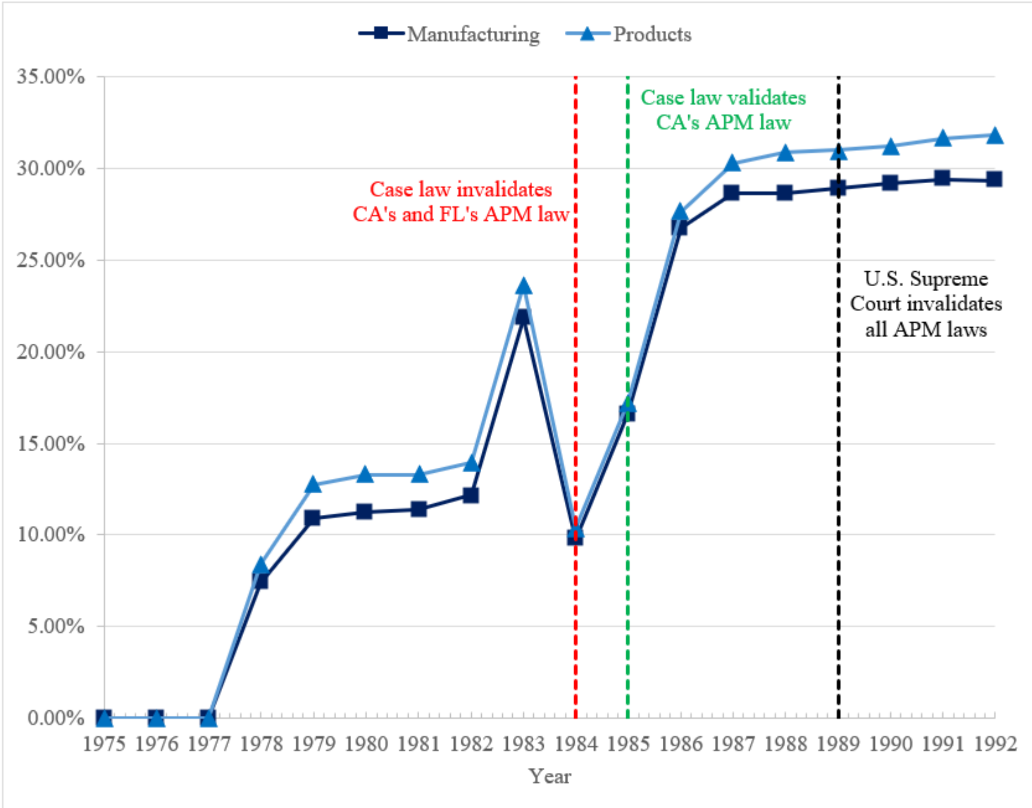


Figure B2. The chart above shows the percentage of firms headquartered in an APM law adopting state in the following samples, each year from 1975 to 1992. The dark blue line with square markers is specific to affected firms in the "Manufacturing" sample with SIC codes ranging from 2000 to 3999. The light blue solid line with triangle markers shows the number of affected firm-year observations in the "Products" sample. Table B2 provides a description of the "Products" dataset. The dashed red vertical line indicates the invalidation of California's and Florida's APM statutes resulting from case law in 1984. The dashed green vertical line denotes 1985 case law validation of California's APM law. The dashed black vertical line signifies the U.S. Supreme Court's 1989 decision in *Bonito Boats v. Thunder Craft Boats*, which effectively invalidates all states' APM statutes.

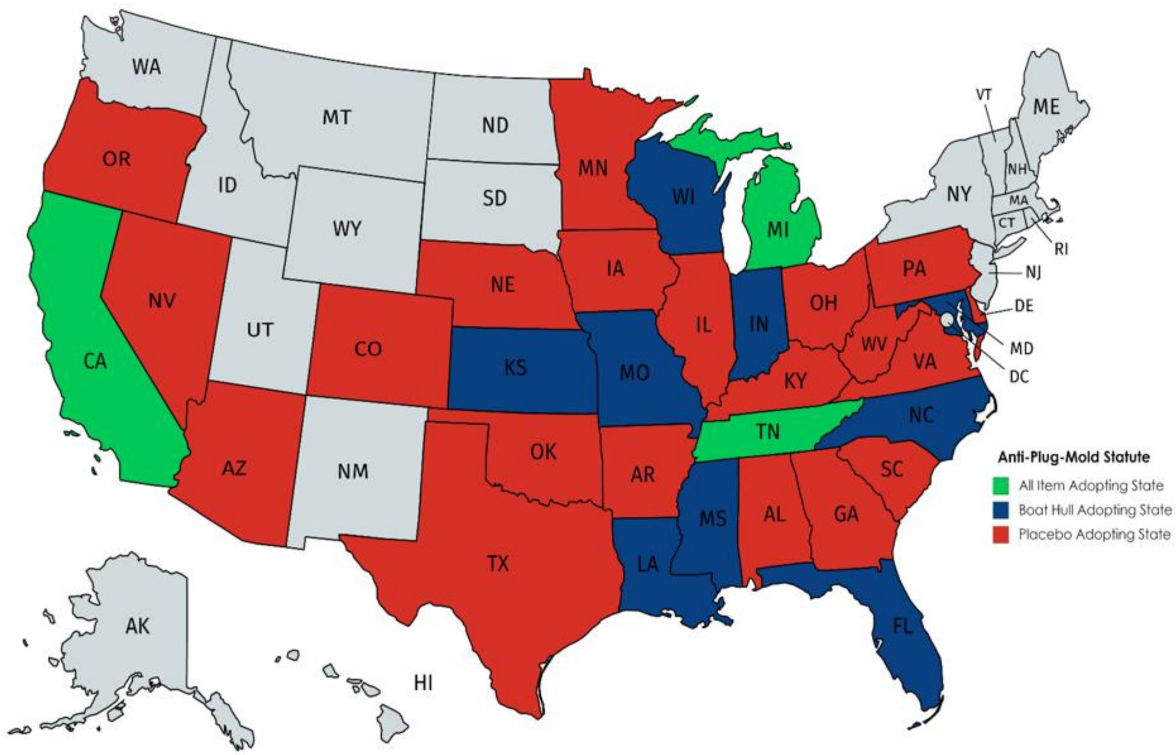


Figure B3. The chart above shows the states that have adopted an APM statute, and those I falsely assign adopting status. States colored with green and dark blue indicates an actual adopting state in the sample, 1975 to 1992. Red colored states denotes neighboring states of actual adopters whom are assigned pseudo affected status as also passing the APM laws. The grey colored states are without such legislation. *Created with*: mapchart.net.

Appendix C: Chapter 2 UTSA Index and Variable Definitions

Table C1: Index of Legal Protection of Trade Secrets

This table is an exact reproduction from Table A1 in the appendix of Png (2017). It provides the criteria used in the construction of the state-level trade secrets protection (UTSA) index. The values specific to each state are summed across the six unique items over time. The first iteration of this summation process yields the level of trade secrets protection provided by common law. Next, if a state passes the UTSA, the items are re-evaluated and a post-enactment index value is calculated. The change in the value of the index captures state-level exogenous variation in trade secrets protection.

Dimension	Item	Coding	Sources
Substantive law	Whether information must be in actual or intended business use to be protected as trade secret.	= 0 if information must be in actual or intended use, = 1 otherwise.	ULA (Uniform Laws Annotated); Pedowitz et al. 1997; Malsberger et al. 2006
Substantive law	Whether reasonable efforts are required to maintain secrecy.	= 0 if reasonable efforts required, = 1 otherwise.	ULA; Pedowitz et al. 1997; Malsberger et al. 2006
Substantive law	Whether information must be used or disclosed for it to be deemed to have been misappropriated.	= 0 if information must be used or disclosed, = 1 if includes mere improper acquisition or no requirement.	ULA; Pedowitz et al. 1997; Malsberger et al. 2006
Civil procedure	Limitation on the time for the owner to take legal action for misappropriation.	Number of years divided by three.	ULA; Pedowitz et al. 1997; Malsberger et al. 2006
Remedies	Whether an injunction is limited to eliminating the advantage from misappropriation.	= 0 if yes, = 1 otherwise.	Pedowitz et al. 1997; Malsberger et al. 2006
Remedies	Multiple of actual damages available in punitive damages.	Number of years divided by six.	Pedowitz et al. 1997; Malsberger et al. 2006

Table C2: Variable Definitions

This table provides definitions for all the variables used in Chapter 2 and Tables D1 through D12.

Variable	Description (variable definitions in parentheses refer to Compustat designations where appropriate)
Assets	The value of total book assets (*at*) in millions.
Book Leverage	The book value of long-term debt (*dltt*) plus debt in current liabilities (*dlc*) divided by book value of assets (*at*).
CF Risk	The operating cash flow volatility for a firm, where cash flow volatility is the standard deviation of the ratio of income before extraordinary items plus depreciation and amortization to book assets ((*ib*+*dp*)/*at*) over the preceding 10 years.
Common Law	State-specific common law trade secrets protection. Measured by Png (2017) and described in Section 4.2 and Table A1 in the appendix. https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/BFP2IC
CW Patents	Citation-weighted patents, as constructed in Hall, Jaffe, and Trajtenberg (2005), Atanassov (2013), and Kogan, Papanikolaou, Seru, and Stoffman (2016). https://iu.app.box.com/v/patents
Div Payer	An indicator variable set to one if a firm pays a common dividend (*dvc*) during a fiscal year, and zero otherwise.
EBIT	Earnings before interest and taxes (*ebit*) in millions.
Employees	The number of firm-level employees (*emp*).
Fixed Assets	The ratio of property, plant, and equipment (*ppent*) to book value of assets (*at*).
IDD	Inevitable Disclosure Doctrine indicator variable, which equals one if it is recognized by a state and zero otherwise; Data come from Klasa, Ortiz-Molina, Serfling and Srinivasan (2017).
IY Book Leverage	Control for industry shocks, measured as the mean of *Book Leverage* in the firm's three-digit SIC industry in a given year, excluding the firm itself.
IY Market Leverage	Control for industry shocks, measured as the mean of *Market Leverage* in the firm's three-digit SIC industry in a given year, excluding the firm itself.
M/B	The market value of assets (book value of assets (*at*) plus market value of equity (*prcc_f**csho*) minus book value of equity (*ceq*) divided by book value of assets (*at*).

Table C2 – *(Continued)*

Market Leverage	The book value of long-term debt (*dltt*) plus debt in current liabilities (*dlc*) divided by market value of debt and equity (long-term debt (*dltt*) plus debt in current liabilities (*dlc*) plus market value of equity (*prcc_f**csho*)).
Median Ln(Sales)	An indicator variable equal to one if a firm has Ln(Sales) greater than the sample median, and zero otherwise, where *Sales* is defined as below.
Median-Year Ln(Sales)	An indicator variable equal to one if a firm has Ln(Sales) in a given year greater than the sample median measured each year, and zero otherwise, where *Sales* is defined as below.
Mod Z-score	The modified Altman's Z-score (1.2*(*wcap/at*)+1.4*(*re/at*)+3.3*(*ebit/at*)+1.0*(*sale/at*)).
Patents	Count variable for patents, as constructed in Hall, Jaffe, and Trajtenberg (2005), Atanassov (2013), and Kogan, Papanikolaou, Seru, and Stoffman (2016). https://iu.app.box.com/v/patents
Prob. of Default	The inverse of modified Altman's Z-score (1.2*(*wcap/at*)+1.4*(*re/at*)+3.3*(*ebit/at*)+1.0*(*sale/at*)).
Profitability	Income before extraordinary items (*ib*) plus depreciation and amortization (*dp*) divided by book value of assets (*at*).
R&D Intensity	An indicator variable set to one if a firm has R&D expenditure greater than 0.02, and zero otherwise, as in Denis and McKeon (2016).
R&D Tax Credit	An indicator variable set to one if a state has adopted a tax credit for research & development expenditure, and zero otherwise; Data comes from Wilson (2009).
Republican	The proportion of state-level representatives in the U.S. House of Representatives whom belong to the Republican party, in a given year; Data from the *Book of the States*.
Sales	The value of sales (*sale*) in millions.
Size	The natural logarithm of the value of total sales (*sale*) in millions, centered by subtracting out its sample mean. We also consider the natural logarithm of the value of total assets (*at*) in millions, centered by subtracting out its sample mean, and the natural logarithm of one plus the total number of employees (*emp*), centered by subtracting out its sample mean.
SM Patents	Stock market-weighted patents, as constructed in Kogan, Papanikolaou, Seru, and Stoffman (2016). https://iu.app.box.com/v/patents

Table C2 – *(Continued)*

State Corruption	State-by-state corruption measures come from Table 2 in Dass, Nanda and Xiao (Working Paper, 2017), who collect the data from the U.S. Department of Justice's Public Integrity Section Reports over the period 1990 to 2011.
State GDPG	The state-level GDP growth rate over the fiscal year; Data from U.S. Bureau of Economic Analysis.
State Per Capita GDP	A state's GDP (in thousands) divided by its total population; Data from U.S. Bureau of Economic Analysis.
State Property Crime Rate	State-by-state total property crime divided by population every year from 1960 to 2014. *Source:* U.S. Department of Justice and the Federal Bureau of Investigation. Data is retrieved from the Uniform Crime Reporting Statistics website: https://www.ucrdatatool.gov/Search/Crime/State/StatebyState.cfm
SY Book Leverage	Control for local shocks, measured as the mean of *Book Leverage* in the firm's state of location in a given year, excluding the firm itself.
SY Ln(Patents)	Measured as the mean of *Ln(1+Patents)* in the firm's state of location in a given year, excluding the firm itself, where *Patents* is defined as above.
SY Ln(Sales)	Measured as the mean of *Ln(Sales)* in the firm's state of location in a given year, excluding the firm itself, where *Sales* is defined as above.
SY Market Leverage	Control for local shocks, measured as the mean of *Market Leverage* in the firm's state of location in a given year, excluding the firm itself.
SY Mod Z-score	Measured as the mean of *Mod Z-score* in the firm's state of location in a given year, excluding the firm itself, where *Mod S Z-Score* is defined as above.
Tobin's Q	Market value of assets (*at* – book equity + market equity (*prcc_f*csho*)) divided by the book value of assets (*at*). Book equity and this measure, in general, follows Fama and French (1992).
Total Tobin's Q	Market value of outstanding equity (*prcc_f*csho*) plus the book value of debt (*dltt* + *dlc*) minus the firm's current assets (*act*) divided by the sum of the book value of property, plant, and equipment (*ppegt*), and the replacement cost of intangible capital (the sum of the firm's externally purchased and internally created intangible capital), follows Peters and Taylor (2017). This measure (*q_tot*) is available on WRDS from 1950 to 2015: http://www.whartonwrds.com/datasets/included/luke-taylors-total-q/

Table C2 – *(Continued)*

UTSA Index	The change in state-specific trade secrets protection after the enactment of the Uniform Trade Secrets Act (UTSA). Measured by Png (2016, 2017) and described in Section 4.2 and Table A1 in the appendix. https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/BFP2IC

Appendix D: Chapter 2 Tables and Figures

Table D1: Importance of Different IP Mechanisms to U.S. Firms in 2013 (%)

This table reports the most recently published responses to the National Science Foundation's National Center for Science and Engineering's annual Business Research and Development and Innovation Survey (BRDIS) question: "how important to your company were the following types of intellectual property protection?" (Form BRDI-1, 2013, p.45). The target responders, which are composed of for-profit companies with at least five or more paid employees, a minimum of one business establishment in operation during the survey year, and performs some form of R&D activity all within the United States in 2013, are provided the following answer choices: "very important," "somewhat important," and "not important." Size is measured by the number of domestic employees. We average the reported BRDIS percentages for businesses with 5 – 499 and 500 – 999, 1,000 – 4,999 and 5,000 – 9,999, and 10,000 -24,999 and 25,000 or more domestic employees to construct the three size categories shown below. The rows may not sum to one hundred due to rounding.

Source: National Science Foundation, National Center for Science and Engineering Statistics, and U.S. Census Bureau, Business R&D and Innovation Survey, 2013.

IP mechanism	Importance by size	Very important	Somewhat important	Not important
Trade secrets	All companies	57.2	19.9	22.8
	5 – 999	56.0	23.3	20.8
	1,000 – 9,999	68.3	20.5	11.4
	10,000 or more	80.5	13.2	6.4
Utility patents	All companies	51.0	15.8	33.2
	5 – 999	49.1	18.2	32.8
	1,000 – 9,999	64.7	17.4	18.0
	10,000 or more	73.5	15.0	11.6
Trademarks	All companies	43.4	31.3	25.3
	5 – 999	47.3	29.7	23.1
	1,000 – 9,999	69.7	19.6	10.7
	10,000 or more	81.7	12.9	5.4
Copyrights	All companies	27.2	33.8	39.0
	5 – 999	27.3	34.5	38.3
	1,000 – 9,999	34.6	42.3	23.2
	10,000 or more	43.9	44.2	12.0
Design patents	All companies	24.3	27.4	48.3
	5 – 999	24.3	27.9	47.9
	1,000 – 9,999	26.4	30.6	43.0
	10,000 or more	28.3	41.1	30.7

Table D2: Explaining the Adoption of UTSA Statutes

This table reports Cox proportional hazard model results for the state-level adoption of the Uniform Trade Secrets Act (UTSA) as a function of the level of firm-specific book leverage, the average state-year book leverage, and the average industry-year book leverage, where firm *i* is excluded from the calculation in the latter two measures, and industry is defined by three-digit SIC code, plus other state-level characteristics. The sample is composed of Compustat industrial firms from 1975 to 2003. The dependent variable is an indicator equal to one if a state has passed the UTSA, and zero otherwise. This passage of the statute is the "failure event" and as such all firms headquartered in a UTSA state are dropped from the sample after adoption. The coefficients reported below are the corresponding hazard ratios. All continuous variables have been winsorized at the 1st and 99th percentiles and then standardized to have zero mean and unit variance. The explanatory variables are lagged one period (t–1). We estimate robust standard errors clustered by state of location and present in parentheses below the coefficients. The dollar values are expressed in 2001 dollars. Table C2 in the appendix provides variable definitions. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	Dependent Variable is an indicator for UTSA adoption			
	(1)	(2)	(3)	(4)
Book Leverage	0.999	0.996	0.998	1.003
	(-0.04)	(-0.13)	(-0.07)	(0.12)
SY Book Leverage	1.356	1.379	1.399	1.414
	(0.98)	(0.89)	(1.20)	(1.07)
IY Book Leverage	0.962	0.964	0.974	0.966
	(-0.79)	(-0.80)	(-0.63)	(-0.91)
SY Ln(Sales)	0.858	0.649	0.699	0.714
	(-1.08)	(-1.45)	(-1.00)	(-0.83)
IDD	0.116***	0.107***	0.105***	0.110**
	(-2.80)	(-2.89)	(-2.84)	(-2.56)
R&D/Sales		1.006	1.011	1.022
		(0.15)	(0.25)	(0.60)
R&D Tax Credit		0.505	0.501	0.429
		(-0.69)	(-0.72)	(-0.68)
SY Ln(Patents)		1.054	1.093	1.175
		(0.12)	(0.18)	(0.32)
SY Mod Z-score		1.541	1.478	1.855
		(0.86)	(0.83)	(1.21)
Ln(State GDPPC)			1.091	1.188
			(0.20)	(0.33)
State GDPG			1.525	1.603
			(1.55)	(1.43)
Percent Republican			1.279	1.338
			(1.27)	(1.45)
State Property Crime Rate				1.246
				(0.55)
State Corruption				0.839
				(-0.40)
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	43,401	43,380	43,380	43,380
Number of Failures	1,376	1,376	1,376	1,376
Pseudo R^2	0.043	0.046	0.055	0.058

Table D3: State-Level Trade Secrets Protection

This table reports the year when the Uniform Trade Secrets Act (UTSA) became effective in each state that passed the legislation.[a] The data on the *level* of common law trade secrets protection is provided by Png (2017a) and can be found on the *Review of Economics and Statistics* webpage: https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/BFP2IC. Further, the *change* in protection granted by the effective UTSA statute are reproductions of Table 1 from Png (2017b). We also provide the number of unique firms located in a given state at any time in the sample, as well as the overall total. For a description of how common law and UTSA protection are measured please see Table C1 in the appendix, which gives a reproduction from Png (2017a).

State	Effective Year of UTSA	Common Law	Effective Statute (Δ in Protection)	# Firms Located in the State
Alabama	1987	0.25	0	48
Alaska	1988	0	0.47	6
Arizona	1990	0.25	0.22	145
Arkansas	1981	0.5	-0.1	32
California	1985	0.22	0.25	1573
Colorado	1986	0	0.77	307
Connecticut	1983	0	0.47	246
Delaware	1982	0	0.47	28
Florida	1988	0.1	0.37	462
Georgia	1990	0	0.7	251
Hawaii	1989	0	0.47	10
Idaho	1981	0	0.47	20
Illinois	1988	0	0.7	385
Indiana	1982	0	0.47	91
Iowa	1990	0	0.47	40
Kansas	1981	0	0.47	68
Kentucky	1990	0	0.47	47
Louisiana	1981	0	0.4	46
Maine	1987	0	0.5	8
Maryland	1989	0.22	0.25	157
Massachusetts		0.27	0	510
Michigan	1998	0.25	0.15	215

[a] New Jersey and Texas also adopted the UTSA in 2012 and 2013, respectively. However, we only have data on the change in trade secretes protection stemming from the UTSA until 2010, which motivates our decision to end the sample period prior to that year. More recently, Massachusetts and New York have introduced bills to legalize the UTSA, but are yet to be voted on (2017 legislative sessions).

Table D3 – *(Continued)*

Minnesota	1980	0	0.47	278
Mississippi	1990	0	0.57	25
Missouri	1995	0	0.63	141
Montana	1985	0	0.57	7
Nebraska	1988	0	0.43	28
Nevada	1987	0	0.47	90
New Hampshire	1990	0.025	0.44	47
New Jersey		0.25	0	505
New Mexico	1989	0	0.47	18
New York		0.1	0	881
North Carolina		0	0	179
North Dakota	1983	0	0.47	5
Ohio	1994	0.25	0.28	334
Oklahoma	1986	0.025	0.44	100
Oregon	1988	0	0.47	88
Pennsylvania	2004	0.24	-0.11	379
Rhode Island	1986	0	0.47	29
South Carolina	1992	0	0.47	55
South Dakota	1988	0	0.47	8
Tennessee	2000	0	0.63	120
Texas		0.23	0	921
Utah	1989	0	0.47	92
Vermont	1996	0	0.57	10
Virginia	1986	0.025	0.44	226
Washington	1982	0	0.47	158
West Virginia	1986	0	0.47	11
Wisconsin		0	0	118
Wyoming	2006	0.5	0	5
Total Number of Unique Firms				9,553

Table D4: Summary Statistics

This table reports summary statistics for the main dependent and explanatory variables used in the main leverage regressions. Panel A presents full sample summary statistics. Panel B reports the temporal distribution of total firms, firms located in UTSA passing states, and the percent of firms affected by the trade secrets legislation by year. The sample is composed of Compustat industrial firms (excluding financials and utilities) over the period 1975 to 2003. All continuous variables, with the exception of the *UTSA*, *Common Law*, state-level economic and political variables, are winsorized at the 1st and 99th percentiles to remove the influence of extreme outliers. The dollar values are expressed in 2001 dollars. Table C2 in the appendix provides variable definitions.

Panel A: Full Sample Descriptive Statistics

	N	Mean	Std. Dev.	P25	Median	P75
Dependent Variables						
Book Leverage	80,691	0.234	0.195	0.066	0.210	0.351
Market Leverage	80,691	0.259	0.238	0.045	0.201	0.418
Main Explanatory Variables						
UTSA Index	80,691	0.236	0.198	0.050	0.247	0.333
Common Law	80,691	0.116	0.117	0.000	0.100	0.225
Other Explanatory Variables						
Sales	80,691	1189.1	3199.5	41.13	172.9	695.7
Profitability	80,691	0.039	0.195	0.030	0.083	0.126
M/B	80,691	1.757	1.514	0.977	1.268	1.879
Fixed Assets	80,691	0.308	0.212	0.144	0.262	0.426
Div Payer	80,691	0.412	0.492	0.000	0.000	1.000
Mod Z-score	80,691	1.660	2.544	1.094	2.129	2.958
IDD	80,691	0.409	0.492	0.000	0.000	1.000
State GDPPC	80,691	38.33	6.637	33.62	37.71	42.81
State GDPG	80,691	0.070	0.035	0.046	0.067	0.090
Republican	80,691	0.420	0.180	0.333	0.419	0.500
IY Book Leverage	80,691	0.244	0.093	0.182	0.234	0.291
SY Book Leverage	80,691	0.246	0.048	0.223	0.251	0.275
IY Market Leverage	80,691	0.260	0.127	0.159	0.250	0.338
SY Market Leverage	80,691	0.259	0.075	0.211	0.258	0.311

Table D4 – *(Continued)*

Panel B: Temporal Distribution

Year	N	UTSA	% of Firms Affected by UTSA in Year
1975	2,177	0	0.00%
1976	2,180	0	0.00%
1977	2,175	0	0.00%
1978	2,108	0	0.00%
1979	2,087	0	0.00%
1980	2,163	55	2.54%
1981	2,265	111	4.90%
1982	2,341	191	8.16%
1983	2,474	298	12.05%
1984	2,472	297	12.01%
1985	2,613	629	24.07%
1986	2,697	842	31.22%
1987	2,672	893	33.42%
1988	2,778	1,193	42.94%
1989	2,919	1,346	46.11%
1990	2,920	1,505	51.54%
1991	2,875	1,480	51.48%
1992	2,940	1,528	51.97%
1993	3,051	1,588	52.05%
1994	3,205	1,822	56.85%
1995	3,438	2,046	59.51%
1996	3,544	2,121	59.85%
1997	3,454	2,080	60.22%
1998	3,486	2,176	62.42%
1999	3,375	2,099	62.19%
2000	3,125	1,987	63.58%
2001	3,113	1,995	64.09%
2002	3,078	1,969	63.97%
2003	2,966	1,902	64.13%
Total	80,691	32,153	39.85%

Table D5: The Uniform Trade Secrets Act, Firm Size, and Financial Leverage

This table reports the results for the panel regressions of financial leverage on the interaction of the Uniform Trade Secrets Act (UTSA) and the natural logarithm of sales over the period 1975 to 2003. Panel A provides OLS estimates with *Book Leverage* as the dependent variable. Panel B reports results with *Market Leverage* specified as the regressand. *UTSA* is a trade secrets protection index first constructed in Png (2017a). It accounts for pre-existing common law by measuring the change in protection granted via passage of the UTSA in state *s* and year *t*. *Ln(Sales)* is a proxy for firm size measured by sales revenue. We center the size proxy for ease of interpretation, since we are interacting two continuous variables. Other variables unreported in Panel A due to statistical insignificance: *State GDPG,* and *Republican*. Table C2 in the appendix provides variable definitions. All continuous variables, with the exception of the *UTSA*, state-level economic and political variables, are winsorized at the 1st and 99th percentiles and dollar values are expressed in 2001 dollars. Robust standard errors are clustered at the state of location level (reported in parentheses). *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Dependent Variable is Book Leverage

	(1)	(2)	(3)	(4)	(5)	(6)
UTSA×Ln(Sales)		0.020***	0.020***	0.019***	0.018***	0.018***
		(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
UTSA	-0.011	-0.020	-0.019	-0.021	-0.020	-0.021**
	(0.012)	(0.015)	(0.015)	(0.012)	(0.012)	(0.010)
Ln(Sales)	0.038***	0.018***	0.035***	0.035***	0.035***	0.035***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Profitability	0.007	-0.207***	0.009	0.009	0.008	0.008
	(0.009)	(0.019)	(0.008)	(0.008)	(0.008)	(0.008)
M/B	-0.007***	-0.005***	-0.008***	-0.008***	-0.007***	-0.007***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Fixed Assets	0.178***	0.227***	0.181***	0.181***	0.179***	0.178***
	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)
Div Payer	-0.048***		-0.047***	-0.047***	-0.047***	-0.047***
	(0.004)		(0.004)	(0.004)	(0.004)	(0.004)
Mod Z-score	-0.034***		-0.034***	-0.034***	-0.034***	-0.034***
	(0.003)		(0.003)	(0.003)	(0.003)	(0.003)
IDD	0.014***			0.013***		0.012***
	(0.004)			(0.004)		(0.003)
Ln(State GDPPC)	0.071***			0.074***		0.065***
	(0.020)			(0.022)		(0.020)
IY Leverage	0.115***				0.110***	0.111***
	(0.015)				(0.016)	(0.016)
SY Leverage	0.139***				0.142***	0.125***
	(0.038)				(0.039)	(0.035)
Firm and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	80,691	80,691	80,691	80,691	80,691	80,691
Adjusted R^2	0.687	0.663	0.687	0.687	0.688	0.688

Table D5 – *(Continued)*

Panel B: Dependent Variable is Market Leverage						
	(1)	(2)	(3)	(4)	(5)	(6)
UTSA×Ln(Sales)		0.013***	0.012***	0.011***	0.011***	0.010**
		(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
UTSA	-0.002	0.000	0.001	-0.001	-0.006	-0.007
	(0.013)	(0.016)	(0.015)	(0.014)	(0.012)	(0.011)
Ln(Sales)	0.045***	0.024***	0.041***	0.042***	0.042***	0.043***
	(0.004)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)
Profitability	-0.036***	-0.237***	-0.037***	-0.036***	-0.036***	-0.035***
	(0.012)	(0.034)	(0.013)	(0.013)	(0.012)	(0.012)
M/B	-0.034***	-0.033***	-0.035***	-0.035***	-0.034***	-0.034***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Fixed Assets	0.185***	0.234***	0.193***	0.192***	0.186***	0.186***
	(0.014)	(0.015)	(0.015)	(0.014)	(0.015)	(0.015)
Div Payer	-0.071***		-0.071***	-0.071***	-0.071***	-0.071***
	(0.005)		(0.005)	(0.005)	(0.005)	(0.005)
Mod Z-score	-0.031***		-0.031***	-0.031***	-0.031***	-0.031***
	(0.004)		(0.004)	(0.004)	(0.004)	(0.004)
IDD	0.013**			0.014**		0.012**
	(0.006)			(0.006)		(0.006)
Ln(State GDPPC)	0.048**			0.046**		0.046**
	(0.020)			(0.023)		(0.020)
State GDPG	-0.179***			-0.288***		-0.178***
	(0.045)			(0.074)		(0.044)
Republican	0.006			0.016*		0.007
	(0.009)			(0.009)		(0.008)
IY Leverage	0.170***				0.172***	0.170***
	(0.021)				(0.022)	(0.021)
SY Leverage	0.227***				0.262***	0.223***
	(0.036)				(0.043)	(0.036)
Firm and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	80,691	80,691	80,691	80,691	80,691	80,691
Adjusted R^2	0.720	0.700	0.716	0.717	0.720	0.720

Table D6: The Uniform Trade Secrets Act, Alternative Size Proxies, and Financial Leverage

This table reports the results for the panel regressions of financial leverage on the interaction of the Uniform Trade Secrets Act (UTSA) and alternative definitions for firm size for Compustat industrial firms from 1975 to 2003. Panel A provides the OLS estimates with *Book Leverage* as the dependent variable. Panel B reports the results with *Market Leverage* specified as the regressand. *UTSA* is a trade secrets protection index first constructed in Png (2017a). It accounts for pre-existing common law by measuring the change in protection granted via passage of the UTSA in state *s* and year *t*; we provide an explanation for how the variable is measured in Section 4.2. *Ln(Assets)* is measured by total assets, and *Ln(1+Employees)* is defined as the total number of employees (both of these continuous measures are specified in logarithm as the difference from its sample mean in order to center the variable). *Median Ln(Sales)* is an indicator variable equal to one for firms with sales greater than the overall sample period median, and zero otherwise. *Median-Year Ln(Sales)* is an indicator variable equal to one for firms with sales greater than the sample median by year, and zero otherwise. The interaction between these alternative size proxies and *UTSA* yields the main coefficients of interest. We center the size proxy for ease of interpretation, when interacting two continuous variables. Table C2 in the appendix provides variable definitions. The other explanatory variables include *Profitability, MB, Fixed Assets, Div Payer, Mod Z-score, Log(State GDPPC), State GDPG, Republican, IY Leverage,* and *SY Leverage*. All continuous variables, with the exception of the *UTSA*, state-level economic and political variables, are winsorized at the 1st and 99th percentiles to remove the influence of extreme outliers. The dollar values are expressed in 2001 dollars. Robust standard errors are clustered at the state of location level and reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Dependent Variable is Book Leverage

	(1)	(2)	(3)	(4)
UTSA×Ln(Assets)	0.020***			
	(0.004)			
UTSA×Ln(1 + Employees)		0.032***		
		(0.008)		
UTSA×[Median Ln(Sales) Dummy]			0.077***	
			(0.018)	
UTSA×[Median-Year Ln(Sales) Dummy]				0.083***
				(0.015)
UTSA	-0.018*	-0.021*	-0.058***	-0.064***
	(0.010)	(0.012)	(0.017)	(0.016)
Ln(Assets)	0.041***			
	(0.003)			
Ln(1+ Employees)		0.020***		
		(0.004)		
Median Ln(Sales) Dummy			0.016**	
			(0.007)	
Median-Year Ln(Sales) Dummy				0.010*
				(0.005)
IDD	0.012***	0.011***	0.012***	0.012***
	(0.003)	(0.003)	(0.003)	(0.003)
All Control Variables	Yes	Yes	Yes	Yes
Firm and Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	80,691	79,427	80,691	80,691
Adjusted R^2	0.691	0.682	0.680	0.679

Table D6 – *(Continued)*

Panel B: Dependent Variable is Market Leverage				
	(1)	(2)	(3)	(4)
UTSA×Ln(Assets)	0.010***			
	(0.003)			
UTSA×Ln(1 + Employees)		0.017***		
		(0.006)		
UTSA×[Median Ln(Sales) Dummy]			0.045***	
			(0.015)	
UTSA×[Median-Year Ln(Sales) Dummy]				0.051***
				(0.015)
UTSA	-0.005	-0.008	-0.030*	-0.035*
	(0.012)	(0.013)	(0.015)	(0.017)
Ln(Assets)	0.052***			
	(0.004)			
Ln(1 + Employees)		0.042***		
		(0.005)		
Median Ln(Sales) Dummy			0.032***	
			(0.008)	
Median-Year Ln(Sales) Dummy				0.021***
				(0.007)
IDD	0.012**	0.012**	0.011**	0.011**
	(0.006)	(0.005)	(0.005)	(0.005)
All Control Variables	Yes	Yes	Yes	Yes
Firm and Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	80,691	79,427	80,691	80,691
Adjusted R^2	0.723	0.715	0.712	0.712

Table D7: The Uniform Trade Secrets Act, Firm Size, and the Timing of Financial Leverage Adjustments

This table reports the results for the panel regressions of financial leverage on the interaction of the Uniform Trade Secrets Act (UTSA) and the natural logarithm of sales for Compustat industrial firms from 1975 to 2003. *Book Leverage* is the dependent variable in columns 1 and 2, and the dependent variable in columns 3 and 4 is *Market Leverage*. $UTSA^{(-1)}$ is the change in trade secrets protection stemming from the UTSA if a firm is located in a state that will pass the law in one year and equal to zero otherwise (i.e., one-year leaded *UTSA*). $UTSA^{(0)}$ is the change in trade secrets protection stemming from the UTSA if a firm is located in a state that passes the UTSA in the current year and equal to zero otherwise (i.e., contemporaneous *UTSA*). $UTSA^{(1+)}$ is the change in trade secrets protection stemming from the UTSA if a firm is located in a state that passed the UTSA one or more years ago and zero otherwise (i.e., one-year or more lagged *UTSA*). Each of these index variables are interacted with the natural logarithm of sales centered by its sample mean, to proxy for the effect of UTSA on large firms. *UTSA* is a trade secrets protection index first constructed in Png (2017a). It accounts for pre-existing common law by measuring the change in protection granted via passage of the UTSA in state *s* and year *t*; we provide an explanation for how the variable is measured in Section 4.2. Columns 1 and 3 controls for *Profitability, M/B, Fixed Assets, Div Payer*, and *Mod Z-score*. Columns 2 and 4 includes all financial controls, but in addition, specifies *IDD, Ln(State GDPPC), State GDPG, Republican, IY Leverage, SY Leverage,* and a state time trend. Table C2 in the appendix provides variable definitions. All continuous variables, with the exception of the *UTSA*, state-level economic and political variables, are winsorized at the 1st and 99th percentiles to remove the influence of extreme outliers. The dollar values are expressed in 2001 dollars. Robust standard errors are clustered at the state of location level and reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	Book Leverage		Market Leverage	
	(1)	(2)	(3)	(4)
$[UTSA \times Ln(Sale)]^{(-1)}$	0.000	0.001	-0.001	-0.000
	(0.002)	(0.002)	(0.002)	(0.002)
$[UTSA \times Ln(Sale)]^{(0)}$	0.001	0.001	-0.003	-0.003
	(0.002)	(0.002)	(0.003)	(0.003)
$[UTSA \times Ln(Sale)]^{(1+)}$	0.020***	0.016***	0.016***	0.012**
	(0.004)	(0.004)	(0.005)	(0.005)
$UTSA^{(-1)}$	-0.003	-0.003	0.004	0.002
	(0.004)	(0.003)	(0.004)	(0.003)
$UTSA^{(0)}$	-0.003	-0.003	-0.004	-0.004
	(0.003)	(0.003)	(0.004)	(0.004)
$UTSA^{(1+)}$	-0.006	-0.005	0.001	0.001
	(0.004)	(0.004)	(0.005)	(0.004)
Financial Control Variables	Yes	Yes	Yes	Yes
State Control Variables	No	Yes	No	Yes
Industry Control Variable	No	Yes	No	Yes
Firm and Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	80,691	80,691	80,691	80,691
Adjusted R^2	0.681	0.682	0.712	0.716

Table D8: The Uniform Trade Secrets Act, Firm Size, and Alternative Definitions of Leverage

This table reports the results for the panel regressions of alternative definitions of financial leverage on the interaction of the Uniform Trade Secrets Act (UTSA) and a proxy for firm size for Compustat industrial firms from 1975 to 2003. The dependent variable in columns 1 and 2 is *Ln(1+Total Debt)* which is the natural logarithm of one plus the book value of long-term debt plus debt in current liabilities. The dependent variable in columns 3 and 4 is *Net Book Leverage* measured as the ratio of book value of long-term debt plus debt in current liabilities minus the book value of cash and short-term investments over the book value of assets. The dependent variable in columns 5 and 6 is *Net Market Leverage* which is constructed as book value of long-term debt plus debt in current liabilities minus the book value of cash and short-term investments divided by the market value of debt and equity. *UTSA* is a trade secrets protection index first constructed in Png (2017a). It accounts for pre-existing common law by measuring the change in protection granted via passage of the UTSA in state *s* and year *t*; we provide an explanation for how the variable is measured in Section 4.2. *Ln(Sales)* is a proxy for firm size measured by sales revenue (specified in logarithm as the difference from its sample mean in order to center the variable). The interaction between the size proxy and *UTSA* yields the main coefficient of interest. We center the size proxy for ease of interpretation, since we are interacting two continuous variables. Table C2 in the appendix provides variable definitions. The other explanatory variables include *Profitability, MB, Fixed Assets, Div Payer, Mod Z-score, IDD, Log(State GDPPC), State GDPG, Republican, IY Leverage,* and *SY Leverage*. All continuous variables, with the exception of the *UTSA*, state-level economic and political variables, are winsorized at the 1st and 99th percentiles to remove the influence of extreme outliers. The dollar values are expressed in 2001 dollars. Robust standard errors are clustered at the state of location level and reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	Ln(1+Total Debt)		Net Book Leverage		Net Market Leverage	
	(1)	(2)	(3)	(4)	(5)	(6)
UTSA	-0.073	-0.145**	-0.007	-0.019*	0.003	-0.003
	(0.070)	(0.055)	(0.012)	(0.010)	(0.016)	(0.015)
UTSA × Ln(Sales)		0.127***		0.021***		0.013***
		(0.021)		(0.004)		(0.005)
Ln(Sales)	0.784***	0.762***	0.076***	0.073***	0.082***	0.078***
	(0.030)	(0.032)	(0.004)	(0.005)	(0.006)	(0.006)
All Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Firm and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	80,691	80,691	80,691	80,691	80,691	80,691
Adjusted R^2	0.885	0.886	0.767	0.768	0.693	0.694

Table D9: The Uniform Trade Secrets Act, Innovative Activity, and Financial Leverage

This table reports the results for the panel regressions of financial leverage on the interaction of the Uniform Trade Secrets Act (UTSA) and firm-level innovative activity measures for Compustat industrial firms from 1975 to 2003. Panel A provides the OLS estimates with *Book Leverage* as the dependent variable. Panel B reports the results with *Market Leverage* specified as the regressand. *UTSA* is a trade secrets protection index first constructed in Png (2017a). It accounts for pre-existing common law by measuring the change in protection granted via passage of the UTSA in state *s* and year *t*; we provide an explanation for how the variable is measured in Section 4.2. The measures of innovative activity include: (1) an indicator for *R&D Intensity* set equal to one if the firm has R&D expenditure greater than 0.02 and zero otherwise; (2) *Ln(1+Patents)* is the natural logarithm of one plus a count variable for firm patents in year *t*; (3) *Ln(1+CW Patents)* is the natural logarithm of one plus citation-weighted patents; and (4) *Ln(1+SM Patents)* is the natural logarithm of one plus stock market-weighted patents; all three continuous measures centered by its sample mean. The other explanatory variables include *Profitability, M/B, Fixed Assets, Div Payer, Mod Z-score, IDD, Log(State GDPPC), State GDPG, Republican, IY Leverage,* and *SY Leverage*. Table C2 in the appendix provides variable definitions. All continuous variables, with the exception of the *UTSA*, state-level economic and political variables, are winsorized at the 1st and 99th percentiles to remove the influence of extreme outliers. The dollar values are expressed in 2001 dollars. Robust standard errors are clustered at the state of location level and reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Dependent Variable is Book Leverage				
	(1)	(2)	(3)	(4)
UTSA	-0.003	-0.017	-0.017	-0.019*
	(0.013)	(0.012)	(0.011)	(0.011)
UTSA×R&D Intensity	-0.028***			
	(0.011)			
UTSA×Ln(1+Patents)		0.060***		
		(0.019)		
UTSA×Ln(1+CW Patents)			0.016***	
			(0.004)	
UTSA×Ln(1+SM Patents)				0.021***
				(0.005)
R&D Intensity	-0.018***			
	(0.004)			
Ln(1+Patents)		-0.033***		
		(0.008)		
Ln(1+CW Patents)			-0.005***	
			(0.002)	
Ln(1+SM Patents)				-0.008***
				(0.003)
All Control Variables	Yes	Yes	Yes	Yes
Firm and Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	80,691	58,704	58,704	58,704
Adjusted R^2	0.688	0.676	0.675	0.676

Table D9 – *(Continued)*

Panel B: Dependent Variable is Market Leverage				
	(1)	(2)	(3)	(4)
UTSA	0.006	-0.007	-0.007	-0.010
	(0.016)	(0.013)	(0.013)	(0.013)
UTSA×R&D Intensity	-0.025*			
	(0.015)			
UTSA×Ln(1+Patents)		0.031**		
		(0.013)		
UTSA×Ln(1+CW Patents)			0.008**	
			(0.003)	
UTSA×Ln(1+SM Patents)				0.017***
				(0.003)
R&D Intensity	-0.022***			
	(0.004)			
Ln(1+Patents)		-0.017**		
		(0.007)		
Ln(1+CW Patents)			-0.003*	
			(0.002)	
Ln(1+SM Patents)				-0.015***
				(0.002)
All Control Variables	Yes	Yes	Yes	Yes
Firm and Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	80,691	58,704	58,704	58,704
Adjusted R^2	0.721	0.717	0.717	0.718

Table 10: The Uniform Trade Secrets Act, Firm Size, and Bankruptcy Costs

This table reports the results for the panel regressions of alternative definitions of financial leverage on the interaction of the Uniform Trade Secrets Act (UTSA) and a proxy for firm size from 1975 to 2003. The dependent variable in columns 1 and 2 is the change in $Ln(EBIT)_t$ which is measured as the one-year change in the natural logarithm of earnings before interest and taxes over period *t* to *t-1*. The dependent variable in columns 3 and 4 is *Prob. of* $Default_{t+1}$ measured as the inverse of one-year ahead value of 1.2 × working capital over assets plus 1.4 × retained earnings over assets plus 3.3 × EBIT over assets plus 1.0 × sales over assets. The dependent variable in columns 5 and 6 is *CF* $Risk_{t+1}$ which is constructed as the rolling standard deviation of operating cash flows over a 10-year window, where operating cash flows equal income before extraordinary expenses plus depreciation and amortization. *UTSA* is a trade secrets protection index first constructed in Png (2017a). It accounts for pre-existing common law by measuring the change in protection granted via passage of the UTSA in state *s* and year *t*. Δ $Ln(Sales)_t$ measures the one-year change in the natural logarithm of sales over the period *t* to *t-1*. The other explanatory variables include *Book Leverage, Profitability, M/B, Fixed Assets, Div Payer, Prob. of Default, Log(State GDPPC), State GDPG, Republican, IY Leverage,* and *SY Leverage*. Table A2 in the appendix provides variable definitions. All continuous variables, with the exception of the *UTSA*, state-level economic and political variables, are winsorized at the 1st and 99th percentiles. The dollar values are expressed in 2001 dollars. Robust standard errors are clustered at the state of location level and reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	Δ Ln(EBIT)$_t$		Prob. of Default$_{t+1}$		CF Risk$_{t+1}$	
	(1)	(2)	(3)	(4)	(5)	(6)
UTSA	-0.006	-0.012	0.062	0.057	-0.001	0.001
	(0.032)	(0.030)	(0.148)	(0.013)	(0.004)	(0.004)
Ln(Sales)	-0.074***	-0.075***	0.069***	0.081***	-0.003***	-0.002*
	(0.008)	(0.009)	(0.024)	(0.024)	(0.001)	(0.001)
UTSA × Ln(Sales)		0.006		-0.049***		-0.004*
		(0.015)		(0.019)		(0.002)
ΔLn(Sales)	1.278***	1.278***				
	(0.040)	(0.039)				
UTSA × ΔLn(Sales)	-0.213**	-0.214**				
	(0.095)	(0.095)				
Prob. of Default			0.589***	0.585***		
			(0.024)	(0.024)		
UTSA × Prob. of Default			0.003	0.005		
			(0.050)	(0.050)		
IDD	0.008	0.007	-0.033	-0.028	0.002	0.002
	(0.018)	(0.018)	(0.027)	(0.026)	(0.002)	(0.002)
All Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Firm and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	51,171	51,171	73,526	73,526	70,604	70,604
Adjusted R^2	0.253	0.253	0.842	0.842	0.851	0.851

Table D11: The Uniform Trade Secrets Act, Probability of Default, and Financial Leverage

This table reports the results for the panel regressions of financial leverage on the interaction of the Uniform Trade Secrets Act (UTSA) and the probability of default for Compustat industrial firms from 1975 to 2003. Columns 1 and 2 are specific to *Book Leverage* as the dependent variables, whereas columns 3 and 4 have *Market Leverage* on the left-hand side of the pooled panel regression. Columns 1 and 3 specify the dependent variable as contemporaneous, while columns 2 and 4 lead the regressand by one period ($t + 1$). *UTSA* is a trade secrets protection index first constructed in Png (2017a). It accounts for pre-existing common law by measuring the change in protection granted via passage of the UTSA in state s and year t; we provide an explanation for how the variable is measured in Section 4.2. *Prob. of Default* is measured as the inverse of 1.2 × working capital over assets plus 1.4 × retained earnings over assets plus 3.3 × EBIT over assets plus 1.0 × sales over assets. Further, it is centered by subtracting out its sample mean. This is done for ease of interpretation, since we are interacting two continuous variables. The other explanatory variables include *Profitability, M/B, Fixed Assets, Div Payer, Prob. of Default, IDD, Log(State GDPPC), State GDPG, Republican, IY Leverage,* and *SY Leverage*. Table A2 in the appendix provides variable definitions. All continuous variables, with the exception of the *UTSA*, state-level economic and political variables, are winsorized at the 1st and 99th percentiles to remove the influence of extreme outliers. The dollar values are expressed in 2001 dollars. Robust standard errors are clustered at the state of location level and reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	Book Leverage$_t$	Book Leverage$_{t+1}$	Market Leverage$_t$	Market Leverage$_{t+1}$
	(1)	(2)	(3)	(4)
UTSA×Prob. of Default	0.025**	0.019**	0.023**	0.018**
	(0.010)	(0.009)	(0.009)	(0.008)
UTSA	-0.020	-0.017	-0.011	-0.007
	(0.013)	(0.013)	(0.013)	(0.013)
Prob. of Default	-0.042***	-0.029***	-0.038***	-0.028***
	(0.004)	(0.004)	(0.005)	(0.004)
All Control Variables	Yes	Yes	Yes	Yes
Firm and Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	80,691	70,604	80,691	70,604
Adjusted R^2	0.689	0.666	0.721	0.691

Table D12: The Uniform Trade Secrets Act, Firm Size, and Value

This table reports the results for the panel regressions of firm value on the interaction of the Uniform Trade Secrets Act (UTSA) and the natural logarithm of sales for Compustat industrial firms from 1975 to 2003. Columns 1 – 3 specify firm value using the standard measure of *Tobin's Q*, whereas columns 4 – 6 have the proxy variable *Total Tobin's Q*. *Tobin's Q* is measured as the market value of assets (total assets – book equity + market equity) divided by the book value of assets, as in Fama and French (1992). *Total Tobin's Q* is measured as the market value of outstanding equity plus the book value of debt minus the firm's current assets divided by the sum of the book value of property, plant, and equipment, and the replacement cost of intangible capital, as in Peters and Taylor (2017). *UTSA* is a trade secrets protection index first constructed in Png (2017a). It accounts for pre-existing common law by measuring the change in protection granted via passage of the UTSA in state s and year t; we provide an explanation for how the variable is measured in Section 4.2. *Ln(Sales)* is a proxy for firm size measured by sales revenue (specified in logarithm as the difference from its sample mean in order to center the variable). The interaction between the size proxy and *UTSA* yields the main coefficient of interest. We center the size proxy for ease of interpretation, since we are interacting two continuous variables. The other explanatory variables include *Profitability, M/B, Fixed Assets, Div Payer, Mod Z-score, Log(State GDPPC), State GDPG, Republican, IY Leverage,* and *SY Leverage*.Table C2 in the appendix provides variable definitions. All continuous variables, with the exception of the *UTSA*, state-level economic and political variables, are winsorized at the 1st and 99th percentiles to remove the influence of extreme outliers. The dollar values are expressed in 2001 dollars. Robust standard errors are clustered at the state of location level and reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	Tobin's Q			Total Tobin's Q		
	(1)	(2)	(3)	(4)	(5)	(6)
UTSA×Ln(Sales)		0.195***	0.198***		0.278***	0.276***
		(0.028)	(0.030)		(0.070)	(0.069)
UTSA	0.034	-0.082	-0.068	-0.032	-0.186*	-0.175*
	(0.092)	(0.064)	(0.066)	(0.112)	(0.101)	(0.096)
Ln(Sales)	-0.106***	-0.153***	-0.158***	-0.249***	-0.315***	-0.318***
	(0.022)	(0.026)	(0.026)	(0.052)	(0.060)	(0.060)
IDD	0.074		0.053	0.123		0.094
	(0.059)		(0.058)	(0.078)		(0.074)
Firm and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	80,691	80,691	80,691	80,570	80,570	80,570
Adjusted R^2	0.489	0.488	0.490	0.408	0.408	0.409

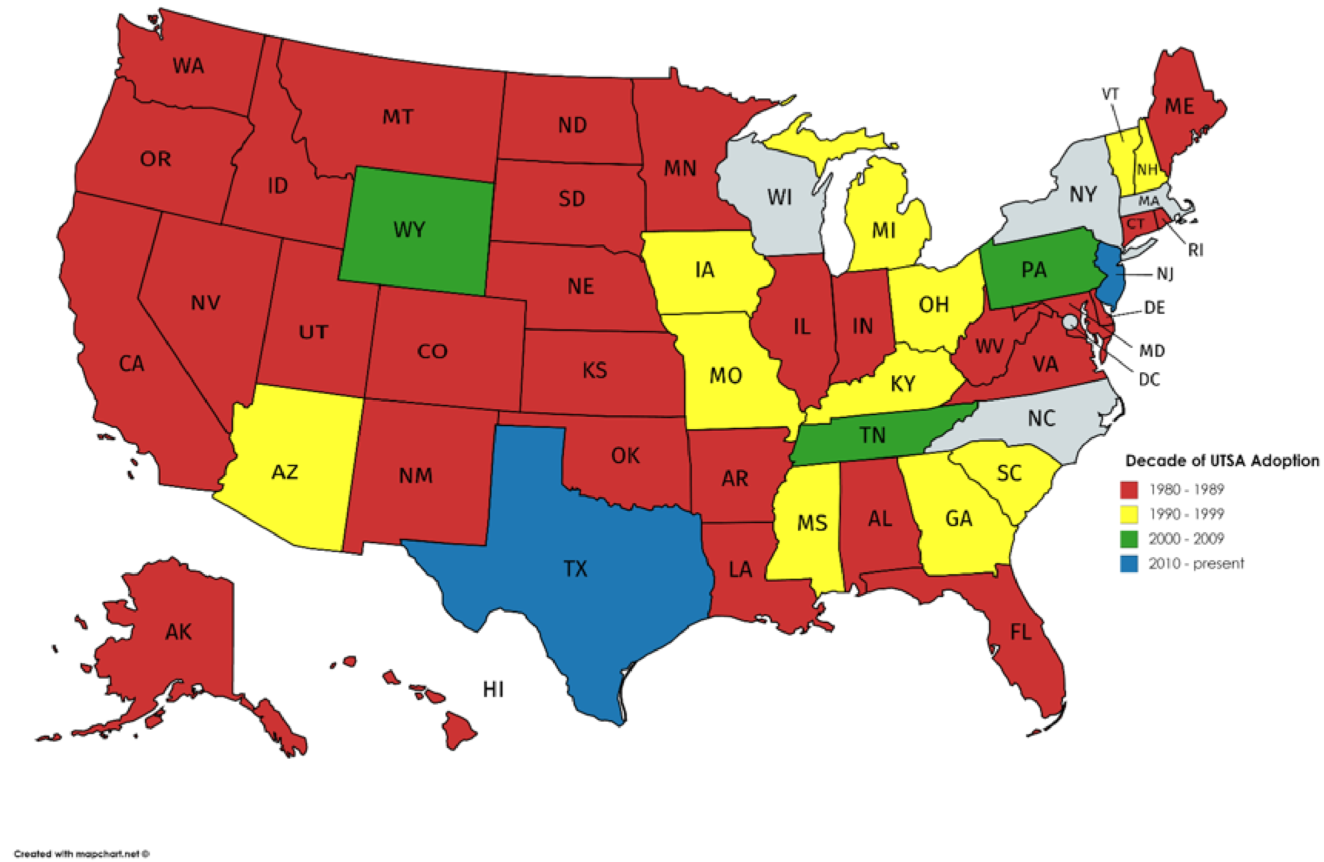


Figure D1: States with a Uniform Trade Secrets Act statute. The chart above shows the states that have adopted a UTSA statute by decade. States colored with red indicates passage of a law during the period 1980 to 1989. Yellow colored states denotes legalization of UTSA from 1990 to 1999. The green colored states adopt UTSA in the 2000 to 2009 period. States in blue passed a UTSA statute from 2010 to the present, and the four grey colored states are without such legislation.

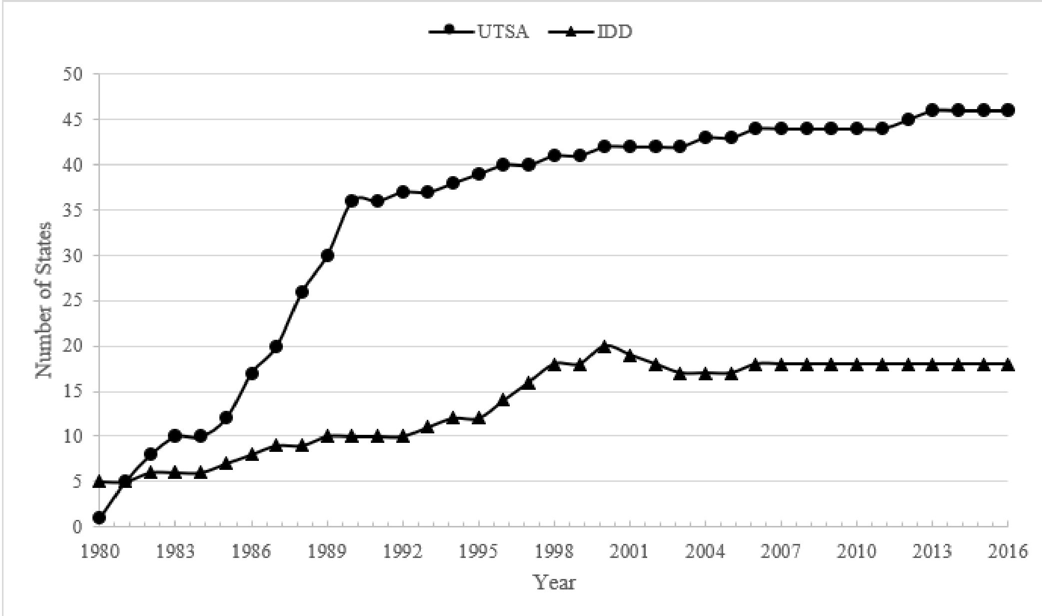


Figure D2. Number of states with trade secrets protection. This figure displays the number of states that have passed the Uniform Trade Secrets Act (UTSA) legislatively, and the number of states that adopted the Inevitable Disclosure Doctrine (IDD) judicially, from 1980 to 2016.

Appendix E: Chapter 3 Variable Definitions

Table E1: Variable Definitions

This table provides definitions for all the variables used in Chapter 3 and Tables F1 through F20, and G9 through G20.

Dependent Variables	Description
Tobin's Q	Market value of assets (*at* – book equity + market equity (*prcc_f*csho*)) divided by the book value of assets (*at*). Book equity and this measure, in general, follows Fama and French (1992).
Monthly Stock Returns	Monthly stock returns of a portfolio created by either (i) longing the stocks of matched firms incorporated in poison pill law adopting states, (ii) shorting the stocks of matched companies incorporated in states without poison pill legislation, and (iii) combining both (i) and (ii) into a long-short investment strategy. In all three portfolios, we begin the holding period 6 months before the adoption date and continue to hold until 24 ("6m24") or 36 ("6m36") months after the laws are enacted.
Takeover Bid (Bid)	*Bid* is an indicator variable equal to one if a firm receives a takeover bid as catalogued by the SDC M&A database and CRSP delisting codes (200s), and zero otherwise.
Takeover Complete (Complete)	*Complete* is an indicator variable equal to one if a firm is successfully acquired as catalogued by the SDC M&A database and CRSP delisting codes (200s), and zero otherwise.
1-Day Premium	Premium of offer price to target closing stock price 1-day prior to the original announcement date, expressed as a percentage. Data comes from the SDC M&A database.
1-Week Premium	Premium of offer price to target closing stock price 1-week prior to the original announcement date, expressed as a percentage. Data comes from the SDC M&A database.
4-Week Premium	Premium of offer price to target closing stock price 4-week prior to the original announcement date, expressed as a percentage. Data comes from the SDC M&A database.
Return on Assets (ROA)	Operating income before depreciation and amortization (*oibdp*) divided by the book value of assets (*at*).
Net Profit Margin (NPM)	Net income (*ni*) divided by the value of sales (*sale*).
Operating Margin (OM)	Operating income after depreciation and amortization (*oiadp*) divided by the value of sales (*sale*).

Table E1 – *(Continued)*

Sales Growth (SG)	The natural logarithm of the value of sales (*sale*) in millions in year *t* divided by the value of sales (*sale*) in millions in year *t*-1; also specified as a control in *Tobin's Q* regressions.
Total Tobin's Q	Market value of outstanding equity (*prcc_f*csho*) plus the book value of debt (*dltt* + *dlc*) minus the firm's current assets (*act*) divided by the sum of the book value of property, plant, and equipment (*ppegt*), and the replacement cost of intangible capital (the sum of the firm's externally purchased and internally created intangible capital), follows Peters and Taylor (2017). This measure (*q_tot*) is available on WRDS from 1950 to 2015.
Main Explanatory Variables	Description
Poison Pill Law	An indicator variable equal to one if a firm is incorporated in a state that has adopted a poison pill law, and zero otherwise. We use adoption dates provided by Cain, McKeon and Solomon (2017) and Karpoff and Wittry (2017).
Poison Pill Law First Wave	An indicator variable equal to one if a firm is incorporated in a state that passes a poison pill law during the period 1986 to 1990, and zero otherwise. We use adoption dates provided by Cain, McKeon and Solomon (2017) and Karpoff and Wittry (2017).
Poison Pill Law Second Wave	An indicator variable equal to one if a firm is incorporated in a state that passes a poison pill law during the period 1995 to 2009, and zero otherwise. We use adoption dates provided by Cain, McKeon and Solomon (2017) and Karpoff and Wittry (2017).
Alpha	Monthly portfolio abnormal returns, estimated using either the four-factor Carhart (1997) and three-factor Fama-French (1993) models, respectively.
Poison Pill Law First Wave Adjusted	An indicator variable equal to one if a firm is incorporated in a state that passes a poison pill law during the period 1986 to 1988, and zero otherwise. We use adoption dates provided by Cain, McKeon and Solomon (2017) and Karpoff and Wittry (2017).
Poison Pill Law Second Wave Adjusted	An indicator variable equal to one if a firm is incorporated in a state that passes a poison pill law during the period 1989 to 2009, and zero otherwise. We use adoption dates provided by Cain, McKeon and Solomon (2017) and Karpoff and Wittry (2017).

Table E1 – *(Continued)*

PPV-Index	We create a poison pill validity index (PPV-Index) using poison pill statute and poison pill case information provided by Cain, McKeon, and Solomon (2017). The PPV-Index captures the relative change or strength of poison pill validity over time and by state of incorporation. For a detailed description of the PPV-Index, see Panel A of Table F17.
Main Interaction Variables	Description
Incorp State-Year M&A Volume	The ratio of mergers & acquisitions' dollar volume in SDC to the total market capitalization from Compustat per state of incorporation, in a given year. We only include ordinary stocks (i.e., we exclude American depositary receipts (ADRs) and real estate investment trusts (REITs)). Further, we only consider SDC transactions that are completed and where the acquirer achieves control of the target; also included as a predictor variable.
Industry-Year M&A Volume	The ratio of mergers & acquisitions' dollar volume in SDC to the total market capitalization from Compustat per Fama-French 49 industry groupings, in a given year. We only include ordinary stocks (i.e., we exclude American depositary receipts (ADRs) and real estate investment trusts (REITs)). Further, we only consider SDC transactions that are completed and where the acquirer achieves control of the target; also included as a predictor variable.
Large Customer	An indicator variable equal to one if a firm has at least one large customer based on the Compustat Customer Segments database.
Strategic Alliance	An indicator variable equal to one if the firm is in an active strategic alliance based on the SDC Strategic Alliances database.
Labor Capital	Selling, general and administrative expenses (*xsga*) scaled by the book value of assets (*at*).
R&D/Sales	Research and development expense (*xrd*) divided by the value of sales (*sale*).
Intangible Capital/Assets	Firm's intangible capital estimated replacement cost scaled by the book value of assets (*at*). The measure (*K_int*) is available on WRDS from 1950 to 2015, from Peters and Taylor (2017).
Knowledge Capital/Assets	Firm's knowledge capital replacement cost scaled by the book value of assets (*at*). The measure (*K_int_Know*) is available on WRDS from 1950 to 2015, from Peters and Taylor (2017).

Table E1 – *(Continued)*

Staggered Board	An indicator variable equal to one if the board is staggered in year *t*, and zero otherwise. Data come from Cremers, Litov, and Sepe (2017).
Control Variables	Description
Poison Pill Firm-Level	An indicator variable equal to one if a firm has adopted a poison pill. We use data from ISS (formerly Riskmetrics), Cremers and Ferrell (2014), Cremers, Litov and Sepe (2017), SDC's Corporate Governance and M&A databases, Comment and Schwert (1995), Caton and Goh (2008) and hand-collected information from Factiva.
Ln(Assets)	The natural logarithm of the value of total book assets (*at*) in millions, where assets are adjusted using 2015 dollars.
Ln(Age)	The natural logarithm of one plus the number of firm-year observations since the firm's first appearance in Compustat.
HHI	The Herfindahl-Hirschman Index for a particular industry defined as the sum of squared market shares for all firms in a three-digit SIC industry. The market share of firm *i* is defined as the value of sales (*sale*) of firm *i* divided by the total value of sales (*sale*) in the industry of firm *i*.
Loss	An indicator variable set to one if a firm has negative net income (*ni*) during a fiscal year, and zero otherwise.
Debt-to-Equity	Long-term debt (*dltt*) divided by book equity, where book equity is calculated as in Fama and French (1992).
Firm Liquidity	Current assets (*act*) minus current liabilities (*lct*) divided by the value of total book assets (*at*).
CAPX/Assets	Capital expenditures (*capx*) divided by the value of total book assets (*at*).
Institutional Ownership	The percent ownership of a firm by its institutional owners, measure by their equity ownership in their 13F holdings reports from Thomson Reuters, weighted by the firm's market capitalization.
State-Year Tobin's Q	Control for local shocks, measured as the mean of *Tobin's Q* in the firm's state of location in a given year, excluding the firm itself.
Industry-Year Tobin's Q	Control for industry shocks, measured as the mean of *Tobin's Q* in the firm's three-digit SIC industry in a given year, excluding the firm itself.

Table E1 – *(Continued)*

Business Combination Law	An indicator variable equal to one if a firm is incorporated in a state that has adopted a business combination law, and zero otherwise. We use adoption dates provided by Cain, McKeon and Solomon (2017) and Karpoff and Wittry (2017).
Control Share Law	An indicator variable equal to one if a firm is incorporated in a state that has adopted a control share law, and zero otherwise. We use adoption dates provided by Cain, McKeon and Solomon (2017) and Karpoff and Wittry (2017).
Directors' Duties Law	An indicator variable equal to one if a firm is incorporated in a state that has adopted a directors' duties law, and zero otherwise. We use adoption dates provided by Karpoff and Wittry (2017).
Fair Price Law	An indicator variable equal to one if a firm is incorporated in a state that has adopted a fair price law, and zero otherwise. We use adoption dates provided by Cain, McKeon and Solomon (2017) and Karpoff and Wittry (2017).
Incorp State-Year Q	The average Tobin's Q of all firms incorporated within a state, in a given year.
Incorp State-Year Poison Pill Firm Level	The average percent of all firms incorporated within a state with an existing poison pill in-place, in a given year.
Incorp State-Year Ln(Assets)	The average natural logarithm of total assets of all firms incorporated within a state, in a given year, where assets are adjusted using 2015 dollars.
Incorp State-Year Ln(Age)	The average natural logarithm of one plus the number of firm-year observations since the firm's first appearance in Compustat of all firms incorporated within a state, in a given year.
Incorp State-Year HHI	The average Herfindahl-Hirschman Index of all firms incorporated within a state, in a given year.
Incorp State-Year Sales Growth	The average sales growth of all firms incorporated within a state, in a given year.
Incorp State-Year Loss	The average percent of all firms incorporated within a state experiencing negative net income, in a given year.
Incorp State-Year Debt-to-Equity	The average debt-to-equity of all firms incorporated within a state, in a given year.
Incorp State-Year Firm Liquidity	The average firm liquidity of all firms incorporated within a state, in a given year.
Incorp State-Year CAPX/Assets	The average ratio of capital expenditure to total assets of all firms incorporated within a state, in a given year.

Table E1 – *(Continued)*

Incorp State-Year R&D/Sales	The average ratio of research and development expenditure to sales of all firms incorporated within a state, in a given year.
Incorp State-Year Institutional Ownership	The average percentage of institutional ownership of all firms incorporated within a state, in a given year.
R&D Tax Credit	An indicator variable set to one if a state has adopted a tax credit for research & development expenditure, and zero otherwise; Data comes from Wilson (2009).
Percent Incorp State Republican	The proportion of incorporated state-level representatives in the U.S. House of Representatives whom belong to the Republican party, in a given year. We use data from the Book of the States for this measure.
Ln(Incorp State Per Capita GDP)	The natural logarithm of an incorporating state's GDP (in thousands) divided by its total population. We use data from the U.S. Bureau of Economic Analysis.
Incorp State GDP Growth	The incorporated state-level GDP growth rate over the fiscal year. We use data from the U.S. Bureau of Economic Analysis.

Appendix F: Chapter 3 Tables and Figures

Table F1: State-Level Poison Pill Laws

This table reports the month and year in which a state adopts a poison pill statute; a blank entry indicates that no law has been passed. The dates listed below on state-level laws comes from Cain, McKeon and Solomon (2017), and Karpoff and Wittry (2017). The number of unique firms' column provides the total number of distinct firms in the respective incorporating state in our sample from 1983 to 2015. The sum of this column exceeds the total number of unique firms in the pooled panel regressions due to reincorporations. Treatment firms are defined as companies incorporated in a state that adopts a poison pill statute, whereas controls incorporate in states without such legislation at the time of the analysis. The first wave measures the period of initial poison pill law passage from 1986 to 1990, whereas the second wave captures the next batch of statute adoptions over the period 1995 to 2009.[a]

State	Month/Year Poison Pill Law Passed	Number of Unique Firms in the Sample	Full Sample		First Wave		Second Wave	
			Treat	Control	Treat	Control	Treat	Control
Alabama		7	No	Yes	No	Yes	No	Yes
Alaska		1	No	Yes	No	Yes	No	Yes
Arizona		16	No	Yes	No	Yes	No	Yes
Arkansas		5	No	Yes	No	Yes	No	Yes
California		264	No	Yes	No	Yes	No	Yes
Colorado	3/1989	12	Yes	No	Yes	No	No	No
Connecticut	6/2003	18	Yes	No	No	Yes	Yes	No
Delaware[b]		2,009	No	Yes	No	Yes	No	Yes
Florida	6/1989	49	Yes	No	Yes	No	No	No
Georgia	4/1988	36	Yes	No	Yes	No	No	No
Hawaii	6/1988	7	Yes	No	Yes	No	No	No
Idaho	3/1988	2	Yes	No	Yes	No	No	No
Illinois	8/1989	13	Yes	No	Yes	No	No	No
Indiana	3/1986	37	Yes	No	Yes	No	No	No
Iowa	6/1989	9	Yes	No	Yes	No	No	No
Kansas		14	No	Yes	No	Yes	No	Yes
Kentucky	7/1988	6	Yes	No	Yes	No	No	No
Louisiana		18	No	Yes	No	Yes	No	Yes
Maine[c]	4/2002	4	Yes	No	No	Yes	Yes	No
Maryland	5/1999	73	Yes	No	No	Yes	Yes	No

[a] The literature typically refers to state antitakeover laws passed after 1982 as second-generation laws, where the first-generation laws were invalidated in 1982 by the *MITE* decision (please see Karpoff and Wittry (2017) for a more detailed discussion); other studies further classify the most recent statutes as third-generation laws. We choose to separate by "waves" since we focus only on poison pill legislation.

[b] The Moran v. Household court decision in Delaware in 1985 provides some legitimacy to poison pills, however, its legality is still debatable and can be challenged by firms, thus we treat Delaware as a control state or exclude from the analysis all together.

[c] The Georgia-Pacific v. Great Northern Nekoosa Corp. court decision in Maine in 1990 provides some legitimacy to poison pills, however, its legality was affirmed when the state passed a law. Thus we consider Maine a treated state since its adoption of a statute, and a control any time before.

Table F1 – *(Continued)*

Massachusetts	7/1989	77	Yes	No	Yes	No	No	No
Michigan	7/2001	72	Yes	No	No	Yes	Yes	No
Minnesota	5/1995	90	Yes	No	No	Yes	Yes	No
Mississippi	4/2005	4	Yes	No	No	Yes	Yes	No
Missouri	7/1999	36	Yes	No	No	Yes	Yes	No
Montana		1	No	Yes	No	Yes	No	Yes
Nebraska		6	No	Yes	No	Yes	No	Yes
Nevada	6/1989	45	Yes	No	Yes	No	No	No
New Hampshire		1	No	Yes	No	Yes	No	Yes
New Jersey	6/1989	52	Yes	No	Yes	No	No	No
New Mexico		2	No	Yes	No	Yes	No	Yes
New York	12/1988	125	Yes	No	Yes	No	No	No
North Carolina	6/1989	25	Yes	No	Yes	No	No	No
North Dakota			No	Yes	No	Yes	No	Yes
Ohio	11/1986	87	Yes	No	Yes	No	No	No
Oklahoma		21	No	Yes	No	Yes	No	Yes
Oregon	3/1989	21	Yes	No	Yes	No	No	No
Pennsylvania	3/1988	89	Yes	No	Yes	No	No	No
Rhode Island	7/1990	2	Yes	No	Yes	No	No	No
South Carolina	6/1998	15	Yes	No	No	Yes	Yes	No
South Dakota	2/1990	2	Yes	No	Yes	No	No	No
Tennessee	5/1989	24	Yes	No	Yes	No	No	No
Texas	5/2003	143	Yes	No	No	Yes	Yes	No
Utah	3/1989	10	Yes	No	Yes	No	No	No
Vermont	6/2008	2	Yes	No	No	Yes	Yes	No
Virginia	4/1990	40	Yes	No	Yes	No	No	No
Washington	3/1998	80	Yes	No	No	Yes	Yes	No
West Virginia		3	No	Yes	No	Yes	No	Yes
Wisconsin	9/1987	32	Yes	No	Yes	No	No	No
Wyoming	3/2009	6	Yes	No	No	Yes	Yes	No

Table F2: Summary Statistics

This table reports summary statistics for the main dependent and explanatory variables used in the pooled panel regressions. Panel A presents full sample summary statistics. Panel B shows the summary statistics by first wave (1983 to 1994) and second wave (1995 to 2012) periods. Panel C reports the summary statistics by treatment and control grouping. If a firm is incorporated in a state that has adopted poison pill legislation it is included in the treatment group, and in the control group otherwise. The sample is composed of Compustat industrial firms over the period 1983 to 2012. This range yields an equidistant three-year window around the first states' and last state's adoption of a poison pill law. Further, prior to 1983 states passed first-generation laws which were invalidated in 1982 by the *MITE* decision (see Karpoff and Wittry, 2017). Thus, to minimize the noise from these inaugural state takeover laws and their repeal, we start the sample in 1983. All continuous variables are winsorized at the 1st and 99th percentiles and the dollar values are expressed in 2015 dollars. Table E1 provides variable definitions. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Panel A: Full Sample

Dependent Variable:	Mean	St. Dev.	P25	Median	P75	Obs.
$Q_{[t]}$	1.859	1.246	1.144	1.471	2.092	33,826
Independent Variables:	Mean	St. Dev.	P25	Median	P75	Obs.
$Poison\ Pill\ Law_{[t]}$	0.284	0.451	0	0	1	33,826
$Poison\ Pill\ Firm\text{-}Level_{[t]}$	0.391	0.488	0	0	1	33,826
$Business\ Combination\ Law_{[t]}$	0.779	0.415	1	1	1	33,826
$Control\ Share\ Law_{[t]}$	0.239	0.427	0	0	0	33,826
$Directors'\ Duties\ Law_{[t]}$	0.283	0.451	0	0	1	33,826
$Fair\ Price\ Law_{[t]}$	0.288	0.453	0	0	1	33,826
$Ln(Assets)_{[t]}$	7.026	1.753	5.933	7.007	8.169	33,826
$Ln(Age)_{[t]}$	3.030	0.557	2.639	3.135	3.466	33,826
$HHI_{[t]}$	0.238	0.180	0.107	0.191	0.294	33,826
$Sales\ Growth_{[t]}$	0.045	0.231	-0.039	0.042	0.130	33,826
$Loss_{[t]}$	0.215	0.411	0	0	0	33,826
$Debt\text{-}to\text{-}Equity_{[t]}$	0.551	1.364	0.026	0.307	0.704	33,826
$Firm\ Liquidity_{[t]}$	0.242	0.206	0.089	0.227	0.378	33,826
$CAPX/Assets_{[t]}$	0.061	0.056	0.025	0.046	0.078	33,826
$R\&D/Sales_{[t]}$	0.034	0.076	0	0.003	0.037	33,826
$Institutional\ Ownserhip_{[t]}$	0.450	0.333	0.061	0.496	0.736	33,826
$State\text{-}Year\ Q_{[t]}$	1.942	0.436	1.625	1.863	2.189	33,826
$Industry\text{-}Year\ Q_{[t]}$	2.010	0.836	1.422	1.793	2.375	33,826

Table F2 – *(Continued)*

Panel B: Full Sample by Wave

	First Wave (1983 to 1994)			Second Wave (1995 to 2012)				
Dependent Variable:	Mean	St. Dev.	Obs.	Mean	St. Dev.	Obs.	Diff.	*t*-stat
$Q_{[t]}$	1.565	0.856	10,242	1.987	1.361	23,584	-0.422***	-28.96

	First Wave (1983 to 1994)			Second Wave (1995 to 2012)				
Independent Variables:	Mean	St. Dev.	Obs.	Mean	St. Dev.	Obs.	Diff.	*t*-stat
$Poison\ Pill\ Law_{[t]}$	0.191	0.393	10,242	0.324	0.468	23,584	-0.133***	-25.23
$Poison\ Pill\ Firm\text{-}Level_{[t]}$	0.410	0.492	10,242	0.383	0.486	23,584	0.027***	4.64
$Business\ Combination\ Law$	0.552	0.497	10,242	0.878	0.328	23,584	-0.325***	-71.06
$Control\ Share\ Law_{[t]}$	0.190	0.393	10,242	0.260	0.439	23,584	-0.070***	-13.88
$Directors'\ Duties\ Law_{[t]}$	0.227	0.419	10,242	0.308	0.462	23,584	-0.081***	-15.28
$Fair\ Price\ Law_{[t]}$	0.247	0.431	10,242	0.307	0.461	23,584	-0.060***	-11.17
$Ln(Assets)_{[t]}$	7.079	1.536	10,242	7.002	1.838	23,584	0.077***	3.72
$Ln(Age)_{[t]}$	3.044	0.423	10,242	3.024	0.605	23,584	0.021***	3.12
$HHI_{[t]}$	0.261	0.173	10,242	0.229	0.182	23,584	0.033***	15.32
$Sales\ Growth_{[t]}$	0.031	0.199	10,242	0.051	0.243	23,584	-0.020***	-7.31
$Loss_{[t]}$	0.178	0.383	10,242	0.231	0.421	23,584	-0.053***	-10.85
$Debt\text{-}to\text{-}Equity_{[t]}$	0.580	1.276	10,242	0.539	1.400	23,584	0.041**	2.54
$Firm\ Liquidity_{[t]}$	0.241	0.185	10,242	0.242	0.214	23,584	-0.001	-0.51
$CAPX/Assets_{[t]}$	0.071	0.051	10,242	0.057	0.057	23,584	0.014***	20.64
$R\&D/Sales_{[t]}$	0.024	0.045	10,242	0.039	0.086	23,584	-0.015***	-16.34
$Institutional\ Ownserhip_{[t]}$	0.303	0.255	10,242	0.514	0.343	23,584	-0.211***	-55.86
$State\text{-}Year\ Q_{[t]}$	1.822	0.329	10,242	1.995	0.466	23,584	-0.173***	-34.06
$Industry\text{-}Year\ Q_{[t]}$	1.807	0.678	10,242	2.098	0.881	23,584	-0.292***	-29.88

Table F2 – *(Continued)*

Panel C: Full Sample by Treatment

	Treat (Poison Pill Law = 1)			Control (Poison Pill Law = 0)				
Dependent Variable:	Mean	St. Dev.	Obs.	Mean	St. Dev.	Obs.	Diff.	*t*-stat
$Q_{[t]}$	1.844	1.159	9,602	1.865	1.278	24,224	-0.021	1.38

	Treat (Poison Pill Law = 1)			Control (Poison Pill Law = 0)				
Independent Variables:	Mean	St. Dev.	Obs.	Mean	St. Dev.	Obs.	Diff.	*t*-stat
$Poison\ Pill\ Firm\text{-}Level_{[t]}$	0.429	0.495	9,602	0.376	0.484	24,224	0.053***	9.05
$Business\ Combination\ Law$	0.856	0.351	9,602	0.749	0.434	24,224	0.107***	21.52
$Control\ Share\ Law_{[t]}$	0.646	0.478	9,602	0.078	0.268	24,224	0.568***	1,400
$Directors'\ Duties\ Law_{[t]}$	0.890	0.313	9,602	0.043	0.203	24,224	0.847***	2,900
$Fair\ Price\ Law_{[t]}$	0.806	0.395	9,602	0.083	0.276	24,224	0.723***	1,900
$Ln(Assets)_{[t]}$	7.031	1.724	9,602	7.023	1.764	24,224	0.008	0.36
$Ln(Age)_{[t]}$	3.191	0.531	9,602	2.966	0.554	24,224	0.225***	34.02
$HHI_{[t]}$	0.254	0.182	9,602	0.232	0.179	24,224	0.021***	9.77
$Sales\ Growth_{[t]}$	0.037	0.206	9,602	0.047	0.240	24,224	-0.010***	-3.59
$Loss_{[t]}$	0.197	0.398	9,602	0.222	0.416	24,224	-0.025***	-5.07
$Debt\text{-}to\text{-}Equity_{[t]}$	0.529	1.247	9,602	0.560	1.407	24,224	-0.030*	-1.85
$Firm\ Liquidity_{[t]}$	0.242	0.204	9,602	0.242	0.207	24,224	0.000	0.16
$CAPX/Assets_{[t]}$	0.056	0.051	9,602	0.063	0.057	24,224	-0.007***	-10.43
$R\&D/Sales_{[t]}$	0.030	0.070	9,602	0.036	0.078	24,224	-0.007***	-7.34
$Institutional\ Ownserhip_{[t]}$	0.480	0.324	9,602	0.438	0.336	24,224	0.041***	10.31
$State\text{-}Year\ Q_{[t]}$	1.880	0.407	9,602	1.967	0.445	24,224	-0.087***	-16.62
$Industry\text{-}Year\ Q_{[t]}$	1.990	0.823	9,602	2.018	0.841	24,224	-0.028***	-2.81

Table F3: Explaining the Adoption of Poison Pill Statutes

This table presents results from linear probability models analyzing the determinants of a state adopting a poison pill law. The sample period in columns (1) and (2) is for the full period 1983 – 2012, whereas columns (3) and (4), and (5) and (6) are split into the "first wave" and "second wave" periods, respectively. We define the dependent variable in the LPM models as the passage of a poison pill statute in a given state. Further, once a firm becomes covered by a poison pill statute they are removed from the analysis in the subsequent annual regressions. The independent variables are lagged one year. We standardize the continuous explanatory variables to have zero mean and unit variance. We also include year and incorporating state fixed effects in the LPM specifications. Table E1 provides variable definitions. All continuous variables are winsorized at the 1% level in both tails, and dollar values are expressed in 2015 dollars. t-statistics are reported in parentheses and are estimated using robust standard errors independently double-clustered at the incorporating state and year level. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Dep. Variable: $Poison\ Pill\ Law_{[t]}$

	1983 – 2012		First Wave (1983 – 1994)		Second Wave (1995 – 2012)	
Variables	(1)	(2)	(3)	(4)	(5)	(6)
$Incorp\ State\text{-}Year\ Q_{[t-1]}$	-0.009	0.009	-0.008	0.106	0.080	0.113
	(-0.09)	(0.09)	(-0.06)	(0.90)	(0.57)	(0.86)
$Incorp\ State\text{-}Year\ M\&A\ Volume_{[t-1]}$	-0.008	-0.004	0.044	0.047	0.002	-0.003
	(-0.57)	(-0.27)	(1.04)	(1.35)	(0.12)	(-0.26)
$Industry\text{-}Year\ M\&A\ Volume_{[t-1]}$	-0.001	-0.001	-0.002	-0.003	-0.001	-0.002
	(-0.83)	(-0.70)	(-1.20)	(-0.84)	(-0.65)	(-0.96)
$Incorp\ State\text{-}Year\ Poison\ Pill\ Firm\text{-}Level_{[t-1]}$	-0.218	-0.180	-0.093	-0.311	0.010	-0.177
	(-1.04)	(-0.82)	(-0.27)	(-1.06)	(0.04)	(-0.58)
$Incorp\ State\text{-}Year\ Ln(Assets)_{[t-1]}$	0.027	0.005	0.238	0.164	-0.076	-0.077
	(0.31)	(0.06)	(0.88)	(0.59)	(-0.53)	(-0.64)
$Incorp\ State\text{-}Year\ Ln(Age)_{[t-1]}$	0.157	0.193	0.518	0.563	0.115	0.112
	(0.78)	(0.68)	(1.07)	(1.06)	(0.38)	(0.27)
$Incorp\ State\text{-}Year\ HHI_{[t-1]}$	-0.184	0.068	-0.476	0.133	0.402	0.204
	(-0.40)	(0.15)	(-0.69)	(0.17)	(0.67)	(0.46)
$Incorp\ State\text{-}Year\ Sales\ Growth_{[t-1]}$	-0.071	-0.050	0.105	0.131	-0.219	-0.316
	(-0.22)	(-0.16)	(0.28)	(0.45)	(-0.46)	(-0.66)
$Incorp\ State\text{-}Year\ Loss_{[t-1]}$	0.022	0.031	0.110	0.067	-0.055	-0.087
	(0.15)	(0.22)	(0.53)	(0.37)	(-0.36)	(-0.49)
$Incorp\ State\text{-}Year\ Debt\text{-}to\text{-}Equity_{[t-1]}$	-0.099	-0.043	-0.223*	-0.089	0.133	0.129
	(-0.82)	(-0.41)	(-1.70)	(-0.81)	(0.64)	(0.69)
$Incorp\ State\text{-}Year\ Firm\ Liquidity_{[t-1]}$	-0.488	-0.582	1.038	0.140	-0.515	-0.141
	(-0.95)	(-1.19)	(1.29)	(0.26)	(-0.57)	(-0.16)
$Incorp\ State\text{-}Year\ CAPX/Assets_{[t-1]}$	-0.930	-0.794	2.094	1.079	-1.807	-1.742
	(-0.90)	(-0.73)	(1.50)	(0.91)	(-1.10)	(-1.06)
$Incorp\ State\text{-}Year\ R\&D/Sales_{[t-1]}$	0.074	0.156	0.872	1.578	-0.654	-0.736
	(0.13)	(0.28)	(0.62)	(0.91)	(-1.01)	(-1.29)
$Incorp\ State\text{-}Year\ Institutional\ Ownership_{[t-1]}$	-0.582	-0.580	0.043	0.114	-0.243	-0.033
	(-1.30)	(-1.38)	(0.06)	(0.17)	(-0.61)	(-0.10)

Table F3 – *(Continued)*

$Business\ Combination\ Law_{[t-1]}$		-0.078		-0.268**		0.150
		(-0.58)		(-2.23)		(1.02)
$Control\ Share\ Law_{[t-1]}$		0.064		0.088		-0.318
		(0.54)		(0.65)		(-1.04)
$Directors'\ Duties\ Law_{[t-1]}$		0.388**		0.429**		0.471***
		(2.28)		(2.43)		(2.61)
$Fair\ Price\ Law_{[t-1]}$		0.033		0.136		-0.568**
		(0.27)		(1.13)		(-2.35)
$R\&D\ Tax\ Credit_{[t-1]}$		0.003		0.109		-0.021
		(0.03)		(0.70)		(-0.17)
$Percent\ Incorp\ State\ Republican_{[t-1]}$		-0.022		0.008		0.064
		(-0.83)		(0.40)		(1.21)
$Ln(Incorp\ State\ Per\ Capita\ GDP)$		0.140		0.307**		-0.254*
		(1.26)		(2.01)		(-1.72)
$Incorp\ State\ GDP\ Growth_{[t-1]}$		-0.015		-0.018		-0.003
		(-0.64)		(-0.57)		(-0.15)
Incorporating state and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
# of firms in regression	2,821	2,821	1,259	1,259	2,306	2,306
N	22,185	22,185	6,871	6,871	15,314	15,314
Adjusted R^2	0.271	0.326	0.348	0.501	0.413	0.456

Table F4: Explaining the Adoption of Firm-Level Poison Pills

This table presents results from linear probability model regressions of a firm-level poison pill indicator variable on predictor variables. The dependent variable *Poison Pill Firm-Level* and main independent variables *Poison Pill Law*, *Poison Pill Law First Wave*, and *Poison Pill Law Second Wave* are measured contemporaneously, whereas the remaining controls are lagged one period. *Poison Pill Law First Wave* is a dummy variable equal to one if a firm is incorporated in a state that passes a poison pill law during the period 1986 to 1990, and zero otherwise. *Poison Pill Law Second Wave* is a dummy variable equal to one if a firm is incorporated in a state that passes a poison pill law during the period 1995 to 2009, and zero otherwise. We include firm and year fixed effects and the coefficient estimates are for the full sample period 1983 to 2012. Other control variables not reported due to economic and statistical insignificance: *Loss, Debt-to-Equity, CAPX/Assets, R&D/Sales, State-Year Q*, and *Industry-Year Q*. Further, columns (3), and (6) specify: *Business Combination Law, Control Share Law, Directors' Duties Law,* and *Fair Price Law* dummies. Table E1 provides variable definitions. The continuous variables are standardized to have a mean of zero, and a standard deviation equal to one. The continuous variables are winsorized at the 1st and 99th percentiles, and the dollar values are expressed in 2015 dollars. t-statistics are reported in parentheses and are estimated using robust standard errors, clustered at the firm level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dep. Variable: $Poison\ Pill\ Firm\text{-}Level_{[t]}$						
	1983 – 2012					
Variables	(1)	(2)	(3)	(4)	(5)	(6)
$Q_{[t-1]}$	-0.038***	-0.021***	-0.021***	-0.038***	-0.021***	-0.021***
	(-8.09)	(-4.46)	(-4.47)	(-8.13)	(-4.50)	(-4.52)
$Poison\ Pill\ Law_{[t]}$	0.073***	0.060***	0.042*			
	(3.60)	(2.95)	(1.78)			
$Poison\ Pill\ Law\ First\ Wave_{[t]}$				0.033	0.034	-0.002
				(1.37)	(1.36)	(-0.09)
$Poison\ Pill\ Law\ Second\ Wave_{[t]}$				0.124***	0.093***	0.070**
				(3.86)	(2.95)	(2.21)
$Ln(Assets)_{[t-1]}$		0.039***	0.038***		0.039***	0.038***
		(3.34)	(3.29)		(3.34)	(3.28)
$Ln(Age)_{[t-1]}$		0.561***	0.559***		0.559***	0.558***
		(13.92)	(13.93)		(13.85)	(13.88)
$HHI_{[t-1]}$		-0.121***	-0.120**		-0.122***	-0.121***
		(-2.58)	(-2.54)		(-2.60)	(-2.58)
$Sales\ Growth_{[t-1]}$		-0.032***	-0.032***		-0.032***	-0.031***
		(-2.84)	(-2.82)		(-2.80)	(-2.76)
$Firm\ Liquidity_{[t-1]}$		-0.113***	-0.116***		-0.113***	-0.115***
		(-3.04)	(-3.11)		(-3.02)	(-3.10)
$Institutional\ Ownserhip_{[t-1]}$		0.098***	0.097***		0.098***	0.097***
		(2.87)	(2.83)		(2.87)	(2.84)
Other law controls	No	No	Yes	No	No	Yes
Firm and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
# of firms in regression	3,423	3,423	3,423	3,423	3,423	3,423
N	33,821	33,821	33,821	33,821	33,821	33,821
Adjusted R^2	0.528	0.558	0.559	0.529	0.558	0.559

Table F5: Poison Pill Laws and Firm Value

This table reports the results for pooled panel regressions of Tobin's Q on poison pill law indicator variables over the sample period 1983 to 2012. The main variables of interest, *Q, Poison Pill Law, Poison Pill Law First Wave,* and *Poison Pill Law Second Wave* are measured contemporaneously, whereas the remaining controls are lagged one period. *Poison Pill Law First Wave* is a dummy variable equal to one if a firm is incorporated in a state that passes a poison pill law during the period 1986 to 1990, and zero otherwise. *Poison Pill Law Second Wave* is a dummy variable equal to one if a firm is incorporated in a state that passes a poison pill law during the period 1995 to 2009, and zero otherwise. Other control variables not reported due to economic and statistical insignificance: *HHI.* Further, columns (3), and (6) specify: *Business Combination Law, Control Share Law, Directors' Duties Law,* and *Fair Price Law* dummies. Table E1 provides variable definitions. All continuous variables are winsorized at the 1st and 99th percentiles and the dollar values are expressed in 2015 dollars. The estimated *t*-statistics are based on robust standard errors clustered by firm and are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Dep. Variable: $Q_{[t]}$						
	1983 – 2012					
Variables	(1)	(2)	(3)	(4)	(5)	(6)
$Poison\ Pill\ Law_{[t]}$	0.110***	0.123***	0.105**			
	(2.81)	(3.22)	(2.20)			
$Poison\ Pill\ Law\ First\ Wave_{[t]}$				0.025	0.026	-0.058
				(0.59)	(0.62)	(-1.20)
$Poison\ Pill\ Law\ Second\ Wave_{[t}$				0.218***	0.244***	0.202***
				(3.15)	(3.61)	(2.90)
$Poison\ Pill\ Firm\text{-}Level_{[t-1]}$	-0.217***	-0.102***	-0.103***	-0.219***	-0.104***	-0.105***
	(-7.40)	(-3.77)	(-3.81)	(-7.45)	(-3.83)	(-3.87)
$Ln(Assets)_{[t-1]}$		-0.421***	-0.421***		-0.421***	-0.421***
		(-14.22)	(-14.24)		(-14.25)	(-14.28)
$Ln(Age)_{[t-1]}$		-0.237***	-0.236***		-0.243***	-0.239***
		(-2.68)	(-2.67)		(-2.74)	(-2.70)
$Sales\ Growth_{[t-1]}$		0.339***	0.339***		0.340***	0.341***
		(8.61)	(8.62)		(8.65)	(8.66)
$Loss_{[t-1]}$		-0.077***	-0.076***		-0.077***	-0.076***
		(-4.36)	(-4.32)		(-4.37)	(-4.32)
$Debt\text{-}to\text{-}Equity_{[t-1]}$		-0.018***	-0.018***		-0.018***	-0.018***
		(-3.51)	(-3.49)		(-3.50)	(-3.49)
$Firm\ Liquidity_{[t-1]}$		0.281***	0.284***		0.283***	0.286***
		(2.89)	(2.93)		(2.91)	(2.95)
$CAPX/Assets_{[t-1]}$		0.576***	0.571***		0.573***	0.567***
		(2.95)	(2.93)		(2.95)	(2.92)
$R\&D/Sales_{[t-1]}$		0.183**	0.183**		0.185**	0.185**
		(1.98)	(1.98)		(2.00)	(2.00)
$Institutional\ Ownserhip_{[t-1]}$		0.217***	0.216***		0.216***	0.217***
		(3.49)	(3.49)		(3.50)	(3.51)
$State\text{-}Year\ Q_{[t-1]}$		0.161***	0.161***		0.159***	0.159***
		(4.50)	(4.51)		(4.46)	(4.45)
$Industry\text{-}Year\ Q_{[t-1]}$		0.154***	0.154***		0.154***	0.154***
		(8.90)	(8.93)		(8.88)	(8.89)
Other law controls	No	No	Yes	No	No	Yes
Firm and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
# of firms in regression	3,423	3,423	3,423	3,423	3,423	3,423
N	33,826	33,826	33,826	33,826	33,826	33,826
Adjusted R^2	0.566	0.602	0.602	0.566	0.602	0.602

Table F6: Poison Pill Laws, Firm-Level Pills and Firm Value

This table reports the results for pooled panel regressions of Tobin's Q on interactions of poison pill law indicator variables and firm-level poison pill indicator variables over the sample period 1983 to 2012. The main variables of interest, *Q, Poison Pill Law, Poison Pill Law First Wave,* and *Poison Pill Law Second Wave* are measured contemporaneously, whereas *Poison Pill Firm-Level* and the remaining controls are lagged one period. *Poison Pill Law First Wave* is a dummy variable equal to one if a firm is incorporated in a state that passes a poison pill law during the period 1986 to 1990, and zero otherwise. *Poison Pill Law Second Wave* is a dummy variable equal to one if a firm is incorporated in a state that passes a poison pill law during the period 1995 to 2009, and zero otherwise. The included controls are: *Ln(Assets), Ln(Age), HHI, Sales Growth, Loss, Debt-to-Equity, Firm Liquidity, CAPX/Assets, R&D/Sales, Institutional Ownership, State-year Q,* and *Industry-year Q*. Further, columns (2), and (4) specify: *Business Combination Law, Control Share Law, Directors' Duties Law,* and *Fair Price Law* dummies. Table E1 provides variable definitions. All continuous variables are winsorized at the 1st and 99th percentiles and the dollar values are expressed in 2015 dollars. The estimated *t*-statistics are based on robust standard errors clustered by firm and are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Dep. Variable: $Q_{[t]}$				
	1983 – 2012			
Variables	(1)	(2)	(3)	(4)
$Poison\ Pill\ Law_{[t]}$	0.115**	0.098*		
	(2.51)	(1.83)		
$Poison\ Pill\ Law_{[t]} \times Poison\ Pill\ Firm\text{-}Level_{[t-1]}$	0.018	0.018		
	(0.37)	(0.37)		
$Poison\ Pill\ Law\ First\ Wave_{[t]}$			0.003	-0.078
			(0.06)	(-1.49)
$Poison\ Pill\ Law\ First\ Wave_{[t]} \times$			0.050	0.047
$Poison\ Pill\ Firm\text{-}Level_{[t-1]}$			(0.97)	(0.91)
$Poison\ Pill\ Law\ Second\ Wave_{[t]}$			0.281***	0.241***
			(3.36)	(2.82)
$Poison\ Pill\ Law\ Second\ Wave_{[t]} \times$			-0.095	-0.096
$Poison\ Pill\ Firm\text{-}Level_{[t-1]}$			(-1.03)	(-1.04)
$Poison\ Pill\ Firm\text{-}Level_{[t-1]}$	-0.107***	-0.108***	-0.112***	-0.112***
	(-3.28)	(-3.31)	(-3.39)	(-3.40)
Control variables	Yes	Yes	Yes	Yes
Other law controls	No	Yes	No	Yes
Firm and year fixed effects	Yes	Yes	Yes	Yes
# of firms in regression	3,423	3,423	3,423	3,423
N	33,826	33,826	33,826	33,826
Adjusted R^2	0.602	0.602	0.602	0.602

Table F7: Matched Sample Summary Statistics

This table reports summary statistics for a propensity score matched sample. Treated firms are defined as companies incorporated in states that adopt poison pill laws, whereas the control firms are incorporated in states without poison pill laws in at least the five-year period following the passage of a law for its matched counterpart. We use nearest-neighbor matching with replacement in year *t-1* to create a sample matched on *Q* and *Ln(Assets)*, and exactly on 2-digit SIC industry codes and firm-level poison pill status for each of the thirty five treated states. Panel A presents the summary statistics for the year prior to treatment. The column "Difference (*t*-stat)" provides the difference between the treat and control sample mean and its test statistic in parentheses. The row "N (by group)" provides the number of unique firms for each treatment and control group. Panel B shows the summary statistics for the full matched panel (*t*-3) to (*t*+3). Table E1 provides variable definitions. All continuous variables are winsorized at the 1st and 99th percentiles and the dollar values are expressed in 2015 dollars. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Panel A: Pre-Treatment Year (*t*-1) Summary Statistics

	Full Sample			First Wave			Second Wave		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Matched Variables:	Treat	Control	Difference	Treat	Control	Difference	Treat	Control	Difference
$Q_{[t]}$	1.564	1.552	0.012	1.418	1.396	0.022	1.752	1.753	-0.001
	(0.992)	(0.924)	(0.20)	(0.555)	(0.468)	(0.52)	(1.343)	(1.269)	(-0.01)
$Poison\ Pill\ Firm\text{-}Level_{[t]}$	0.346	0.346	0.000	0.332	0.332	0.000	0.363	0.363	0.000
	(0.476)	(0.476)	(0.00)	(0.472)	(0.472)	(0.00)	(0.482)	(0.482)	(0.00)
$Ln(Assets)_{[t]}$	6.391	6.437	-0.046	7.075	6.941	0.133	5.505	5.784	0.279
	(1.898)	(1.766)	(-0.40)	(1.642)	(1.489)	(1.02)	(1.844)	(1.883)	(1.58)
Other Control Variables:									
$Ln(Age)_{[t]}$	2.954	2.936	0.018	3.105	3.066	0.039	2.760	2.767	-0.008
	(0.519)	(0.511)	(0.57)	(0.302)	(0.357)	(1.40)	(0.659)	(0.621)	(-0.13)
$HHI_{[t]}$	0.250	0.254	-0.003	0.261	0.269	-0.009	0.237	0.233	0.003
	(0.176)	(0.185)	(-0.30)	(0.161)	(0.169)	(-0.63)	(0.193)	(0.202)	(0.18)
$Sales\ Growth_{[t]}$	0.040	0.035	0.006	0.044	0.056	-0.012	0.035	0.007	0.029
	(0.236)	(0.279)	(0.35)	(0.212)	(0.238)	(-0.65)	(0.265)	(0.322)	(1.03)
$Loss_{[t]}$	0.213	0.260	-0.047*	0.128	0.159	-0.031	0.323	0.390	-0.067
	(0.410)	(0.439)	(-1.77)	(0.335)	(0.366)	(-1.07)	(0.469)	(0.489)	(-1.48)
$Debt\text{-}to\text{-}Equity_{[t]}$	0.493	0.507	-0.014	0.467	0.461	0.007	0.545	0.477	0.068
	(1.018)	(1.295)	(-0.19)	(1.005)	(0.976)	(0.08)	(1.164)	(1.486)	(0.54)
$Firm\ Liquidity_{[t]}$	0.269	0.264	0.005	0.271	0.249	0.022	0.266	0.284	-0.018
	(0.201)	(0.224)	(0.34)	(0.184)	(0.190)	(1.40)	(0.220)	(0.261)	(-0.78)
$CAPX/Assets_{[t]}$	0.067	0.062	0.005	0.068	0.066	0.002	0.066	0.057	0.009
	(0.056)	(0.051)	(1.49)	(0.051)	(0.048)	(0.53)	(0.063)	(0.055)	(1.56)
$R\&D/Sales_{[t]}$	0.036	0.030	-0.006	0.021	0.019	0.002	0.041	0.057	-0.016*
	(0.074)	(0.062)	(-1.43)	(0.034)	(0.034)	(0.58)	(0.085)	(0.102)	(-1.83)
$Institutional\ Ownership_{[t]}$	0.304	0.295	0.010	0.315	0.267	0.048**	0.291	0.330	-0.040
	(0.259)	(0.270)	(0.58)	(0.244)	(0.238)	(2.37)	(0.277)	(0.304)	(-1.44)
N (by group)	512	512		289	289		223	223	

Table F7 – *(Continued)*

Panel B: Matched Sample Summary Statistics (*t*-3) to (*t*+3)

	Full Sample		First Wave		Second Wave	
Dependent Variable:	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
$Q_{[t]}$	1.638	1.048	1.458	0.639	1.892	1.402
$Bid_{[t]}$	0.030	0.169	0.020	0.139	0.043	0.204
$Complete_{[t]}$	0.020	0.140	0.013	0.113	0.030	0.172
$ROA_{[t]}$	0.130	0.105	0.143	0.083	0.111	0.127
$NPM_{[t]}$	0.005	0.175	0.029	0.110	-0.028	0.234
$OM_{[t]}$	0.055	0.152	0.076	0.086	0.025	0.211
$Sales\ Growth_{[t]}$	0.032	0.237	0.022	0.198	0.046	0.282
Independent Variables:	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
$Treat_{[t]} \times Post_{[t]}$	0.187	0.390	0.185	0.388	0.190	0.392
$Poison\ Pill\ Firm\text{-}Level_{[t]}$	0.411	0.492	0.437	0.496	0.375	0.484
$Ln(Assets)_{[t]}$	6.551	1.808	7.058	1.559	5.834	1.890
$Ln(Age)_{[t]}$	3.018	0.476	3.123	0.321	2.869	0.604
$HHI_{[t]}$	0.261	0.186	0.273	0.171	0.245	0.204
$Loss_{[t]}$	0.223	0.416	0.168	0.374	0.302	0.459
$Debt\text{-}to\text{-}Equity_{[t]}$	0.507	1.184	0.551	1.181	0.445	1.185
$Firm\ Liquidity_{[t]}$	0.259	0.205	0.253	0.188	0.268	0.226
$CAPX/Assets_{[t]}$	0.066	0.056	0.067	0.048	0.065	0.066
$Institutional\ Ownserhip_{[t]}$	0.321	0.269	0.308	0.244	0.338	0.300
$State\text{-}Year\ Q_{[t]}$	1.896	0.420	1.795	0.322	2.039	0.494
$Industry\text{-}Year\ Q_{[t]}$	1.910	0.811	1.731	0.644	2.165	0.945
Interaction Variables:	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
$Incorp\ State\text{-}Year\ M\&A\ Volume_{[t]}$	0.019	0.031	0.018	0.021	0.022	0.041
$Industry\text{-}Year\ M\&A\ Volume_{[t]}$	0.037	0.053	0.035	0.050	0.039	0.057
$Large\ Customer_{[t]}$	0.451	0.498	0.395	0.489	0.530	0.499
$Strategic\ Alliance_{[t]}$	0.332	0.471	0.198	0.399	0.521	0.500
$Labor\ Capital_{[t]}$	0.308	0.213	0.290	0.187	0.334	0.243
$R\&D/Sales_{[t]}$	0.030	0.064	0.021	0.036	0.043	0.089
$Intangible\ Capital/Assets_{[t]}$	0.533	0.339	0.497	0.312	0.583	0.367
$Knowledge\ Capital/Assets_{[t]}$	0.123	0.213	0.104	0.157	0.150	0.271
Obs.	6,117		3,581		2,536	

Table F8: Poison Pill Laws and Firm Value in the Matched Sample

This table reports the results for matched sample regressions of Tobin's Q on a *Treat* × *Post* interaction term. *Treat* is an indicator variable equal to one if the firm is incorporated in a state that adopts a poison pill law. *Post* is an indicator variable equal to one in the year of and post treatment period, and zero otherwise. The main variables of interest Q, *Treat* × *Post*, and *Post* are measured contemporaneously, whereas the remaining controls are lagged one period. *Treat* is omitted in the regression because of collinearity with its firm fixed effect. Columns (1) – (2) regresses Tobin's Q on *Treat* × *Post* for the full sample period, columns (3) – (4) provides coefficient estimates for the "first wave", columns (5) – (6) shows the matched sample DID results for the "second wave" period, and columns (7) – (8) reports the DID estimates for the full sample period where *Treat* × *Post* is interacted with the *Poison Pill Law First Wave* dummy. *Poison Pill Law First Wave* is a dummy variable equal to one if a firm is incorporated in a state that passes a poison pill law during the period 1986 to 1990, and zero otherwise. Table E1 provides variable definitions. The included controls are: *Ln(Assets), Ln(Age), HHI, Sales Growth, Loss, Debt-to-Equity, Firm Liquidity, CAPX/Assets, R&D/Sales, Institutional Ownership, State-year Q,* and *Industry-year Q*. Further, columns (2), (4), and (6) specify: *Business Combination Law, Control Share Law, Directors' Duties Law,* and *Fair Price Law* dummies. All other interaction terms are unreported to conserve space. Continuous variables are winsorized at the 1st and 99th percentiles and the dollar values are expressed in 2015 dollars. The estimated *t*-statistics are based on robust standard errors clustered by firm and are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Dep. Variable: $Q_{[t]}$								
	(t-3) to (t+3)							
	Full Sample		First Wave (law adopted: 1986-1990)		Second Wave (law adopted: 1995-2009)		Full Sample with First Wave Dummy	
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Treat_{[t]} \times Post_{[t]}$	0.114**	0.103*	0.005	0.022	0.244**	0.228**	0.243***	0.227**
	(2.25)	(1.69)	(0.09)	(0.42)	(2.30)	(1.97)	(2.60)	(2.40)
$Treat_{[t]} \times Post_{[t]} \times$							-0.011	-0.008
$Poison\ Pill\ Law\ First\ Wave_{[t]}$							(-0.03)	(-0.02)
$Post_{[t]}$	0.007	0.009	0.013	0.002	-0.022	-0.022	0.001	0.006
	(0.18)	(0.21)	(0.39)	(0.05)	(-0.25)	(-0.24)	(0.03)	(0.14)
$Poison\ Pill\ Firm\text{-}Level_{[t-1]}$	0.012	0.012	0.011	0.011	0.014	0.015	0.006	0.006
	(0.28)	(0.27)	(0.29)	(0.28)	(0.11)	(0.12)	(0.14)	(0.14)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other law controls	No	Yes	No	Yes	No	Yes	No	Yes
Firm and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
# of firms in regression	873	873	504	504	401	401	873	873
N	6,117	6,117	3,581	3,581	2,536	2,536	6,117	6,117
Adjusted R^2	0.662	0.662	0.702	0.702	0.637	0.638	0.664	0.664

Table F9: Portfolio Analysis: Poison Pill Laws and Abnormal Returns in the Matched Sample

This table reports abnormal returns of value weighted monthly portfolios of firms that are incorporated in states that adopt poison pill statutes. We construct the portfolios using the treated and control firms from the propensity score matched sample around the passage of these laws. The long portfolios are composed in the following manner. For portfolios *6m24*, and *6m36* we include all stocks of matched firms that are incorporated in states starting 6 months before the fiscal year-end of the year in which the incorporating state adopts a poison pill law, and hold these stocks for 24 or 36 months. Similarly, the short portfolios are constructed by including all stocks of control firms that are matched to a treated company incorporated in states starting 6 months before the fiscal year-end of the year in which that treated incorporating state adopts a poison pill law, and short these control group stocks for 24 or 36 months. The long-short portfolios are then created by differencing the portfolio returns of the long and short portfolios, for each respective month. We use two models: the four-factor Carhart (1997) model (i.e., momentum, high minus low book-to-market (HML), small minus big (SMB), and market return), and the three-factor Fama-French model (i.e., HML, SMB, and market return). Further, we calculate the portfolio return with each stock weighted by its market capitalization immediately preceding its inclusion in the portfolio. The estimated *t*-statistics are based on robust standard errors and presented in parentheses below the coefficients. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively. The number of stocks in the long and short portfolios are averaged across all months and displayed in the "Average # firms" row. The "M" row shows the total number of monthly observations, and the "N" row shows the total number of firms with useable returns.

Panel A: Full Sample

	Four-factor model			Three-factor model		
Portfolio "6m24"						
	Long	Short	Long - Short	Long	Short	Long - Short
Alpha (monthly)	0.851**	0.041	0.704*	0.802**	0.005	0.688*
	(2.21)	(0.15)	(1.91)	(2.09)	(0.02)	(1.82)
Average # firms	70.69	71.60	-	70.69	71.60	-
M	253	248	248	253	248	248
N	490	487	-	490	487	-
Adjusted R^2	0.341	0.628	0.040	0.342	0.629	0.043

	Four-factor model			Three-factor model		
Portfolio "6m36"						
	Long	Short	Long - Short	Long	Short	Long - Short
Alpha (monthly)	0.734*	-0.113	0.743*	0.679*	-0.146	0.726*
	(1.76)	(-0.42)	(1.92)	(1.71)	(-0.56)	(1.85)
Average # firms	61.63	64.92	-	61.63	64.92	-
M	294	277	277	294	277	277
N	491	488	-	491	488	-
Adjusted R^2	0.324	0.612	0.017	0.325	0.612	0.020

Table F9 – (*Continued*)

Panel B: First Wave

	Four-factor model			Three-factor model		
Portfolio "6m24"						
	Long	Short	Long - Short	Long	Short	Long - Short
Alpha (monthly)	-0.097	0.030	-0.127	-0.153	0.005	-0.158
	(-0.52)	(0.12)	(-0.54)	(-0.81)	(0.02)	(-0.68)
Average # firms	128.25	128.80	-	128.25	128.80	-
M	81	81	81	81	81	81
N	279	273	-	279	273	-
Adjusted R^2	0.885	0.860	0.067	0.883	0.861	0.074

	Four-factor model			Three-factor model		
Portfolio "6m36"						
	Long	Short	Long - Short	Long	Short	Long - Short
Alpha (monthly)	0.186	-0.035	0.220	0.120	-0.139	0.260
	(0.86)	(-0.12)	(0.62)	(0.56)	(-0.49)	(0.71)
Average # firms	112.91	113.38	-	112.91	113.38	-
M	93	93	93	93	93	93
N	279	274	-	279	274	-
Adjusted R^2	0.822	0.761	-0.011	0.822	0.759	-0.001

Panel C: Second Wave

	Four-factor model			Three-factor model		
Portfolio "6m24"						
	Long	Short	Long - Short	Long	Short	Long - Short
Alpha (monthly)	1.273**	0.004	1.119**	1.221**	-0.037	1.104**
	(2.22)	(0.01)	(2.09)	(2.18)	(-0.10)	(2.01)
Average # firms	43.59	43.86	-	43.59	43.86	-
M	172	167	167	172	167	167
N	211	214	-	211	214	-
Adjusted R^2	0.249	0.549	0.034	0.251	0.550	0.040

	Four-factor model			Three-factor model		
Portfolio "6m36"						
	Long	Short	Long - Short	Long	Short	Long - Short
Alpha (monthly)	0.942	-0.162	0.966*	0.892	-0.183	0.945*
	(1.61)	(-0.43)	(1.74)	(1.58)	(-0.50)	(1.68)
Average # firms	37.90	40.43	-	37.90	40.43	-
M	201	184	184	201	184	184
N	212	214	-	212	214	-
Adjusted R^2	0.253	0.564	0.011	0.256	0.566	0.016

Table F10: Poison Pill Laws and M&A Activity

This table reports the results for matched sample regressions of *M&A Activity* on a *Treat* × *Post* interaction term. *M&A Activity* dependent variables include the following: *Bid* and *Complete*. *Bid* is an indicator variable equal to one if a firm receives a takeover bid as catalogued by the SDC M&A database and CRSP delisting codes (200s), and zero otherwise. *Complete* is an indicator variable equal to one if a firm is successfully acquired as catalogued by the SDC M&A database and CRSP delisting codes (200s), and zero otherwise. *Treat* is an indicator variable equal to one if the firm is incorporated in a state that adopts a poison pill law. *Post* is an indicator variable equal to one in the year of and post treatment period, and zero otherwise. The main variables of interest, Bid, $Complete$, *Treat* × *Post*, and *Post* are measured contemporaneously, and the controls are lagged one period. *Treat* is omitted in the regression because of collinearity with its firm fixed effect. Panel A is specific to the full matched sample. Panel B provides coefficient estimates for the "first wave", and Panel C shows the matched sample DID results for the "second wave" period. Table E1 provides variable definitions. The included controls are: *Ln(Assets), Ln(Age), HHI, Loss, Debt-to-Equity, Firm Liquidity, CAPX/Assets, R&D/Sales, Institutional Ownership, State-year Q, Industry-year Q,* and *Business Combination Law, Control Share Law, Directors' Duties Law,* and *Fair Price Law* dummies. All continuous variables are winsorized at the 1st and 99th percentiles and the dollar values are expressed in 2015 dollars. Industry fixed effects are defined at the three-digit SIC code level. The estimated *t*-statistics are based on robust standard errors clustered by firm and are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.[d]

Panel A: Full Sample

	Full Sample: (t-3) to (t+3)			
Dep. Variables:	$Bid_{[t]}$		$Complete_{[t]}$	
Variables	(1)	(2)	(3)	(4)
$Treat_{[t]} \times Post_{[t]}$	-0.007	-0.015	0.002	-0.001
	(-0.69)	(-1.32)	(0.23)	(-0.01)
$Post_{[t]}$	0.009	0.015	-0.004	-0.002
	(0.93)	(1.44)	(-0.53)	(-0.25)
$Poison\ Pill\ Firm\text{-}Level_{[t-1]}$	0.001	0.001	-0.004	-0.004
	(0.12)	(0.16)	(-0.79)	(-0.72)
Control variables	Yes	Yes	Yes	Yes
Other law controls	Yes	Yes	Yes	Yes
Industry fixed effects	No	Yes	No	Yes
Year fixed effects	Yes	Yes	Yes	Yes
# of firms in regression	873	873	873	873
N	6,117	6,117	6,117	6,117
Adjusted R^2	0.019	0.022	0.011	0.017

Table F10 – *(Continued)*

Panel B: First Wave (law adopted: 1986-1990)

	First Wave: (t-3) to (t+3)			
Dep. Variables:	$Bid_{[t]}$		$Complete_{[t]}$	
Variables	(1)	(2)	(3)	(4)
$Treat_{[t]} \times Post_{[t]}$	-0.010	-0.014	-0.007	-0.011
	(-0.89)	(-1.05)	(-0.68)	(-1.00)
$Post_{[t]}$	-0.004	-0.004	-0.010	-0.007
	(-0.30)	(-0.27)	(-1.16)	(-0.80)
$Poison\ Pill\ Firm\text{-}Level_{[t-1]}$	0.002	-0.003	0.001	-0.002
	(0.29)	(-0.35)	(0.22)	(-0.32)
Control variables	Yes	Yes	Yes	Yes
Other law controls	Yes	Yes	Yes	Yes
Industry fixed effects	No	Yes	No	Yes
Year fixed effects	Yes	Yes	Yes	Yes
# of firms in regression	504	504	504	504
N	3,581	3,581	3,581	3,581
Adjusted R^2	0.011	0.030	0.007	0.030

Panel C: Second Wave (law adopted: 1995-2009)

	Second Wave: (t-3) to (t+3)			
Dep. Variables:	$Bid_{[t]}$		$Complete_{[t]}$	
Variables:	(1)	(2)	(3)	(4)
$Treat_{[t]} \times Post_{[t]}$	-0.007	-0.016	0.006	0.007
	(-0.32)	(-0.70)	(0.43)	(0.40)
$Post_{[t]}$	0.018	0.029	0.002	0.009
	(1.10)	(1.59)	(0.13)	(0.68)
$Poison\ Pill\ Firm\text{-}Level_{[t-1]}$	-0.001	0.003	-0.010	-0.009
	(-0.16)	(0.30)	(-1.27)	(-0.96)
Control variables	Yes	Yes	Yes	Yes
Other law controls	Yes	Yes	Yes	Yes
Industry fixed effects	No	Yes	No	Yes
Year fixed effects	Yes	Yes	Yes	Yes
# of firms in regression	401	401	401	401
N	2,536	2,536	2,536	2,536
Adjusted R^2	0.015	0.030	0.005	0.021

Table F11: Poison Pill Laws, M&A Activity, and Firm Value

This table reports results for matched sample regressions analyzing the effect of poison pill statutes on target firm value. Panel A regresses Tobin's Q on a *Treat* × *Post* ×*M&A Activity* interaction term. *M&A Activity* interaction variables include the following: *Incorp State-Year M&A Volume* and *Industry-Year M&A Volume*. *Incorp State-Year M&A Volume* is measured as the ratio of completed M&A dollar volume to total market capitalization per state of incorporation. *Industry-Year M&A Volume* is defined as the ratio of completed M&A dollar volume to total market capitalization per Fama-French 49 industry grouping. *Treat* is an indicator variable equal to one if the firm is incorporated in a state that adopts a poison pill law. *Post* is an indicator variable equal to one in the year of and post treatment period, and zero otherwise. Panel B presents the estimates of *Takeover Premium* values on *Treat* × *Post*. We use three *Takeover Premium* dependent variables: *1-Day Premium, 1-Week Premium,* and *4-Week Premium*, all of which come from the SDC M&A database and measures the premium of the offer price to the target closing price 1-day, 1-week, or 4-weeks prior to the announcement date, respectively. *Treat* is omitted in the regression because of collinearity with its firm fixed effect. Table E1 provides variable definitions. Included controls are lagged one period: *Ln(Assets), Ln(Age), HHI, Loss, Debt-to-Equity, Firm Liquidity, CAPX/Assets, R&D/Sales, Institutional Ownership, State-year Q, Industry-year Q,* and *Business Combination Law, Control Share Law, Directors' Duties Law,* and *Fair Price Law* dummies. All other interaction terms from Panel A are unreported to conserve space. Continuous variables are winsorized at the 1st and 99th percentiles and the dollar values are expressed in 2015 dollars. Industry fixed effects are defined at the three-digit SIC code level. The estimated *t*-statistics are based on robust standard errors clustered by firm and are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively

Panel A: Poison Pill Laws, M&A Volume and Tobin's Q

Dep. Variable: $Q_{[t]}$	(*t*-3) to (*t*+3)					
	Full Sample		First Wave (law adopted: 1986-1990)		Second Wave (law adopted: 1995-2009)	
Variables	(1)	(2)	(3)	(4)	(5)	(6)
$Treat_{[t]} \times Post_{[t]} \times Incorp\ State\text{-}Year\ M\&A\ Volume_{[t]}$	-4.912		1.013		-4.452	
	(-1.32)		(0.33)		(-0.94)	
$Treat_{[t]} \times Post_{[t]} \times Industry\text{-}Year\ M\&A\ Volume_{[t]}$		-0.066		-0.901*		0.695
		(-0.11)		(-1.77)		(0.58)
$Treat_{[t]} \times Post_{[t]}$	0.119*	0.104	0.030	0.055	0.245*	0.202
	(1.67)	(1.61)	(0.40)	(0.91)	(1.69)	(1.60)
$Incorp\ State\text{-}Year\ M\&A\ Volume_{[t]}$	-0.487		-0.668		-0.269	
	(-0.38)		(-0.37)		(-0.16)	
$Industry\text{-}Year\ M\&A\ Volume_{[t]}$		-0.020		-0.724***		0.863
		(-0.06)		(-2.62)		(1.29)
$Post_{[t]}$	0.020	-0.002	-0.021	-0.019	-0.001	-0.015
	(0.34)	(-0.04)	(-0.35)	(-0.49)	(-0.01)	(-0.18)
$Poison\ Pill\ Firm\text{-}Level_{[t-1]}$	0.012	0.011	0.010	0.008	0.016	0.017
	(0.28)	(0.25)	(0.25)	(0.22)	(0.13)	(0.14)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Other law controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
# of firms in regression	873	873	504	504	401	401
N	6,117	6,117	3,581	3,581	2,536	2,536
Adjusted R^2	0.662	0.662	0.703	0.704	0.639	0.639

Table F11 – *(Continued)*

Panel B: Poison Pill Laws and Takeover Premiums

	Full sample: (t-3) to (t+3)					
Dep. Variable:	$1\text{-}Day\ Premium_{[t]}$		$1\text{-}Week\ Premium_{[t]}$		$4\text{-}Week\ Premium_{[t]}$	
Variables	(1)	(2)	(3)	(4)	(5)	(6)
$Treat_{[t]} \times Post_{[t]}$	-0.136	-0.282	-0.157	-0.205	-0.117	-0.381
	(-0.69)	(-0.99)	(-0.81)	(-0.73)	(-0.44)	(-0.95)
$Post_{[t]}$	-0.061	-0.011	0.033	0.092	-0.066	0.000
	(-0.43)	(-0.08)	(0.25)	(0.65)	(-0.38)	(0.00)
$Poison\ Pill\ Firm\text{-}Level_{[t-1]}$	0.236*	0.166	0.244**	0.190	0.128	0.001
	(1.90)	(1.22)	(1.99)	(1.43)	(0.83)	(0.00)
Dep. Variable average (standard deviation)	0.364 (0.299)		0.405 (0.313)		0.484 (0.380)	
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Other law controls	No	Yes	No	Yes	No	Yes
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
# of firms in regression	129	129	129	129	129	129
N	129	129	129	129	129	129
Adjusted R^2	0.093	0.123	0.182	0.231	-0.094	-0.069

Table F12: Poison Pill Laws and Operational Efficiency

This table reports the results for matched sample regressions of proxies for *Operational Efficiency* on a *Treat* × *Post* interaction term. *Operational Efficiency* proxies include the following: *ROA, NPM, OM,* and *SG*. *ROA* (return on assets) is measured as operating income before depreciation and amortization divided by total assets. *NPM* (net profit margin) is defined as net income scaled by sales. *OM* (operating margin) equals operating income after depreciation and amortization over sales. *SG* (sales growth) is measured as the difference between next period's sales and the current period's sales divided by this period's sales. *Treat* is an indicator variable equal to one if the firm is incorporated in a state that adopts a poison pill law. *Post* is an indicator variable equal to one in the year of and post treatment period, and zero otherwise. The main variables of interest, ROA, NPM, OM, and SG are led one year (t+1). *Treat* × *Post*, and *Post* are measured contemporaneously, and the controls are lagged one period. *Treat* is omitted in the regression because of collinearity with its firm fixed effect. Panel A is specific to the full matched sample. Panel B provides coefficient estimates for the "first wave", and Panel C shows the matched sample DID results for the "second wave" period. Table E1 provides variable definitions. The included controls are: *Ln(Assets), Ln(Age), HHI, Loss, Debt-to-Equity, Firm Liquidity, CAPX/Assets, R&D/Sales, Institutional Ownership, State-year Q, Industry-year Q*, and *Business Combination Law, Control Share Law, Directors' Duties Law,* and *Fair Price Law* dummies. All continuous variables are winsorized at the 1st and 99th percentiles and the dollar values are expressed in 2015 dollars. The estimated t-statistics are based on robust standard errors clustered by firm and are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Panel A: Full Sample

	Full Sample: (t-3) to (t+3)			
Dep. Variables:	$ROA_{[t+1]}$	$NPM_{[t+1]}$	$OM_{[t+1]}$	$SG_{[t+1]}$
Variables	(1)	(2)	(3)	(4)
$Treat_{[t]} \times Post_{[t]}$	0.009**	0.016***	0.008	0.024*
	(2.62)	(2.67)	(1.41)	(1.76)
$Post_{[t]}$	-0.003*	-0.005	-0.002	0.001
	(-1.82)	(-1.23)	(-1.04)	(0.15)
$Poison\ Pill\ Firm\text{-}Level_{[t-1]}$	-0.008*	-0.014	-0.013*	-0.012
	(-1.89)	(-1.58)	(-1.96)	(-0.83)
Control variables	Yes	Yes	Yes	Yes
Other law controls	Yes	Yes	Yes	Yes
Firm and year fixed effects	Yes	Yes	Yes	Yes
# of firms in regression	869	869	869	869
N	5,897	5,897	5,896	5,897
Adjusted R^2	0.723	0.610	0.779	0.251

Table F12 – *(Continued)*

Panel B: First Wave (law adopted: 1986-1990)

	First Wave: (t-3) to (t+3)			
Dep. Variables:	$ROA_{[t+1]}$	$NPM_{[t+1]}$	$OM_{[t+1]}$	$SG_{[t+1]}$
Variables	(1)	(2)	(3)	(4)
$Treat_{[t]} \times Post_{[t]}$	0.006	0.008	0.003	0.002
	(1.30)	(1.29)	(0.43)	(0.12)
$Post_{[t]}$	-0.004	-0.007	-0.007	-0.013
	(-1.17)	(-1.23)	(-1.54)	(-1.23)
$Poison\ Pill\ Firm\text{-}Level_{[t-1]}$	-0.003	0.002	-0.001	-0.015
	(-0.79)	(0.25)	(-0.33)	(-1.15)
Control variables	Yes	Yes	Yes	Yes
Other law controls	Yes	Yes	Yes	Yes
Firm and year fixed effects	Yes	Yes	Yes	Yes
# of firms in regression	504	504	504	504
N	3,502	3,502	3,502	3,502
Adjusted R^2	0.689	0.295	0.642	0.217

Panel C: Second Wave (law adopted: 1995-2009)

	Second Wave: (t-3) to (t+3)			
Dep. Variables:	$ROA_{[t+1]}$	$NPM_{[t+1]}$	$OM_{[t+1]}$	$SG_{[t+1]}$
Variables	(1)	(2)	(3)	(4)
$Treat_{[t]} \times Post_{[t]}$	0.010*	0.021**	0.011*	0.037**
	(1.84)	(2.45)	(1.82)	(2.23)
$Post_{[t]}$	-0.001	-0.003	0.003	0.019
	(-0.34)	(-0.26)	(0.76)	(1.23)
$Poison\ Pill\ Firm\text{-}Level_{[t-1]}$	-0.020*	-0.056**	-0.042**	-0.013
	(-1.81)	(-2.43)	(-2.05)	(-1.15)
Control variables	Yes	Yes	Yes	Yes
Other law controls	Yes	Yes	Yes	Yes
Firm and year fixed effects	Yes	Yes	Yes	Yes
# of firms in regression	397	397	397	397
N	2,395	2,395	2,394	2,395
Adjusted R^2	0.732	0.695	0.804	0.268

Table F13: Poison Pill Laws, Innovative Activity, and Firm Value

This table reports the results for matched sample regressions of Tobin's Q on a *Treat* × *Post* × *Innovative Activity* interaction term. *Treat* is an indicator variable equal to one if the firm is incorporated in a state that adopts a poison pill law. *Post* is an indicator variable equal to one in the year of and post treatment period, and zero otherwise. *Innovative Activity* measures include the following: *R&D/Sales, Intangible Capital/Assets,* and *Knowledge Capital/Assets*. The main variables of interest, *Q*, *Treat* × *Post*, *Treat* × *Post* × *Innovative Activity*, and *Post* are measured contemporaneously, whereas the remaining controls are lagged one period. *Treat* is omitted in the regression because of collinearity with its firm fixed effect. Panel A regresses Tobin's Q on *Treat* × *Post* and *Treat* × *Post*× *Innovative Activity* for the full sample. Panel B, columns (1) – (3), provides coefficient estimates for the "first wave", whereas columns (4) – (6) shows the matched sample DID results for the "second wave" period. The treatment window is plus or minus three years around the adoption year. Table E1 provides variable definitions. The included controls are: *Business Combination Law, Control Share Law, Directors' Duties Law, Fair Price Law, Ln(Assets), Ln(Age), HHI, Sales Growth, Loss, Debt-to-Equity, Firm Liquidity, CAPX/Assets, R&D/Sales, Institutional Ownership, State-year Q,* and *Industry-year Q*. All other interaction terms are unreported to conserve space. Continuous variables are winsorized at the 1st and 99th percentiles and the dollar values are expressed in 2015 dollars. The estimated *t*-statistics are based on robust standard errors clustered by firm and are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Panel A: Full Sample

Dep. Variable: $Q_{[t]}$			
	Full Sample: (t-3) to (t+3)		
Variables	(1)	(2)	(3)
$Treat_{[t]} \times Post_{[t]} \times \frac{R\&D}{Sales_{[t]}}$	3.012**		
	(2.52)		
$Treat_{[t]} \times Post_{[t]} \times \frac{Intangible\ Capital}{Assets}_{[t]}$		0.394**	
		(2.41)	
$Treat_{[t]} \times Post_{[t]} \times \frac{Knowledge\ Capital}{Assets}_{[t]}$			0.803**
			(2.47)
$\frac{R\&D}{Sales_{[t]}}$	2.073**		
	(1.20)		
$\frac{Intangible\ Capital}{Assets}_{[t]}$		-0.026	
		(-0.13)	
$\frac{Knowledge\ Capital}{Assets}_{[t]}$			0.602
			(1.54)
$Treat_{[t]} \times Post_{[t]}$	0.010	-0.144	-0.004
	(0.15)	(-1.52)	(-0.06)
$Post_{[t]}$	0.073	0.203***	0.100**
	(1.62)	(2.69)	(1.98)
$Poison\ Pill\ Firm\text{-}Level_{[t-1]}$	0.014	0.010	0.015
	(0.31)	(0.23)	(0.36)
Control Variables	Yes	Yes	Yes
Other Law Controls	Yes	Yes	Yes
Firm and year fixed effects	Yes	Yes	Yes
# of firms in regression	873	873	873
N	6,117	6,117	6,117
Adjusted R^2	0.664	0.666	0.666

Table F13 – (*Continued*)

Panel B: First and Second Waves

Dep. Variable: $Q_{[t]}$	(t-3) to (t+3)					
	First Wave			Second Wave		
Variables	(1)	(2)	(3)	(4)	(5)	(6)
$Treat_{[t]} \times Post_{[t]} \times \frac{R\&D}{Sales_{[t]}}$	3.336*			2.714*		
	(1.85)			(1.81)		
$Treat_{[t]} \times Post_{[t]} \times \frac{Intangible\ Capital}{Assets}_{[t]}$		0.143			0.522**	
		(1.04)			(2.13)	
$Treat_{[t]} \times Post_{[t]} \times \frac{Knowledge\ Capital}{Assets}_{[t]}$			0.753**			0.878**
			(2.23)			(2.33)
$\frac{R\&D}{Sales_{[t]}}$	0.530			2.631		
	(0.23)			(1.37)		
$\frac{Intangible\ Capital}{Assets}_{[t]}$		-0.238			0.110	
		(-1.28)			(0.36)	
$\frac{Knowledge\ Capital}{Assets}_{[t]}$			0.371			0.792*
			(0.85)			(1.79)
$Treat_{[t]} \times Post_{[t]}$	-0.053	-0.056	-0.059	0.113	-0.126	0.090
	(-0.87)	(-0.65)	(-0.97)	(0.91)	(-0.72)	(0.74)
$Post_{[t]}$	0.059	0.014	0.046	0.068	0.306**	0.115
	(1.29)	(0.28)	(1.08)	(0.71)	(2.07)	(1.16)
$Poison\ Pill\ Firm\text{-}Level_{[t-1]}$	0.016	0.013	0.013	0.010	0.014	0.001
	(0.41)	(0.34)	(0.34)	(0.05)	(0.13)	(0.01)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Other Law Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
# of firms in regression	504	504	504	401	401	401
N	3,581	3,581	3,581	2,536	2,536	2,536
Adjusted R^2	0.705	0.703	0.704	0.640	0.643	0.644

Table F14: Poison Pill Laws, Stakeholder Relationships, and Firm Value

This table reports the results for matched sample regressions of Tobin's Q on a *Treat* × *Post* × *Shareholder Relationship Proxy* interaction term. *Treat* is an indicator variable equal to one if the firm is incorporated in a state that adopts a poison pill law. *Post* is an indicator variable equal to one in the year of and post treatment period, and zero otherwise. *Shareholder Relationship* Proxies include the following: *Large Customer, Strategic Alliance,* and *Labor Capital*. The main variables of interest, Q, *Treat* × *Post*, *Treat* × *Post* × *Shareholder Commitment Proxy*, and *Post* are measured contemporaneously, whereas the remaining controls are lagged one period. *Treat* is omitted in the regression because of collinearity with its firm fixed effect. Panel A regresses Tobin's Q on *Treat* × *Post* and *Treat* × *Post* × *Stakeholder Relationship Proxy* for the full sample. Panel B, columns (1) – (3), provides coefficient estimates for the "first wave", whereas columns (4) – (6) shows the matched sample DID results for the "second wave" period. The treatment window is plus or minus three years around the adoption year. Table E1 provides variable definitions. The included controls are: *Business Combination Law, Control Share Law, Directors' Duties Law, Fair Price Law, Ln(Assets), Ln(Age), HHI, Sales Growth, Loss, Debt-to-Equity, Firm Liquidity, CAPX/Assets, R&D/Sales, Institutional Ownership, State-year Q,* and *Industry-year Q*. All other interaction terms are unreported to conserve space. Continuous variables are winsorized at the 1st and 99th percentiles and the dollar values are expressed in 2015 dollars. The estimated *t*-statistics are based on robust standard errors clustered by firm and are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Panel A: Full Sample

Dep. Variable: $Q_{[t]}$			
		(t-3) to (t+3)	
Variables	(1)	(2)	(3)
$Treat_{[t]} \times Post_{[t]} \times Large\ Customer_{[t]}$	0.104*		
	(1.77)		
$Treat_{[t]} \times Post_{[t]} \times Strategic\ Alliance_{[t]}$		0.130*	
		(1.66)	
$Treat_{[t]} \times Post_{[t]} \times Labor\ Capital_{[t]}$			0.635***
			(2.67)
$Large\ Customer_{[t]}$	0.010		
	(0.16)		
$Strategic\ Alliance_{[t]}$		0.001	
		(0.00)	
$Labor\ Capital_{[t]}$			0.166
			(0.43)
$Treat_{[t]} \times Post_{[t]}$	-0.011	-0.005	-0.139*
	(-0.26)	(-0.11)	(-1.65)
$Post_{[t]}$	0.031	0.032	0.071
	(0.94)	(0.96)	(1.12)
$Poison\ Pill\ Firm\text{-}Level_{[t-1]}$	0.003	0.007	0.017
	(0.10)	(0.21)	(0.42)
Control Variables	Yes	Yes	Yes
Other Law Controls	Yes	Yes	Yes
Firm and year fixed effects	Yes	Yes	Yes
# of firms in regression	873	873	839
N	6,117	6,117	5,813
Adjusted R^2	0.711	0.715	0.658

Table F14 – (*Continued*)

Panel B: First and Second Waves

Dep. Variable: $Q_{[t]}$						
	(t-3) to (t+3)					
	First Wave			Second Wave		
Variables	(1)	(2)	(3)	(4)	(5)	(6)
$Treat_{[t]} \times Post_{[t]} \times Large\ Customer_{[t]}$	0.078			0.134*		
	(1.06)			(1.67)		
$Treat_{[t]} \times Post_{[t]} \times Strategic\ Alliance_{[t]}$		-0.083			0.237**	
		(-0.61)			(1.99)	
$Treat_{[t]} \times Post_{[t]} \times Labor\ Capital_{[t]}$			0.316			1.009**
			(1.46)			(2.28)
$Large\ Customer_{[t]}$	-0.009			-0.007		
	(-0.21)			(-0.17)		
$Strategic\ Alliance_{[t]}$		-0.092			0.033	
		(-0.88)			(0.36)	
$Labor\ Capital_{[t]}$			0.294			0.261
			(0.69)			(0.40)
$Treat_{[t]} \times Post_{[t]}$	-0.011	0.033	-0.090	-0.005	-0.050	-0.170
	(-0.26)	(0.64)	(-1.18)	(-0.09)	(-0.54)	(-1.04)
$Post_{[t]}$	0.034	-0.002	-0.023	0.004	0.049	0.157
	(0.91)	(-0.06)	(-0.45)	(0.06)	(0.76)	(1.15)
$Poison\ Pill\ Firm\text{-}Level_{[t-1]}$	0.030	0.011	0.006	-0.057	-0.044	0.066
	(1.01)	(0.28)	(0.18)	(-1.09)	(-0.70)	(0.55)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Other Law Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
# of firms in regression	504	504	504	401	401	401
N	3,581	3,581	3,581	2,536	2,536	2,536
Adjusted R^2	0.739	0.702	0.726	0.691	0.699	0.629

Table F15: Poison Pill Laws, Wave Adjustments, and Firm Value

This table reports the results for regressions of Tobin's Q on poison pill law indicator variables for Compustat firms. The main variables of interest, Q, $Poison\ Pill\ Law$, $First\ Wave\ Poison\ Pill\ Law\ Adjusted$, $Second\ Wave\ Poison\ Pill\ Law\ Adjusted$, $Treat \times Post$, $Treat\ First\ Wave\ Adjusted \times Post$, and $Treat\ Second\ Wave\ Adjusted \times Post$ are measured contemporaneously, whereas the remaining controls are lagged one period. We adjust the waves to capture the uncertainty stemming from Delaware case law. In 1985, the *Moran* decision effectively validates the use of the pill. However, subsequent Delaware case law in 1988 in *Interco* creates uncertainty about the validity of the poison pill. We therefore adjust the first wave to span 1986 to 1988, and allow the second wave adjustment to range from 1989 to 2009. Panel A provides pooled panel and matched sample regression estimates for the wave adjusted poison pill law indicator variables, where Delaware firms are excluded in the first wave, and included as control firms in the second wave. Panel B shows the pooled panel and matched sample regression estimates for wave adjusted poison pill law indicator variables, excluding firms incorporated in Delaware entirely. Included control variables: *Ln(Assets), Ln(Age), HHI, Sales Growth, Loss, Debt-to-Equity, Firm Liquidity, CAPX/Assets, R&D/Sales, Institutional Ownership, State-year Q, Industry-year Q*, as well as *Business Combination Law, Control Share Law, Directors' Duties Law*, and *Fair Price Law* indicators. Table E1 provides variable definitions. All continuous variables are winsorized at the 1st and 99th percentiles a dollar values are expressed in 2015 dollars. The estimated *t*-statistics are based on robust standard errors clustered by firm and are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Panel A: Poison Pill Laws Adjusted by Wave

Dep. Variable: $Q_{[t]}$					
	Pooled Panel: 1983 to 2012		Matched Sample: (*t*-3) to (*t*+3)		
Variables	(1)	(2)	(4)	(5)	(6)
$Poison\ Pill\ Law_{[t]}$	0.125**				
	(2.54)				
$Poison\ Pill\ Law\ First\ Wave\ Adjusted_{[t]}$		-0.025			
		(-0.43)			
$Poison\ Pill\ Law\ Second\ Wave\ Adjusted_{[t]}$		0.155***			
		(2.76)			
$Treat_{[t]} \times Post_{[t]}$			0.098**		
			(2.23)		
$Treat\ First\ Wave\ Adjusted_{[t]} \times Post_{[t]}$				-0.017	
				(-0.42)	
$Treat\ Second\ Wave\ Adjusted_{[t]} \times Post_{[t]}$					0.171**
					(2.15)
$Poison\ Pill\ Firm\text{-}Level_{[t-1]}$	-0.103***	-0.104***	-0.030	0.004	-0.039
	(-3.61)	(-3.64)	(-0.62)	(0.08)	(-0.55)
Control variables	Yes	Yes	Yes	Yes	Yes
Other law controls	Yes	Yes	Yes	Yes	Yes
Firm and year fixed effects	Yes	Yes	Yes	Yes	Yes
# of firms in regression	3,319	3,319	808	298	586
N	31,526	31,526	6,089	2,240	3,849
Adjusted R^2	0.603	0.603	0.663	0.707	0.645

Table F15 – (*Continued*)

Panel B: Poison Pill Laws Adjusted by Wave and Excluding Delaware Firms

Dep. Variable: $Q_{[t]}$					
	Pooled Panel: 1983 to 2012		Matched Sample: (t-3) to (t+3)		
Variables	(1)	(2)	(4)	(5)	(6)
$Poison\ Pill\ Law_{[t]}$	0.090*				
	(1.68)				
$Poison\ Pill\ Law\ First\ Wave\ Adjusted_{[t]}$		-0.056			
		(-0.87)			
$Poison\ Pill\ Law\ Second\ Wave\ Adjusted_{[t]}$		0.113*			
		(1.89)			
$Treat_{[t]} \times Post_{[t]}$			0.117*		
			(1.82)		
$Treat\ First\ Wave\ Adjusted_{[t]} \times Post_{[t]}$				-0.030	
				(-0.54)	
$Treat\ Second\ Wave\ Adjusted_{[t]} \times Post_{[t]}$					0.214**
					(2.36)
$Poison\ Pill\ Firm\text{-}Level_{[t-1]}$	-0.129***	-0.131***	0.004	0.033	-0.048
	(-3.34)	(-3.38)	(0.07)	(0.68)	(-0.56)
Control variables	Yes	Yes	Yes	Yes	Yes
Other law controls	Yes	Yes	Yes	Yes	Yes
Firm and year fixed effects	Yes	Yes	Yes	Yes	Yes
# of firms in regression	1,659	1,659	666	240	472
N	16,025	16,025	5,705	1,935	3,770
Adjusted R^2	0.605	0.605	0.655	0.744	0.642

Table F16: PPV-Index and Firm Value

This table describes the construction of the poison pill validity index (PPV-Index) and reports the results for pooled panel regressions of Tobin's Q on the PPV-Index over the sample period 1983 to 2012. We create the PPV-Index using poison pill statute and poison pill case information provided by Cain, McKeon, and Solomon (2017). The aim of this measure is to capture the relative change or strength in the validity of the right to adopt a poison pill or its effectiveness as a takeover defense over time and by state of incorporation. Panel A provides a description of the PPV-index. Panel B then tests the effect of the PPV-Index on firm value. The main variables of interest, *Q,* and *PPV-Index* are measured contemporaneously, whereas the remaining controls are lagged one period. All four columns include the following control variables: *Ln(Assets), Ln(Age), HHI, Sales Growth, Loss, Debt-to-Equity, Firm Liquidity, CAPX/Assets, R&D/Sales, Institutional Ownership, State-year Q,* and *Industry-year Q*. Column's (2) and (4) further specify: *Business Combination Law, Control Share Law, Directors' Duties Law,* and *Fair Price Law* indicators. Additionally, the odd-numbered columns include Arizona firms in the regression analysis, while the odd-numbered versions exclude them entirely. We consider our results with and without Arizona corporations since the language in the statute is ambiguous. Table E1 provides variable definitions. All continuous variables are winsorized at the 1st and 99th percentiles and the dollar values are expressed in 2015 dollars. The estimated *t*-statistics are based on robust standard errors clustered by firm and are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Panel A: PPV-Index Description

Poison Pill Validity Event	Code	Explanation
Moran v. Household (Delaware case)	= 0.5 or 1	If a firm is incorporated in Delaware after the Moran decision, we adjust the index to equal "1". Moreover, since Delaware court decisions are often applied *de facto* to even non-Delaware incorporated firms we increment the index up to equal "0.5" for all corporations outside Delaware and without a poison pill statute or a poison pill court case.
Georgia-Pacific v. Great Northern (Maine case)	= 1	If a firm is incorporated in Maine after the Georgia-Pacific decision, but before the state adopts a poison pill statute, we adjust the index to equal "1". Moreover, since this is the last court case that challenges the validity of the poison pill, we increment the index up by "0.5" to equal "1" for all corporations incorporated in a state without a poison pill statute or without a poison pill case.
State specific court cases (11 cases excluding *Moran* and *Georgia-Pacific*)	= 0 or 1	If a state has a court case, before or after *Moran* or *Georgia-Pacific*, that invalidates the poison pill, and does not have a poison pill statute, we adjust the index to equal "0". In contrast, if a state has a court case which validates a poison pill, but does not have a poison pill statute we increment the index value to equal "1".
State statutes (35 statutes)	= 2	If a state adopts a poison pill statute, we increment the index to equal "2".
State cases or statutes validating strong pills (3 cases and 2 statutes)	= 3	If a state has a court case or adopts a poison pill statute that allows for strong poison pills, we adjust the index value to equal "3".
Total	= 0 - 3	We then divide the index value by "3", which is the maximum possible points, to scale the measure between 0 and 1. This measure captures the change or relative strength of poison pill validity over time by state of incorporation.

Table F16 – (*Continued)*

Panel B: Pooled Panel Regressions

Dep. Variable: $Q_{[t]}$				
	1983 – 2012			
Variables	(1)	(2)	(3)	(4)
$PPV\text{-}Index_{[t]}$	0.201***	0.133*	0.201***	0.132*
	(2.95)	(1.72)	(2.95)	(1.70)
$Poison\ Pill\ Firm\text{-}Level_{[t-1]}$	-0.102***	-0.103***	-0.102***	-0.103***
	(-3.76)	(-3.80)	(-3.78)	(-3.81)
Arizona firms	Included	Included	Excluded	Excluded
Control variables	Yes	Yes	Yes	Yes
Other law controls	No	Yes	No	Yes
Firm and year fixed effects	Yes	Yes	Yes	Yes
# of firms in regression	3,423	3,423	3,407	3,407
N	33,826	33,826	33,074	33,074
Adjusted R^2	0.601	0.602	0.602	0.602

Table F17: Poison Pill Laws and Firm Value with Higher Dimensional Fixed Effects

This table reports the results for higher dimensional fixed effects pooled panel regressions of Tobin's Q on poison pill law indicator variables over the sample period 1983 to 2012. The main variables of interest, *Q, Poison Pill Law, Poison Pill Law First Wave,* and *Poison Pill Law Second Wave* are measured contemporaneously, whereas the remaining controls are lagged one period. *Poison Pill Law First Wave* is a dummy variable equal to one if a firm is incorporated in a state that passes a poison pill law during the period 1986 to 1990, and zero otherwise. *Poison Pill Law Second Wave* is a dummy variable equal to one if a firm is incorporated in a state that passes a poison pill law during the period 1995 to 2009, and zero otherwise. Industry fixed effects are defined at the three-digit SIC code level (following Catan, 2017, and Karpoff and Wittry, 2018). Columns (2) – (3), and (5) and (6) include control variables: *Ln(Assets), Ln(Age), HHI, Sales Growth, Loss, Debt-to-Equity, Firm Liquidity, CAPX/Assets, R&D/Sales, Institutional Ownership, State-year Q, Industry-year Q*. Further, columns (3) and (6) append controls for: *Business Combination Law, Control Share Law, Directors' Duties Law,* and *Fair Price Law*. Table E1 provides variable definitions. All continuous variables are winsorized at the 1st and 99th percentiles and the dollar values are expressed in 2015 dollars. The estimated t-statistics are based on robust standard errors clustered by firm and are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Dep. Variable: $Q_{[t]}$

	1983 – 2012					
Variables	(1)	(2)	(3)	(4)	(5)	(6)
$Poison\ Pill\ Law_{[t]}$	0.123***	0.115***	0.120**			
	(2.86)	(2.77)	(2.22)			
$Poison\ Pill\ Law\ First\ Wave_{[t]}$				-0.003	-0.016	-0.074
				(-0.06)	(-0.36)	(-1.42)
$Poison\ Pill\ Law\ Second\ Wave_{[t}$				0.291***	0.285***	0.257***
				(3.55)	(3.64)	(3.17)
$Poison\ Pill\ Firm\text{-}Level_{[t-1]}$	-0.201***	-0.106***	-0.106***	-0.204***	-0.108***	-0.108***
	(-6.60)	(-3.75)	(-3.76)	(-6.66)	(-3.80)	(-3.81)
Control variables	No	Yes	Yes	No	Yes	Yes
Other law controls	No	No	Yes	No	No	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry×Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
# of firms in regression	3,035	3,035	3,035	3,035	3,035	3,035
N	33,826	33,826	33,826	33,826	33,826	33,826
Adjusted R^2	0.588	0.620	0.620	0.589	0.620	0.620

Table F18: Poison Pill Laws and Firm Value without same year, Multi-Law Adopters

This table reports the results for regressions of Tobin's Q on a poison pill law indicator variable, where firms incorporated in states that adopt a poison pill statute and either a business combination, control share, or fair price law in the same year are excluded from the analysis. The main variables of interest, Q, $Poison\ Pill\ Law$, and $Treat \times Post$ are measured contemporaneously, whereas the remaining controls are lagged one period. Columns (1) and (2) provides pooled panel regression estimates over the full sample period, 1983 to 2012. Columns (3) and (4) shows the matched sample regression estimates for the full sample. Further, the odd-numbered columns include Delaware firms as controls, where the even-numbered versions exclude these firms. Included control variables: *Ln(Assets), Ln(Age), HHI, Sales Growth, Loss, Debt-to-Equity, Firm Liquidity, CAPX/Assets, R&D/Sales, Institutional Ownership, State-year Q, Industry-year Q*, as well as *Business Combination Law, Control Share Law, Directors' Duties Law,* and *Fair Price Law* indicators. Table E1 provides variable definitions. All continuous variables are winsorized at the 1st and 99th percentiles a dollar values are expressed in 2015 dollars. The estimated *t*-statistics are based on robust standard errors clustered by firm and are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Dep. Variable: $Q_{[t]}$				
	Pooled Panel: 1983 to 2012		Matched Sample: (*t*-3) to (*t*+3)	
Variables	(1)	(2)	(3)	(4)
$Poison\ Pill\ Law_{[t]}$	0.114**	0.086*		
	(2.05)	(1.70)		
$Treat_{[t]} \times Post_{[t]}$			0.117*	0.141**
			(1.72)	(2.02)
$Poison\ Pill\ Firm\text{-}Level_{[t-1]}$	-0.107***	-0.146***	0.010	0.003
	(-3.65)	(-3.13)	(0.20)	(0.04)
Delaware firms	Control	Excluded	Control	Excluded
Control variables	Yes	Yes	Yes	Yes
Other Law Controls	Yes	Yes	Yes	Yes
Firm and year fixed effects	Yes	Yes	Yes	Yes
# of firms in regression	3,175	1,385	771	571
N	30,633	12,832	5,485	5,125
Adjusted R^2	0.603	0.609	0.659	0.653

Table F19: Poison Pill Laws and the Timing of Firm Value Implications

This table reports the results for pooled panel regressions of Tobin's Q on poison pill law indicator variables for Compustat firms over the period 1983 to 2012. *Poison Pill Law*$^{[-1]}$ is an indicator variable equal to one if a firm is incorporated in a state that will adopt a poison pill law in one year and equal to zero otherwise. *Poison Pill Law*$^{[0]}$ is an indicator variable equal to one if a firm is incorporated in a state that adopts a poison pill law in the current year and equal to zero otherwise. *Poison Pill Law*$^{[1+]}$ is an indicator variable equal to one if a firm is incorporated in a state that adopted a poison pill law one or more years ago and equal to zero otherwise. *Poison Pill Law First Wave*$^{[t]}$ and *Poison Pill Law Second Wave*$^{[t]}$ dynamics are defined in a similar manner. All control variables are lagged one-period and those included in columns (2) – (3), and (5) – (6) are: *Ln(Assets), Ln(Age), HHI, Sales Growth, Loss, Debt-to-Equity, Firm Liquidity, CAPX/Assets, R&D/Sales, Institutional Ownership, State-year Q,* and *Industry-year Q*. Further, columns (3) and (6) specify: *Business Combination Law, Control Share Law, Directors' Duties Law,* and *Fair Price Law* dummies. Table E1 provides variable definitions. All continuous variables are winsorized at the 1st and 99th percentiles and the dollar values are expressed in 2015 dollars. The estimated *t*-statistics are based on robust standard errors clustered by firm and are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Dep. Variable: $Q_{[t]}$

	1983 – 2012					
Variables	(1)	(2)	(3)	(4)	(5)	(6)
$Poison\ Pill\ Law_{[t]}^{[-1]}$	0.019	0.017	0.029			
	(0.52)	(0.48)	(0.79)			
$Poison\ Pill\ Law_{[t]}^{[0]}$	0.063	0.060	0.068			
	(1.26)	(1.23)	(1.27)			
$Poison\ Pill\ Law_{[t]}^{[1+]}$	0.120***	0.134***	0.124**			
	(2.67)	(3.07)	(2.08)			
$Poison\ Pill\ Law\ First\ Wave_{[t]}^{[-1]}$				0.003	-0.007	-0.013
				(0.10)	(-0.24)	(-0.42)
$Poison\ Pill\ Law\ First\ Wave_{[t]}^{[0]}$				0.049	0.041	0.007
				(1.25)	(1.04)	(0.15)
$Poison\ Pill\ Law\ First\ Wave_{[t]}^{[1+]}$				0.023	0.024	-0.068
				(0.49)	(0.51)	(-1.14)
$Poison\ Pill\ Law\ Second\ Wave_{[t]}^{[-1]}$				0.036	0.044	0.045
				(0.49)	(0.63)	(0.64)
$Poison\ Pill\ Law\ Second\ Wave_{[t]}^{[0]}$				0.076	0.076	0.071
				(0.80)	(0.84)	(0.78)
$Poison\ Pill\ Law\ Second\ Wave_{[t]}^{[1+]}$				0.252***	0.285***	0.236***
				(3.20)	(3.71)	(2.91)
$Poison\ Pill\ Firm\text{-}Level_{[t-1]}$	-0.217***	-0.102***	-0.103***	-0.220***	-0.104***	-0.105***
	(-7.40)	(-3.78)	(-3.81)	(-7.46)	(-3.85)	(-3.88)
Control variables	No	Yes	Yes	No	Yes	Yes
Other law controls	No	No	Yes	No	No	Yes
Firm and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
# of firms in regression	3,423	3,423	3,423	3,423	3,423	3,423
N	33,826	33,826	33,826	33,826	33,826	33,826
Adjusted R^2	0.566	0.602	0.602	0.566	0.602	0.602

Table F20: Poison Pill Laws, Staggered Boards, and Firm Value

This table reports the results for pooled panel regressions of Tobin's Q on poison pill law and staggered board indicator and interaction variables over the period 1983 to 2012. The main variables of interest, Q, and $Poison\ Pill\ Law$, are measured contemporaneously, whereas $Staggered\ Board$, $Poison\ Pill\ Firm\text{-}Level$, and the remaining controls, are lagged one period. We also interact $Poison\ Pill\ Law \times Staggered\ Board$ and $Poison\ Pill\ Firm\text{-}Level \times Staggered\ Board$ in the last two columns. Each of the four columns include the following control variables: *Ln(Assets), Ln(Age), HHI, Sales Growth, Loss, Debt-to-Equity, Firm Liquidity, CAPX/Assets, R&D/Sales, Institutional Ownership, State-year Q,* and *Industry-year Q*. The even-numbered columns further specify *Business Combination Law, Control Share Law, Directors' Duties Law,* and *Fair Price Law* indicators. Table E1 provides variable definitions. All continuous variables are winsorized at the 1st and 99th percentiles a dollar values are expressed in 2015 dollars. The estimated *t*-statistics are based on robust standard errors clustered by firm and are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Dep. Variable: $Q_{[t]}$				
	1983 to 2012			
Variables	(1)	(2)	(3)	(4)
$Poison\ Pill\ Law_{[t]}$	0.119***	0.104**	0.123***	0.106**
	(3.15)	(2.18)	(2.81)	(2.03)
$Poison\ Pill\ Firm\text{-}Level_{[t-1]}$	-0.110***	-0.111***	-0.141***	-0.141***
	(-4.07)	(-4.10)	(-3.81)	(-3.81)
$Staggered\ Board_{[t]}$	0.111***	0.111***	0.089**	0.088**
	(3.24)	(3.22)	(2.25)	(2.23)
$Poison\ Pill\ Law_{[t]} \times Staggered\ Board_{[t]}$			-0.012	-0.009
			(-0.21)	(-0.16)
$Poison\ Pill\ Firm\text{-}Level_{[t-1]} \times Staggered\ Board_{[t]}$			0.065	0.064
			(1.55)	(1.53)
Control variables	Yes	Yes	Yes	Yes
Other Law Controls	No	Yes	No	Yes
Firm and year fixed effects	Yes	Yes	Yes	Yes
# of firms in regression	3,423	3,423	3,423	3,423
N	33,826	33,826	33,826	33,826
Adjusted R^2	0.602	0.602	0.602	0.602

Figure F1: States With a Poison Pill Statute

The chart below shows the states that have adopted a poison pill statute. States colored with red indicates passage of a law during the "first wave" period in our sample, 1986 to 1990. Green colored states denotes the legalization of pills from 1995 to 2009, which we label the "second wave." The grey colored states are without such legislation. *Created with*: https://mapchart.net/.

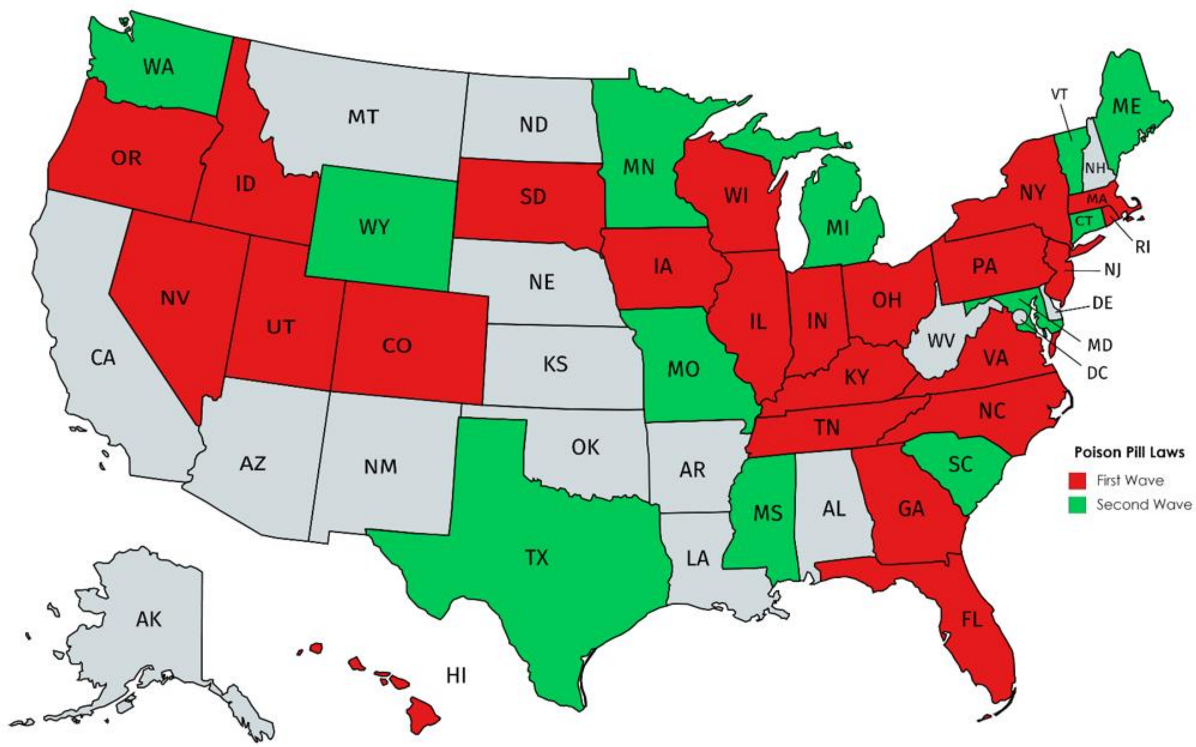


Figure F2: Percentage of Firms With a Poison Pill

The chart below shows the percentage of firms with a poison pill in our sample, each year from 1983 to 2015. Further, we partition the sample into the percentage of firms with a poison pill incorporated at any time in a first wave poison pill law adopting state (defined as 1986 to 1990), and those at any time from states passing the legislation duing the second wave period (defined as 1995 to 2009). Excluded from the sample are financial and utility firms.

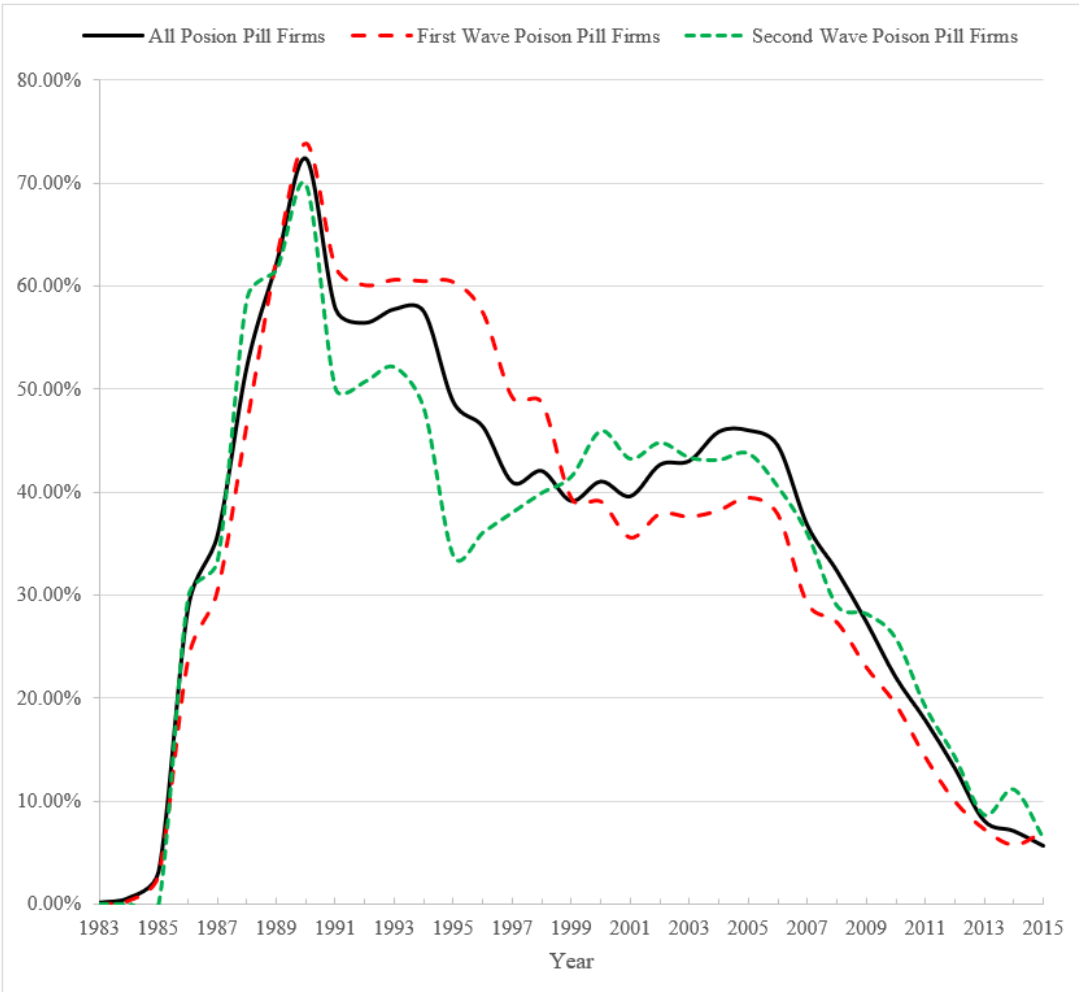
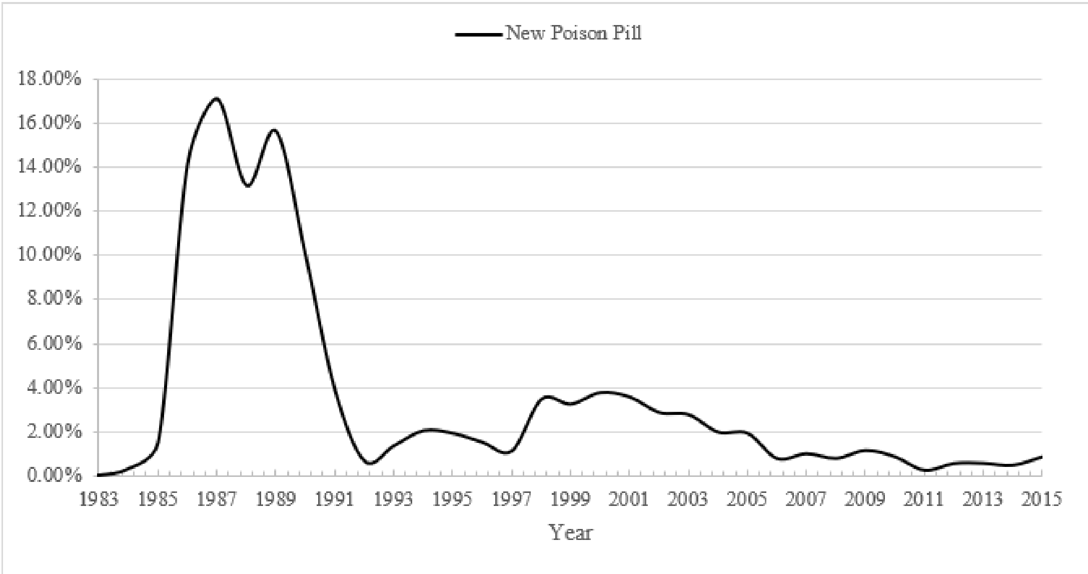


Figure F3: Percentage of Firms Adopting a New or Dropping an Existing Poison Pill

Panel A of the chart below shows the percentage of firms adopting a new poison pill in our sample, each year from 1983 to 2015. Panel B of the figure below depicts the percentage of firms dropping an existing poison pill in our dataset, each year between 1983 and 2015. We graph the two-year percentage averages to smooth the plot lines. Excluded from the sample are financial and utility firms.

Panel A: Adopting a New Poison Pill



Panel B: Dropping an Existing Poison Pill

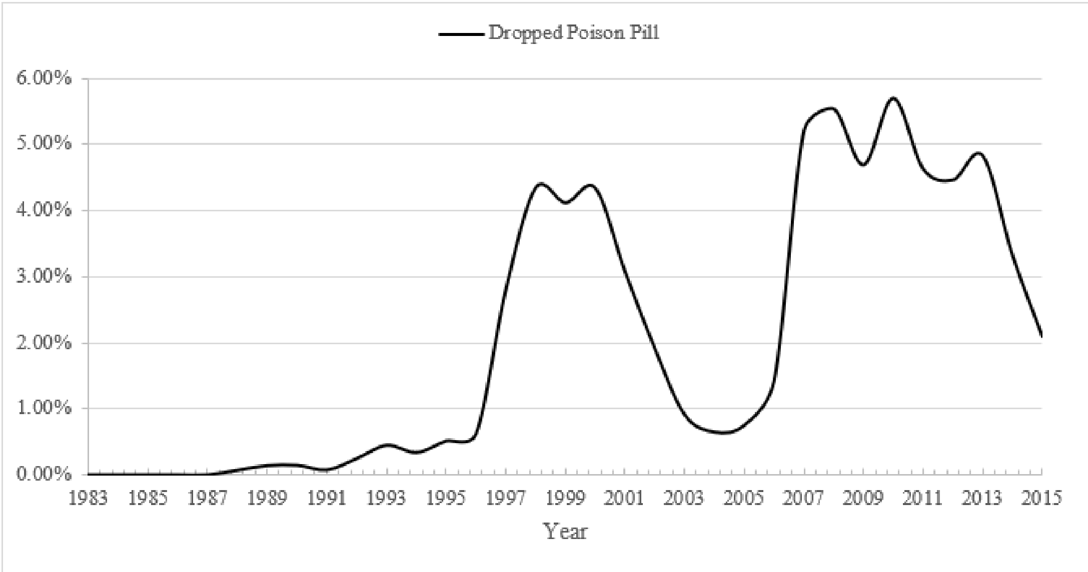


Figure F4: Percentage of Firms Affected by Poison Pill Laws

The chart below shows the percentage of firms incorporated in a poison pill law adopting state in our sample, each year from 1983 to 2015. Excluded from the sample are financial and utility firms.

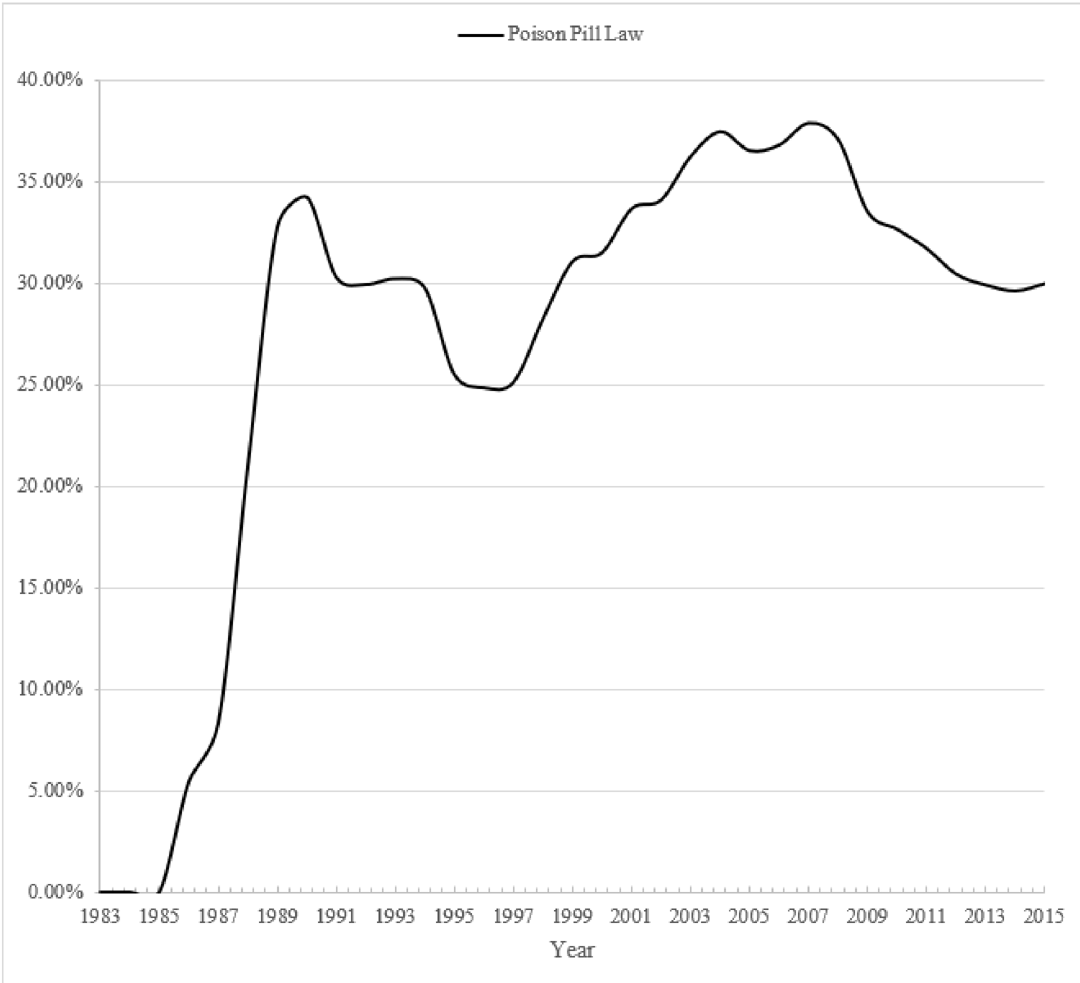
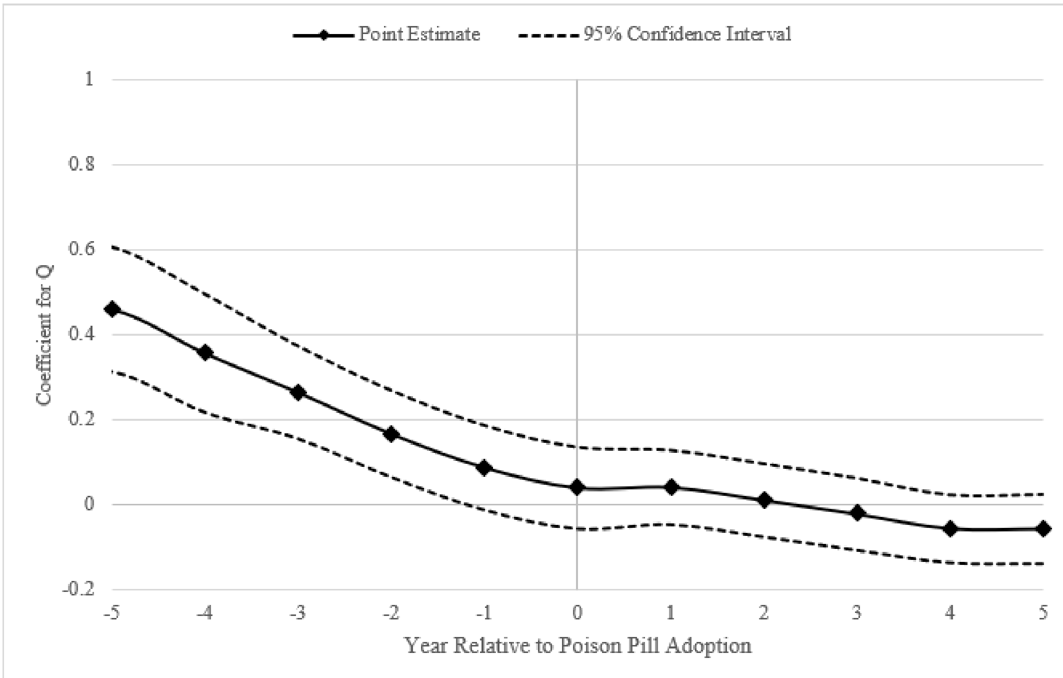


Figure F5: Tobin's Q and Poison Pill Adoption

This figure shows the association between poison pill adoption and Q. On the y-axis, the graph plots the coefficient estimates from regressing Q on year fixed effects, industry-year fixed effects, and dummy variables indicating the year relative to the adoption of a poison pill (following Catan, 2017). We create dummies for up to 10 years before and after poison pill adoption. The x-axis shows the time relative to the adoption of a poison pill. The dashed lines correspond to the 95% confidence intervals of the coefficient estimates. Confidence intervals are calculated from standard errors clustered by firm. The sample period is from 1983 to 2012 and consists of 33,826 firm-year observations. Industry dummies are defined at the three-digit SIC code level.



Appendix G: Chapter 1 and 3 Supplementary Tables

Table G1. Explaining the Adoption of All Item APM Statutes

This table reports results for regressions of *All Item APM Law* on state-year average firm characteristics and state-level macro and legal factors over the period 1975 to 1992. Panel A, columns (1) – (2) presents estimates from the "Manufacturing" sample, while columns (3) – (4) are specific to the "Products" dataset. Table B2 describes the "Products" samples. I drop all firms located in an all item APM law passing state after its adoption, and boat hull APM law firms are always excluded. All continuous variables have been winsorized at the 1st and 99th percentiles and standardized to have zero mean and unit variance. The predictor variables are lagged one period. Table A1 provides variable definitions. The estimated *t*-statistics are based on robust standard errors independently double clustered by state of location and year, and are reported in parentheses. The dollar values are expressed in 2015 dollars. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Dep. Variable: $All\ Item\ APM\ Law_{[t]}$	1975 – 1992			
Sample:	"Manufacturing"		"Products"	
	(1)	(2)	(3)	(4)
$SY\ Tobin's\ Q_{[t-1]}$	0.000	0.002	0.000	0.002
	(-0.01)	(0.12)	(-0.01)	(0.12)
$SY\ \Delta\ Tobin's\ Q_{[t-1]}$	0.006	0.004	0.006	0.004
	(0.59)	(0.37)	(0.59)	(0.37)
$SY\ Industry\text{-}Year\ Tobin's\ Q_{[t-1]}$	-0.002	0.006	-0.002	0.006
	(-0.12)	(0.30)	(-0.12)	(0.30)
$SY\ Size_{[t-1]}$	0.043	0.043	0.043	0.043
	(1.08)	(1.13)	(1.08)	(1.13)
$SY\ Ln(Age)_{[t-1]}$	-0.036	-0.035	-0.036	-0.035
	(-0.90)	(-0.93)	(-0.90)	(-0.93)
$SY\ Debt\text{-}to\text{-}Equity_{[t-1]}$	0.004	0.002	0.004	0.002
	(0.74)	(0.42)	(0.74)	(0.42)
$SY\ ROA_{[t-1]}$	-0.051	-0.059	-0.051	-0.059
	(-1.23)	(-1.25)	(-1.23)	(-1.25)
$SY\ Operating\ Cash\text{-}Flow_{[t-1]}$	0.004	0.003	0.004	0.003
	(0.56)	(0.42)	(0.56)	(0.42)
$SY\ HHI_{[t-1]}$	0.004	0.002	0.004	0.002
	(0.35)	(0.14)	(0.35)	(0.14)
$SY\ Sales\ Growth_{[t-1]}$	-0.004	-0.003	-0.004	-0.003
	(-0.65)	(-0.39)	(-0.65)	(-0.39)
$SY\ Loss_{[t-1]}$	-0.192	-0.191	-0.192	-0.191
	(-1.04)	(-1.03)	(-1.04)	(-1.03)
$SY\ Firm\ Liquidity_{[t-1]}$	0.025	0.025	0.025	0.025
	(1.07)	(1.15)	(1.07)	(1.15)
$SY\ R\&D/Sales_{[t-1]}$	-0.015	-0.012	-0.015	-0.012
	(-1.13)	(-1.03)	(-1.13)	(-1.03)
$SY\ CAPX/Assets_{[t-1]}$	-0.001	0.002	-0.001	0.002
	(-0.09)	(0.21)	(-0.09)	(0.21)
$Ln(State\ GDPPC)_{[t-1]}$		-0.026		-0.026
		(-1.14)		(-1.14)
$State\ GDPG_{[t-1]}$		0.001		0.001
		(0.09)		(0.09)
$UTSA\ Index_{[t-1]}$		0.010		0.010
		(0.84)		(0.84)
$IDD_{[t-1]}$		-0.003		-0.003
		(-0.13)		(-0.13)
$R\&D\ Tax\ Credit_{[t-1]}$		-0.031		-0.031
		(-0.97)		(-0.97)
State and year fixed effects	Yes	Yes	Yes	Yes
Number of firms	3,037	3,037	2,089	2,089
Firm-year obs.	22,837	22,837	14,732	14,732
Adjusted R^2	0.420	0.425	0.449	0.452

Table G2. Event Study: 1989 U.S. Supreme Court Ruling, Invalidating All Item APM Laws

This table reports cumulative abnormal returns (CARs) surrounding the U.S. Supreme Court's ruling in *Bonito Boats v. Thunder Craft Boats* for firms located in all item APM law adopting states. CARs are estimated over the event windows' [-0,+0] and [-2,+2] and pre-event windows' [-17,-3] and [-12,-3]. The first four columns provides CARs for the "Manufacturing" sample, while the last four columns shows CARs for the "Products" dataset. Table B2 details the "Products" samples. The odd numbered columns specify the four-factor Carhart (1997) model (i.e., momentum, high minus low book-to-market (HML), small minus big (SMB), and market return), while the even numbered columns use the three-factor Fama-French (1993) model (i.e., HML, SMB, and market return). Columns (1) – (2) and (5) – (6) employ the CRSP equal-weighted index as the market factor, and columns (3) – (4) and (7) – (8) use the CRSP value-weighted index. The parameters for all models are estimated over the window [-271, -21] relative to the event day. The estimated *t*-statistics have been corrected for cross-sectional correlation (Kolari and Pynnönen (2010)) and are shown in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively. Boat hull APM law firms are excluded.

Sample:	"Manufacturing"				"Products"			
	Equal-Weighted Index		Value-Weighted Index		Equal-Weighted Index		Value-Weighted Index	
	4-Factor	3-Factor	4-Factor	3-Factor	4-Factor	3-Factor	4-Factor	3-Factor
CAR Window:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
[-17,-3]	-0.50%	-0.61%	-0.83%	-0.97%	-1.05%	-1.20%	-1.39%	-1.56%
	(-0.32)	(-0.43)	(-0.85)	(-0.96)	(-0.65)	(-0.80)	(-1.11)	(-1.25)
[-12,-3]	-0.21%	-0.23%	-0.57%	-0.61%	-0.45%	-0.47%	-0.82%	-0.86%
	(-0.25)	(-0.23)	(-0.53)	(-0.56)	(-0.33)	(-0.30)	(-0.37)	(-0.40)
[-0,+0]	-0.34%**	-0.37%**	-0.31%*	-0.34%*	-0.40%*	-0.43%**	-0.37%*	-0.41%*
	(-1.78)	(-1.90)	(-1.53)	(-1.64)	(-1.56)	(-1.70)	(-1.34)	(-1.48)
[-2,+2]	-0.31%*	-0.35%*	-0.26%*	-0.30%*	-0.41%*	-0.45%*	-0.36%*	-0.41%*
	(-1.50)	(-1.56)	(-1.31)	(-1.36)	(-1.50)	(-1.56)	(-1.34)	(-1.40)
Number of firms	446	446	446	446	350	350	350	350

Table G3. APM Laws, Supreme Court's Ruling, Patent Activity and Firm Value

This table reports the results for panel regressions of *Tobin's Q* on a *Post 88* × *All Item APM Law* × *Patent Activity* triple interaction term over the period 1975 to 1992. Columns (1) – (3) presents the estimates for the "Manufacturing" sample, while columns (4) – (6) are specific to the "Products" dataset. Table B2 describes the "Products" sample. The included controls are: *Size, Ln(Age), Debt-to-Equity, ROA, Operating Cash-Flow, HHI, Sales Growth, Loss, Firm Liquidity, R&D/Sales, CAPX/Assets,* and *Industry-Year Tobin's Q*. Table A1 provides variable definitions. The industry fixed effects are defined at the two-digit SIC code level. All continuous variables are winsorized at the 1st and 99th percentiles and the dollar values are expressed in 2015 dollars. The row "Sample mean" provides the average value of the respective *Patent Activity* measure. The row "Test for joint significance" shows the results from a test of whether the loss of *All Item APM Law* protection for a firm with an average level of *Patent Activity* is different from an affected non-patenting firm. The estimated *t*-statistics are based on robust standard errors clustered by state of location (reported in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Dep. Variable: *Tobin's Q*	1975 – 1992					
Sample:	"Manufacturing"			"Products"		
Variables	(1)	(2)	(3)	(4)	(5)	(6)
$Post\ 88 \times All\ Item\ APM\ Law \times Ln(1 + Patent)$	-0.075			-0.080		
	(-1.04)			(-1.33)		
$Post\ 88 \times All\ Item\ APM\ Law \times Ln(1 + CW\ Patent)$		0.007			0.006	
		(0.39)			(0.30)	
$Post\ 88 \times All\ Item\ APM\ Law \times Ln(1 + SM\ Patent)$			-0.010			-0.026
			(-0.63)			(-1.22)
$Post\ 88 \times All\ Item\ APM\ Law$						
$Post\ 88 \times Ln(1 + Patent)$	0.145**			0.081		
	(2.12)			(1.39)		
$Post\ 88 \times Ln(1 + CW\ Patent)$		0.051***			0.034**	
		(3.04)			(2.08)	
$Post\ 88 \times Ln(1 + SM\ Patent)$			0.042**			0.026**
			(2.49)			(2.02)
$All\ Item\ APM\ Law \times Ln(1 + Patent)$	-0.184***			-0.191***		
	(-2.96)			(-2.74)		
$All\ Item\ APM\ Law \times Ln(1 + CW\ Patent)$		-0.036*			-0.035*	
		(-1.87)			(-1.79)	
$All\ Item\ APM\ Law \times Ln(1 + SM\ Patent)$			-0.053***			-0.066***
			(-3.04)			(-3.35)
$All\ Item\ APM\ Law$	0.108***	0.118***	0.124***	0.110***	0.118***	0.131***
	(3.11)	(2.64)	(3.64)	(3.88)	(3.19)	(4.66)
$Boat\ Hull\ APM\ Law$	0.075	0.073	0.075	0.070	0.068	0.071
	(1.24)	(1.23)	(1.29)	(1.31)	(1.29)	(1.31)
$Ln(1 + Patent)$	0.077*			0.019		
	(1.88)			(0.33)		
$Ln(1 + CW\ Patent)$		0.002			-0.001	
		(0.19)			(-0.09)	
$Ln(1 + SM\ Patent)$			0.143***			0.162***
			(11.98)			(8.38)
Patent Activity sample mean:	0.155	1.010	0.686	0.145	1.026	0.604
Test for joint significance:						
$[Post\ 88 \times All\ Item\ APM\ Law \times Patent\ Activity] + [Post\ 88 \times All\ Item\ APM\ Law]$	-0.068	-0.068	-0.066	-0.026	-0.028	-0.025
	(-1.02)	(-1.03)	(-1.00)	(-0.43)	(-0.49)	(-0.46)
All control variables	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of firms	3,837	3,837	3,837	2,640	2,640	2,640
Firm-year obs.	32,808	32,808	32,808	21,791	21,791	21,791
Adjusted R^2	0.712	0.712	0.714	0.691	0.691	0.693

Table G4. APM Laws, Supreme Court's Ruling and Patent Activity

This table reports the results for panel regressions of *Patent Activity* on a *Post 88* × *All Item APM Law* over the period 1975 to 1992. Panel A presents the estimates for the "Manufacturing" sample, while Panel B is for the "Products" dataset. Table B2 describes the "Products" sample. The dependent variables are leaded either one (t+1) or three (t+3) years. The included controls are: *Tobins's Q, Size, Ln(Age), Debt-to-Equity, ROA, Operating Cash-Flow, HHI, Sales Growth, Loss, Firm Liquidity, R&D/Sales, CAPX/Assets,* and *Industry-Year Tobin's Q*. Table A1 provides variable definitions. Industry fixed effects are defined at the two-digit SIC code level. All continuous variables are winsorized at the 1st and 99th percentiles and dollar values are expressed in 2015 dollars. The estimated t-statistics are based on robust standard errors clustered by state of location (reported in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Panel A: "Manufacturing" Sample

Period:	1975 – 1992					
Dep. Variables:	$Ln(1 + Patent)_{t+1}$	$Ln(1 + CW\ Patent)_{t+1}$	$Ln(1 + SM\ Patent)_{t+1}$	$Ln(1 + Patent)_{t+3}$	$Ln(1 + CW\ Patent)_{t+3}$	$Ln(1 + SM\ Patent)_{t+3}$
Variables	(1)	(2)	(3)	(4)	(5)	(6)
Post 88 × All Item APM Law	0.021***	0.075**	0.067***	0.029***	0.063**	0.064**
	(6.68)	(2.50)	(3.04)	(4.68)	(2.49)	(2.43)
All Item APM Law	-0.001	-0.006	-0.048**	-0.004	-0.049*	-0.045***
	(-0.42)	(-0.33)	(-2.46)	(-1.34)	(-1.80)	(-2.92)
Boat Hull APM Law	-0.010	-0.032	-0.015	-0.004	0.015	-0.008
	(-1.29)	(-1.00)	(-0.35)	(-0.40)	(0.30)	(-0.20)
All control variables	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of firms	3,546	3,546	3,546	2,969	2,969	2,969
Firm-year obs.	30,536	30,536	30,536	25,122	25,122	25,122
Adjusted R^2	0.926	0.843	0.919	0.924	0.850	0.917

Panel B: "Products" Sample

Period:	1975 – 1992					
Dep. Variables:	$Ln(1 + Patent)_{t+1}$	$Ln(1 + CW\ Patent)_{t+1}$	$Ln(1 + SM\ Patent)_{t+1}$	$Ln(1 + Patent)_{t+3}$	$Ln(1 + CW\ Patent)_{t+3}$	$Ln(1 + SM\ Patent)_{t+3}$
Variables	(1)	(2)	(3)	(4)	(5)	(6)
Post 88 × All Item APM Law	0.027***	0.096**	0.081**	0.034***	0.068**	0.071**
	(5.98)	(2.70)	(3.35)	(3.77)	(2.12)	(2.45)
All Item APM Law	-0.001	-0.006	-0.023***	-0.003	-0.028	-0.011
	(-0.01)	(-0.24)	(-1.33)	(-0.77)	(-1.02)	(-0.68)
Boat Hull APM Law	-0.006	-0.032	-0.033	0.007	0.059	0.013
	(-0.54)	(-0.83)	(-1.18)	(0.67)	(1.13)	(0.40)
All control variables	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of firms	2,425	2,425	2,425	2,021	2,021	2,021
Firm-year obs.	20,238	20,238	20,238	16,606	16,606	16,606
Adjusted R^2	0.911	0.819	0.912	0.906	0.826	0.912

Table G5. Portfolio Analysis: APM Laws and Abnormal Returns

This table reports abnormal returns of equally weighted monthly portfolios of firms located in states that adopt all item APM statutes. The long portfolios are composed in the following manner. Portfolio *12m36* includes all stocks of affected firms located in California, Michigan and Tennessee starting 12 months before the fiscal year-end of the year in which the headquartering state adopts an all item APM law, and holds these stocks for 36 months. The short portfolios are created by including all stocks of falsely affected firms either from the neighboring state(s) or non-manufacturing firm placebo tests starting 12 months before the fiscal year-end of the year in which the corresponding affected headquartering state adopts an APM law, and shorts these stocks for 36 months. The long-short portfolios are then created by differencing the portfolio returns of the long and short portfolios for each respective month. Table B2 describes the "Products" sample. I employ two models: the four-factor Carhart (1997) model (i.e., momentum, high minus low book-to-market (HML), small minus big (SMB), and market return), and the three-factor Fama-French (1993) model (i.e., HML, SMB, and market return). Further, I calculate the portfolio return with each stock weighted by its market capitalization immediately preceding its inclusion in the portfolio. The estimated *t*-statistics are based on robust standard errors (presented in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively. The "Average # of firms" row displays the mean number of stocks in the long and short portfolios across all months. Boat hull APM affected firms are excluded.

Panel A: "Manufacturing" v. "Neighboring State Manufacturing"

Portfolio "12m36"	Four-factor model			Three-factor model		
	Long	Short	Long - Short	Long	Short	Long - Short
Alpha (monthly)	0.727**	-0.037	0.765**	1.017***	0.002	1.016***
	(2.32)	(-0.11)	(2.00)	(2.99)	(0.01)	(2.61)
Average # firms	140.39	87.13	-	140.39	87.13	-
Monthly obs.	98	98	98	98	98	98
Number of firms	320	192	-	320	192	-
Adjusted R^2	0.775	0.769	0.202	0.752	0.771	0.169

Panel B: "Manufacturing" v. "Non-Manufacturing"

Portfolio "12m36"	Four-factor model			Three-factor model		
	Long	Short	Long - Short	Long	Short	Long - Short
Alpha (monthly)	0.727**	0.282	0.445	1.017***	0.261	0.756*
	(2.32)	(0.73)	(1.13)	(2.99)	(0.70)	(1.90)
Average # firms	140.39	91.11	-	140.39	91.11	-
Monthly obs.	98	98	98	98	98	98
Number of firms	320	220	-	320	220	-
Adjusted R^2	0.775	0.716	0.104	0.752	0.719	0.046

Panel C: "Products" v. "Neighboring State Products"

Portfolio "12m36"	Four-factor model			Three-factor model		
	Long	Short	Long - Short	Long	Short	Long - Short
Alpha (monthly)	0.743	0.054	0.690*	0.936**	-0.072	1.008***
	(1.56)	(0.14)	(1.89)	(2.09)	(-0.21)	(2.66)
Average # firms	102.78	59.76	-	102.78	59.76	-
Monthly obs.	98	98	98	98	98	98
Number of firms	234	133	-	234	133	-
Adjusted R^2	0.664	0.757	0.127	0.659	0.755	0.046

Table G6. APM Laws and Firm Value with Non-Product Companies

This table reports results for a falsification test regressing *Tobin's Q* on an *APM Law* indicator variable on a sample of non-products firms located in states adopting APM statutes. "Non-Products" firms include corporations in the manufacturing sample but excluded from the products dataset. Table 2 describes the "Products" sample. Columns (1) – (2) present the estimates corresponding to the period 1975 to 1988 for which the APM statutes are enforceable, while columns (3) – (4) are specific to the entire sample period, 1975 to 1992, where the APM statutes lose their enforceability after the Supreme Court's preemption ruling in February of 1989. The included controls are: *Size, Ln(Age), Debt-to-Equity, ROA, Operating Cash-Flow, HHI, Sales Growth, Loss, Firm Liquidity, R&D/Sales, CAPX/Assets,* and *Industry-Year Tobin's Q*. Table A1 provides variable definitions. Industry fixed effects are defined at the two-digit SIC code level. All continuous variables are winsorized at the 1st and 99th percentiles and dollar values are expressed in 2015 dollars. The row "Test for joint significance" shows the results from a test of whether the summation of the value effect before and after 1988 is different from zero. The estimated *t*-statistics are based on robust standard errors clustered by state of location (reported in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Dep. Variable: *Tobin's Q*				
Sample:	"Non-Products"			
Period:	1975 – 1988		1975 – 1992	
Variables	(1)	(2)	(3)	(4)
Post 88 × All Item APM Law			-0.244	-0.186
			(-1.25)	(-1.42)
All Item APM Law	0.034	0.050	0.057	0.073
	(1.26)	(1.18)	(1.40)	(1.43)
Boat Hull APM Law	0.068	0.071	0.045	0.072
	(0.45)	(0.51)	(0.42)	(0.67)
Test for joint significance:				
[*Post 88 × All Item APM Law*] +			-0.188	-0.113
[*All Item APM Law*]			(-1.15)	(-1.08)
All control variables	No	Yes	No	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes
Number of firms	1,066	1,066	1,258	1,258
Firm-year obs.	8,461	8,461	10,974	10,974
Adjusted R^2	0.755	0.773	0.736	0.753

Table G7. APM Laws, Neighboring State Placebo and Firm Value

This table reports the results for a neighboring state falsification test, regressing *Tobin's Q* on a *Neighboring State All Item APM Law* indicator variable equal to one according to the falsified treatment assignment outlined in Panel C of Table B1, and zero otherwise. Moreover, firms located in actual all item and boat hull APM statute states are excluded from these analyses. Columns (1) – (2) present the estimates in the "Manufacturing" sample, while columns (3) – (4) are specific to the "Products" dataset. Table B2 describes the "Products" sample. The included controls are: *Size, Ln(Age), Debt-to-Equity, ROA, Operating Cash-Flow, HHI, Sales Growth, Loss, Firm Liquidity, R&D/Sales, CAPX/Assets,* and *Industry-Year Tobin's Q*. Table A1 provides variable definitions. The industry fixed effects are defined at the two-digit SIC code level. All continuous variables are winsorized at the 1st and 99th percentiles and the dollar values are expressed in 2015 dollars. The estimated *t*-statistics are based on robust standard errors clustered by state of location (reported in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Dep. Variable: *Tobin's Q*	1975 – 1988			
Sample:	"Neighboring State Manufacturing"		"Neighboring State Products"	
Variables	(1)	(2)	(3)	(4)
Neighboring State All Item APM Law	0.060	0.022	0.033	-0.038
	(0.65)	(0.34)	(0.25)	(-0.43)
Neighboring State Boat Hull APM Law	0.025	0.015	0.035	0.027
	(0.65)	(0.36)	(0.80)	(0.58)
All control variables	No	Yes	No	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes
Number of firms	3,037	3,045	2,040	2,040
Firm-year obs.	21,001	21,109	13,477	13,477
Adjusted R^2	0.709	0.739	0.682	0.721

Table G8. APM Laws, Supreme Court's Ruling and Total Tobin's Q

This table reports the results for panel regressions of *Total Tobin's Q* (Peters and Taylor (2017)) on an *All Item APM Law* indicator variable. Columns (1) – (2) present the estimates in the "Manufacturing" sample, while columns (3) – (4) are specific to the "Products" dataset. Table B2 describes the "Products" sample. Further, the odd numbered columns correspond to the period for which the APM statutes are enforceable, 1975 to 1988, while the even numbered versions are for the entire sample period, 1975 to 1992, where the APM statutes lose their enforceability after the Supreme Court's preemption ruling. The included controls are: *Size, Ln(Age), Debt-to-Equity, ROA, Operating Cash-Flow, HHI, Sales Growth, Loss, Firm Liquidity, R&D/Sales, CAPX/Assets,* and *Industry-Year Tobin's Q*. Table A1 provides variable definitions. The industry fixed effects are defined at the two-digit SIC code level. All continuous variables are winsorized at the 1st and 99th percentiles and the dollar values are expressed in 2015 dollars. The row "Test for joint significance" shows the results from a test of whether the summation of the value effect before and after 1988 is different from zero. The estimated *t*-statistics are based on robust standard errors clustered by state of location (reported in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Dep. Variable: *Total Tobin's Q*				
Sample:	"Manufacturing"		"Products"	
Period:	1975 – 1988	1975 – 1992	1975 – 1988	1975 – 1992
Variables	(1)	(2)	(3)	(4)
Post 88 × *All Item APM Law*		-0.197*		-0.123
		(-1.76)		(-1.20)
All Item APM Law	0.104***	0.106**	0.098***	0.085**
	(2.75)	(2.28)	(2.90)	(2.10)
Boat Hull APM Law	0.050	0.070	0.096	0.100
	(0.49)	(0.84)	(1.44)	(1.57)
Test for joint significance:				
[*Post* 88 × *All Item APM Law*] +		-0.091		-0.039
[*All Item APM Law*]		(-1.17)		(-0.53)
All control variables	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes
Number of firms	3,277	3,277	2,264	2,264
Firm-year obs.	24,829	24,829	16,359	16,359
Adjusted R^2	0.678	0.649	0.652	0.626

Table G9: Poison Pill Laws and Firm Value by Time Split

This table reports the results for pooled panel regressions of Tobin's Q on a poison pill law indicator variable for Compustat firms by time split: 1983 to 1991 and 1994 to 2012. The main variables of interest, Q and $Poison\ Pill\ Law$, are measured contemporaneously, whereas the remaining controls are lagged one period. The pooled panel results below are specific to each "wave". Columns (1) – (2) is for the "first wave" period from 1983 to 1991, and the "second wave" results are shown in columns (3) – (4), which corresponds to the period 1994 to 2012. Table E1 provides variable definitions. All continuous variables are winsorized at the 1st and 99th percentiles and the dollar values are expressed in 2015 dollars. The estimated t-statistics are based on robust standard errors clustered by firm and are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Dep. Variable: $Q_{[t]}$				
	1983 – 1991		1994 – 2012	
Variables	(1)	(2)	(3)	(4)
$Poison\ Pill\ Law_{[t]}$	-0.014	-0.019	0.304***	0.235***
	(-0.36)	(-0.49)	(4.97)	(3.17)
$Poison\ Pill\ Firm\text{-}Level_{[t-1]}$	0.006	0.007	-0.082**	-0.079**
	(0.20)	(0.23)	(-2.34)	(-2.25)
Control variables	Yes	Yes	Yes	Yes
Other Law Controls	No	Yes	No	Yes
Firm and year fixed effects	Yes	Yes	Yes	Yes
# of firms in regression	1,348	1,348	3,057	3,057
N	7,144	7,144	24,670	24,670
Adjusted R^2	0.755	0.756	0.612	0.612

Table G10: Portfolio Analysis: Poison Pill Laws and Abnormal Returns in the Matched Sample

This table reports abnormal returns of equally weighted monthly portfolios of firms that are incorporated in states that adopt poison pill statutes. We construct the portfolios using the treated and control firms from the propensity score matched sample around the passage of these laws. The long portfolios are composed in the following manner. For portfolios *6m24*, and *6m36* we include all stocks of matched firms that are incorporated in states starting 6 months before the fiscal year-end of the year in which the incorporating state adopts a poison pill law, and hold these stocks for 24 or 36 months. Similarly, the short portfolios are constructed by including all stocks of control firms that are matched to a treated company incorporated in states starting 6 months before the fiscal year-end of the year in which that treated incorporating state adopts a poison pill law, and short these control group stocks for 24 or 36 months. The long-short portfolios are then created by differencing the portfolio returns of the long and short portfolios, for each respective month. We use two models: the four-factor Carhart (1997) model (i.e., momentum, high minus low book-to-market (HML), small minus big (SMB), and market return), the three-factor Fama-French model (i.e., HML, SMB, and market return), and the market model (i.e., including only the market return). Further, we calculate the portfolio return with each stock weighted by its market capitalization immediately preceding its inclusion in the portfolio. The estimated *t*-statistics are based on robust standard errors and presented in parentheses below the coefficients. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively. The number of stocks in the long and short portfolios are averaged across all months and displayed in the "Average # firms" row. The "N" row shows the total number of security-events with useable returns.

Panel A: Full Sample

	Four-factor model			Three-factor model		
Portfolio "6m24"						
	Long	Short	Long - Short	Long	Short	Long - Short
Alpha (monthly)	0.629*	-0.145	0.671*	0.751*	0.008	0.634
	(1.67)	(-0.56)	(1.79)	(1.97)	(0.03)	(1.62)
Average # firms	70.69	71.60	-	70.69	71.60	-
M	253	248	248	253	248	248
N	490	487	-	490	487	-
Adjusted R^2	0.339	0.559	0.027	0.336	0.550	0.030

	Four-factor model			Three-factor model		
Portfolio "6m36"						
	Long	Short	Long - Short	Long	Short	Long - Short
Alpha (monthly)	0.520	-0.313	0.707*	0.639	-0.151	0.675*
	(1.29)	(-1.17)	(1.80)	(1.61)	(-0.54)	(1.68)
Average # firms	61.63	64.92	-	61.63	64.92	-
M	294	277	277	294	277	277
N	491	488	-	491	488	-
Adjusted R^2	0.311	0.539	0.008	0.309	0.529	0.011

Table G10 – (*Continued*)

Panel B: First Wave

	Four-factor model			Three-factor model		
Portfolio "6m24"						
	Long	Short	Long - Short	Long	Short	Long - Short
Alpha (monthly)	-0.047	0.098	-0.145	-0.080	0.101	-0.181
	(-0.18)	(0.31)	(-0.61)	(-0.31)	(0.33)	(-0.76)
Average # firms	128.25	128.80	-	128.25	128.80	-
M	81	81	81	81	81	81
N	279	273	-	279	273	-
Adjusted R^2	0.808	0.764	0.050	0.809	0.767	0.056

	Four-factor model			Three-factor model		
Portfolio "6m36"						
	Long	Short	Long - Short	Long	Short	Long - Short
Alpha (monthly)	0.231	0.035	0.196	0.206	-0.021	0.227
	(0.86)	(0.10)	(0.55)	(0.80)	(-0.06)	(0.62)
Average # firms	112.91	113.38	-	112.91	113.38	-
M	93	93	93	93	93	93
N	279	274	-	279	274	-
Adjusted R^2	0.755	0.670	-0.021	0.758	0.673	-0.011

Panel C: Second Wave

	Four-factor model			Three-factor model		
Portfolio "6m24"						
	Long	Short	Long - Short	Long	Short	Long - Short
Alpha (monthly)	0.899*	-0.341	1.093**	1.057*	-0.159	1.060*
	(1.66)	(-0.93)	(2.00)	(1.91)	(-0.42)	(1.87)
Average # firms	43.59	43.86	-	43.59	43.86	-
M	172	167	167	172	167	167
N	211	214	-	211	214	-
Adjusted R^2	0.259	0.499	0.022	0.257	0.490	0.028

	Four-factor model			Three-factor model		
Portfolio "6m36"						
	Long	Short	Long - Short	Long	Short	Long - Short
Alpha (monthly)	0.614	-0.514	0.937	0.766	-0.303	0.900
	(1.07)	(-1.38)	(1.64)	(1.35)	(-0.79)	(1.55)
Average # firms	37.90	40.43	-	37.90	40.43	-
M	201	184	184	201	184	184
N	212	214	-	212	214	-
Adjusted R^2	0.253	0.514	0.004	0.251	0.500	0.009

Table G11: Poison Pill Laws and Total Q

This table reports results for pooled panel regressions of Total Tobin's Q on poison pill law indicators. *Total Q* is from Peters and Taylor (2017). Panel A provides pooled panel regression estimates. Columns (1) – (2) correspond to the period 1983 to 2012, columns (3) – (4) to 1983 to 1991 or the "first wave", and columns (5) – (6) to the "second wave" period from 1994 to 2012. Panel B shows the matched sample DID results. Columns (1) – (2) are for the full sample, columns (3) – (4) are specific to the "first wave", columns (5) – (6) to the "second wave" period, and, finally, columns (7) – (8) include an interaction of *Treat* × *Post* with a *Poison Pill Law First Wave* dummy. Control variables are lagged one period and those included in columns (1) – (6): *Ln(Assets), Ln(Age), HHI, Sales Growth, Loss, Debt-to-Equity, Firm Liquidity, CAPX/Assets, R&D/Sales, Institutional Ownership, State-year Q,* and *Industry-year Q*. Further, columns (2), (4), and (6) specify: *Business Combination Law, Control Share Law, Directors' Duties Law,* and *Fair Price Law* dummies. Table E1 provides variable definitions. All continuous variables are winsorized at the 1st and 99th percentiles and the dollar values are expressed in 2015 dollars. The estimated *t*-statistics are based on robust standard errors clustered by firm (reported in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Panel A: Pooled Panel Regressions

Dep. Variable: $Total\ Q_{[t]}$

	1983 – 2012					
Variables	(1)	(2)	(3)	(4)	(5)	(6)
$Poison\ Pill\ Law_{[t]}$	0.160**	0.143*				
	(2.39)	(1.66)				
$Poison\ Pill\ Law\ First\ Wave_{[t]}$			-0.012	-0.188	0.021	-0.159
			(-0.22)	(-1.56)	(0.36)	(-1.38)
$Poison\ Pill\ Law\ Second\ Wave_{[t]}$			0.369***	0.334**	0.362***	0.318**
			(2.87)	(2.52)	(2.82)	(2.41)
$Post\ 94_{[t]} \times Poison\ Pill\ Law\ First\ Wave_{[t]}$					-0.061	-0.087
					(-0.79)	(-1.05)
$Poison\ Pill\ Firm\text{-}Level_{[t-1]}$	-0.153***	-0.147***	-0.156***	-0.151***	-0.156***	-0.151***
	(-2.81)	(-2.84)	(-2.85)	(-2.89)	(-2.85)	(-2.89)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Other law controls	No	Yes	No	Yes	No	Yes
Firm and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
# of firms in regression	3,419	3,419	3,419	3,419	3,419	3,419
N	33,791	33,791	33,791	33,791	33,791	33,791
Adjusted R^2	0.357	0.357	0.357	0.357	0.357	0.357

Table G11 – (*Continued*)

Panel B: Matched Sample Regressions

Dep. Variable: $Total\ Q_{[t]}$

	(t-3) to (t+3)							
	Full Sample		First Wave (law adopted: 1986-1990)		Second Wave (law adopted: 1995-2009)		Full Sample with First Wave Dummy	
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Treat_{[t]} \times Post_{[t]}$	0.103*	0.135*	-0.015	0.008	0.265**	0.271*	0.241**	0.249**
	(1.77)	(1.72)	(-0.25)	(0.14)	(2.19)	(1.65)	(2.13)	(2.02)
$Treat_{[t]} \times Post_{[t]} \times$							-0.337	-0.333
$Poison\ Pill\ Law\ First\ Wave_{[t]}$							(-1.48)	(-1.45)
$Post_{[t]}$	0.004	-0.008	0.028	0.015	-0.004	-0.001	-0.004	-0.011
	(0.09)	(-0.17)	(0.77)	(0.43)	(-0.05)	(-0.01)	(-0.08)	(-0.23)
$Poison\ Pill\ Firm\text{-}Level_{[t-1]}$	-0.019	-0.019	0.009	0.008	-0.073	-0.076	-0.024	-0.023
	(-0.38)	(-0.38)	(0.19)	(0.17)	(-0.58)	(-0.60)	(-0.48)	(-0.48)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other law controls	No	Yes	No	Yes	No	Yes	No	Yes
Firm and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
# of firms in regression	873	873	504	504	401	401	873	873
N	6,112	6,112	3,578	3,578	2,534	2,534	6,112	6,112
Adjusted R^2	0.707	0.707	0.689	0.689	0.702	0.702	0.708	0.708

Table G12: Poison Pill Laws, Innovative Activity, and Firm Value by Wave

This table reports matched sample regressions of Tobin's Q on *Treat* × *Post* × *Innovative Activity*. *Treat* is an indicator variable equal to one if the firm is incorporated in a state that adopts a poison pill law. *Post* is an indicator variable equal to one in the year of and post treatment period, and zero otherwise. *Innovative Activity* measures include: *R&D/Sales, Intangible Capital/Assets,* and *Knowledge Capital/Assets*. Q, *Treat* × *Post, Treat* × *Post* × *Innovative Activity*, and *Post* are measured contemporaneously, and the controls are lagged one year. *Treat* is omitted in the regression because of collinearity with its firm fixed effect. The fourth interacted variable is *Poison Pill Law First Wave.* Table E1 provides variable definitions. Included controls: *Business Combination Law, Control Share Law, Directors' Duties Law, Fair Price Law, Firm-Level Poison Pill, Ln(Assets), Ln(Age), HHI, Sales Growth, Loss, Debt-to-Equity, Firm Liquidity, CAPX/Assets, R&D/Sales, Institutional Ownership, State-year Q,* and *Industry-year Q*. Continuous variables are winsorized at the 1st and 99th percentiles and dollar values are expressed in 2015 dollars. Estimated *t*-statistics are based on robust standard errors clustered by firm (reported in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Dep. Variable: $Q_{[t]}$	Full Sample: (t-3) to (t+3)		
Variables	(1)	(2)	(5)
$Treat_{[t]} \times Post_{[t]} \times \frac{R\&D}{Sales_{[t]}} \times Poison\ Pill\ Law\ First\ Wave_{[t]}$	-0.234 (-0.05)		
$Treat_{[t]} \times Post_{[t]} \times \frac{Intangible\ Capital}{Assets}_{[t]} \times Poison\ Pill\ Law\ First\ Wave_{[t]}$		-1.927 (-1.42)	
$Treat_{[t]} \times Post_{[t]} \times \frac{Knowledge\ Capital}{Assets}_{[t]} \times Poison\ Pill\ Law\ First\ Wave_{[t]}$			1.870 (0.34)
$Treat_{[t]} \times Post_{[t]} \times \frac{R\&D}{Sales_{[t]}}$	0.854 (1.31)		
$Treat_{[t]} \times Post_{[t]} \times \frac{Intangible\ Capital}{Assets}_{[t]}$		0.369* (1.88)	
$Treat_{[t]} \times Post_{[t]} \times \frac{Knowledge\ Capital}{Assets}_{[t]}$			0.767** (2.31)
$\frac{R\&D}{Sales_{[t]}}$	1.255** (2.02)		
$\frac{Intangible\ Capital}{Assets}_{[t]}$		0.003 (0.01)	
$\frac{Knowledge\ Capital}{Assets}_{[t]}$			0.633 (1.63)
$Treat_{[t]} \times Post_{[t]}$	0.067 (1.22)	-0.028 (-0.22)	0.114 (1.23)
$Post_{[t]}$	0.009 (0.34)	0.206*** (2.67)	0.096* (1.91)
$Poison\ Pill\ Law\ First\ Wave_{[t]}$	-0.053 (-0.49)	1.387 (1.07)	0.321 (0.85)
Control Variables (including other laws)	Yes	Yes	Yes
Firm and year fixed effects	Yes	Yes	Yes
# of firms in regression	873	873	873
N	6,117	6,117	6,117
Adjusted R^2	0.715	0.667	0.669

Table G13: Poison Pill Laws, Stakeholder Relationships, and Firm Value by Wave

This table reports matched sample regressions of Tobin's Q on *Treat* × *Post* × *Stakeholder Relationship Proxy*. *Treat* is an indicator variable equal to one if the firm is incorporated in a state that adopts a poison pill law. *Post* is an indicator variable equal to one in the year of and post treatment period, and zero otherwise. *Stakeholder Relationship* Proxies include the following: *Large Customer, Strategic Alliance,* and *Labor Capital*. *Q*, *Treat* × *Post*, *Treat* × *Post* × *Stakeholder Relationship Proxy*, and *Post* are measured contemporaneously, whereas the remaining controls are lagged one period. *Treat* is omitted in the regression because of collinearity with its firm fixed effect. We report the results from adding a quadruple interaction term, where the fourth interacted variable is *Poison Pill Law First Wave*. *Poison Pill First Wave Law* is a dummy variable equal to one if a firm is incorporated in a state that adopts a poison pill law in the "first wave" period from 1986 to 1990. Table E1 provides variable definitions. Included controls: *Business Combination Law, Control Share Law, Directors' Duties Law, Fair Price Law, Ln(Assets), Poison Pill Firm-Level, Ln(Age), HHI, Sales Growth, Loss, Debt-to-Equity, Firm Liquidity, CAPX/Assets, R&D/Sales, Institutional Ownership, State-year Q,* and *Industry-year Q*. Continuous variables are winsorized at the 1st and 99th percentiles and dollar values are expressed in 2015 dollars. Estimated *t*-statistics are based on robust standard errors clustered by firm (reported in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Dep. Variable: $Q_{[t]}$	Full Sample: (t-3) to (t+3)		
Variables	(1)	(2)	(3)
$Treat_{[t]} \times Post_{[t]} \times Large\ Customer_{[t]} \times$	-0.176		
$Poison\ Pill\ Law\ First\ Wave_{[t]}$	(-0.61)		
$Treat_{[t]} \times Post_{[t]} \times Strategic\ Alliance_{[t]} \times$		0.097	
$Poison\ Pill\ Law\ First\ Wave_{[t]}$		(0.24)	
$Treat_{[t]} \times Post_{[t]} \times Labor\ Capital_{[t]} \times$			-0.572
$Poison\ Pill\ Law\ First\ Wave_{[t]}$			(-1.01)
$Treat_{[t]} \times Post_{[t]} \times Large\ Customer_{[t]}$	0.134*		
	(1.74)		
$Treat_{[t]} \times Post_{[t]} \times Strategic\ Alliance_{[t]}$		0.153	
		(1.50)	
$Treat_{[t]} \times Post_{[t]} \times Labor\ Capital_{[t]}$			0.311*
			(1.69)
$Large\ Customer_{[t]}$	0.017		
	(0.48)		
$Strategic\ Alliance_{[t]}$		0.012	
		(0.17)	
$Labor\ Capital_{[t]}$			0.378**
			(2.00)
$Treat_{[t]} \times Post_{[t]}$	0.025	0.040	-0.017
	(0.41)	(0.51)	(-0.21)
$Post_{[t]}$	0.028	0.023	0.034
	(0.86)	(0.70)	(0.79)
$Poison\ Pill\ Law\ First\ Wave_{[t]}$	0.236	0.428	0.415
	(1.27)	(1.43)	(1.32)
Control Variables (including other laws)	Yes	Yes	Yes
Firm and year fixed effects	Yes	Yes	Yes
# of firms in regression	873	873	837
N	6,117	6,117	5,801
Adjusted R^2	0.712	0.717	0.710

Table G14: Matched Sample Summary Statistics across Wave

This table reports summary statistics for a propensity score matched sample. Treated firms are defined as companies incorporated in states that adopt poison pill laws, whereas the control firms are incorporated in states without poison pill laws in at least the five-year period following the passage of a law for its matched counterpart. We use nearest-neighbor matching with replacement in year *t-1* to create a sample matched on *Q* and *Ln(Assets)*, and exactly on 2-digit SIC industry codes and firm-level poison pill status for each of the thirty five treated states. We show the summary statistics for the year prior to treatment, comparing first-wave treated (control) firms with second-wave treated (control) firms. The column "Difference (*t*-stat)" provides the difference between the wave-specific treated (control) firms' sample means and their test statistics in parentheses. The row "N (by group)" provides the number of unique firms for each treatment and control group. Table E1 provides variable definitions. All continuous variables are winsorized at the 1st and 99th percentiles and the dollar values are expressed in 2015 dollars. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	Treat			Control		
	(1)	(2)	(3)	(4)	(5)	(6)
Matched Variables:	First Wave	Second Wave	Difference	First Wave	Second Wave	Difference
$Q_{[t]}$	1.418	1.752	-0.334***	1.396	1.753	-0.357***
	(0.555)	(1.343)	(-3.83)	(0.468)	(1.269)	(-4.41)
$Poison\ Pill\ Firm\text{-}Level_{[t]}$	0.332	0.363	-0.031	0.332	0.363	-0.031
	(0.472)	(0.482)	(-0.73)	(0.472)	(0.482)	(-0.73)
$Ln(Assets)_{[t]}$	7.075	5.505	1.570***	6.941	5.784	1.157***
	(1.642)	(1.844)	(10.17)	(1.489)	(1.883)	(7.76)
Other Control Variables:						
$Ln(Age)_{[t]}$	3.105	2.760	0.345***	3.066	2.767	0.299***
	(0.302)	(0.659)	(7.89)	(0.357)	(0.621)	(6.85)
$HHI_{[t]}$	0.261	0.237	0.024	0.269	0.233	0.036**
	(0.161)	(0.193)	(1.53)	(0.169)	(0.202)	(2.19)
$Sales\ Growth_{[t]}$	0.044	0.035	0.009	0.056	0.007	0.049**
	(0.212)	(0.265)	(0.43)	(0.238)	(0.322)	(1.98)
$Loss_{[t]}$	0.128	0.323	-0.195***	0.159	0.390	-0.231***
	(0.335)	(0.469)	(-5.48)	(0.366)	(0.489)	(-6.11)
$Firm\ Liquidity_{[t]}$	0.271	0.266	0.005	0.249	0.284	-0.035*
	(0.184)	(0.220)	(0.28)	(0.190)	(0.261)	(-1.76)
$CAPX/Assets_{[t]}$	0.068	0.066	0.002	0.066	0.057	0.009**
	(0.051)	(0.063)	(0.40)	(0.048)	(0.055)	(1.97)
$Institutional\ Owenrship_{[t]}$	0.315	0.291	0.024	0.267	0.330	-0.063***
	(0.244)	(0.277)	(1.04)	(0.238)	(0.304)	(-2.63)
Interacted Variables:						
$Large\ Customer_{[t]}$	0.356	0.511	-0.155***	0.439	0.565	-0.126***
	(0.480)	(0.501)	(-3.55)	(0.497)	(0.497)	(-2.84)
$Strategic\ Alliance_{[t]}$	0.149	0.511	-0.362***	0.107	0.489	-0.382***
	(0.356)	(0.501)	(-9.55)	(0.310)	(0.501)	(-10.47)

Table G14 – *(Continued)*

$Labor\ Capital_{[t]}$	0.290	0.338	-0.048**	0.291	0.362	-0.071***
	(0.179)	(0.247)	(-2.55)	(0.194)	(0.256)	(-3.57)
$R\&D/Sales_{[t]}$	0.021	0.041	-0.020***	0.019	0.057	-0.038***
	(0.034)	(0.085)	(-3.64)	(0.034)	(0.102)	(-5.92)
$Intangible\ Capital/Assets_{[t]}$	0.497	0.614	-0.117***	0.502	0.709	-0.207***
	(0.354)	(0.550)	(-2.92)	(0.329)	(0.569)	(-5.17)
$Knowledge\ Capital/Assets_{[t]}$	0.106	0.154	-0.048**	0.095	0.206	-0.111***
	(0.163)	(0.310)	(-2.26)	(0.145)	(0.348)	(-4.90)
N (by group)	289	223		289	223	

Table G15: Poison Pill Laws, Heterogeneous Provisions and Firm Value

This table reports regressions of Tobin's Q on a poison pill law, and, where applicable, additional provision indicators. The main variables of interest, Q, $Poison\ Pill\ Law$, $Dead\text{-}Hand\ Provision$, and $Weak\ Pill\ Provision$ are measured contemporaneously, whereas the controls are lagged one period. Columns (1) – (3) provides pooled panel regression estimates over the full sample period, 1983 to 2012. Columns (4) – (6) shows the matched sample regression estimates over the three samples: full sample, and first and second wave samples. $Dead\text{-}Hand\ Provision$ is a dummy variable equal to one if a firm is incorporated in a state that passes a poison pill law with or later amends earlier legislation to allow dead-hand poison pills, and zero otherwise.[a]

$Weak\ Pill\ Provision$ is a dummy variable equal to one if a firm is incorporated in a state that passes a poison pill law with a provision that allows explicitly for judicial review of poison pills, and zero otherwise.[b] Control variables: *Ln(Assets), Ln(Age), HHI, Sales Growth, Loss, Debt-to-Equity, Firm Liquidity, CAPX/Assets, R&D/Sales, Institutional Ownership, State-year Q, Industry-year Q, Business Combination Law, Control Share Law, Directors' Duties Law*, and *Fair Price Law*. Table E1 provides variable definitions. Continuous variables are winsorized at the 1st and 99th percentiles and dollar values are expressed in 2015 dollars. Estimated *t*-statistics are based on robust standard errors clustered by firm (reported in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Dep. Variable: $Q_{[t]}$	Pooled Panel: Full Sample			Matched Sample: Full Sample		
Variables	(1)	(2)	(3)	(4)	(5)	(6)
$Poison\ Pill\ Law_{[t]}$	0.111**	0.112**	0.120**			
	(2.29)	(2.28)	(2.38)			
$Treat_{[t]} \times Post_{[t]}$				0.104*	0.103*	0.104*
				(1.71)	(1.69)	(1.71)
$Dead\text{-}Hand\ Provision_{[t]}$	-0.082		-0.094	-0.104		-0.104
	(-0.96)		(-1.09)	(-1.02)		(-1.02)
$Weak\text{-}\ Pill\ Provision_{[t]}$		-0.114	-0.124		0.001	-0.003
		(-1.24)	(-1.34)		(0.01)	(-0.04)
$Poison\ Pill\ Firm\text{-}Level_{[t-1]}$	-0.103***	-0.103***	-0.103***	0.012	0.012	0.012
	(-3.80)	(-3.81)	(-3.80)	(0.27)	(0.27)	(0.27)
Control variables (including other laws)	Yes	Yes	Yes	Yes	Yes	Yes
Firm and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
# of firms in regression	3,423	3,423	3,423	873	873	873
N	33,826	33,826	33,826	6,117	6,117	6,117
Adjusted R^2	0.602	0.602	0.602	0.662	0.662	0.662

[a] There are three states with dead-hand pill provisions: Georgia, after it amended its earlier statute in 2000, as well as Maryland and Virginia.

[b] There are two states with weak-pill provisions: Both New York and North Carolina explicitly admit judicial review of poison pills.

Table G16: Poison Pill Laws and Firm Value without Delaware Firms

This table reports the results for pooled panel regressions of Tobin's Q on poison pill law indicator variables over the sample period 1983 to 2012, excluding firms incorporated in Delaware. The main variables of interest, *Q, Poison Pill Law, Poison Pill Law First Wave,* and *Poison Pill Law Second Wave* are measured contemporaneously, whereas the remaining controls are lagged one period. *Poison Pill Law First Wave* is a dummy variable equal to one if a firm is incorporated in a state that passes a poison pill law during the period 1986 to 1990, and zero otherwise. *Poison Pill Law Second Wave* is a dummy variable equal to one if a firm is incorporated in a state that passes a poison pill law during the period 1995 to 2009, and zero otherwise. All four columns include the following control variables: *Ln(Assets), Ln(Age), HHI, Sales Growth, Loss, Debt-to-Equity, Firm Liquidity, CAPX/Assets, R&D/Sales, Institutional Ownership, State-year Q,* and *Industry-year Q*. Column's (2) and (4) further specify: *Business Combination Law, Control Share Law, Directors' Duties Law,* and *Fair Price Law* indicators. Table E1 provides variable definitions. All continuous variables are winsorized at the 1st and 99th percentiles and the dollar values are expressed in 2015 dollars. The estimated *t*-statistics are based on robust standard errors clustered by firm and are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Dep. Variable: $Q_{[t]}$

	1983 – 2012			
Variables	(1)	(2)	(3)	(4)
$Poison\ Pill\ Law_{[t]}$	0.136***	0.090*		
	(2.76)	(1.68)		
$Poison\ Pill\ Law\ First\ Wave_{[t]}$			0.026	-0.052
			(0.48)	(-0.97)
$Poison\ Pill\ Law\ Second\ Wave_{[t]}$			0.193***	0.151**
			(2.68)	(2.04)
$Poison\ Pill\ Firm\text{-}Level_{[t-1]}$	-0.127***	-0.129***	-0.130***	-0.133***
	(-3.28)	(-3.34)	(-3.35)	(-3.43)
Control variables	Yes	Yes	Yes	Yes
Other law controls	No	Yes	No	Yes
Firm and year fixed effects	Yes	Yes	Yes	Yes
# of firms in regression	1,659	1,659	1,659	1,659
N	16,025	16,025	16,025	16,025
Adjusted R^2	0.605	0.605	0.605	0.606

Table G17: Matched Sample without Delaware Firms Summary Statistics

This table reports summary statistics for a propensity score matched sample in the year prior to treatment, excluding firms incorporated in Delaware from the pool of possible controls. Treated firms are defined as companies incorporated in states that adopt poison pill laws, whereas the control firms are incorporated in states without poison pill laws in at least the five-year period following the passage of a law for its matched counterpart. We use nearest-neighbor matching with replacement in year *t-1* to create a sample matched on *Q* and *Ln(Assets)*, and exactly on 2-digit SIC industry codes and firm-level poison pill status for each of the thirty five treated states. Columns (1) – (3) presents the results of the matching algorithm for the 35 treatment states in the full sample. Columns (4) – (6) presents the results of the matching algorithm for the 23 treatment states in the "first wave" sample. Columns (7) – (9) provides the summary statistics for the matched treated and control firms in year *t-1* for the 12 treatment states in the "second wave" sample. The column "Difference" provides the difference between the treat and control sample mean and its test statistic in parentheses. The row "N (by group)" provides the number of unique firms for each treatment and control group. Table E1 provides variable definitions. All continuous variables are winsorized at the 1st and 99th percentiles and the dollar values are expressed in 2015 dollars. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	Full Sample			First Wave			Second Wave		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Matched Variables:	Treat	Control	Difference	Treat	Control	Difference	Treat	Control	Difference
$Q_{[t]}$	1.598	1.648	-0.050	1.439	1.475	-0.036	1.788	1.856	-0.068
	(1.048)	(1.086)	(-0.74)	(0.637)	(0.503)	(0.72)	(1.367)	(1.489)	(-0.50)
$Poison\ Pill\ Firm\text{-}Level_{[t]}$	0.348	0.348	0.000	0.336	0.336	0.000	0.362	0.362	0.000
	(0.477)	(0.477)	(0.00)	(0.473)	(0.473)	(0.00)	(0.482)	(0.482)	(0.00)
$Ln(Assets)_{[t]}$	6.305	6.245	0.060	7.063	6.943	0.120	5.395	5.407	-0.012
	(1.902)	(1.692)	(0.52)	(1.607)	(1.341)	(0.93)	(1.831)	(1.693)	(-0.074)
Other Control Variables:									
$Ln(Age)_{[t]}$	2.938	2.879	0.059*	3.100	3.098	0.001	2.744	2.617	0.127**
	(0.534)	(0.510)	(1.75)	(0.310)	(0.273)	(0.06)	(0.667)	(0.598)	(2.11)
$HHI_{[t]}$	0.246	0.240	0.007	0.257	0.266	-0.009	0.233	0.208	0.025
	(0.175)	(0.172)	(0.59)	(0.156)	(0.168)	(-0.63)	(0.195)	(0.172)	(1.44)
$Sales\ Growth_{[t]}$	0.042	0.054	-0.012	0.043	0.040	0.003	0.041	0.071	-0.030
	(0.249)	(0.278)	(0.70)	(0.214)	(0.207)	(0.17)	(0.285)	(0.344)	(-0.99)
$Loss_{[t]}$	0.208	0.210	-0.002	0.128	0.106	0.023	0.303	0.335	-0.032
	(0.406)	(0.408)	(-0.08)	(0.335)	(0.308)	(0.81)	(0.461)	(0.473)	(-0.71)
$Debt\text{-}to\text{-}Equity_{[t]}$	0.478	0.600	-0.121	0.447	0.623	-0.176*	0.515	0.571	-0.056
	(1.009)	(1.475)	(-1.50)	(0.860)	(1.345)	(-1.79)	(1.163)	(1.621)	(-0.42)
$Firm\ Liquidity_{[t]}$	0.274	0.274	0.000	0.274	0.268	0.006	0.274	0.280	-0.006
	(0.203)	(0.196)	(0.01)	(0.179)	(0.159)	(0.37)	(0.228)	(0.231)	(-0.29)
$CAPX/Assets_{[t]}$	0.068	0.065	0.003	0.070	0.066	0.004	0.065	0.064	0.001
	(0.055)	(0.050)	(0.76)	(0.051)	(0.041)	(0.93)	(0.059)	(0.059)	(0.20)
$R\&D/Sales_{[t]}$	0.035	0.049	-0.014**	0.022	0.028	-0.006**	0.074	0.050	-0.024**
	(0.081)	(0.094)	(-2.52)	(0.034)	(0.035)	(2.08)	(0.131)	(0.112)	(-2.05)
$Institutional\ Owenrship_{[t]}$	0.309	0.315	-0.006	0.323	0.333	-0.010	0.293	0.294	-0.001
	(0.260)	(0.268)	(-0.34)	(0.242)	(0.237)	(-0.46)	(0.281)	(0.301)	(-0.03)
N (by group)	486	486		265	265		221	221	

Table G18: Poison Pill Laws and Firm Value in the Matched Sample without Delaware Firms

This table reports the results for matched sample regressions of Tobin's Q on a *Treat* × *Post* interaction term, in which we exclude firms incorporated in Delaware from the pool of potential controls. *Treat* is an indicator variable equal to one if the firm is incorporated in a state that adopts a poison pill law. *Post* is an indicator variable equal to one in the year of and post treatment period, and zero otherwise. The main variables of interest, *Q*, *Treat* × *Post*, and *Post* are measured contemporaneously, whereas the remaining controls are lagged one period. *Treat* is omitted in the regression because of collinearity with its firm fixed effect. Columns (1) – (2) regresses Tobin's Q on *Treat* × *Post* for the full sample period, columns (3) – (4) provides coefficient estimates for the "first wave", columns (5) – (6) shows the matched sample DID results for the "second wave" period, and columns (7) – (8) reports the DID estimates for the full sample period where *Treat* × *Post* is interacted with the *Poison Pill Law First Wave* dummy. *Poison Pill Law First Wave* is a dummy variable equal to one if a firm is incorporated in a state that passes a poison pill law during the period 1986 to 1990, and zero otherwise. Table E1 provides variable definitions. The included controls are: *Ln(Assets), Ln(Age), HHI, Sales Growth, Loss, Debt-to-Equity, Firm Liquidity, CAPX/Assets, R&D/Sales, Institutional Ownership, State-year Q*, and *Industry-year Q*. Further, columns (2), (4), and (6) specify: *Business Combination Law, Control Share Law, Directors' Duties Law*, and *Fair Price Law* dummies. All continuous variables are winsorized at the 1st and 99th percentiles and the dollar values are expressed in 2015 dollars. The estimated *t*-statistics are based on robust standard errors clustered by firm and are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Dep. Variable: $Q_{[t]}$	(t-3) to (t+3)							
	Full Sample		First Wave (law adopted: 1986-1990)		Second Wave (law adopted: 1995-2009)		Full Sample with First Wave Dummy	
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Treat_{[t]} \times Post_{[t]}$	0.128**	0.117*	-0.044	-0.052	0.303***	0.311**	0.245**	0.232**
	(2.14)	(1.82)	(-0.55)	(-0.67)	(2.72)	(2.49)	(2.29)	(2.17)
$Treat_{[t]} \times Post_{[t]} \times$							-0.350	-0.343
$Poison\ Pill\ Law\ First\ Wave_{[t]}$							(-1.49)	(-1.47)
$Post_{[t]}$	0.040	0.044	0.041	0.041	-0.103	-0.102	0.037	0.042
	(0.68)	(0.71)	(0.83)	(0.89)	(-0.82)	(-0.81)	(0.62)	(0.68)
$Poison\ Pill\ Firm\text{-}Level_{[t-1]}$	0.006	0.004	0.026	0.028	-0.191	-0.192	0.001	-0.001
	(0.09)	(0.07)	(0.44)	(0.47)	(-1.63)	(-1.63)	(0.01)	(-0.01)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other law controls	No	Yes	No	Yes	No	Yes	No	Yes
Firm and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
# of firms in regression	666	666	357	357	344	344	666	666
N	5,705	5,705	3,136	3,136	2,569	2,569	5,705	5,705
Adjusted R^2	0.655	0.655	0.725	0.726	0.646	0.646	0.656	0.657

Table G19: Matched Sample Placebo Test Summary Statistics

This table reports summary statistics for a propensity score matched sample in the year prior to placebo treatment. We purposely move back treatment five years to serve as a matched sample falsification test. For example, Minnesota adopted a poison pill law in 1995, however, in this analysis we assume the law was passed in 1990. We then consider a plus or minus three-year window. Thus, actual treatment never occurs. We provide summary statistics for the full sample, first, and second waves, respectively. The standard deviation is included in the parentheses below the mean value of each variable. We indicate significant differences between the two groups with *, **, and ***, which denotes significance at the 10%, 5%, and 1% level, respectively. The column "Difference" provides the difference between the treat and control sample mean and its test statistic in parentheses. The row "N (by group)" provides the number of unique firms for each group. Table E1 provides variable definitions. All continuous variables are winsorized at the 1st and 99th percentiles and dollar values are expressed in 2015 dollars.

	Full Sample			First Wave			Second Wave		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Matched Variables:	Treat	Control	Difference	Treat	Control	Difference	Treat	Control	Difference
$Q_{[t]}$	1.496	1.497	-0.001	1.319	1.309	0.010	1.794	1.813	-0.019
	(0.759)	(0.759)	(-0.02)	(0.598)	(0.584)	(0.20)	(0.963)	(0.901)	(-0.19)
$Poison\ Pill\ Firm\text{-}Level_{[t]}$	0.138	0.138	0.000	0.000	0.000	0.000	0.370	0.370	0.000
	(0.346)	(0.346)	(0.00)	(0.000)	(0.000)	(0.000)	(0.484)	(0.484)	(0.00)
$Ln(Assets)_{[t]}$	6.463	6.580	-0.117	6.908	6.917	-0.008	5.718	6.016	-0.298
	(1.719)	(1.635)	(-1.08)	(1.536)	(1.447)	(-0.07)	(1.755)	(1.775)	(1.61)
Other Control Variables:									
$Ln(Age)_{[t]}$	2.886	2.900	-0.013	2.917	2.905	0.012	2.835	2.891	-0.056
	(0.423)	(0.415)	(-0.50)	(0.216)	(0.242)	(0.64)	(0.631)	(0.603)	(-0.86)
$HHI_{[t]}$	0.251	0.245	0.006	0.257	0.260	-0.003	0.242	0.221	0.022
	(0.173)	(0.181)	(0.54)	(0.172)	(0.190)	(-0.21)	(0.174)	(0.162)	(1.22)
$Sales\ Growth_{[t]}$	0.001	0.019	-0.018	-0.052	-0.016	-0.036**	0.088	0.077	0.011
	(0.237)	(0.228)	(-1.22)	(0.205)	(0.206)	(-2.16)	(0.261)	(0.251)	(0.43)
$Loss_{[t]}$	0.190	0.186	0.004	0.162	0.155	0.007	0.238	0.238	0.000
	(0.393)	(0.389)	(0.16)	(0.369)	(0.363)	(0.222)	(0.427)	(0.427)	(0.00)
$Debt\text{-}to\text{-}Equity_{[t]}$	0.481	0.477	0.004	0.471	0.505	-0.034	0.498	0.430	0.068
	(0.921)	(0.895)	(0.07)	(0.862)	(0.798)	(-0.50)	(1.013)	(1.036)	(0.63)
$Firm\ Liquidity_{[t]}$	0.279	0.289	-0.010	0.301	0.299	0.002	0.242	0.271	-0.029
	(0.185)	(0.191)	(-0.81)	(0.165)	(0.164)	(0.14)	(0.210)	(0.228)	(-1.26)
$CAPX/Assets_{[t]}$	0.076	0.075	0.001	0.072	0.077	-0.005	0.082	0.072	0.010
	(0.060)	(0.060)	(0.18)	(0.047)	(0.051)	(-1.23)	(0.077)	(0.072)	(1.28)
$R\&D/Sales_{[t]}$	0.016	0.021	-0.005*	0.014	0.014	0.000	0.019	0.325	-0.013**
	(0.031)	(0.053)	(-1.71)	(0.025)	(0.025)	(0.19)	(0.040)	(0.078)	(-2.03)
$Institutional\ Owenrship_{[t]}$	0.238	0.221	0.017	0.199	0.177	0.022	0.304	0.294	0.010
	(0.230)	(0.232)	(1.18)	(0.214)	(0.200)	(1.30)	(0.241)	(0.261)	(0.38)
N (by group)	484	484		303	303		181	181	

Table G20: Placebo Test

This table reports results from matched sample regressions of Tobin's Q on a *Treat* × *Post* interaction term. *Treat* is an indicator variable equal to one if the firm is incorporated in a state that adopts a poison pill law, and zero otherwise. *Post* is an indicator variable equal to one in the year of and post pseudo-treatment period, and zero otherwise. The main variables of interest, Q, *Treat* × *Post*, and *Post* are measured contemporaneously, whereas the remaining controls are lagged one period. In this falsification test, we move back the treatment year five years and then consider a plus or minus three-year window. Thus, actual treatment never occurs. Columns (1) – (2) correspond to the full sample, Columns (3) – (4) specific to the "first wave" period, and Columns (5) – (6) to the "second wave" period. Table E1 provides variable definitions. The included controls are: *Poison Pill Firm-Level, Ln(Assets), Ln(Age), HHI, Sales Growth, Loss, Debt-to-Equity, Firm Liquidity, CAPX/Assets, R&D/Sales, Institutional Ownership, State-year Q,* and *Industry-year Q*. Columns (2), (4), and (6) specify: *First Generation Law, Business Combination Law, Control Share Law, Directors' Duties Law,* and *Fair Price Law* dummies. All continuous variables are winsorized at the 1st and 99th percentiles and dollar values are expressed in 2015 dollars. The estimated t-statistics are based on robust standard errors clustered by firm and are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Dep. Variable: $Q_{[t]}$	(*t*-3) to (*t*+3)					
	Full Sample		First Wave (law pseudo adopted: 1986-1990)		Second Wave (law pseudo adopted: 1986-1990)	
Variables	(1)	(2)	(3)	(4)	(5)	(6)
$Treat_{[t]} \times Post_{[t]}$	0.020	0.015	0.036	0.031	-0.058	-0.065
	(0.55)	(0.41)	(1.13)	(0.96)	(-0.62)	(-0.66)
$Post_{[t]}$	0.017	0.017	-0.024	-0.022	0.064	0.066
	(0.50)	(0.49)	(-1.09)	(-1.01)	(0.82)	(0.83)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Other Law Controls	No	Yes	No	Yes	No	Yes
Firm and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
# of firms in regression	809	809	514	514	339	339
N	6,023	6,023	4,003	4,003	2,020	2,020
Adjusted R^2	0.652	0.652	0.709	0.710	0.596	0.596