Shadow Pills and Long-Term Firm Value

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Abstract

This paper 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.

Keywords: poison pill, shadow pill, poison pill statutes, takeover defenses, corporate governance, M&A, innovation, limited commitment, firm value

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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]).[1] 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[2] since the use of the pill was validated by the Delaware Supreme Court in 1985.[3] 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, 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

[1] 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.
[2] 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).
[3] This was the landmark decision in *Moran v. Household*, 500 A.2d 1346 (Del. 1985).

"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.[4] 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)

[4] 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).

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 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.[5] 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 Wittry, 2017). These laws belong to the broader category of

[5] 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.

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.[6]

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[7] 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*.),[8] 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 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

[6] 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.

[7] 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.

[8] *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).

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.[9]

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)[10] and *Grand Metropolitan PLC v. Pillsbury Co.* (November 1, 1988)[11] – 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]).[12] In both of these decisions, the Delaware court halted the continued use of a visible poison pill that 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

[9] 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).
[10] 551 A.2d 787 (Del. Ch. 1988).
[11] 558 A.2d 1049 (1988).
[12] 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.

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.*,[13] 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 poison pills, covering 4,796 unique firms between 1976 and 2016.[14] 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.

[13] 571 A.2d 1140, 1152-55 (Del. 1990).
[14] Firms with missing firm-level poison pill data are excluded from the main sample.

The institutional data sources are the SDC Corporate Governance and the Institutional Shareholder Services (ISS) Governance databases,[15] 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 1 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 1 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 2. To 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.[16] 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.

[15] 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.

[16] 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.

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 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.[17] Table 3, 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 2 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.

[17] 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).

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 3 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 early 2000s, as shown in Figure 2, 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 3. 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 2 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 3, 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 4 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 4 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 3, 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.[18] We provide the mean, standard

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

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*-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 3, 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 4.

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.[19]

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 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 5 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

[19] 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 16 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).

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 2, 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 5 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:

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

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:

$$Q_{ist} = \gamma_i + \omega_t + \beta_1 Post_t + \beta_2 Treat_i \times Post_t + \alpha X_{ist} + \varepsilon_{ist}\,, \quad (4)$$

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 6 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 5, where we find that having a relatively lower *Q* is a statistically significant predictor of the adoption of a visible pill, and Figure 5, 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 6 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.[20] 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

[20] Table A1 in the internet appendix reports the pooled panel regression results split by the time periods 1983 to 1991 and 1994 to 2012, as opposed to Table 6, which considers the entire sample period 1983 to 2012, but splits the waves using indicator variables. While we prefer the specification in Table 6 as it requires that all of the controls have the same coefficients, Table A1 shows that the results are robust to either design.

certain validity of the pill after the 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 7 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 5 and Figure 5 on the likely reverse causality of the negative association between the adoption of a visible poison pill and firm value, the results of

Table 7 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 6 and columns (1) and (2) of Table 7. 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 8 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 8 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 8 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 9 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* × *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 9 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 6 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 10 presents the abnormal returns of value weighted portfolios for the long, short, and long-short portfolios, respectively.[21] Consistent with our *Q* analysis, we split the portfolio results by full sample, first- and second- wave periods, in the respective panels.

In Panel A of Table 10, 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).

[21] We provide results pertaining to equally weighted portfolios in Table A2 of the internet appendix, where the findings are qualitatively similar to those using the CRSP value weighted market factor.

In Panels B and C of Table 10, 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.[22]

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

[22] 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 A3 of the internet appendix. Our main pooled panel and matched sample results hold in these specifications.

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 (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 11 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 11, 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 11 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*.[23] 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 12, where we analyze the value implications of poison pill laws for firms at risk of takeover bids.

In particular, Table 12 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).[24] 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.

[23] The standalone evidence from Table 11 is, however, inconsistent with the managerial entrenchment hypothesis (Cary, 1969).

[24] 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.

Panel A of Table 12 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 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 12 then shows results for the effect of poison pill law treatment status on target firms' takeover premiums in the full matched sample.[25] 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 incorporated in states

[25] 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.

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

6.2.2 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.

6.2.2.1. Poison Pill Laws and Operational Efficiency

In Table 13, 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 13 shows the matched sample regression estimates for our four operational efficiency measures on *Treat* × *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 13 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.

6.2.2.2. 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 14 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 14 reports the estimates from splitting the matched samples into the first- and second- wave adoption periods.[26] 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

[26] Table A4 of the internet 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*.

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.

6.2.2.3. 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 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 15 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 15 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 × 0.213/1.638) gain in *Q* for firms covered by poison pill laws.

In Panel B of Table 15, we then disentangle our analysis for the first- and second- wave matched samples.[27] 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

[27] Table A5 of the internet 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*.

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.

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.[28]

7.1 Poison Pill Laws, Wave Adjustments and Firm Value

[28] 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 A6 of the internet 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 12, 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*.

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 all adopters after 1988 (i.e., from 1989 to 2009). Table 16 reports the results from this robustness test.

Panel A of Table 16 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 6. 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 16 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 17 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*[29] 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).[30] Finally, we scale this total score by three to have a measure that ranges between zero and one.

In Panel B of Table 17, 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

[29] Georgia-Pacific Corp. v. Great N. Nekoosa Corp., 728 F. Supp. 807, 811 (D. Me. 1990) (Maine law).
[30] Dead-hand and no-hand pills, which are prohibited under Delaware case law, allow for a board to provide that the pill survives for a certain period even after the adopting directors are voted off the board.

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.[31]

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*.

8. Robustness Analysis

8.1 Poison Pill Laws and Firm Value with 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.

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

We also include all of the control variables we have maintained throughout our analysis and estimate robust standard errors with firm-level clustering.

Table 18 presents the pooled panel results.[32] 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.

8.2 Poison Pill Laws and Firm Value 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.[33]

[32] 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.

[33] 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

Table 19 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.[34]

8.3 Poison Pill Laws and the 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 20 presents results from these tests. Following the existing literature (e.g., Giroud and Mueller, 2010; Serfling, 2016; Klasa, Ortiz-Molina, Serfling, and Srinivasan, 2017), 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

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 19: Georgia, Idaho, Illinois, Indiana, Massachusetts, Pennsylvania, Rhode Island, South Dakota, and Wisconsin.

[34] Further, Table A7 of the internet appendix investigates the effect of poison pill laws with heterogenous provisions on firm value. We find no differential effect on *Q*.

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 Pill 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 21 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 21 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 internet appendix (Tables A8 – A12) verifying the strength of our main results. In particular, in Tables A8 – A10 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 A11 – A12 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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Figure 1: 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/.

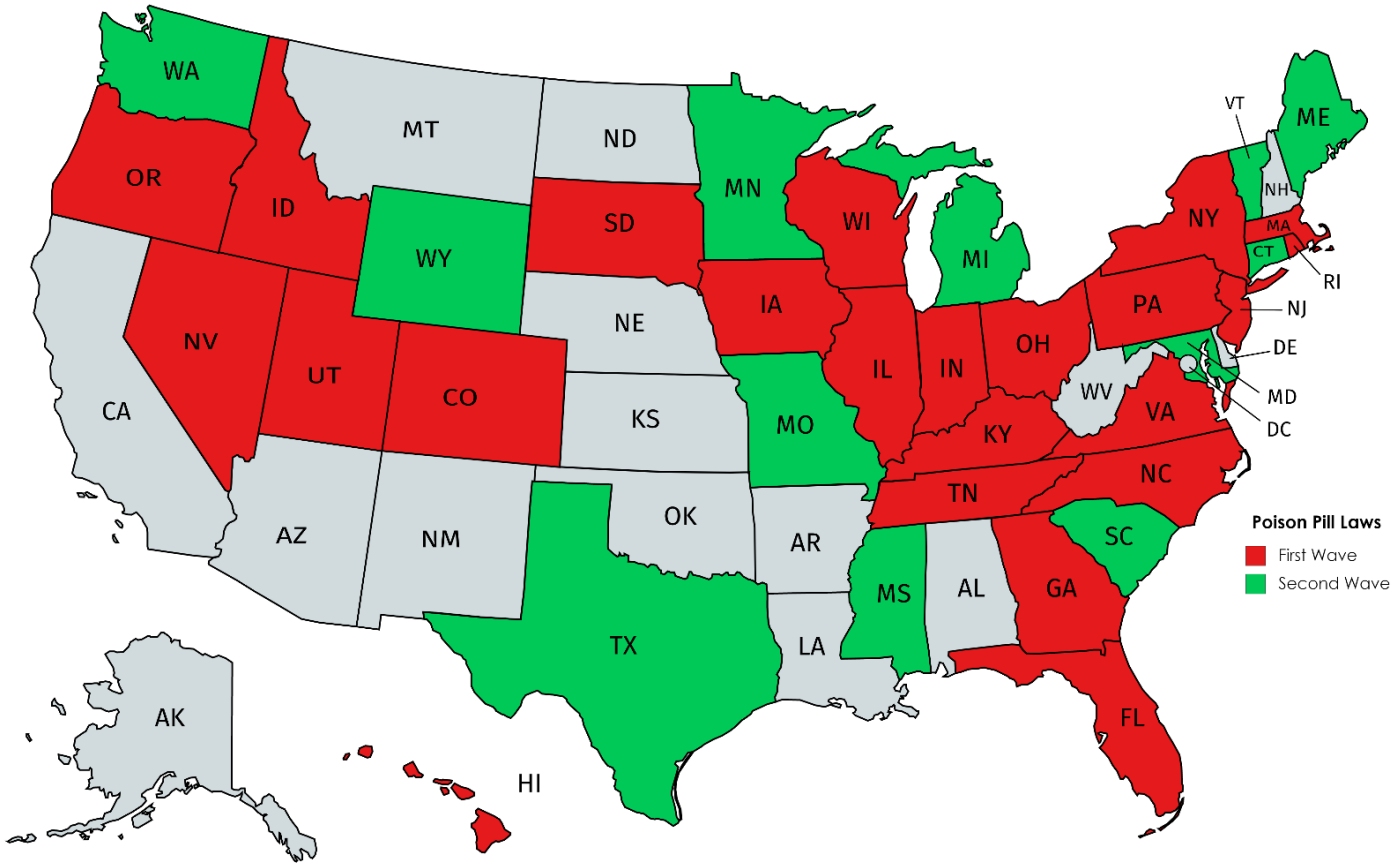


Figure 2: Percentage of Firms With a Poison Pill

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.

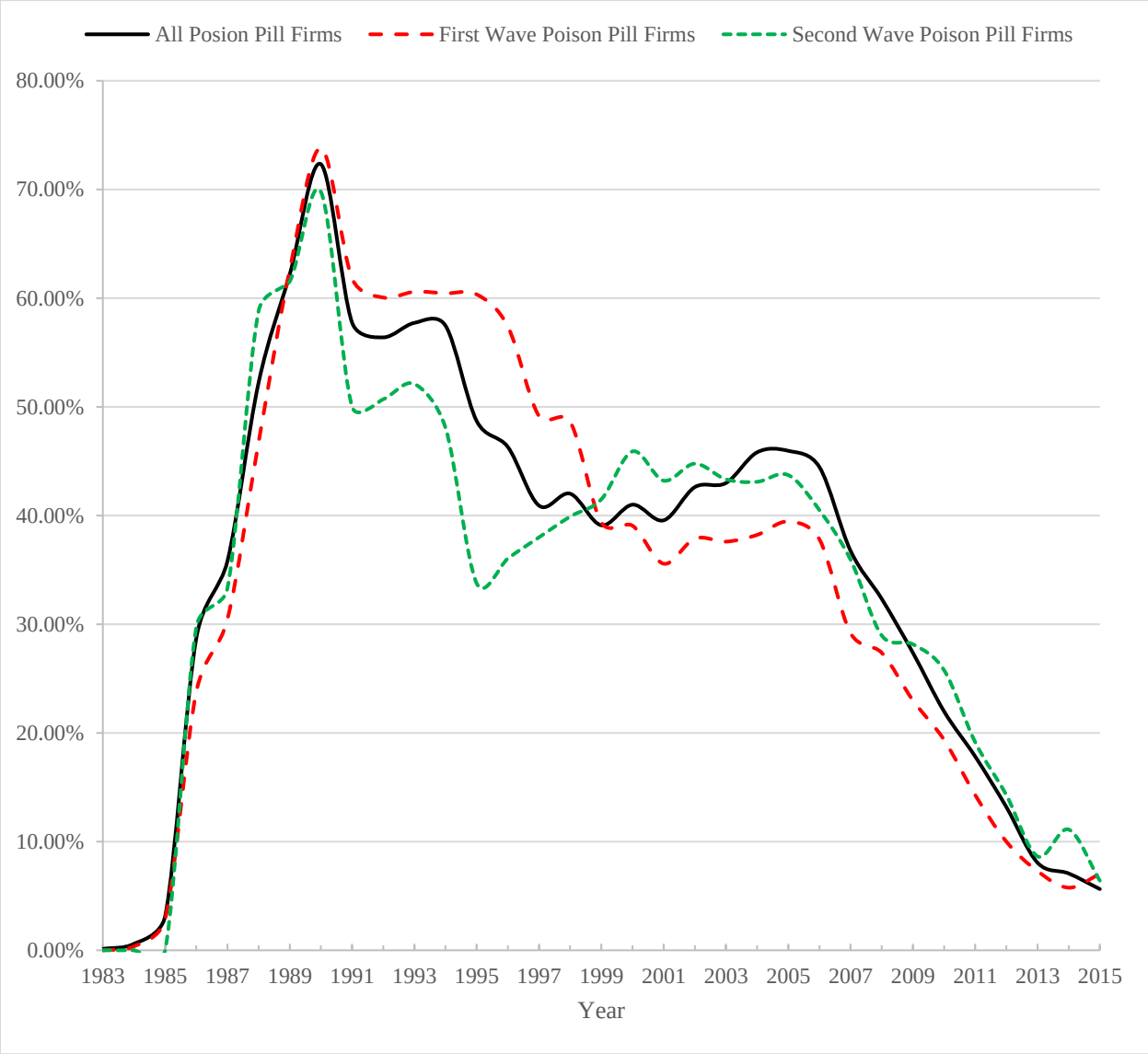
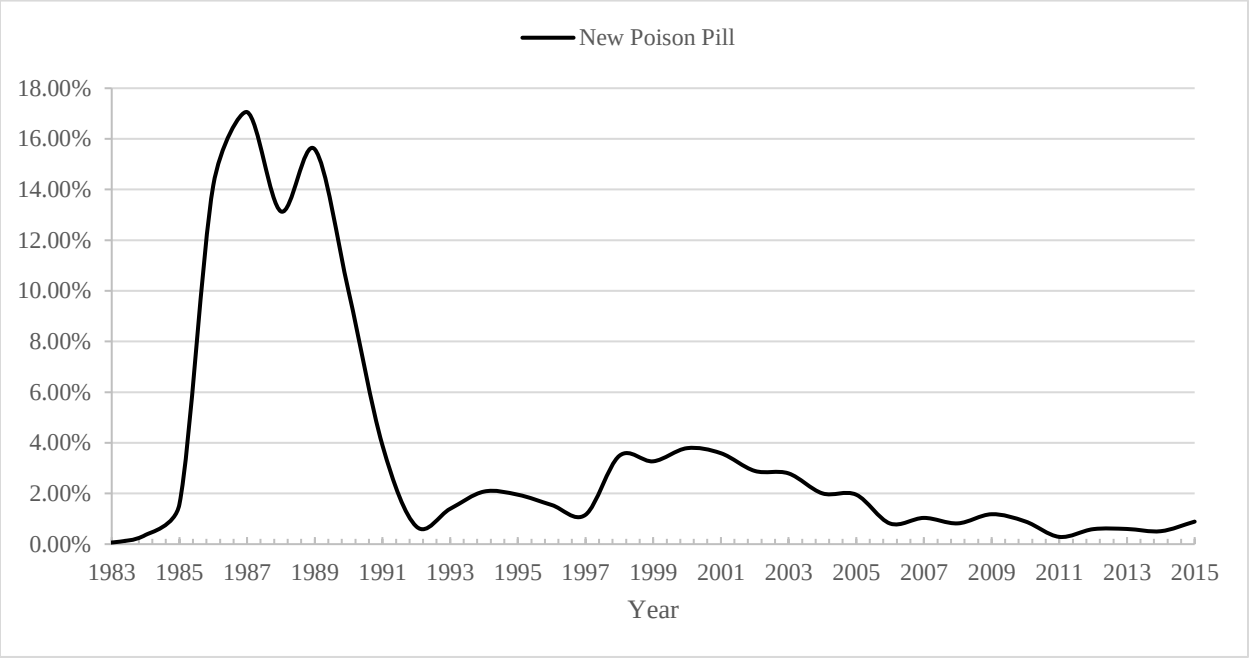


Figure 3: Percentage of Firms Adopting a New or Dropping an Existing Poison Pill

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

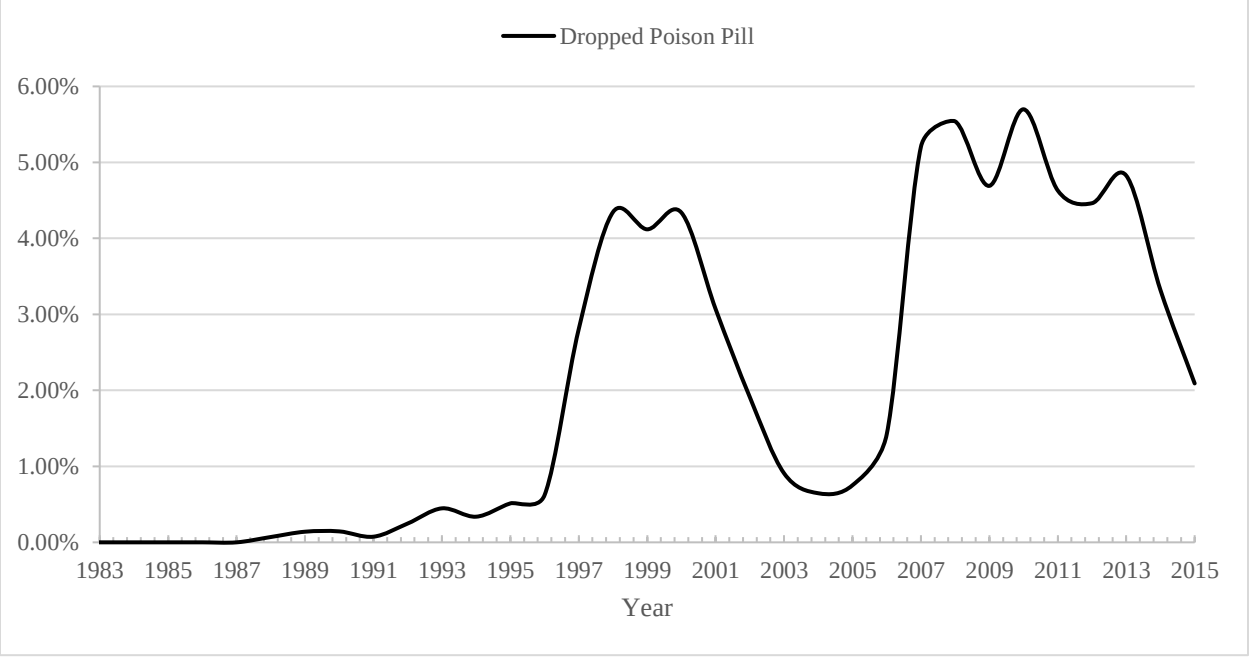


Figure 4: Percentage of Firms Affected by Poison Pill Laws

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.

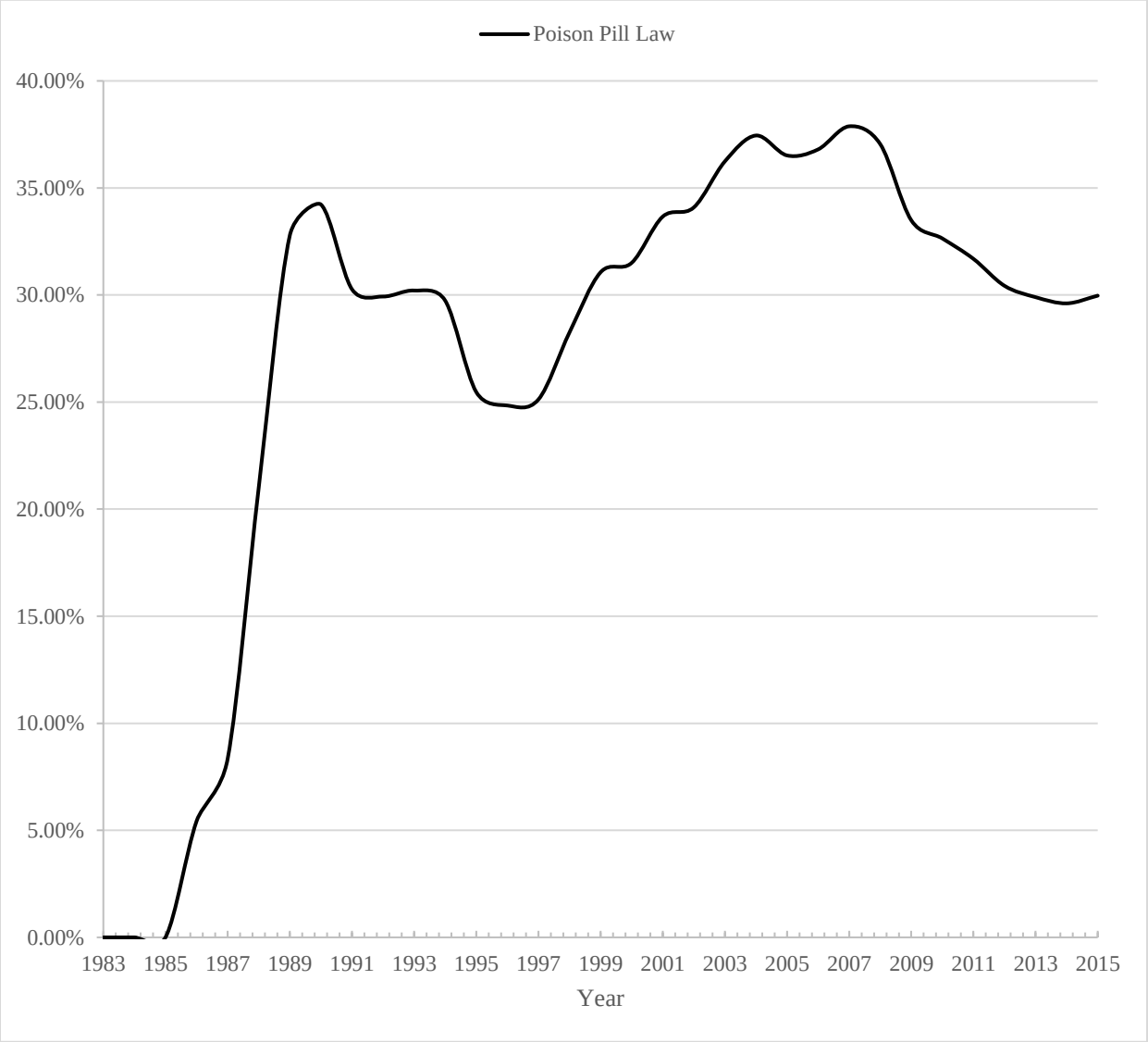


Figure 5: Tobin's Q and Poison Pill Adoption

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.

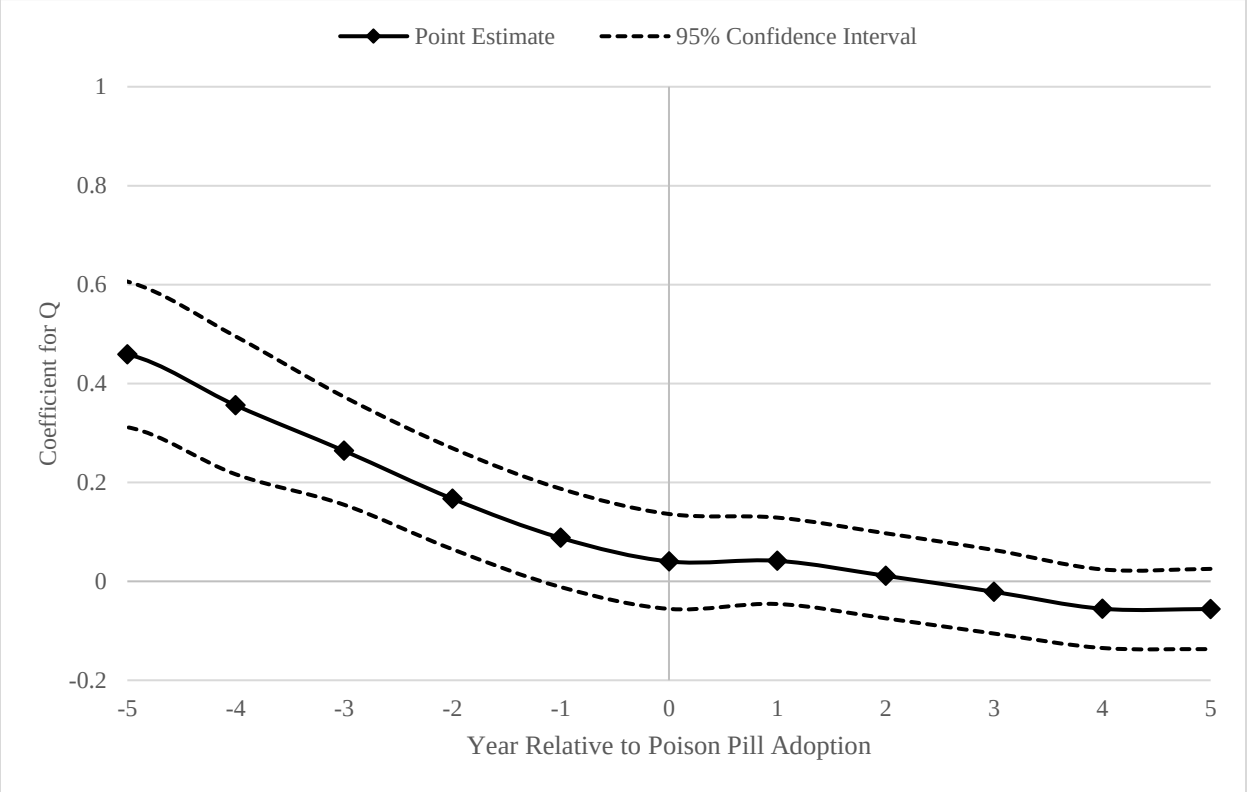


Table 1: Variable Descriptions

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*).
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 (2016) and Karpoff and Wittry (2016).

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 (2016) and Karpoff and Wittry (2016).
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 (2016) and Karpoff and Wittry (2016).
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 (2016) and Karpoff and Wittry (2016).
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 (2016) and Karpoff and Wittry (2016).
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 18.

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).
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.
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 (2016) and Karpoff and Wittry (2016).
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 (2016) and Karpoff and Wittry (2016).
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 Cain, McKeon and Solomon (2016) and Karpoff and Wittry (2016).
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 (2016) and Karpoff and Wittry (2016).
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.
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.

Table 2: 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 (2018). 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.[35]

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[36]		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[37]	4/2002	4	Yes	No	No	Yes	Yes	No
Maryland	5/1999	73	Yes	No	No	Yes	Yes	No

[35] 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 (2018) 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.

[36] 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.

[37] 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.

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 3: 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, 2018). 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 1 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 3 – *(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 3 – *(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 4: 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 1 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)

$Business\ Combination\ Law_{[t-1]}$		-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 5: 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 1 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 6: 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 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 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 7: 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 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 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 8: 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 1 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 8 – *(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 9: 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 1 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)
$Treat_{[t]} \times Post_{[t]} \times$ $Poison\ Pill\ Law\ First\ Wave_{[t]}$							-0.011	-0.008
							(-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 10: 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 10 – (*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 11: 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 1 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.[38]

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

[38] SDC M&A database filter: We require U.S. targets with size of at least $100M, with deal status indicators of either completed, pending, or withdrawn, and the acquisition announcement is for at least control stake of 50% in the target.

Table 11 – *(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 12: 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 1 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 12 – *(Continued)*

Panel B: Poison Pill Laws and Takeover Premiums

	Full sample: (t-3) to (t+3)					
Dep. Variable:	1-$Day\ Premium_{[t]}$		1-$Week\ Premium_{[t]}$		4-$Week\ Premium_{[t]}$	
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		0.405		0.484	
	(0.299)		(0.313)		(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 13: 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 1 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 13 – *(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 14: 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 1 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 14 – (*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 15: 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 1 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 15 – (*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 16: 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 1 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 16 – (*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 17: 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 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 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 17 – (*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 18: 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 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 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 19: 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 1 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 20: 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 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 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 21: 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 1 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

Internet Appendix for
"POISON PILLS AND LONG-TERM FIRM VALUE"
by K.J. Martijn Cremers, Scott B. Guernsey, Lubomir P. Litov and Simone M. Sepe

This Internet Appendix contains 12 supplementary tables to the main article.

Table A1: 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 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 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 A2: 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 A2 – (*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 A3: 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 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 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 A3 – (*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 A4: 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 1 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	-0.028	0.114
	(1.22)	(-0.22)	(1.23)
$Post_{[t]}$	0.009	0.206***	0.096*
	(0.34)	(2.67)	(1.91)
$Poison\ Pill\ Law\ First\ Wave_{[t]}$	-0.053	1.387	0.321
	(-0.49)	(1.07)	(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 A5: 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 1 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)
$Treat_{[t]} \times Post_{[t]} \times Large\ Customer_{[t]} \times Poison\ Pill\ Law\ First\ Wave_{[t]}$	-0.176 (-0.61)		
$Treat_{[t]} \times Post_{[t]} \times Strategic\ Alliance_{[t]} \times Poison\ Pill\ Law\ First\ Wave_{[t]}$		0.097 (0.24)	
$Treat_{[t]} \times Post_{[t]} \times Labor\ Capital_{[t]} \times Poison\ Pill\ Law\ First\ Wave_{[t]}$			-0.572 (-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 (0.41)	0.040 (0.51)	-0.017 (-0.21)
$Post_{[t]}$	0.028 (0.86)	0.023 (0.70)	0.034 (0.79)
$Poison\ Pill\ Law\ First\ Wave_{[t]}$	0.236 (1.27)	0.428 (1.43)	0.415 (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 A6: 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 1 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)
$Labor\ Capital_{[t]}$	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 A7: 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.[39] $Weak\ Pill\ Provision$ 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.[40] 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 1 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

[39] There are three states with dead-hand pill provisions: Georgia, after it amended its earlier statute in 2000, as well as Maryland and Virginia.
[40] There are two states with weak-pill provisions: Both New York and North Carolina explicitly admit judicial review of poison pills.

Table A8: 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 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 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 A9: 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 1 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 A10: 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 1 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)
$Treat_{[t]} \times Post_{[t]} \times$ $Poison\ Pill\ Law\ First\ Wave_{[t]}$							-0.350	-0.343
							(-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 A11: 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 1 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 A12: 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 1 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