

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 quasi-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 matching, higher dimensional fixed effects and portfolio analysis, and support the bonding hypothesis of takeover defenses.

Keywords: poison pill, shadow pill, anti-takeover statutes, firm value, bonding hypothesis, innovation.

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1. Introduction

Law and finance scholars generally agree that the poison pill (formally known as a “shareholder rights plan”) is among the most powerful anti-takeover defenses (Comment and Schwert, 1995; Carney, 2000; Coates, 2000; Daines, 2001; Bebchuk, Coates, and Subramanian, 2002; Cremers and Ferrell, 2014). While details vary across different implementations, the basic defensive mechanism of the pill provides existing shareholders with stock purchase rights that entitle them to acquire newly issued shares at a substantial discount in the “trigger” event that a hostile bidder obtains more than a pre-specified percentage of the company’s outstanding shares.¹ As a result, poison pills grant 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 interests² since the Delaware Supreme Court validated the use of the pill in 1985.³ While earlier findings were inconclusive, over the past decade empirical 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 and poison pills can be unilaterally adopted by the board of directors, so that even firms that do not currently have a poison pill in place still have a “shadow pill” (Coates, 2000). The availability of the shadow pill exacerbates endogeneity concerns, as reverse causality or other omitted variables might explain both the board’s decision to adopt a poison pill and the reported negative association between the adoption of a pill and firm value (Bhagat and Jefferis, 1993; Comment and Schwert, 1995; Catan, 2017).

In this paper, we contribute to the debate on the association between poison pills and firm value by shifting the focus from “visible” pills to shadow pills – i.e., studying the effect that arises from the *right to adopt* a poison pill (which right constitutes the shadow pill) rather than the *actual*

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, see Fleischer & Sussman (2013, §5.01[B][1][2]).

² For example, see Ryngaert (1988); Malatesta and Walkling (1988); Karpoff and Malatesta (1989); Ambrose and Megginson (1992); Bhagat and Jefferis (1993); Brickley, Coles, and Terry (1994); Downen, Johnson and Jensen (1994); Comment and Schwert (1995); Bizjak and Marquette (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).

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 quasi-natural experiment (see e.g., Bertrand and Mullainathan, 2003; Giroud and Mueller, 2010; Gormley and Matsa, 2016; and, more generally, see Karpoff and Wittry, 2018 for a description of these studies). Poison pill laws sanction 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 (2018) and Catan and Kahan (2016) argue that poison pill laws provide plausible exogenous variation in firms' takeover protection and thus constitute a valid quasi-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. We further document that the increase in Tobin's Q is more pronounced for more innovative firms or firms where stakeholder investments are more relevant (e.g., with a large customer or in a strategic alliance). Additionally, we confirm the findings of the previous literature that actual poison pill adoption is negatively associated with Tobin's Q (Gompers, Ishii, and Metrick, 2003; Chi, 2005; Bebchuk, Cohen and Ferrell, 2009; Cremers and Ferrell, 2014), emphasizing the importance of our study's endeavor at disentangling the value implications of the ex-ante *right* to adopt a poison pill relative to the ex-post endogenous decision to put a pill in place.

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 – by strengthening a board's ability to resist a takeover attempt – promotes the undertaking of long-term projects and stronger stakeholder relationships, increasing firm value. Other recent papers that have documented empirical support for the bonding hypothesis include Johnson, Karpoff and Yi (2015, 2018) 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 characteristics. With the exception of the prior adoption of directors' duties statutes (which allow the board to consider non-shareholder interests in corporate decision-making), we find no other significant predictor for the adoption of poison pill laws. Consistent with our identifying assumptions, this suggests that the adoption of these laws is largely exogenous to the legal and economic environment in which they 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. We further confirm that having lower ex-ante firm value also strongly predicts the adoption of a poison pill, as previously documented in Cremers and Ferrell (2014). This 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), in contrast to the view that the adoption of a poison pill causes value-reducing entrenchment of corporate insiders (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 5.2% relative to the sample average Tobin's Q .

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 adopting a poison pill law are matched to "control" firms with similar observable ex-ante characteristics but incorporated in a state without a poison pill law. Confirming the results in our pooled panel Q regressions, the difference in the Tobin's Q between treated and control firms in the matched sample – 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, while the difference in Tobin's Q is significantly positive in the three-year period following the law passage. Additionally, we document that our empirical approach is robust to the inclusion of firm, state of location-by-year and industry-by-year fixed effects, which controls

for unobserved, time-invariant differences across firms as well as unobserved, time-varying differences across headquarter states and industries. Further, we show that stock returns give similar results in a long-term 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.

When we separately consider first-wave poison pill laws (passed in 1986 – 1990) from second-wave poison pill laws (passed during 1995 – 2009), we find that only the second-wave laws are associated with a positive and statistically significant increase in firm value, while the first-wave laws have an insignificant association. We explain this result by 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), the validity of the pill outside Delaware was arguably fairly clear from 1985 – when the Delaware Supreme Court validated the poison pill – 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, many firms, whether incorporated in Delaware or elsewhere, already had access to an effective shadow pill and, in many cases, also already had adopted a visible pill – two circumstances that 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.

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).

Lastly, we examine two possible economic channels through which a shadow pill could contribute to firm value, namely the “bargaining power hypothesis” of Stulz (1988) and Harris (1990), and the “bonding hypothesis” of Shleifer and Summers (1988) and Laffont and Tirole (1988). Under the bargaining power hypothesis, having the right to adopt a poison pill strengthens the bargaining power of the board vis-à-vis any potential bidder, thereby allowing directors to obtain a higher offer price for the target’s shareholders. Alternatively, under the bonding hypothesis, limiting the ability of shareholders to disrupt a firm’s long-term strategy serves to bond other stakeholders more closely to the firm, which helps improve firm value. Consistent with the latter hypothesis, we find that firms incorporated in a state with a poison pill law and for 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 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 these laws.

While ours is the first systematic study to consider the value implications of poison pill laws, and, thus, the strengthening of the ex-ante *right* to adopt a poison pill (i.e., 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 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 the specific impact of these laws on firm value. Karpoff and Wittry (2018) and Fich, Harford and Yore (2017) also consider the adoption of poison pill laws. However, in comparison with Karpoff and Wittry (2018), 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 includes first-wave state laws.⁵ Further, we focus exclusively on the effect of poison pill laws, whereas Fich, Harford and Yore (2017) only use these as a robustness

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, 2018 for a more detailed discussion); other studies further classify the most recent statutes as “third-generation” state takeover laws.

check within their study of the impact of antitakeover protection more generally on the marginal value of cash.

We further contribute to the literature by assembling a comprehensive panel dataset on firm-level visible poison pills. We do so by unifying firm-year pill sample points from two data providers, the SDC Corporate Governance and Institutional Shareholder Services (ISS) Governance databases, four previous academic studies (Comment and Schwert, 1995; Caton and Goh, 2008; Cremers and Ferrell, 2014; and Cremers, Litov and Sepe, 2017), and our own hand-collected Factiva searches. In total, our dataset covers more than 4,700 unique firms and spans 41 fiscal years, allowing us to confirm the findings of the previous literature that the endogenous firm decision to adopt a poison pill is negatively associated with Tobin's Q (Gompers, Ishii, and Metrick, 2003; Chi, 2005; Bebchuk, Cohen and Ferrell, 2009; Cremers and Ferrell, 2014). This affirmation of previous findings highlights the importance of our contribution to disentangle the value implications of the *right* to adopt a poison pill (i.e., the shadow pill) relative to the actual adoption of a pill.

Finally, our results also contribute to the broader 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) or the “quiet life” hypothesis (Hicks, 1935; Holmström, 1979; Grossman and Hart, 1983; Bertrand and Mullainathan, 2003), 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, 2018; 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 firms incorporated in the state of Delaware and promoted the widespread adoption of the pill for both firms incorporated in Delaware and outside of Delaware (Helman and Junewicz, 1986; Fleischer, Hazard, and Klipper, 1988). Most law and finance scholars, however, describe the legal status of the pill for firms incorporated in states other than Delaware as uncertain until these states adopted poison pill laws that validated

the use of the pill (Catan and Kahan, 2016; Cain, McKeon, Solomon, 2017; Karpoff and Wittry, 2018). These laws belong to the broader category of antitakeover laws that a large number of states enacted during the takeover era. In particular, the most prevalent forms of other antitakeover laws are business combination statutes, control share acquisition statutes, fair price statute and directors' duties (or corporate constituency) statutes.⁶

The argument usually given to defend the uncertain status of the poison pill for firms incorporated outside Delaware before the enactment of poison pill laws is that state courts' decisions invalidated the use of the poison pill in the states of New York, New Jersey, Georgia, Wisconsin, Colorado, Virginia and Indiana⁷ between 1986 and 1989 (Karpoff and Wittry, 2018; Catan and Kahan, 2016). 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 often attributed to Delaware judicial decisions over non-Delaware corporations (see, for example, Cremers and Ferrell, 2014) points to the opposite conclusion, namely that the validity of the poison pill was fairly certain in the immediate aftermath of *Moran* for both firms incorporated in Delaware and outside of Delaware. Indeed, the widespread adoption of visible poison pills, even in non-Delaware firms, in the years immediately following *Moran* supports the view that *Moran* was understood to apply to non-Delaware firms as well. This interpretation also finds support in the evidence that state courts' decisions frequently referenced *Moran* in their own poison pill rulings.⁹

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).

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).

Further, state courts' decisions in the period 1986-1990 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 not only that the validity of the pill was possibly not uncertain *before* those decisions, but also that the pill certainly gained validity in those states *after* approval by a favorable state court's decision.

In fact, 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 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 unexpectedly increased uncertainty about the use of the poison pill, although mostly in regard to the redemption of the pill rather than its validity *per se*.¹² In both of these decisions, the Delaware court halted the continued use of a visible poison pill that prevented an unsolicited tender offer, which prompted considerable comment at the time and even induced corporate lawyers to recommend firms to move out of Delaware (Fleischer & Sussman, 2013, §5.08[B][2][A]). This could plausibly explain why several states decided to adopt poison pill laws around 1988-1990, as the viability of the poison pill as a strong defense was no longer assured after *Interco* and *Pillsbury*.

The *Interco* and *Pillsbury* decisions were later reversed by the 1990 Delaware court decision in *Paramount Communications, Inc. v. Time Inc.*,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 tends to depend on 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

¹⁰ 551 A.2d 787 (Del. Ch. 1988).

¹¹ 558 A.2d 1049 (1988).

¹² See Fleischer & Sussman (2013, §5.08[B][2][A]). 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.

¹³ 571 A.2d 1140, 1152-55 (Del. 1990).

plausibly came to completion between 1990 and 1995, 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 data providers, four previous academic studies, and our own hand-collected sample.

The data providers 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 A.1 in the internet appendix provides a brief definition for *Poison Pill Firm-Level* as well as all other variables used in this 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 (2018). Figure A.1 in the internet appendix provides a U.S. map depicting the dispersion of adopting states. The adoption month and years provided by Karpoff and Wittry (2018) are reported in Table A.2 of the internet appendix. To ensure that we employ historically accurate accounts of firms' incorporation status, we use the historical incorporation information from Compact Disclosure for the period 1988 to 2006 and the CRSP Historical U.S.

¹⁴ Firms with missing firm-level poison pill data are excluded from the main sample.

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.

Stock database from 1990 to 2012, as supplemented by the (current) incorporation data provided by Compustat.¹⁶ Combining the poison pill adoption dates and historical incorporation data, we construct the indicator variable, *Poison Pill Law*, which is set equal to one for all affected firms in the year of, and all years after, the respective adoption date and is equal to zero otherwise. Accordingly, this indicator variable is set to zero for all firms incorporated in states without a poison pill law.

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 laws between 1986 and 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 use data from CRSP to analyze stock returns in the period surrounding the adoption of poison pill statutes (see Section 5.4 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*, and *Institutional Ownership*. Data for most of the controls come from Compustat, with the exception of the institutional ownership variable, which is obtained from Thomson Reuters. In some additional specifications, we control for other common forms of state-level takeover laws, including *Business Combination Law*, *Control Share Law*, *Directors’ Duties Law*, and *Fair Price Law* (all defined as in Karpoff and Wittry, 2018).¹⁷

3.2 Descriptive Statistics

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.

¹⁷ Additionally, we exclude firms with observed lobbying activity for specific state antitakeover legislation (Karpoff and Wittry, 2018, Table 3, p. 53).

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 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.¹⁸ Table 1 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 1 provides a more detailed view of the substantial time variation in firm-level visible poison pills over the period 1983 to 2015. Over the period 1983 to 1990, which roughly covers the so-called takeover era of the corporation, there is a precipitous increase in the fraction of firms in the sample with a visible poison pill, with this fraction going from less than 10% in 1985 to more than 70% by 1990. This is followed by a gradual decline, where in 1999 the fraction of firms in the sample with a poison pill in place is roughly 40%. After that, the fraction of firms with a visible poison pill registers only slight variations until 2005, when it starts to decline steadily, with less than 10% of the firms in the sample having a poison pill in place by 2015.

We further refine our investigation into the time series variation of firm-level poison pill adoptions in our sample by considering new pill adoptions. Panel A of Figure A.2 in the internet appendix depicts the percentage of corporations that adopt a new poison pill each year in the period from 1983 to 2015. 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 is fairly clear under the ruling in *Moran*. After 1988, the instances of new adoptions becomes less frequent, with fewer than 4% of sample firms adopting a new poison pill between 1992 and 2015. This suggests that the majority of poison pills in place in the late 1990s through early 2000s, as shown in Figure 1, are likely existing pills that had yet to expire or were reinstated from earlier initial adoptions.

In Panel B of Figure A.2, we also decompose the percentage of firms dropping an existing pill in our sample from 1983 to 2015. Firms begin dropping (either by expiration or through early

¹⁸ Beginning the sample period in 1983 also has the advantage of not overlapping firm-year observations with the so-called first-generation state antitakeover laws, and their effective 1982 invalidation by the U.S. Supreme Court in *Edgar v. Mite Corp.* (Karpoff and Wittry, 2018).

removal) existing poison pills more commonly from 1997 to 2002, and also from 2008 to 2015; a stylized fact which is undetectable from Figure 1 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).

Next, Table 1 shows that the average proportion of firms in our full sample that are incorporated in states that have adopted a poison pill law is 28.4%. Relatedly, Figure A.3 of the internet appendix shows the average percentage 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. This is followed by a gradual decline in covered firms until the second wave of state-level poison pill 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%.¹⁹

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 a plausibly exogenous shock to the takeover protection of firms incorporated in the enacting states (Karpoff and Wittry, 2018), with this shock affecting firm value. An important step in providing evidence for the validity of our identification strategy is thus to investigate whether states were more likely to adopt poison pill laws based on differences in the ex-ante value of the affected firms. If we were to find that states are more likely to adopt poison pill laws when the firms incorporated in the state have relatively high (low) value before the law's adoption, this 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

¹⁹ Panel A of Table A.3 of the internet appendix reports 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 covers the sample from 1995 to 2012. Panel B of Table A.3 shows the full sample summary statistics split by treatment status, where a firm is “treated” if it is incorporated in a state that adopted a poison pill law.

explain the propensity of states to pass a poison pill law, this would undermine the notion that poison pill laws provided an exogenous shock to the takeover protection of the affected firms.

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 state of incorporation and year fixed effects, and estimate standard errors using independent double clustering at both state of incorporation and year levels. We lag all our predictor variables one year and standardize continuous variables to have a mean of zero and unit variance to facilitate comparisons across coefficients. The results of these tests are presented in Table 2.²⁰

Columns (1) and (2) report 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 previously passed a directors’ duties law. In particular, the average incorporating state-year Q is not a significant determinant of passing a poison pill law, which mitigates concerns of reverse causality.

Columns (3) and (4) repeat the analysis for the 1983 –1994 sub-sample, 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 adopts a poison pill law. However, this result is not robust to controlling for other state institutional and macro factors (such as *Business Combination Law*, *Directors’ Duties Law* and $\text{Ln}(\text{Incorp State Per Capita GDP})$) 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. 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

²⁰ To conserve space, Table 2 only presents the main predictor variable coefficients. In the internet appendix, we do include Table A.4, which reports all of the predictor variable estimates and test statistics.

likely to enact poison pill legislation if it has already passed a directors' duties law, and is less likely to adopt a poison pill law if it already has a fair price law or has 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 generally consistent with our main identification assumption.²¹

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

The next step in our identification strategy is verifying whether poison pill laws *did* affect the actual adoption of poison pills by firms incorporated in the enacting states. Specifically, as poison pill laws sanction a firm's right to adopt a visible poison pill – thus strengthening the shadow pill of covered firms – we would expect firms in states with a poison pill law to have a poison pill in place more frequently. To verify this hypothesis, in Table 3 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. We cluster the standard errors by either firm or state of incorporation.

In columns (1) through (3), we examine the marginal effect of a poison pill law on the firm-level decision to adopt a pill provision over the entire period 1983 to 2012. The first two columns indicate that firms incorporated in a state with a poison pill law are 6% to 7.3% (depending on the specification) more likely to have a visible poison pill in place than companies incorporated in states without such legislation. Column (3) includes controls for the existence of other state antitakeover laws (Karpoff and Wittry, 2018) and still finds a positive relation between poison pill laws and the adoption of firm-level pills (albeit the increased probability of having a visible pill has a reduced economic magnitude of 4.2%, as well as reduced statistical significance). These results are again consistent with the assumption that poison pill laws identify valid external shocks to firms' takeover protection.

²¹ We provide additional evidence for the validity of our identification strategy in Figure 2 and Table 5 of Section 5.1.2 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 Figure 2 and Table 5 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).

We next examine whether the documented relationship is wave-specific, separately considering *Poison Pill Law First Wave* and *Poison Pill Law Second Wave*, which are indicator variables that, 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) of Table 3 present the estimates from these linear probability model specifications, where the final column adds controls for other state antitakeover laws.

With or without the additional state antitakeover laws' controls, we find that the adoption of a visible pill for firms incorporated in the first-wave enacting states are not related to 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.²² These findings are consistent with Figure 1, 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 the poison pill laws was likely significantly reduced (consistent with Karpoff and Malatesta, 1989; Karpoff and Wittry, 2018). Conversely, the proportion of firms with a visible poison pill decreases significantly in the second-wave period, suggesting that the poison pill laws enacted during this period have 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 an economically and 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 likely attributable to reverse causality (Catan, 2017).

We 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 "relative year" dummy variables indicating the number of years prior to, and after, the year in which a firm adopts a poison pill, along with year and industry-year fixed effects (following similar specifications in Catan, 2017). The relative year dummies include indicators for up to 10 years before and after a pill's adoption, and the industry grouping is defined at the three-digit SIC code level. We also estimate robust

²² We note that clustering standard errors by firm, instead of state of incorporation, yields the more conservative test statistics.

standard errors with clustering performed by firm. The resulting point estimates and 95% confidence intervals of the relative year dummies are plotted in Figure A.4 of the internet appendix. Consistent with the reverse causality hypothesis, Figure A.4 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 association with Tobin's Q is insignificant in the year before, year of, and up to 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 difference-in-differences pooled panel regressions 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 untreated firms until their state of incorporation passes such a law. Once these previously untreated firms become covered by poison pill laws, they enter the treated group. Accordingly, companies incorporated in states that never passed a poison pill law are always coded as untreated firms. Specifically, we estimate the following pooled panel regression model:

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

where Q_{ist} measures firm value for firm i in incorporating state s during year t , and $\text{Poison Pill Law}_{st}$ is an indicator variable for whether the state in which a company is incorporated has adopted a poison pill law as of year t (i.e., the firm is treated). 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 be correlated with firm value. In addition, we control for time-invariant unobserved heterogeneity across 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, we estimate robust standard errors by either clustering at the firm level (following Petersen, 2009) or state of incorporation (as in Gormley and Matsa, 2016, and Karpoff and Wittry, 2018).

The *Poison Pill Law* coefficient in regression model (1) captures the average effect of poison pill laws on Q over the entire period 1983 to 2012. Given that 23 of the states adopted poison pill legislation prior to 1991 and 12 states enacted this legislation after 1994, with significant differences in legal and business circumstances across these two periods, we also explore whether poison pill laws are differentially associated with firm value in the two waves of

law adoptions by modifying equation (1) with a decomposition of *Poison Pill Law* into *Poison Pill Law First Wave* and *Poison Pill Law Second Wave*.

4.4 Matched Sample

A potential concern about 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 took place over the long-time period of our sample, 1983 to 2012. Therefore, we additionally employ the difference-in-differences methodology in a matched sample that consists of treated and (untreated) 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 incorporated in 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.²³

In constructing our matched sample, we match all treated firms (incorporated in one of the 35 adopting states) to an untreated control firm in the year prior to the year in which the state of incorporation of the treated firm adopted a poison pill law, and where the control firm is incorporated in a state that does not have a poison pill law during the full five-year period afterwards. We assign the controls based on nearest-neighbor matching of propensity scores constructed from 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, for the sample of firms in the period starting three years before the year in which the state where the treated firm is incorporated adopts a poison pill law and ending three years afterwards:

$$Q_{ist} = \beta_1 Post_t + \beta_2 Treated_i \times Post_t + \alpha X_{ist} + \gamma_i + \omega_t + \varepsilon_{ist}, \quad (2)$$

where $Post_t$ is an indicator variable equal to one in the year in which, and the three-year period after, a poison pill law is passed for both treated and control firms, and zero otherwise, and $Treated_i \times Post_t$ 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 . $Treated_i$ is omitted from model (2) due to multicollinearity with

²³ Moreover, using a matched sample allows us to address the concern of firms' reincorporation into (out of) states with (without) state level antitakeover laws.

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 we estimate standard errors clustered by firm. Lastly, we also consider differences in the value relevance of poison pill laws in the matched sample across the two waves.

4.5 Higher Dimensional Fixed Effects

Both the pooled panel and matched sample research designs described above exploit the staggered adoption of poison pill laws across states and time, comparing changes in the value of firms incorporated in states that enact poison pill legislation with those of firms incorporated in states without such laws. Nevertheless, one cannot rule out the possibility that some unobserved time-varying political economy or business cycle factor or both could coincide with or lead to the adoption of poison pill laws, muddying our identification of the value relevance of the shadow pill.²⁴ To assuage concerns that our principal identification strategies are subject to this obfuscation, we implement two additional research designs to control for unobserved, time-varying differences across states of location and industry.

First, in our pooled panel setting, we follow the pioneering approach laid out in Gormley and Matsa (2016), and estimate the following difference-in-differences specification with higher dimensional fixed effects:

$$Q_{ijl st} = \beta \text{Poison Pill Law}_{st} + \alpha X_{ist} + \gamma_i + \omega_{lt} + \lambda_{jt} + \varepsilon_{ijl st} , \quad (3)$$

where $Q_{ijl st}$ measures firm value for firm i , operating in industry j , located in state l , incorporated in state s , during year t . The two new terms, not included in equation (1), are ω_{lt} and λ_{jt} , which denote state-by-year and three-digit standard industrial classification (SIC) industry-by-year fixed effects, respectively. Otherwise, all other terms and fixed effects are as delineated in Section 4.3. Using regression model (3), we are able to compare the changes of firms incorporated in states adopting poison pill laws with firms incorporated elsewhere but sharing the same state of location and three-digit SIC industry, effectively controlling for a multitude of potential sources of unobserved time-varying factors that could confound the analysis. We estimate standard errors clustered by either firm or state of location.

Next, in our matched sample setting, we modify our matching algorithm outlined in Section 4.4 to match exactly on state of location, instead of two-digit SIC industry. Otherwise, the

²⁴ Although, the evidence from Table 2 doesn't suggest that this is the case.

matching strategy is the same, matching exactly on firm-level poison pill status and propensity scores constructed from Q and $Ln(Assets)$. With this alternative matched sample, we estimate:

$$Q_{ijst} = \beta_1 Post_t + \beta_2 Treated_i \times Post_t + \alpha X_{ist} + \gamma_i + \lambda_{jt} + \varepsilon_{ijst}, \quad (4)$$

where model (4) is nearly identical to model (2), except now we add the term λ_{jt} to specify three-digit SIC industry-by-year fixed effects (since we no longer match exactly on industry in favor of exact state of location matching). Whenever possible, we estimate standard errors clustered by either firm or state of location.²⁵

Unfortunately, as mentioned in Section 6.2 of Gormley and Matsa (2016), there are two important limitations with the use of state-by-year fixed effects as an identification strategy in studies that use state-level antitakeover laws. First, there could be a challenge with the amount of available variation if firms affected by antitakeover laws are incorporated and located in the same state. For studies that use business combination laws as a quasi-natural experiment (e.g., Bertrand and Mullainathan, 2003; Giroud and Mueller, 2010; Gormley and Matsa, 2016; and Karpoff and Wittry, 2018), this is less of an empirical concern since Delaware (where 56.4% of the firms in our pooled panel are incorporated, but less than 1% are located) has adopted one of these types of laws.²⁶ In contrast, the super-majority of firms incorporated in a poison pill law state are also located in the same state. For instance, of the 1,035 (9,602) unique treated firms (firm-year observations) in our pooled panel, 71.8% (73.2%) are incorporated and located in the same state. Therefore, employing state-by-year fixed effects greatly reduces the amount of variation available to estimate the effect of poison pill laws on Q .

Second, of the 2,930 (24,224) unique control firms (firm-year observations) in our pooled panel 1,095 (6,423) are specific to firms (firm-years) incorporated in non-Delaware states. Of these unique non-Delaware firms (firm-years), 71.5% (73.1%) correspond to control firms (firm-years) that also are incorporated and located in the same state. By contrast, less than 1% of unique Delaware firms (firm-years) incorporate and locate in the same state. Thus, the use of state-by-year fixed effects effectively reduces our comparison of treated firms that incorporate and locate

²⁵ It is not feasible to cluster by state of incorporation in the second wave since the number of clusters (21) is too few to provide accurate cluster-robust inference (Cameron, Gelbach and Miller, 2008).

²⁶ Specific to the study of Gormley and Matsa (2016, p.437), the author's state: "We are able to obtain estimates for the BC laws' effect even after including state-by-year fixed effects because more than 60% of our firms are incorporated and located in different states." By comparison, only 28.2% of the firms that are affected by poison pill laws in our sample are incorporated and located in different states.

in different states with Delaware firms, and creates a potential “Delaware bias” in our estimates of the effect of poison pill laws on Tobin’s Q .

We address these two sources of limited variation in both the pooled panel with state-by-year fixed effects and the matched sample with exact state of location matching by interacting *Poison Pill Law* and *Treated* \times *Post* with an indicator variable, *Inc-HQ State*, set equal to one if a firm’s state of incorporation and state of location are the same (as in Gormley and Matsa, 2016). Thus, we are still able to account for unobserved, time-varying state of location factors with either fixed effects or matching, while also retaining the available variation from both our affected and unaffected firms that incorporate and operate in the same state.

5. Main Results

5.1. Pooled Sample

5.1.1 Poison Pill Laws and Firm Value

Table 4 reports the difference-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 using the full sample over the period 1983 to 2012. In separate specifications, we decompose the effect of the first-wave (1986 – 1990) from the second-wave (1995 – 2009) poison pill laws in order to assess the importance of the different legal contexts in which first- and second-wave laws were introduced (see Section 2).

Preliminarily, and 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 3 (where we find that having a relatively lower Q is a statistically significant predictor of the adoption of a visible pill) and of Figure A.4 (where we show that firm value is significantly higher in the two to five years before the adoption of a visible pill, but does not significantly decline in the year before, and immediately following, the pill’s adoption), the negative association between visible poison pills and firm value in Table 4 seems plausibly 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 state antitakeover laws in column (3), following Karpoff and Wittry (2018). Economically, and relative to the sample mean’s Tobin’s

Q of 1.859, our estimates suggest an increase in value of 5.2% ($=0.096/1.859$) 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.²⁷ Focusing on column (6), which controls for other state antitakeover laws, 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 an economic magnitude of 10.8% ($=0.200/1.859$). Conversely, the coefficient for firms incorporated in states that adopted poison pill laws during the first wave is insignificant, such that the positive effect of poison pill laws on firm value is entirely driven by the second-wave laws.²⁸

As we argue in Section 2, the different results across waves can be likely explained by the different legal contexts underlying the enactment of the first-wave and second-wave poison pill laws (Karpoff and Wittry, 2018). In particular, the results for the 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 relatively certain validity of the pill after the Delaware courts' 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, second-wave laws plausibly added greater incremental protection when they were introduced as the sorting between pro-pill and anti- (or no) pill states plausibly came to completion when the first-wave of poison pill laws ended in 1990, so that the legal status of the poison pill was uncertain in the states that later enacted the second-wave poison pill laws until the enactment of these laws.²⁹

²⁷ Table A.5 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 4, which considers the entire sample period 1983 to 2012, but splits the waves using indicator variables. While we prefer the specification in Table 4 as it requires that all of the controls have the same coefficients, Table A.5 shows that the results are robust to either design.

²⁸ Inspecting the test statistics by clustering method indicates that clustering by firm is the more conservative approach when decomposing the effect of poison pill laws by wave. This is an important distinction since we find that the second wave laws drive our results. Moreover, as mentioned in footnote 25, when we move to the matched sample, it is no longer feasible to cluster by state of incorporation in the second wave since the number of clusters is too few to provide accurate cluster-robust inference (Cameron, Gelbach and Miller, 2008). Finally, when we explore possible sources of value using heterogeneous effects analysis, the variation we exploit comes from firm-level proxy variables. Therefore, our default approach for estimating standard errors is to cluster by firm.

²⁹ Table A.6 of the internet appendix considers whether the passage of poison pill laws (i.e., the strengthening of the shadow pill) has different value implications depending on whether a firm has adopted a visible poison pill. However,

5.1.2 *Poison Pill Laws and Firm Value Dynamics*

We now move to investigating the timing of the changes in firm value relative to the timing of poison pill law adoptions. These tests are particularly important for assessing the validity of our identification strategy, where we make the assumption that firms incorporated in both affected and unaffected states have similar trends in value prior to the enactment of poison pill legislation (i.e., parallel trends).

Following a similar approach to Gormley and Matsa (2016), we create Figure 2 by regressing Q on firm and year fixed effects, dummies for other state antitakeover laws (Karpoff and Wittry, 2018), and indicator variables signifying the year relative to the adoption date of the poison pill laws. We construct these indicators for up to 10 years before and 26 years after a poison pill law is adopted, with the final dummy equal to one if 7 or more years have elapsed since the poison pill legislation was first passed. We then plot the corresponding coefficient estimates for the \pm five relative year indicators. Furthermore, to display statistical significance, we include plots of the 90% confidence intervals for the coefficient estimates, which we create using robust standard errors clustered by firm. We plot red triangular markers to denote where on the figure we reject the null hypothesis at the 10% significance level. Panel A of Figure 2 shows the results for the full period, while Panels B and C further decompose our test of parallel trends by first and second waves.

All three panels provide graphical evidence that affected firms had similar pre-treatment Q as firms incorporated in unaffected states in the five years prior to the adoption of the respective poison pill laws. However, in Panels A and C, which represents the full and second wave periods, the coefficient estimates for the relative year indicator variables become significantly different from zero in the one through five years after the adoption of the poison pill laws, suggesting that firms with a strengthened right to adopt a poison pill (i.e., a shadow pill in place) experienced significant increases in value relative to firms unaffected by poison pill legislation.³⁰

Table 5 is included as supplementary evidence to the evidence shown in Figure 2. In these regressions, we falsely assign affected status to firms incorporated in states with poison pill

the results from this analysis seems 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).

³⁰ Meanwhile, consistent with our results in Table 4, affected firms in the first wave period (Panel B) do not have differential Q s post-poison pill law enactment.

legislation one and two years before the laws are actually adopted. These indicators, *Poison Pill Law*^[-1 or -2], serve as placebo estimators in our test for parallel trends. Second, we construct the indicator *Poison Pill Law*^[0] to signify the year in which poison pill laws are truly adopted in the firm's state of incorporation. Finally, we create indicators, *Poison Pill Law*^[1, 2, or 3+], which are set equal to one in the respective one, two, and three or more years since the actual adoption of a poison pill law took place.

In all three columns of Table 5, we find a lack of statistical evidence for the placebo estimators (*Poison Pill Law*^[-1 or -2]), consistent with the parallel trends assumption of our research design. In contrast, in the years post-poison pill law adoption (*Poison Pill Law*^[1, 2, or 3+]), we find statistical evidence that firms affected by this legislation experience differentially higher increases in Q relative to firms incorporated elsewhere.³¹ Overall, the evidence in Figure 2 and Table 5, provides reassuring support for the assumption that, absent the adoption of poison pill laws, firms incorporated in states with and without the legislation would have experienced similar trends in Q .

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 relate 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, as well as satisfying 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 incorporated in states that did not pass a poison pill law up to five years after the year that the state of incorporation of the treated firm adopted a poison pill law. 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 one of the 15 states that never passed a poison pill law or a state that adopted this law after July of 2006 (Vermont and Wyoming). We further again distinguish matched samples pertaining to the first and second wave of poison pill laws, respectively.

³¹ Columns (4) – (6) decomposes the dynamics by wave and document that the positive increase in value is attributable to the second wave laws.

Panel A of Table 6 provides summary statistics for the matched variables as of the year prior to treatment ($t-1$).³² Columns (1) – (3) are for the full sample, whereas columns (4) – (6) and (7) – (9) are for the sub-samples covering the first- and second- wave periods, respectively. For each sample, we show the averages for the group of treated and control firms, along with the corresponding differences in means. We find that, in all cases, the groups of treated and control firms are similar. In particular, Q , *Poison Pill Firm-Level*, and $\ln(\text{Assets})$ are never significantly different across the two groups.³³

5.2.2 *Poison Pill Laws and Firm Value*

Panel B of Table 6 reports the results for matched sample Q regressions with pre- and post-treatment windows of three years.³⁴ In columns (1) and (2), we regress Q on $Treated \times Post$, where *Treated* is the indicator variable for treated firms (always equal to one for firms incorporated in poison pill law states, for both the years before and after the adoption of these laws, and always equal to zero for the control firms), and *Post* is an indicator variable equal to one in the year of the adoption and afterwards for all firms, and zero otherwise. We omit the stand-alone variable *Treated* from the regression specification due to multicollinearity with its firm fixed effect, while the stand-alone variable *Post* is estimated with year fixed effects. As done throughout our study, the robust standard errors are clustered at the firm level.³⁵

The results of Table 6, Panel B, shows that our main finding – that firm value on average increases after the state in which a 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 coefficient on $Treated \times Post$ suggest that the treated firms experience an increase in Q of 10.1 percentage points after the adoption of a poison pill law in the state where they are incorporated.

Columns (3) and (4) show the association of poison pill laws and firm value for the 23 first-wave enacting states. Consistent with the pooled panel regressions, we find no significant association in this earlier period. Moving to columns (5) and (6) for the second-wave period, we

³² Table A.7 of the internet appendix presents a similar table of descriptive statistics for the remaining control variables.

³³ For completeness, Panel B of Table A.7 in the internet appendix presents the summary statistics for all firm-year observations over the full period contained in the full, first wave, and second wave matched samples.

³⁴ The use of three years before and after is consistent with our pooled panel Q regressions, which also begin three years before the enactment of poison pill laws by the first adopting states (i.e., Indiana and Ohio) and end three years after the last passage of a poison pill law.

³⁵ As mentioned in footnotes 25 and 28, clustering by state of incorporation is not feasible for the second wave estimates in the matched sample on account of too few clusters.

find that firms incorporated in second-wave enacting states have an average increase in Q of 12.3% ($=0.232/1.892$) to 13.2% ($=0.249/1.892$), relative to the sample mean. This provides further support that our findings in the pooled panel regressions are not an artifact of spurious correlation.

Lastly, similar to Table 4, we test for differential value implications of first- versus second-wave laws in columns (7) and (8) in the matched sample over the full period. These results provide more evidence that the positive value implications of poison pill laws are driven by the 12 second-wave enacting states, while the *Treated* \times *Post* \times *Poison Pill Law First Wave* triple interaction term is statistically and economically insignificant.

5.3 Higher Dimensional Fixed Effects

5.3.1 Pooled Panel

In our next assessment of the robustness of the relation between Q and poison pill laws, we follow Gormley and Matsa (2016) and Karpoff and Wittry (2018) and include state-of-location-by-year and three-digit SIC industry-by-year fixed effects to our baseline model (1) to control for unobserved, time-varying factors at the headquarter state and industry level. What makes this a compelling identification strategy is that this approach allows us to compare two firms operating in the same state and industry where one of them is incorporated in a state that passes a poison pill law (and hence, is treated) while the other is not. We cluster standard errors using either firm or states of incorporation. Panel A of Table 7 presents the results.

Inspecting column (1), we find that the positive value implications of poison pill laws holds with the addition of these higher order fixed effects. In particular, over the full sample period (1983 to 2012) and relative to the sample mean, firms incorporated in states that adopt poison pill legislation experiences an increase in Q of 6.2% ($=0.115/1.859$) relative to firms operating in the same state of location and within the same three-digit SIC industry but incorporated in a state without such a law. As in all previous analyses, column (3) suggests that the source of the documented positive value effect of the enhanced shadow pill stems from laws adopted during the second wave period, and not the first wave.

Next, for added robustness, we also carefully consider where the majority of the available variation in the data is housed. Ideally, the entirety of the treated and control firms would be incorporated and located in different states. Unfortunately, in using poison pill laws only 28.2%³⁶

³⁶ By comparison, in Gormley and Matsa (2016) this figure exceeds 60%.

(28.5%) of our treated (non-Delaware control) firms are incorporated and located in different states. Therefore, to ensure that we do not exclude the supermajority of our variation in treated firms and limit our comparisons to Delaware control firms (“Delaware bias”), we follow a similar approach to Gormley and Matsa (2016, Section 6.2, p.437) and interact *Poison Pill Law* with an indicator variable, *Inc-HQ State*, set equal to one if a firm’s state of incorporation is the same as its state of location. The even-number columns of Panel A, Table 7, document our findings.

In column (2), we show that firms incorporated and headquartered in states that adopt poison pill laws explain the finding in column (1) and exhibit differential increases in value of 8% ($=0.149/1.859$), relative to the sample mean, even after controlling for unobserved, time-varying factors at the state of location and industry levels. In contrast, the standalone effect of *Poison Pill Law* in this specification is statistically insignificant. We attribute this to a lack of power as more than 73.2% of our treated firm-year observations are captured by the interaction term, *Poison Pill Law* \times *Inc-HQ State*, leaving virtually no remaining variation for the instances of poison pill law firms that incorporate and operate in different states. Column (4) finds a similar result to columns (2) and (3), where the second-wave treated firms, incorporated and located in the same state, experience the entirety of the gains in value from the strengthening of the shadow pill.

5.3.2 Matched Sample

To account for unobserved, time-varying political economy and business cycle factors that might confound our matched sample analysis in Subsection 5.2.2, we modify our matching algorithm in the following way. As before, we continue to use exact matching on firm-level poison pill status, and nearest neighbor matching on propensity scores created from Q and $\ln(Assets)$, except now, instead of matching exactly on two-digit SIC industry, we match exactly on state of location. Further, as noted above, almost all of our variation is attributable to treated firms that are incorporated and located in the same state. Therefore, we either interact the *Treated* \times *Post* regressor term with an *Inc-HQ State* dummy variable (column (1)) or cut our matched sample by only including treated firms where their incorporation and location states are the same (columns (2) – (4)). In all our specifications, we include firm and three-digit SIC industry-by-year fixed effects. Panel B of Table 7 reports the results.

We find in column (1) that, using this alternative matching technique to account for state of location unobserved, time-variant factors, the main result holds. That is, firms incorporated and operating in the same state with a poison pill law experience increases in value relative to control

firms operating in the same state and three-digit SIC industry, but without such legislation in their state of incorporation. Specifically, this subset of treated firms exhibit differentially higher Q s, relative to the sample mean, by 6.7% ($=0.125/1.859$). A qualitatively similar result obtains in column (2), where as opposed to interacting $Treated \times Post$ with $Inc-HQ\ State$, we only retain treated firm-year observations where this triple interaction is equal to one. Finally, columns (3) and (4), document, respectively, that it is only the second wave treated firms that are incorporated and located in the same state that display higher Q s post-treatment (column (4)), and not the firms treated during the first wave (column (3)).

Overall, the evidence from Section 5.3 suggests that other unobserved, time-varying sources of variation are not confounding the primary analysis in Sections 5.1 and 5.2.³⁷ Therefore, given the consistency of these supplementary findings and, in order to exploit all of the variation stemming from the enactment of the poison pill laws, and not just those limited cases where the incorporation and operating state are different, and to retain the full pool of control firms (i.e., minimize the “Delaware bias”), we maintain our primary pooled panel and matched sample research design with firm and year fixed effects for the remaining sections.

5.4 Portfolio Analysis

As a robustness check to the Q regressions, we follow previous studies (Gompers, Ishii, and Metrick, 2003; Bebchuk, Cohen, and Ferrell, 2009; Cremers and Ferrell, 2014; Cremers, Litov, and Sepe, 2017), and perform a long-term stock return event study on the firms in the matched sample in the period surrounding the adoption of poison pill laws. We construct long (short) portfolios in calendar time of stocks from the treated (control) group that are held in the period around the time – i.e., the event date – that their (matched sample counterpart’s) state of incorporation adopts a poison pill law. We always start including stocks six months before the event date, and then hold these until two years after the event date (“6m24,” with the numbers referring to months) or until three years after the event date (“6m36”). Table 8 presents the abnormal returns of value weighted portfolios for the long, short, and long-short portfolios, respectively,³⁸ and we also again show results for the full sample period, as well as first- and second- wave periods, in Panels A, B and C, respectively.

³⁷ This is also consistent with the evidence in Table 2.

³⁸ We provide results pertaining to equally weighted portfolios in Table A.8 of the internet appendix, where the findings are qualitatively similar to those using the value weighted portfolios.

In Panel A of Table 8, we report the results from the above portfolios for the matched sample over the full period. 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 has insignificant abnormal returns that are close to zero. For example, the value-weighted stock portfolio of treated firms has a four-factor abnormal stock return of 0.85% per month (t-statistic of 2.21) over the 30-month period starting six months before the event date and ending two years after, while the analogous stock portfolio of control firms has a monthly alpha of 0.04% (t-statistic of 0.15). The long-short portfolio that buys stocks of treated firms and shorts stocks of control firms has positive and significant abnormal returns. These results are consistent with the Q regressions, in spite of the inherently noisy estimation of abnormal returns of portfolios containing a limited number of stocks (on average 62 to 72 stocks, depending on the length of our holding period).

In Panels B and C of Table 8, we consider the portfolios in the first- and second-wave periods, respectively. Consistent with our considerations about 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, in the second-wave period, the long portfolio of stocks of treated firms has a positive and strongly statistically significant abnormal return using the “6m24” holding period portfolios, and a positive and marginally statistically significant abnormal return using the “6m36” holding period. Meanwhile, the short portfolios of stocks of control firms have insignificant alphas, whereas the long-short portfolios are positive and significant (sometimes marginally so) in both the four-factor and three-factor models and in both holding periods. Overall, we conclude that the portfolio analysis yields congruent results to the Q regressions and provides further robustness to our main finding that poison pill laws have positive corporate value implications.³⁹

6. Two Channels

6.1 Hypotheses

³⁹ We additionally provide results for a second alternative measure of firm value, *Total Q* (as proposed by Peters and Taylor, 2017) in Table A.9 of the internet appendix. Our main pooled panel and matched sample results hold in these specifications.

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 hypotheses for this relation: the “bargaining power hypothesis” and the “bonding hypothesis,” respectively. The first hypothesis is rooted in the rationale that strengthening the board’s ability to resist a hostile takeover attempt increases the board’s power to “bargain” with a potential bidder and, ultimately, to extract a higher purchasing price for the benefit of the target’s shareholders (Stulz, 1988; Harris, 1990). Conversely, the second hypothesis posits that board’s power to deter hostile takeovers benefit shareholders by allowing a firm to more credibly “bond” itself to longer-term operational strategies, strategies that would otherwise be at risk of reversal if the firm underwent a change of control (Shleifer and Summers, 1988; Laffont and Tirole, 1988).

6.2 *The Bargaining Power Hypothesis: Discussion*

In order to test the bargaining power hypothesis, we conduct the following empirical tests. First, we examine in Table A.10 of the internet appendix whether the right to adopt a poison pill, as strengthened by the adoption of a poison pill law, alters the likelihood that a treated firm will receive a bid (*Bid*) and/or be successfully acquired (*Acquired*). However, we do not find any evidence that firms incorporated in poison pill law adopting states, irrespective of wave, are more or less likely to receive a takeover bid or be acquired.

Next, since the evidence from the first test can only verify a necessary but insufficient condition of the bargaining power hypothesis, we also explore the value ramifications in the following two ways. In Table A.11, Panel A, of the internet appendix, we investigate whether treated firms incorporated in states or industries with a greater threat of takeover have differentially higher *Q*s. In general, we do not find evidence that the association between firm valuations and poison pill laws is stronger during times with more takeover activity in the state of incorporation or the firm’s industry.⁴⁰ As a final test of the bargaining power hypothesis, we then consider

⁴⁰ For the first-wave period, we do find that treated firms with higher levels of takeover activity in their respective industries experience significantly negative changes in *Q*. This suggests that in addition to our main institutional justification for the differential impact of the first-wave and second-wave poison pill laws, there could also be a complementary explanation, namely that the firms affected by the first-wave and second-wave poison pill laws had different economic characteristics. We consider this in Table A.12 of the internet appendix, where we test for pre-treatment year (*t*-1) differences between first- and second-wave treated firms (as well as between 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,

whether the takeover premium itself is positively related to the adoption of poison pill laws, as that hypothesis would suggest. The results in Panel B of Table A.11 of the internet appendix show no evidence that takeover premiums are higher after the adoption of a poison pill law, irrespective of wave, and whether we control for the other four state antitakeover laws.⁴¹

In summary, we do not find evidence that poison pill laws increase the treated firms' bargaining power relative to firms incorporated in states without such legislation, as neither takeover activity nor takeover premiums⁴² can explain our main result of a positive association between firm value and the passage of these laws.

6.3 *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 channel. As mentioned above, this hypothesis posits that companies that are relatively better shielded from the threat of a takeover can more credibly commit to specific long-term operational strategies, which would promote increased firm value. In order to test this hypothesis, we explore whether a firm's ability to bond to given corporate policies through a more certain right to adopt a poison pill is related to gains in either operational efficiency or Tobin's Q.

6.3.1 *Poison Pill Laws and Operational Efficiency*

In Table 9, we employ four different proxies of operational efficiency as dependent variables. The first proxy is return on assets (*ROA*) calculated as operating income before depreciation and amortization 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 (*SG*), which is defined as the natural logarithm of current-period's sales divided by last-period's sales.

2003), the takeover wave in the 1980s might have emerged as a response to managerial entrenchment or the existence of inefficient conglomerates. Under this view, takeover defenses may reduce the likelihood that under-utilized assets might be put to more efficient uses through a takeover. This interpretation could explain the result in column (4) of Table A.10 for the interaction between first-wave poison pill laws (which were enacted during the apex of the 1980s takeover era, unlike the second-wave laws) with Industry-Year M&A Volume, which has a negative and statistically significant association with Q.

⁴¹ Given that our pool of matched firms is restricted to firms with non-missing firm-level pill data and that we are estimating regressions around tight three-year windows, we only have 129 deals with non-missing premium data. Due to having such a small sample, we only consider the matched sample over the full period.

⁴² In additional tests, we scale the three premium measures by proxies for firm fundamentals (e.g., book equity, earnings, and ROA) to alleviate concerns that the non-result is biased by market anticipation of higher bargaining power. Even after this transformation, we do not find that poison pill laws increased takeover premiums for treated firms.

In our specifications, we consider whether the adoption of poison pill laws help predict improvements in operational efficiency for the matched sample, using difference-in-differences regressions with the standard set of controls, firm and year fixed effects, and also controlling for the presence of other state antitakeover laws.

Panel A of Table 9 shows the matched sample regression estimates for our four operational efficiency measures on $Treated \times Post$ over the full period. We find that firms incorporated in states adopting a poison pill law experience statistically significant increases in three out of four of these measures of operational efficiency. For example, in column (1), the adoption of a poison pill law is associated with an increase in *ROA* of 6.9% ($=0.009/0.130$) over the sample mean. Similar increases in operational efficiency hold for *NPM* (column (2)) and *SG* (column (4)).

Further, we separately consider first- versus second-wave poison pill laws in Panels B and C, respectively, and find that the increases in operational efficiency only occur for firms incorporated in states adopting laws during the second-wave period (1995 to 2009). In particular, the results in Panel C suggest that all four proxies of operational efficiency – *ROA*, *NPM*, *OM*, and *SG* – experience an economically meaningful and statistically significant increase after the adoption of a poison pill law. On the other hand, the coefficient of $Treated \times Post$ is insignificant in each of columns (1) – (4) in Panel B. In sum, Table 9 provides some initial evidence supporting the bonding hypothesis, indicating that treated firms, which are arguably better able to commit to longer-term corporate strategies because they have a stronger shadow pill, experience increases in operational efficiency.

6.3.2 Poison Pill Laws, Innovation and Firm Value

If the shadow pill serves as a commitment device that better enables boards of directors to consider the long-term interests of a firm's stakeholders, as implied by the bonding hypothesis, the adoption of poison pill laws should matter more for innovation-intense firms. Innovation often requires firm-specific investments by employees, suppliers, customers, or strategic alliance partners. These stakeholders themselves may be more reluctant to commit to significant firm-specific investments if they worry about a possible reduction of their investment in case the firm is taken over. Therefore, the bonding hypothesis suggests that having a stronger shadow pill could be useful to reduce the risk of ex-post expropriation of the stakeholders' specific investments in the firm, which may be particularly relevant for firms that are 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 for the importance of innovation and other long-term firm-specific investments. The first proxy is *R&D/Sales*, which measures the intensity of corporate expenditures in research and development activities (Bushee, 1998; Chan, Lakonishok, and Sougiannis, 2001; Eberhart, Maxwell, and Siddique, 2004) and which we construct using financial data from Compustat. The second proxy, *Intangible Capital/Assets*, is a "catch-all" measure for the importance of intangible capital, which is more likely to require firm-specific investments (Core, Holthausen, and Larcker, 1999; Duru, Wang, and Zhao, 2013). *Intangible Capital/Assets* comes from the data provided by Peters and Taylor (2017) on WRDS, where it is a component of their Total Tobin's Q (*Total Q*) measure. Our third proxy *Knowledge Capital/Assets* is another "catch-all" measure for both the significance of knowledge capital like R&D and intellectual property assets, as well as the complex nature of operations behind the use of these assets, which may require more firm-specific investments. This measure is also provided by Peters and Taylor (2017) on WRDS, as it constitutes another input of *Total Q*.

Panel A of Table 10 shows the results for each of these proxies interacted with *Treated* \times *Post* in the matched sample over the full period. Consistent with the bonding hypothesis of the shadow pill, columns (1) – (3) indicate that all three of our proxies for innovation 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 8.3% ($=0.399 \times 0.339 / 1.638$) for firms incorporated in states with a poison pill law relative to matched controls with average intangible assets.

Panel B of Table 10 reports the estimates from splitting the matched samples into the first- and second-wave adoption periods.⁴³ Columns (1) and (3) show that companies with higher levels of *R&D/Sales* and *Knowledge Capital/Assets* experience a larger 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% ($=3.336 \times 0.036 / 1.458$) higher *Q* after their state of incorporation adopts a first-wave poison pill law relative to firms with average R&D and absent such legislation. In columns (4) – (6), all three triple interaction coefficients are again positive and statistically significant. Hence, while on average first-wave poison pill laws

⁴³ Table A.13 of the internet appendix further splits our results in the full sample by wave using the quadruple interaction term *Treated* \times *Post* \times *Innovation Proxy* \times *Poison Pill Law First Wave*.

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.

6.3.3 Poison Pill Laws, Stakeholder Relationships and Firm Value

Our next set of specifications to test the shadow pill's bonding hypothesis consider three proxies intended to measure more directly the importance of stakeholder relationships. The first proxy variable, *Large Customer*, is an indicator variable equal to one if the firm has a large customer of at least 5% of total sales, based on customer sales from the Compustat Segment database. We interpret this variable as a measure of the significance of customers who are likely to have a longer-term association with the firm (Johnson, Karpoff, and Yi, 2015; Fich, Harford, and Yore, 2017). The second proxy, *Strategic Alliance*, is an indicator variable of whether the business has a long-term partnership with another company (Bodnaruk, Massa, and Simonov, 2013), set equal to one if the firm participates in an active strategic alliance, and zero otherwise. The data for this measure comes from the Thomson Reuters SDC M&A database. The third proxy aims to capture the level of importance of employees through the ratio of selling, general and administrative expenses over the book value of total assets, *Labor Capital*, based on Compustat data (Lev and Radhakrishnan, 2005; Eisfeldt and Papanikolaou, 2013).

Table 11 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. Panel A considers the full period using the full set of control variables including the indicator variables for other state antitakeover laws, as well as firm and year fixed effects. Consistent with the bonding hypothesis of the shadow pill, we find in column (1) that firms with a *Large Customer* that are incorporated in a state adopting a poison pill law experience an additional increase in Q of 6.2% ($=0.101/1.638$) 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% ($=0.641 \times 0.213/1.638$) gain in Q for firms covered by poison pill laws. This latter result is strongly statistically significant, while the results in columns (1) and (2) are only marginally statistically significant.

In Panel B of Table 11, we again disentangle our analysis for the first- and second-wave periods.⁴⁴ The results indicate 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. Our justification for this in Section 2 considers the different legal contexts underlying the enactment of the first-wave and second-wave poison pill laws. In brief, we argue that under the pervasive influence of Delaware case law, the validity of the pill even outside Delaware was fairly clear after the 1985 decision in *Moran* and until at least 1988, when subsequent Delaware decisions (*Interco* and *Pillsbury Co.*) re-injected uncertainty into the validity of the pill. Therefore, during the 1985-1988 period in which most of the first-wave poison pill laws were enacted, many firms arguably already had an effective shadow pill in place, which likely reduced the importance of introducing poison pill laws. Conversely, by the start of the second wave of poison pill laws in 1995, states that had not yet adopted a poison pill law had clearly selected an anti- (or at least not-openly favorable) poison pill policy, so that second-wave laws significantly strengthened the shadow pill for the firms incorporated in the enacting states.

In this section, we conduct two 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 (rather than 1986 to 1990) and the latter to consist of laws adopted from 1989 to 2009 (rather than 1995 to 2009). Additionally, we consider the results when we either exclude Delaware firms from the sample during the first wave of poison pill laws and include them as controls during the second wave or exclude them entirely from the sample. 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 verify whether our results are stronger for firms incorporated in states with a relatively stronger legality of the shadow pill given the details of that state's poison pill law.

7.1 Poison Pill Laws, Wave Adjustments and Firm Value

⁴⁴ Table A.14 of the internet appendix further splits our results in the full sample by wave using the quadruple interaction term $Treated \times Post \times Stakeholder\ Relationship\ Proxy \times Poison\ Pill\ Law\ First\ Wave$.

In this subsection, we test whether our main results are robust to redefining the first and second wave periods around the two 1988 Delaware court decisions that injected novel uncertainty on firms' ability to maintain a pill (*Interco* and *Pillsbury Co.*). Indeed, shortly after these decisions, eleven states (or 31.4% of the total affected states) adopted poison pill laws in 1989. As a robustness check, we redefine the first-wave period from 1986 to 1988 and the second wave period as all years after 1988 (i.e., from 1989 to 2009) and treat Delaware firms differently. In particular, we exclude Delaware firms in the first-wave period, because even if Delaware firms were subject to the *Moran* decision during this period, Delaware never adopted a poison pill law. We then include Delaware firms as control observations in the second-wave period, reflecting the uncertainty injected over the use of the pill, and thus the strength of the shadow pill, by the 1988 Delaware courts' decisions. Table 12 reports the results from this robustness test, using both the full sample and the matched samples.

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 4. 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 8% ($=0.149/1.859$), relative to the sample mean, after their state of incorporation adopts a poison pill law. We further obtain similar results in the matched sample regressions (columns (4) – (6)).⁴⁵

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 across states of incorporation in the legal validity of the shadow pill. Methodologically, we use poison pill laws and poison pill case law information from Cain, McKeon, and Solomon (2017) to build an index ranging 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.

⁴⁵ Table A.15 of the internet appendix 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.

Table A.16 of the internet appendix describes the construction of the *PPV-Index* in detail. 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 incorporate the widely-held view that the validity of the pill remained more uncertain in non-Delaware states in the period after *Moran* and before the enactment of state-level poison pill laws (Catan & Kahan, 2016; Karpoff and Wittry, 2018, Cain, McKeon, and Solomon, 2017). Hence, the *PPV-Index* is set equal to one for Delaware companies after *Moran* and to one-half for all others before the incorporating state has adopted a poison pill law. Next, the impact of validating or invalidating state court decisions is captured by (i) increasing the value of the *PPV-Index* to one whenever a state experiences a court case that reinforces the validity of the shadow pill, and (ii) setting the *PPV-Index* to zero for firms incorporated in a state after a court case in that state invalidates the use of poison pills (as an example, a New Jersey court ruled against pill provisions in 1985, the same year as *Moran*).

Further, following Cain, McKeon, and Solomon (2017, p.471), we incorporate the view that the legal status of the poison pill outside Delaware was subsequently clarified by the 1990 *Georgia-Pacific v. Great Northern*⁴⁶ decision under Maine law, which ruled that the poison pill is valid. Accordingly, we set the *PPV-Index* equal to one for firms incorporated in Maine (i.e., similar to firms incorporated in Delaware after *Moran*). We also set the index value to one for all 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 general legal validity of the shadow pill was no longer in doubt after *Georgia-Pacific*).

Finally, we set the value of the *PPV-Index* to two for firms incorporated in states that adopted a poison pill law, as these state statutes sanctioned the legal certainty of the poison pill's validity above and beyond the decisions of state courts, and set the index to three for firms incorporated in states that have either a poison pill law or a court case that validates the use of “strong” poison pills (e.g., a dead-hand or no-hand pill).⁴⁷

⁴⁶ *Georgia-Pacific Corp. v. Great N. Nekoosa Corp.*, 728 F. Supp. 807, 811 (D. Me. 1990) (Maine law).

⁴⁷ Dead-hand and no-hand pills, which are prohibited under Delaware case law, are such that the pill survives for a certain period even after the adopting directors are voted off the board.

Another complication pertains to the state of Arizona. We set the value of the *PPV-Index* of firms incorporated in the state of Arizona to two after Arizona adopts a poison pill law (again as in Cain, McKeon, and Solomon, 2017). However, Karpoff and Wittry (2018) do not list Arizona as a state that adopted pill legislation, and in our own reading of the Arizona law, the language seems ambiguous. Therefore, we consider the association between the *PPV-Index* and firm value in *Q* regressions using two different matched samples: one in which we include Arizona firms (see columns (1) and (2) of Table 13), and another in which we exclude Arizona firms (columns (3) and (4)), in order to verify that our results are robust to this possible ambiguity.⁴⁸

In columns (1) and (2) of Table 13, we find that increases in the *PPV-index* (i.e., the strengthening of the shadow pill) is associated with increases in firm value. For instance, in the second column, which include controls for other state antitakeover laws as well as firm and year fixed effects, *Q* increases by 1.7% ($=0.032/1.859$) when a firm is incorporated in a state that goes from the Georgia-Pacific levels of certainty (*PPV-Index*=1) to that engendered by a poison pill law (*PPV-Index*=2). The coefficients of *PPV-Index* in columns (3) and (4) for the sample that excludes Arizona firms are identical to those in columns (1) and (2) for the sample that includes Arizona firms. Overall, these results confirm 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. Conclusion

This paper contributes 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 quasi-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. Moreover, using a comprehensive dataset of firm-level visible pills, we confirm the findings of the previous literature on the negative association between Tobin's *Q* and actual adoption of a pill.

⁴⁸ The entirety of our analysis is robust to the inclusion or exclusion of the 16 sample firms incorporated in Arizona.

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 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: 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 (i.e., from 1986 to 1990), and those incorporated at any time in states passing a poison pill law during the second wave period (i.e., from 1995 to 2009). Excluded from the sample are financial and utility firms.

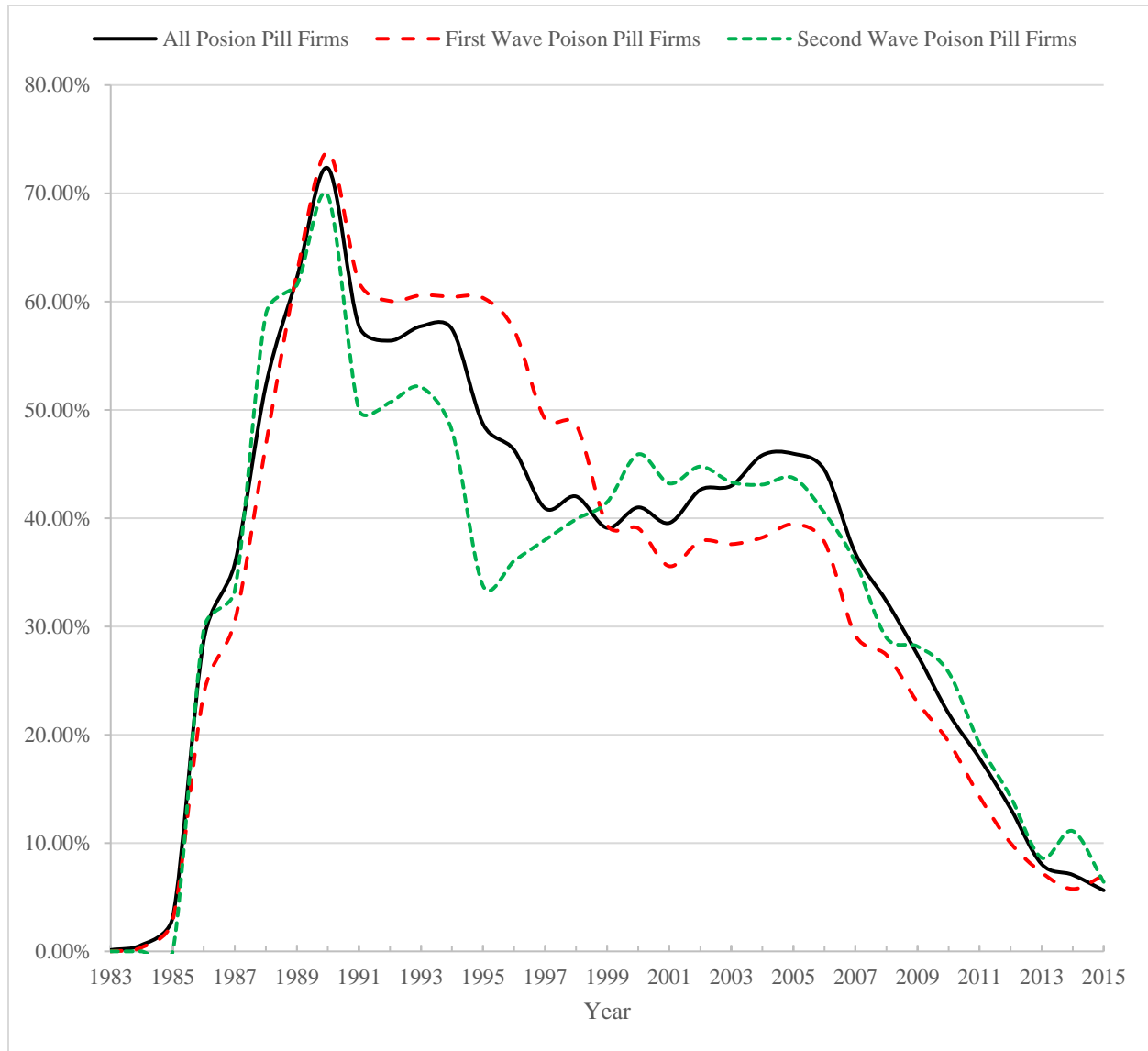


Figure 2: Poison Pill Laws and the Timing of Firm Value Implications

This figure shows the impact of a poison pill law on Q . Panel A plots the coefficient estimates from regressing Q on firm and year fixed effects, four other antitakeover laws, and dummy variables indicating the year relative to the adoption date of the poison pill law on the y-axis. Our dummies are created for up to 10 years before and 26 years after their adoption dates. The last dummy is set to one if 7 or more years have expired after the adoption date of the poison pill law and zero otherwise. Panels B and C supplement the top panel by decomposing poison pill law adoptions into the first (1986 to 1990) and second (1995 to 2009) waves. The x-axis in all three panels shows the time relative to the adoption date of the respective poison pill law. Dashed lines correspond to the 90% confidence intervals of the coefficient estimates, calculated from robust standard errors clustered by firm. Red triangles denote significance at the 10% level. The sample period is from 1983-2012.

Panel A: Full Sample Poison Pill Laws

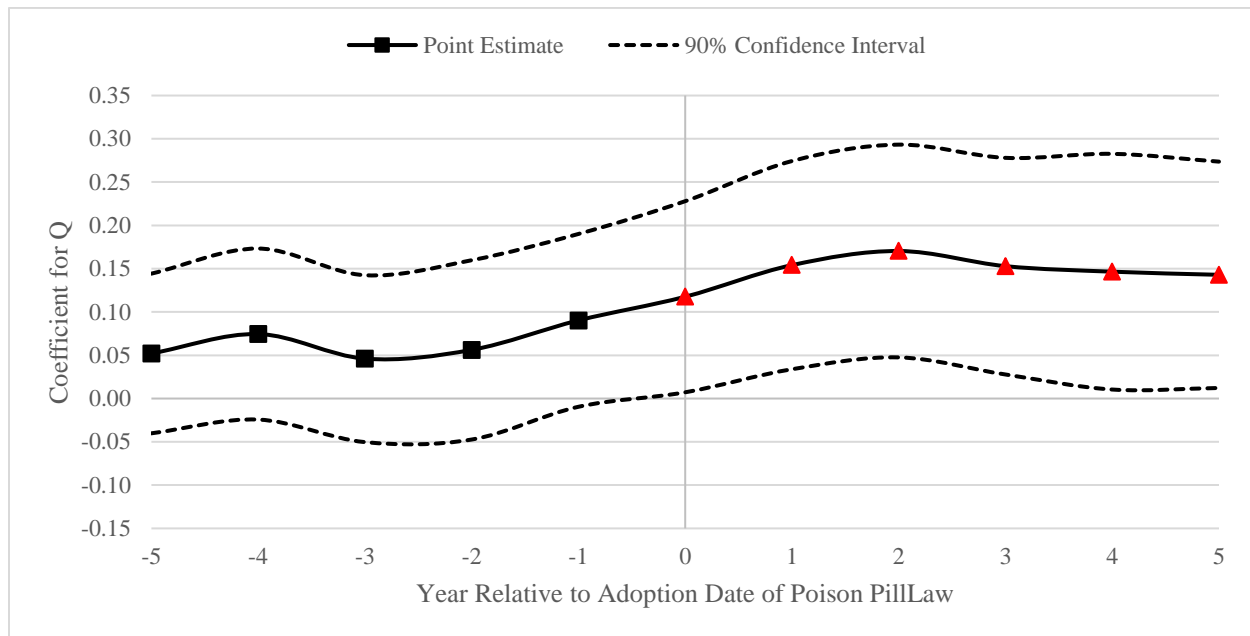
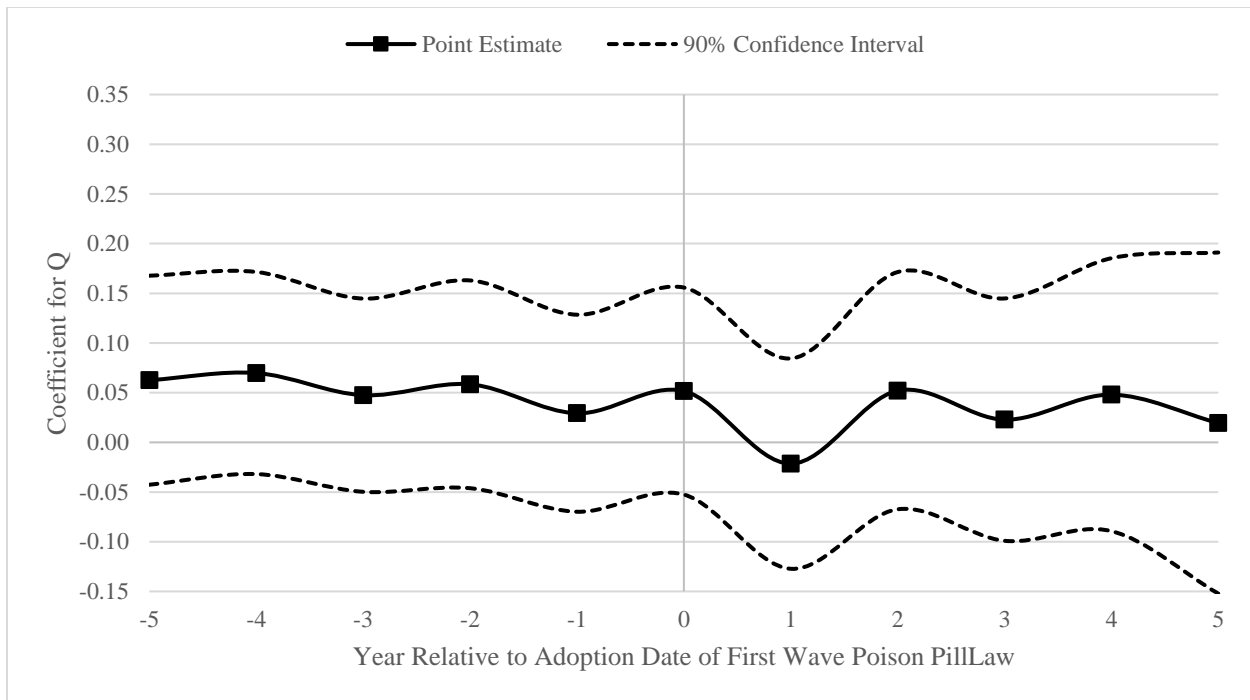


Figure 2 – (Continued)

Panel B: First Wave Poison Pill Laws



Panel C: Second Wave Poison Pill Laws

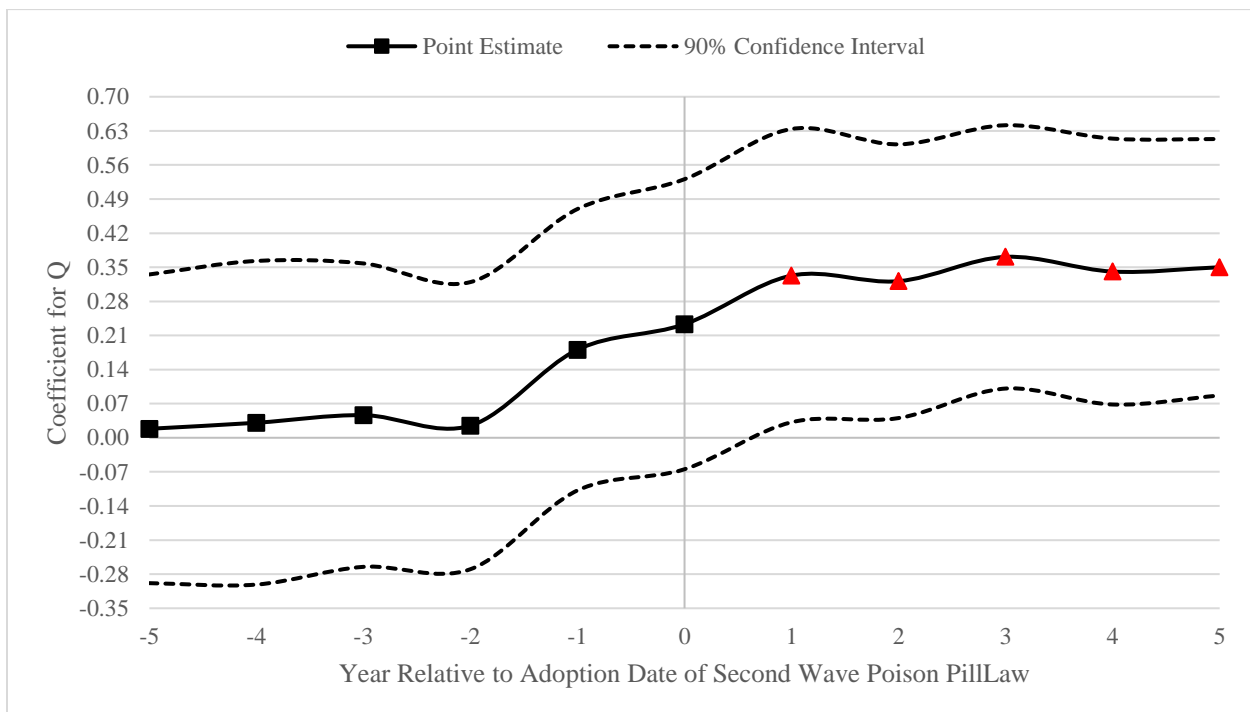


Table 1: Summary Statistics

This table reports full sample summary statistics for the main dependent and explanatory variables used in the pooled panel regressions. 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 that 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 A.1 provides variable definitions. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

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.
<i>Poison Pill Law</i> _[t]	0.284	0.451	0	0	1	33,826
<i>Poison Pill Firm-Level</i> _[t]	0.391	0.488	0	0	1	33,826
<i>Business Combination Law</i> _[t]	0.779	0.415	1	1	1	33,826
<i>Control Share Law</i> _[t]	0.239	0.427	0	0	0	33,826
<i>Directors' Duties Law</i> _[t]	0.283	0.451	0	0	1	33,826
<i>Fair Price Law</i> _[t]	0.288	0.453	0	0	1	33,826
$\ln(\text{Assets})_{[t]}$	7.026	1.753	5.933	7.007	8.169	33,826
$\ln(\text{Age})_{[t]}$	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
<i>Sales Growth</i> _[t]	0.045	0.231	-0.039	0.042	0.130	33,826
<i>Loss</i> _[t]	0.215	0.411	0	0	0	33,826
<i>Debt- to- Equity</i> _[t]	0.551	1.364	0.026	0.307	0.704	33,826
<i>Firm Liquidity</i> _[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
<i>Institutional Ownership</i> _[t]	0.450	0.333	0.061	0.496	0.736	33,826

Table 2: 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. Other insignificant predictors specified but unreported to conserve space include *R&D Tax Credit*, *Percent Incorp State Republican*, *Incorp State GDP Growth*, and incorporation state-year averages of: *Ln(Assets)*, *Ln(Age)*, *HHI*, *Sales Growth*, *Loss*, *Firm Liquidity*, *CAPX/Assets*, *R&D/Sales* and *Institutional Ownership*. Table A.1 provides variable definitions. Table A.4 shows this table in its entirety with all predictor variable estimates. 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 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: <i>Poison Pill Law</i> _[<i>t</i>]						
	1983 – 2012		First Wave (1983 – 1994)		Second Wave (1995 – 2012)	
Variables	(1)	(2)	(3)	(4)	(5)	(6)
<i>Incorp State-Year Q</i> _[<i>t</i>-1]	-0.009 (-0.09)	0.009 (0.09)	-0.008 (-0.06)	0.106 (0.90)	0.080 (0.57)	0.113 (0.86)
<i>Incorp State-Year M&A Volume</i> _[<i>t</i>-1]	-0.008 (-0.57)	-0.004 (-0.27)	0.044 (1.04)	0.047 (1.35)	0.002 (0.12)	-0.003 (-0.26)
<i>Industry-Year M&A Volume</i> _[<i>t</i>-1]	-0.001 (-0.83)	-0.001 (-0.70)	-0.002 (-1.20)	-0.003 (-0.84)	-0.001 (-0.65)	-0.002 (-0.96)
<i>Incorp State-Year Poison Pill Firm-Level</i> _[<i>t</i>-1]	-0.218 (-1.04)	-0.180 (-0.82)	-0.093 (-0.27)	-0.311 (-1.06)	0.010 (0.04)	-0.177 (-0.58)
<i>Incorp State-Year Debt-to-Equity</i> _[<i>t</i>-1]	-0.099 (-0.82)	-0.043 (-0.41)	-0.223* (-1.70)	-0.089 (-0.81)	0.133 (0.64)	0.129 (0.69)
<i>Business Combination Law</i> _[<i>t</i>-1]		-0.078 (-0.58)		-0.268** (-2.23)		0.150 (1.02)
<i>Control Share Law</i> _[<i>t</i>-1]		0.064 (0.54)		0.088 (0.65)		-0.318 (-1.04)
<i>Directors' Duties Law</i> _[<i>t</i>-1]		0.388** (2.28)		0.429** (2.43)		0.471*** (2.61)
<i>Fair Price Law</i> _[<i>t</i>-1]		0.033 (0.27)		0.136 (1.13)		-0.568** (-2.35)
<i>Ln(Incorp State Per Capita GDP)</i>		0.140 (1.26)		0.307** (2.01)		-0.254* (-1.72)
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 3: 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* and *R&D/Sales*. Further, columns (3), and (6) specify “Other law controls”: *Business Combination Law*, *Control Share Law*, *Directors’ Duties Law*, and *Fair Price Law* dummies. Table A.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 estimated using robust standard errors with either firm level or state of incorporation clustering. We report in parentheses both clustering strategies *t*-statistics for the main variables of interest and, to conserve space, firm-level clustering *t*-statistics for the remaining controls. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dep. Variable: <i>Poison Pill Firm-Level</i> _[t]						
1983 – 2012						
Variables	(1)	(2)	(3)	(4)	(5)	(6)
<i>Q</i> _[t-1]	-0.038***	-0.021***	-0.021***	-0.038***	-0.021***	-0.021***
Firm clustering	(-8.09)	(-4.59)	(-4.60)	(-8.13)	(-4.63)	(-4.65)
Incorporation state clustering	(-12.51)	(-9.83)	(-9.76)	(-12.64)	(-9.83)	(-9.78)
<i>Poison Pill Law</i> _[t]	0.073***	0.060***	0.042*			
Firm clustering	(3.60)	(2.96)	(1.79)			
Incorporation state clustering	(3.62)	(4.15)	(1.96)			
<i>Poison Pill Law First Wave</i> _[t]				0.033	0.034	-0.002
Firm clustering				(1.37)	(1.37)	(-0.08)
Incorporation state clustering				(1.41)	(1.60)	(-0.07)
<i>Poison Pill Law Second Wave</i> _[t]				0.124***	0.093***	0.070**
Firm clustering				(3.86)	(2.95)	(2.21)
Incorporation state clustering				(3.75)	(4.84)	(2.73)
<i>Ln(Assets)</i> _[t-1]		0.038***	0.038***		0.038***	0.038***
		(3.32)	(3.27)		(3.32)	(3.26)
<i>Ln(Age)</i> _[t-1]		0.562***	0.560***		0.560***	0.558***
		(13.92)	(13.93)		(13.85)	(13.88)
<i>HHI</i> _[t-1]		-0.121***	-0.120**		-0.122***	-0.121***
		(-2.57)	(-2.54)		(-2.59)	(-2.57)
<i>Sales Growth</i> _[t-1]		-0.032***	-0.032***		-0.032***	-0.031***
		(-2.85)	(-2.83)		(-2.81)	(-2.77)
<i>Firm Liquidity</i> _[t-1]		-0.113***	-0.116***		-0.113***	-0.115***
		(-3.03)	(-3.11)		(-3.02)	(-3.09)
<i>Institutional Ownership</i> _[t-1]		0.098***	0.097***		0.098***	0.097***
		(2.87)	(2.83)		(2.86)	(2.83)
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 4: 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 "Other law controls": *Business Combination Law*, *Control Share Law*, *Directors' Duties Law*, and *Fair Price Law* dummies. Table A.1 provides variable definitions. All continuous variables are winsorized at the 1st and 99th percentiles and the dollar values are expressed in 2015 dollars. t -statistics are estimated using robust standard errors with either firm level or state of incorporation clustering. We report in parentheses both clustering strategies t -statistics for the main variables of interest and, to conserve space, firm-level clustering t -statistics for the remaining controls. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Dep. Variable: $Q_{[t]}$						
1983 – 2012						
Variables	(1)	(2)	(3)	(4)	(5)	(6)
<i>Poison Pill Law</i> $_{[t]}$	0.110***	0.120***	0.096**			
Firm clustering	(2.81)	(3.13)	(2.01)			
Incorporation state clustering	(2.22)	(2.19)	(1.78)			
<i>Poison Pill Law First Wave</i> $_{[t]}$				0.025	0.020	-0.076
Firm clustering				(0.59)	(0.48)	(-1.45)
Incorporation state clustering				(0.84)	(0.64)	(-1.58)
<i>Poison Pill Law Second Wave</i> $_{[t]}$				0.218***	0.245***	0.200**
Firm clustering				(2.72)	(2.70)	(2.57)
Incorporation state clustering				(3.15)	(3.61)	(2.86)
<i>Poison Pill Firm-Level</i> $_{[t-1]}$	-0.217***	-0.105***	-0.106***	-0.219***	-0.107***	-0.108***
	(-7.40)	(-3.82)	(-3.85)	(-7.45)	(-3.88)	(-3.92)
$\ln(\text{Assets})_{[t-1]}$		-0.416***	-0.416***		-0.416***	-0.417***
		(-13.95)	(-13.97)		(-13.98)	(-14.01)
$\ln(\text{Age})_{[t-1]}$		-0.264***	-0.262***		-0.270***	-0.265***
		(-2.95)	(-2.94)		(-3.02)	(-2.97)
<i>Sales Growth</i> $_{[t-1]}$		0.357***	0.357***		0.359***	0.359***
		(8.95)	(8.96)		(8.99)	(9.00)
<i>Loss</i> $_{[t-1]}$		-0.079***	-0.078***		-0.079***	-0.078***
		(-4.46)	(-4.42)		(-4.47)	(-4.42)
<i>Debt- to- Equity</i> $_{[t-1]}$		-0.018***	-0.018***		-0.018***	-0.018***
		(-3.54)	(-3.52)		(-3.53)	(-3.52)
<i>Firm Liquidity</i> $_{[t-1]}$		0.292***	0.295***		0.294***	0.296***
		(2.96)	(3.00)		(2.99)	(3.02)
<i>CAPX/Assets</i> $_{[t-1]}$		0.620***	0.615***		0.617***	0.610***
		(3.15)	(3.13)		(3.14)	(3.11)
<i>R&D/Sales</i> $_{[t-1]}$		0.187**	0.186**		0.188**	0.188**
		(1.98)	(1.98)		(2.00)	(2.00)
<i>Institutional Ownserhip</i> $_{[t-1]}$		0.223***	0.223***		0.223***	0.224***
		(3.55)	(3.54)		(3.55)	(3.57)
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	0.597	0.597	0.567	0.597	0.598

Table 5: Poison Pill Laws and the Timing of Firm Value Implications

This table reports results for pooled panel regressions of Tobin's Q on poison pill law indicator variables over the period 1983 to 2012. *Poison Pill Law*^[-1 or -2] is an indicator variable equal to one if a firm is incorporated in a state that will adopt a poison pill law in one or two years 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 adopted a poison pill law in the current year and equal to zero otherwise. *Poison Pill Law*^[1 or 2] is an indicator variable equal to one if a firm is incorporated in a state that adopted a poison pill law one or two years ago and equal to zero otherwise. *Poison Pill Law*^[3+] is an indicator variable equal to one if a firm is incorporated in a state that adopted a poison pill law three or more years ago and equal to zero otherwise. *Poison Pill Law First Wave*^[1] and *Poison Pill Law Second Wave*^[1] are defined in a similar manner. Control variables are lagged one-year and those included in columns (2) – (3) and (5) – (6) are: *Poison Pill Firm-Level*, *Ln(Assets)*, *Ln(Age)*, *HHI*, *Sales Growth*, *Loss*, *Debt-to-Equity*, *Firm Liquidity*, *CAPX/Assets*, *R&D/Sales* and *Institutional Ownership*. Columns (3) and (6) further specify: *Business Combination Law*, *Control Share Law*, *Directors' Duties Law*, and *Fair Price Law*. Table A.1 provides variable definitions. Continuous variables are winsorized at the 1% level in both tails and dollar values are expressed in 2015 dollars. *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: $Q_{[t]}$						
1983 – 2012						
Variables	(1)	(2)	(3)	(4)	(5)	(6)
<i>Poison Pill Law</i> ^[-2] _[t]	-0.010 (-0.26)	-0.012 (-0.31)	0.010 (0.25)			
<i>Poison Pill Law</i> ^[-1] _[t]	0.020 (0.53)	0.001 (0.03)	0.020 (0.49)			
<i>Poison Pill Law</i> ^[0] _[t]	0.060 (1.18)	0.051 (1.03)	0.066 (1.19)			
<i>Poison Pill Law</i> ^[1] _[t]	0.120** (2.17)	0.112** (2.08)	0.109* (1.72)			
<i>Poison Pill Law</i> ^[2] _[t]	0.140*** (2.58)	0.146*** (2.74)	0.147** (2.17)			
<i>Poison Pill Law</i> ^[3+] _[t]	0.115** (2.40)	0.127*** (2.74)	0.129** (1.96)			
<i>Poison Pill Law First Wave</i> ^[-2] _[t]				0.025 (0.97)	0.019 (0.73)	0.031 (1.14)
<i>Poison Pill Law First Wave</i> ^[-1] _[t]				0.014 (0.45)	0.009 (0.29)	-0.002 (-0.05)
<i>Poison Pill Law First Wave</i> ^[0] _[t]				0.054 (1.34)	0.045 (1.12)	0.010 (0.24)
<i>Poison Pill Law First Wave</i> ^[1] _[t]				-0.007 (-0.16)	-0.006 (-0.15)	-0.085 (-1.62)
<i>Poison Pill Law First Wave</i> ^[2] _[t]				0.067 (1.22)	0.074 (1.36)	-0.008 (-0.12)
<i>Poison Pill Law First Wave</i> ^[3+] _[t]				0.028 (0.57)	0.018 (0.38)	-0.080 (-1.28)
<i>Poison Pill Law Second Wave</i> ^[-2] _[t]				-0.062 (-0.93)	-0.049 (-0.73)	-0.052 (-0.77)
<i>Poison Pill Law Second Wave</i> ^[-1] _[t]				0.014 (0.19)	-0.018 (-0.27)	-0.024 (-0.35)
<i>Poison Pill Law Second Wave</i> ^[0] _[t]				0.064 (0.70)	0.057 (0.63)	0.050 (0.55)
<i>Poison Pill Law Second Wave</i> ^[1] _[t]				0.236** (2.30)	0.230** (2.32)	0.182* (1.78)
<i>Poison Pill Law Second Wave</i> ^[2] _[t]				0.216** (2.33)	0.220** (2.38)	0.170* (1.77)

<i>Poison Pill Law Second Wave</i> ^[3+] _[t]				0.248*** (2.85)	0.296*** (3.48)	0.246*** (2.77)
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	0.587	0.587	0.566	0.588	0.588

Table 6: Poison Pill Laws and Firm Value in the Matched Sample

This table reports summary statistics and regression results for a matched sample. *Treated* firms are defined as firms incorporated in states that adopt poison pill laws, whereas control firms are incorporated in states without such laws. We use nearest-neighbor matching with replacement in year $t-1$ to create a sample matched on Q and $\ln(\text{Assets})$, and exactly on two-digit SIC codes and firm-level poison pill status. Panel A presents the pre-treatment year summary statistics for the matched variables. The column “Difference (t -stat)” shows the difference between treated and control sample means (test statistic in parentheses). Panel B provides matched sample regression estimates. *Post* is an indicator variable equal to one in the year of and post treatment period. The main variables of interest, Q , $Treated \times Post$, and $Post$ are measured contemporaneously, whereas remaining controls are lagged one year. *Treated* is omitted due to multicollinearity with its firm fixed effect. *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. Table A.1 provides variable definitions. Included controls: $\ln(\text{Assets})$, $\ln(\text{Age})$, *HHI*, *Sales Growth*, *Loss*, *Debt-to-Equity*, *Firm Liquidity*, *CAPX/Assets*, *R&D/Sales* and *Institutional Ownership*. Even numbered columns specify the other law controls. All other interaction terms are unreported to conserve space. Continuous variables are winsorized at the 1% level in both tails, 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.

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

	Full Period			First Wave Period			Second Wave Period		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Matched Variables:	Treated	Control	Difference	Treated	Control	Difference	Treated	Control	Difference
$Q_{[t]}$	1.564 (0.992)	1.552 (0.924)	0.012 (0.20)	1.418 (0.555)	1.396 (0.468)	0.022 (0.52)	1.752 (1.343)	1.753 (1.269)	-0.001 (-0.01)
<i>Poison Pill Firm-Level</i> $_{[t]}$	0.346 (0.476)	0.346 (0.476)	0.000 (0.00)	0.332 (0.472)	0.332 (0.472)	0.000 (0.00)	0.363 (0.482)	0.363 (0.482)	0.000 (0.00)
$\ln(\text{Assets})_{[t]}$	6.391 (1.898)	6.437 (1.766)	-0.046 (-0.40)	7.075 (1.642)	6.941 (1.489)	0.133 (1.02)	5.505 (1.844)	5.784 (1.883)	0.279 (1.58)

Panel B: Matched Sample Regression Results with ($t-3$) to ($t+3$) Estimation Windows

Dep. Variable: $Q_{[t]}$	Full Period		First Wave Period		Second Wave Period		Full Period w/Interaction	
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Treated_{[t]} \times Post_{[t]}$	0.113** (2.22)	0.101* (1.65)	0.004 (0.08)	0.021 (0.40)	0.254** (2.37)	0.236** (2.02)	0.249*** (2.66)	0.232** (2.44)
$Treated_{[t]} \times Post_{[t]} \times Poison\ Pill\ Law\ First\ Wave_{[t]}$							-0.036 (-0.08)	-0.033 (-0.08)
$Post_{[t]}$	0.013 (0.33)	0.015 (0.36)	0.014 (0.42)	0.003 (0.08)	-0.001 (-0.02)	-0.001 (-0.01)	0.006 (0.15)	0.011 (0.26)
<i>Poison Pill Firm-Level</i> $_{[t-1]}$	0.012 (0.28)	0.012 (0.27)	0.011 (0.29)	0.011 (0.28)	0.011 (0.09)	0.012 (0.10)	0.006 (0.14)	0.006 (0.13)
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	0.661	0.702	0.702	0.636	0.637	0.663	0.663

Table 7: Poison Pill Laws and Firm Value with Higher Dimensional Fixed Effects

This table reports the results for higher dimensional fixed effects regressions. Panel A provides pooled panel regression estimates of Tobin's Q on poison pill law indicator variables and their interactions with an incorporation-headquarter state indicator variable. *Inc-HQ State* equals one if a firm's state of incorporation is the same as its state of location, and zero otherwise. Panel B shows the matched sample DID results of Tobin's Q on a *Treated* \times *Post* indicator variable and its interaction with *Inc-HQ State*. *Treated* firms are defined as firms incorporated in states that adopt poison pill laws, whereas control firms are incorporated in states without such laws. We use nearest-neighbor matching with replacement in year $t-1$ to create a sample matched on Q and $\ln(\text{Assets})$, and exactly on state of location and firm-level poison pill status. The main variables of interest in the panels, Q , *Poison Pill Law*, *Poison Pill Law First Wave*, *Poison Pill Law Second Wave*, *Treated* \times *Post*, *Post*, *Inc-HQ State* and all of the corresponding interactions, 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. State fixed effects are defined using a firm's state of location (as in Gormley and Matsa, 2016, and Karpoff and Wittry, 2018), while industry fixed effects are measured using three-digit SIC codes (following Catan, 2017, Gormley and Matsa, 2016, and Karpoff and Wittry, 2018). Control variables include: *Poison Pill Firm-Level*, $\ln(\text{Assets})$, $\ln(\text{Age})$, *HHI*, *Sales Growth*, *Loss*, *Debt-to-Equity*, *Firm Liquidity*, *CAPX/Assets*, *R&D/Sales*, *Institutional Ownership*, *Business Combination Law*, *Control Share Law*, *Directors' Duties Law* and *Fair Price Law*. Table A.1 provides variable definitions. All continuous variables are winsorized at the 1st and 99th percentiles and the dollar values are expressed in 2015 dollars. t -statistics are estimated using robust standard errors with either firm level or state of incorporation clustering. We report in parentheses both clustering strategies t -statistics. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Panel A: Pooled Panel Regressions

Dep. Variable: $Q_{[t]}$				
1983 – 2012				
Variables	(1)	(2)	(3)	(4)
<i>Poison Pill Law</i> _[t]	0.115*	-0.026		
Firm clustering	(1.94)	(-0.48)		
Incorporation state clustering	(1.76)	(-0.47)		
<i>Poison Pill Law</i> _[t] \times <i>Inc-HQ State</i>		0.149**		
Firm clustering		(2.42)		
Incorporation state clustering		(2.53)		
<i>Poison Pill Law First Wave</i> _[t]			-0.061	-0.155
Firm clustering			(-1.19)	(-1.37)
Incorporation state clustering			(-1.47)	(-1.39)
<i>Poison Pill Law Second Wave</i> _[t]			0.206***	0.036
Firm clustering			(2.69)	(0.37)
Incorporation state clustering			(3.97)	(0.39)
<i>Poison Pill Law First Wave</i> _[t] \times <i>Inc-HQ state</i>				0.105
Firm clustering				(1.58)
Incorporation state clustering				(1.58)
<i>Poison Pill Law Second Wave</i> _[t] \times <i>Inc-HQ state</i>				0.204**
Firm clustering				(2.08)
Incorporation state clustering				(2.14)
Control variables (including other law controls)	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
State-year fixed effects	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes
# of firms in regression	3,405	3,405	3,405	3,405
N	33,640	33,640	33,640	33,640
Adjusted R ²	0.612	0.612	0.613	0.613

Table 7 – (Continued)

Panel B: Matched Sample Regressions

Dep. Variable: $Q_{[t]}$				
(t-3) to (t+3)				
Variables	(1)	(2)	(3)	(4)
$Treated_{[t]} \times Post_{[t]} \times Inc-HQ\ State$	0.125*			
Firm clustering	(2.00)			
Incorporation state clustering	(1.85)			
$Treated_{[t]} \times Post_{[t]}$	-0.020	0.142*	-0.008	0.257*
Firm clustering	(-0.25)	(1.91)	(-0.20)	(1.83)
Incorporation state clustering	(-0.23)	(1.80)		
$Post_{[t]}$	0.008	-0.016	-0.031	0.138
Firm clustering	(0.16)	(-0.25)	(-0.56)	(1.000)
Incorporation state clustering	(0.15)	(-0.24)		
$Poison\ Pill\ Firm-Level_{[t-1]}$	-0.046	-0.045	0.022	0.027
Firm clustering	(-1.05)	(-0.64)	(0.74)	(0.87)
Incorporation state clustering	(-0.98)	(-0.60)		
Full matched sample	Yes	No	No	No
Inc-HQ state only matched sample	No	Yes	Yes	Yes
First wave only matched firms	No	No	Yes	No
Second wave only matched firms	No	No	No	Yes
Control variables	Yes	Yes	Yes	Yes
Other law controls	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes
# of firms in regression	603	371	204	167
N	4,729	3,149	1,586	1,563
Adjusted R ²	0.747	0.766	0.891	0.758

Table 8: 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. Table A.1 provides variable definitions. 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 Period

Portfolio “6m24”	Four-factor model			Three-factor model		
	Long	Short	Long - Short	Long	Short	Long - Short
Alpha (monthly)	0.851** (2.21)	0.041 (0.15)	0.704* (1.91)	0.802** (2.09)	0.005 (0.02)	0.688* (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

Portfolio “6m36”	Four-factor model			Three-factor model		
	Long	Short	Long - Short	Long	Short	Long - Short
Alpha (monthly)	0.734* (1.76)	-0.113 (-0.42)	0.743* (1.92)	0.679* (1.71)	-0.146 (-0.56)	0.726* (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 8 – (Continued)**Panel B: First Wave Period**

Four-factor model				Three-factor model		
Portfolio “6m24”	Long	Short	Long - Short	Long	Short	Long - Short
Alpha (monthly)	-0.097 (-0.52)	0.030 (0.12)	-0.127 (-0.54)	-0.153 (-0.81)	0.005 (0.02)	-0.158 (-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 (0.86)	-0.035 (-0.12)	0.220 (0.62)	0.120 (0.56)	-0.139 (-0.49)	0.260 (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 Period

Four-factor model				Three-factor model		
Portfolio “6m24”	Long	Short	Long - Short	Long	Short	Long - Short
Alpha (monthly)	1.273** (2.22)	0.004 (0.01)	1.119** (2.09)	1.221** (2.18)	-0.037 (-0.10)	1.104** (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 (1.61)	-0.162 (-0.43)	0.966* (1.74)	0.892 (1.58)	-0.183 (-0.50)	0.945* (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 9: Poison Pill Laws and Operational Efficiency

This table reports the results for matched sample regressions of proxies for *Operational Efficiency* on a *Treated* \times *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 natural logarithm of current period's sales divided by last period's sales. *Treated* 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 ($t+1$). *Treated* \times *Post*, and *Post* are measured contemporaneously, and the controls are lagged one period. *Treated* 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 A.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*, *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 Period

Dep. Variables:	Full Sample: ($t-3$) to ($t+3$)			
	<i>ROA</i> _[$t+1$]	<i>NPM</i> _[$t+1$]	<i>OM</i> _[$t+1$]	<i>SG</i> _[$t+1$]
Variables	(1)	(2)	(3)	(4)
<i>Treated</i> _[t] \times <i>Post</i> _[t]	0.009* (1.66)	0.017** (2.01)	0.008 (1.49)	0.024* (1.71)
<i>Post</i> _[t]	-0.004 (-1.01)	-0.006 (-0.79)	-0.003 (-0.63)	0.003 (0.27)
<i>Poison Pill Firm-Level</i> _[$t-1$]	-0.008** (-2.06)	-0.014* (-1.72)	-0.013** (-2.12)	-0.012 (-0.89)
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	0.722	0.610	0.779	0.249

Table 9 – (Continued)**Panel B: First Wave Period (law adopted: 1986-1990)**

First Wave: (<i>t</i> -3) to (<i>t</i> +3)				
Dep. Variables:	$ROA_{[t+1]}$	$NPM_{[t+1]}$	$OM_{[t+1]}$	$SG_{[t+1]}$
Variables	(1)	(2)	(3)	(4)
$Treated_{[t]} \times Post_{[t]}$	0.007 (1.17)	0.009 (1.03)	0.003 (0.55)	0.002 (0.09)
$Post_{[t]}$	-0.004 (-0.94)	-0.008 (-0.74)	-0.007 (-1.62)	-0.011 (-0.66)
$Poison\ Pill\ Firm-Level_{[t-1]}$	-0.003 (-0.81)	0.002 (0.24)	-0.002 (-0.35)	-0.015 (-1.13)
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	0.641	0.216

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

Second Wave: (<i>t</i> -3) to (<i>t</i> +3)				
Dep. Variables:	$ROA_{[t+1]}$	$NPM_{[t+1]}$	$OM_{[t+1]}$	$SG_{[t+1]}$
Variables	(1)	(2)	(3)	(4)
$Treated_{[t]} \times Post_{[t]}$	0.010* (1.76)	0.023** (2.64)	0.011* (1.91)	0.042** (2.51)
$Post_{[t]}$	-0.002 (-0.49)	-0.001 (-0.10)	0.003 (0.63)	0.027 (1.52)
$Poison\ Pill\ Firm-Level_{[t-1]}$	-0.019* (-1.80)	-0.055** (-2.41)	-0.041** (-2.02)	-0.014 (-1.13)
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	0.694	0.803	0.265

Table 10: Poison Pill Laws, Innovation, and Firm Value

This table reports the results for matched sample regressions of Tobin's Q on a $Treated \times Post \times Innovation$ interaction term. *Treated* 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. *Innovation* measures include the following: *R&D/Sales*, *Intangible Capital/Assets*, and *Knowledge Capital/Assets*. The main variables of interest, *Q*, $Treated \times Post$, $Treated \times Post \times Innovation$, and *Post* are measured contemporaneously, whereas the remaining controls are lagged one period. *Treated* is omitted in the regression because of collinearity with its firm fixed effect. Panel A regresses Tobin's Q on $Treated \times Post$ and $Treated \times Post \times Innovation$ 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 A.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*, *Business Combination Law*, *Control Share Law*, *Directors' Duties Law*, and *Fair Price Law*. 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 Period

Dep. Variable: $Q_{[t]}$			
	Full Sample: $(t-3)$ to $(t+3)$		
Variables	(1)	(2)	(3)
$Treated_{[t]} \times Post_{[t]} \times \frac{R\&D}{Sales_{[t]}}$	3.061** (2.55)		
$Treated_{[t]} \times Post_{[t]} \times \frac{Intangible\ Capital}{Assets_{[t]}}$		0.399** (2.44)	
$Treated_{[t]} \times Post_{[t]} \times \frac{Knowledge\ Capital}{Assets_{[t]}}$			0.813** (2.51)
$\frac{R\&D}{Sales_{[t]}}$	2.002 (1.15)		
$\frac{Intangible\ Capital}{Assets_{[t]}}$		-0.031 (-0.16)	
$\frac{Knowledge\ Capital}{Assets_{[t]}}$			0.604 (1.54)
$Treated_{[t]} \times Post_{[t]}$	0.006 (0.10)	-0.149 (-1.58)	-0.007 (-0.11)
$Post_{[t]}$	0.079* (1.76)	0.210*** (2.79)	0.106** (2.13)
$Poison\ Pill\ Firm-Level_{[t-1]}$	0.013 (0.30)	0.010 (0.23)	0.015 (0.35)
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	0.665	0.666

Table 10 – (Continued)

Panel B: First and Second Wave Periods

Dep. Variable: $Q_{[t]}$						
$(t-3)$ to $(t+3)$						
First Wave			Second Wave			
Variables	(1)	(2)	(3)	(4)	(5)	(6)
$Treated_{[t]} \times Post_{[t]} \times \frac{R\&D}{Sales_{[t]}}$	3.336* (1.85)			2.784* (1.84)		
$Treated_{[t]} \times Post_{[t]} \times \frac{Intangible\ Capital}{Assets_{[t]}}$		0.143 (1.03)			0.532** (2.17)	
$Treated_{[t]} \times Post_{[t]} \times \frac{Knowledge\ Capital}{Assets_{[t]}}$			0.754** (2.23)			0.890** (2.39)
$\frac{R\&D}{Sales_{[t]}}$	0.536 (0.23)			2.502 (1.29)		
$\frac{Intangible\ Capital}{Assets_{[t]}}$		-0.238 (-1.28)			0.105 (0.34)	
$\frac{Knowledge\ Capital}{Assets_{[t]}}$			0.376 (0.86)			0.796* (1.79)
$Treated_{[t]} \times Post_{[t]}$	-0.054 (-0.88)	-0.057 (-0.66)	-0.061 (-0.99)	0.118 (0.95)	-0.125 (-0.71)	0.095 (0.78)
$Post_{[t]}$	0.060 (1.31)	0.016 (0.30)	0.047 (1.11)	0.087 (0.93)	0.328** (2.27)	0.136 (1.41)
$Poison\ Pill\ Firm-Level_{[t-1]}$	0.016 (0.41)	0.013 (0.35)	0.013 (0.35)	0.003 (0.02)	0.012 (0.11)	-0.002 (-0.02)
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	0.704	0.704	0.639	0.643	0.643

Table 11: Poison Pill Laws, Stakeholder Relationships, and Firm Value

This table reports the results for matched sample regressions of Tobin's Q on a *Treated* \times *Post* \times *Shareholder Relationship* proxy interaction term. *Treated* 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*, *Treated* \times *Post*, *Treated* \times *Post* \times *Shareholder Commitment Proxy*, and *Post* are measured contemporaneously, whereas the remaining controls are lagged one period. *Treated* is omitted in the regression because of collinearity with its firm fixed effect. Panel A regresses Tobin's Q on *Treated* \times *Post* and *Treated* \times *Post* \times *Stakeholder Relationship Proxy* for the full period. 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 A.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*, *Business Combination Law*, *Control Share Law*, *Directors' Duties Law*, and *Fair Price Law*. 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 Period

Dep. Variable: $Q_{[t]}$			
	(t-3) to (t+3)		
Variables	(1)	(2)	(3)
$Treated_{[t]} \times Post_{[t]} \times Large\ Customer_{[t]}$	0.101* (1.65)		
$Treated_{[t]} \times Post_{[t]} \times Strategic\ Alliance_{[t]}$		0.130* (1.80)	
$Treated_{[t]} \times Post_{[t]} \times Labor\ Capital_{[t]}$			0.641*** (2.69)
$Large\ Customer_{[t]}$	0.010 (0.15)		
$Strategic\ Alliance_{[t]}$		0.001 (0.00)	
$Labor\ Capital_{[t]}$			0.147 (0.38)
$Treated_{[t]} \times Post_{[t]}$	-0.012 (-0.28)	-0.005 (-0.12)	-0.143* (-1.70)
$Post_{[t]}$	0.015 (0.36)	0.032 (1.04)	0.077 (1.22)
$Poison\ Pill\ Firm-Level_{[t-1]}$	0.012 (0.27)	0.007 (0.23)	0.017 (0.40)
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	0.661	0.715	0.657

Table 11 – (Continued)

Panel B: First and Second Wave Periods

Dep. Variable: $Q_{[t]}$						
$(t-3)$ to $(t+3)$						
First Wave			Second Wave			
Variables	(1)	(2)	(3)	(4)	(5)	(6)
$Treated_{[t]} \times Post_{[t]} \times Large\ Customer_{[t]}$	0.029 (0.61)			0.144* (1.76)		
$Treated_{[t]} \times Post_{[t]} \times Strategic\ Alliance_{[t]}$		-0.083 (-0.66)			0.232** (2.12)	
$Treated_{[t]} \times Post_{[t]} \times Labor\ Capital_{[t]}$			0.313 (1.45)			1.044** (2.36)
$Large\ Customer_{[t]}$	-0.009 (-0.22)			-0.010 (-0.34)		
$Strategic\ Alliance_{[t]}$		-0.092 (-0.95)			0.033 (0.39)	
$Labor\ Capital_{[t]}$			0.292 (0.68)			0.245 (0.37)
$Treated_{[t]} \times Post_{[t]}$	-0.009 (-0.19)	0.032 (0.68)	-0.091 (-1.18)	-0.005 (-0.09)	-0.044 (-0.51)	-0.174 (-1.05)
$Post_{[t]}$	0.034 (0.91)	-0.001 (-0.04)	-0.022 (-0.43)	0.006 (0.09)	0.057 (0.94)	0.180 (1.33)
$Poison\ Pill\ Firm-Level_{[t-1]}$	0.021 (0.61)	0.011 (0.31)	0.007 (0.19)	-0.069 (-0.80)	-0.045 (-0.76)	0.060 (0.50)
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	0.732	0.702	0.726	0.684	0.698	0.628

Table 12: 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*, *Treated* \times *Post*, *Treated First Wave Adjusted* \times *Post*, and *Treated 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* and *Pillsbury* 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. Columns (1) – (2) pertain to the pooled panel and columns (3) – (5) are specific to the 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. Included control variables: *Ln(Assets)*, *Ln(Age)*, *HHI*, *Sales Growth*, *Loss*, *Debt-to-Equity*, *Firm Liquidity*, *CAPX/Assets*, *R&D/Sales*, *Institutional Ownership*, *Business Combination Law*, *Control Share Law*, *Directors' Duties Law*, and *Fair Price Law* indicators. Table A.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)	(5)
<i>Poison Pill Law</i> $_{[t]}$	0.119** (2.41)				
<i>Poison Pill Law First Wave Adjusted</i> $_{[t]}$		-0.032 (-0.56)			
<i>Poison Pill Law Second Wave Adjusted</i> $_{[t]}$		0.149*** (2.64)			
<i>Treated</i> $_{[t]} \times$ <i>Post</i> $_{[t]}$			0.100** (2.26)		
<i>Treated First Wave Adjusted</i> $_{[t]} \times$ <i>Post</i> $_{[t]}$				-0.014 (-0.34)	
<i>Treated Second Wave Adjusted</i> $_{[t]} \times$ <i>Post</i> $_{[t]}$					0.182** (2.29)
<i>Poison Pill Firm-Level</i> $_{[t-1]}$	-0.106*** (-3.65)	-0.107*** (-3.68)	-0.029 (-0.61)	0.007 (0.15)	-0.039 (-0.53)
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	0.598	0.598	0.662	0.705	0.644

Table 13: PPV-Index and Firm Value

This table 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. Table A.14 of the internet appendix provides a description of the PPV-index. The main variables of interest, Q , and $PPV\text{-}Index$ are measured contemporaneously, whereas the remaining controls are lagged one period. All four columns include the following control variables: $Ln(Assets)$, $Ln(Age)$, HHI , $Sales\ Growth$, $Loss$, $Debt\text{-}to\text{-}Equity$, $Firm\ Liquidity$, $CAPX/Assets$, $R\&D/Sale$ and $Institutional\ Ownership$. Column's (2) and (4) further specify: *Business Combination Law*, *Control Share Law*, *Directors' Duties Law*, and *Fair Price Law* indicators. Additionally, the first two columns include Arizona firms in the regression analysis, while the last two columns exclude them entirely. We consider our results with and without Arizona corporations since the language in the statute is ambiguous. Table A.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)
$PPV\text{-}Index_{[t]}$	0.048*** (2.96)	0.032* (1.73)	0.048*** (2.96)	0.032* (1.71)
$Poison\ Pill\ Firm\text{-}Level_{[t-1]}$	-0.104*** (-3.80)	-0.105*** (-3.83)	-0.105*** (-3.82)	-0.106*** (-3.85)
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	33,704	33,704
Adjusted R^2	0.597	0.597	0.597	0.597

**Internet Appendix for
“SHADOW 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 the following material:

- **Section A includes 4 supplementary figures and 16 supplementary tables.**
- **Section B includes a supplementary robustness analysis section with 8 corresponding tables.**

Figure A.1: States With a Poison Pill Law

The chart below shows the states that have adopted a poison pill law. 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/>.

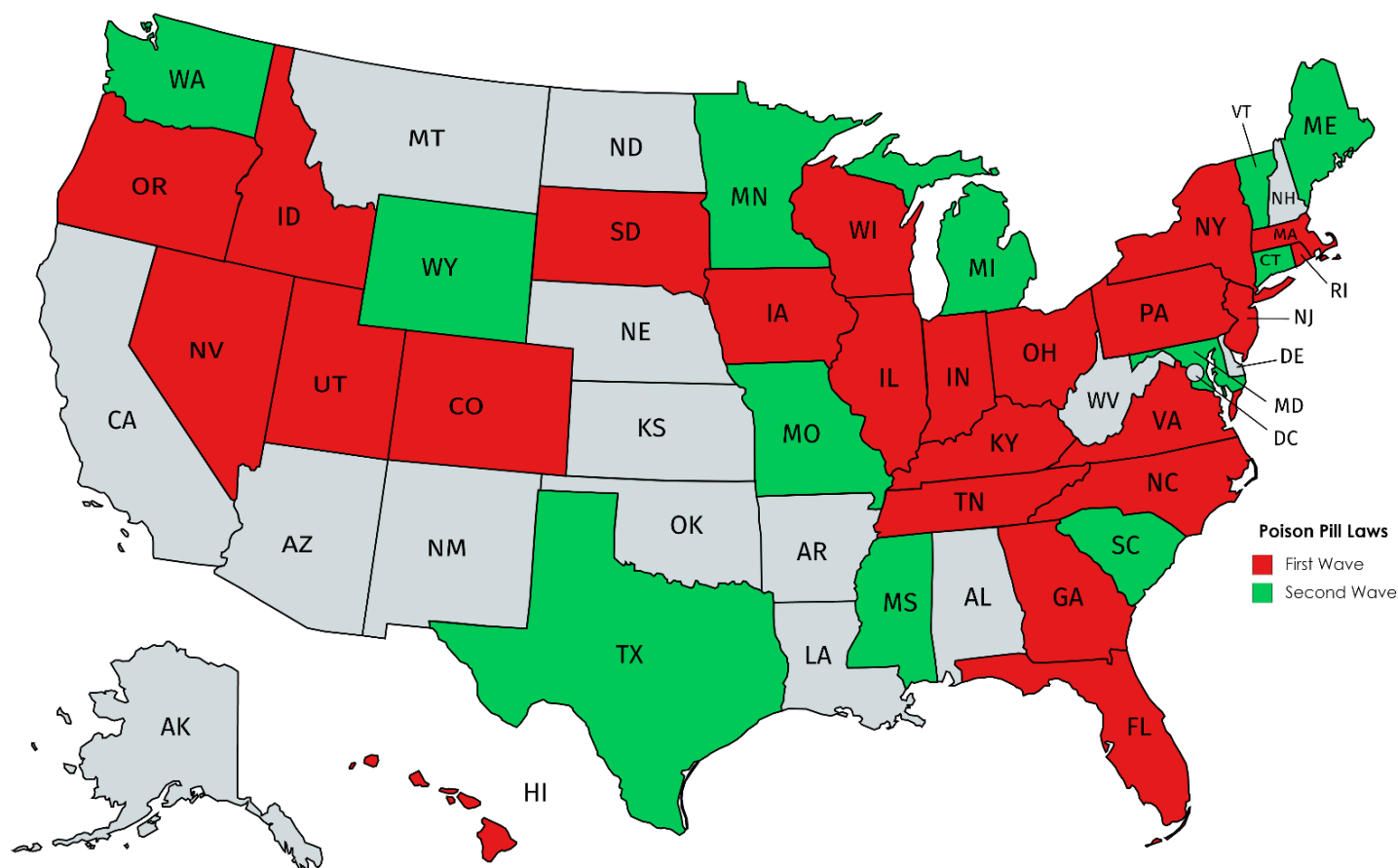
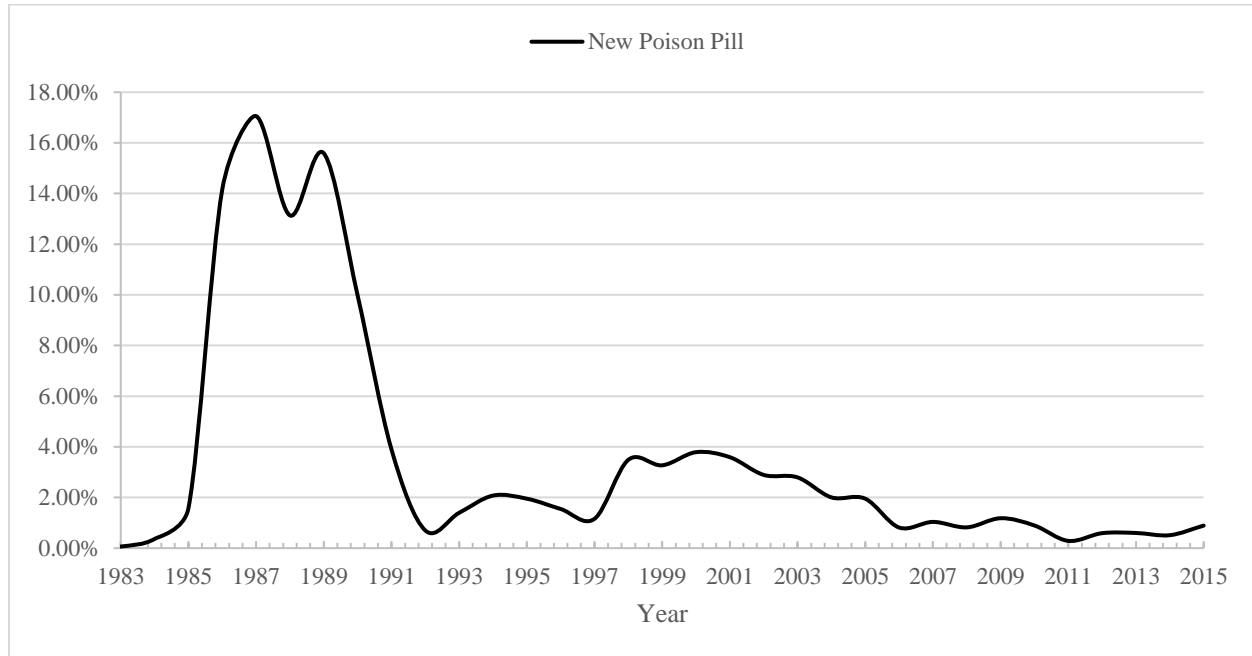


Figure A.2: Percentage of Firms Adopting a New or Dropping an Existing Poison Pill

Panel A of Figure 2 shows the percentage of firms adopting a new poison pill in our sample, each year from 1983 to 2015. Panel B of Figure 2 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

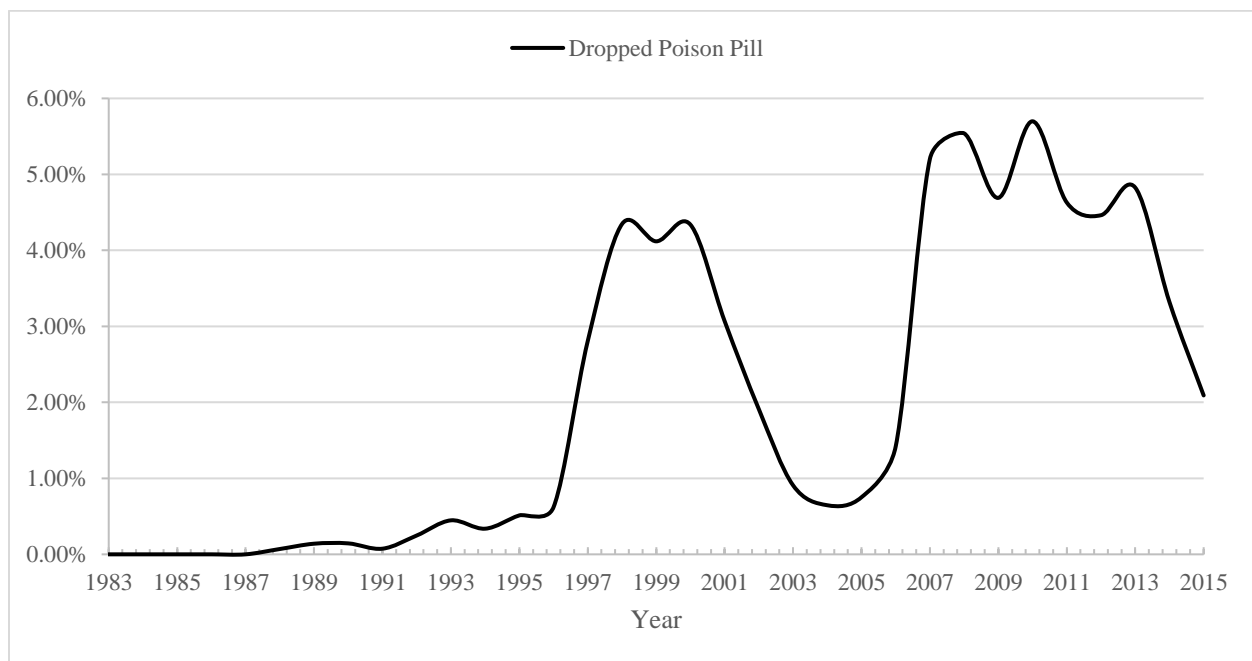


Figure A.3: Percentage of Firms Affected by Poison Pill Laws

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.

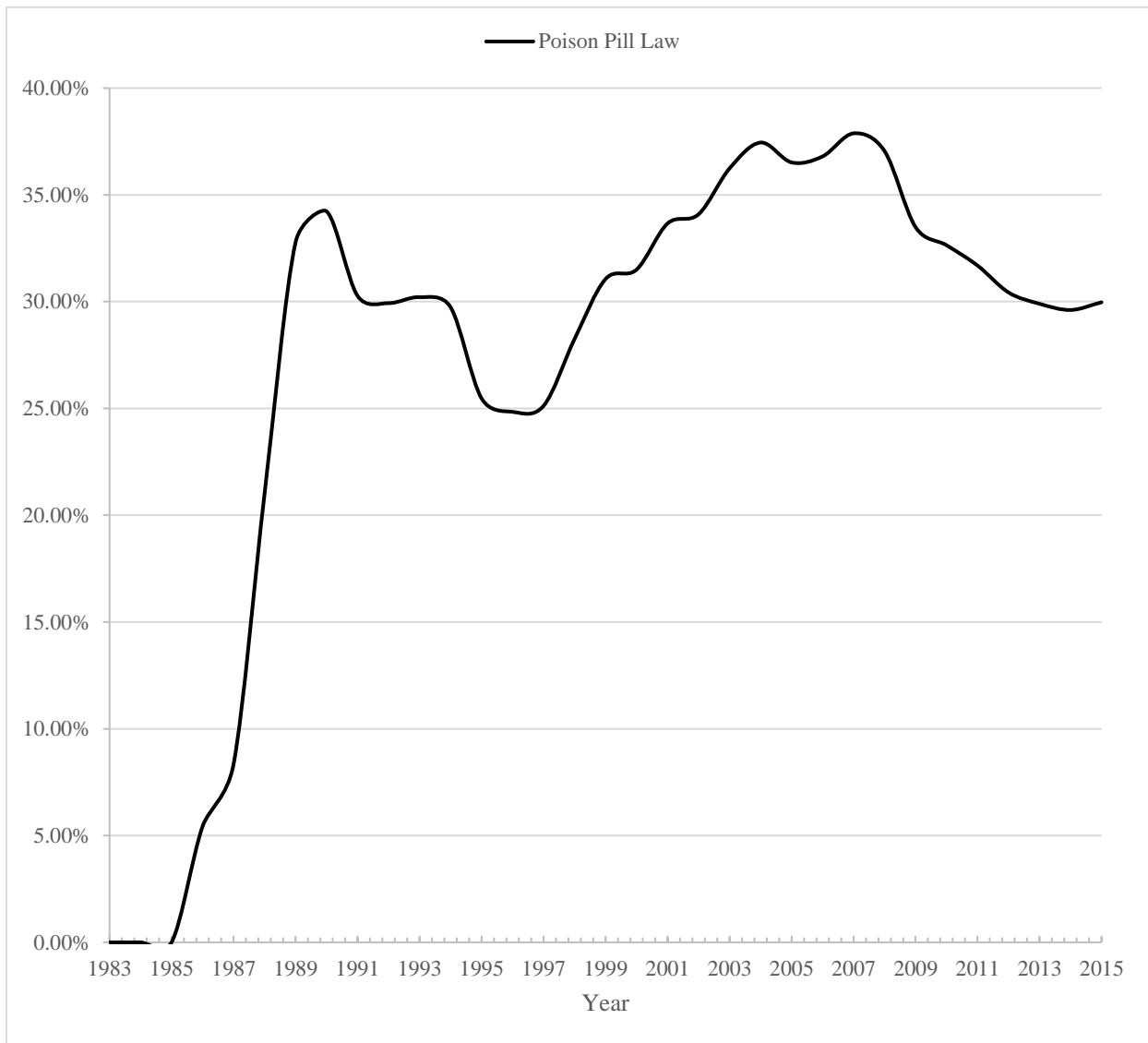


Figure A.4: Tobin's Q and Poison Pill Adoption

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.

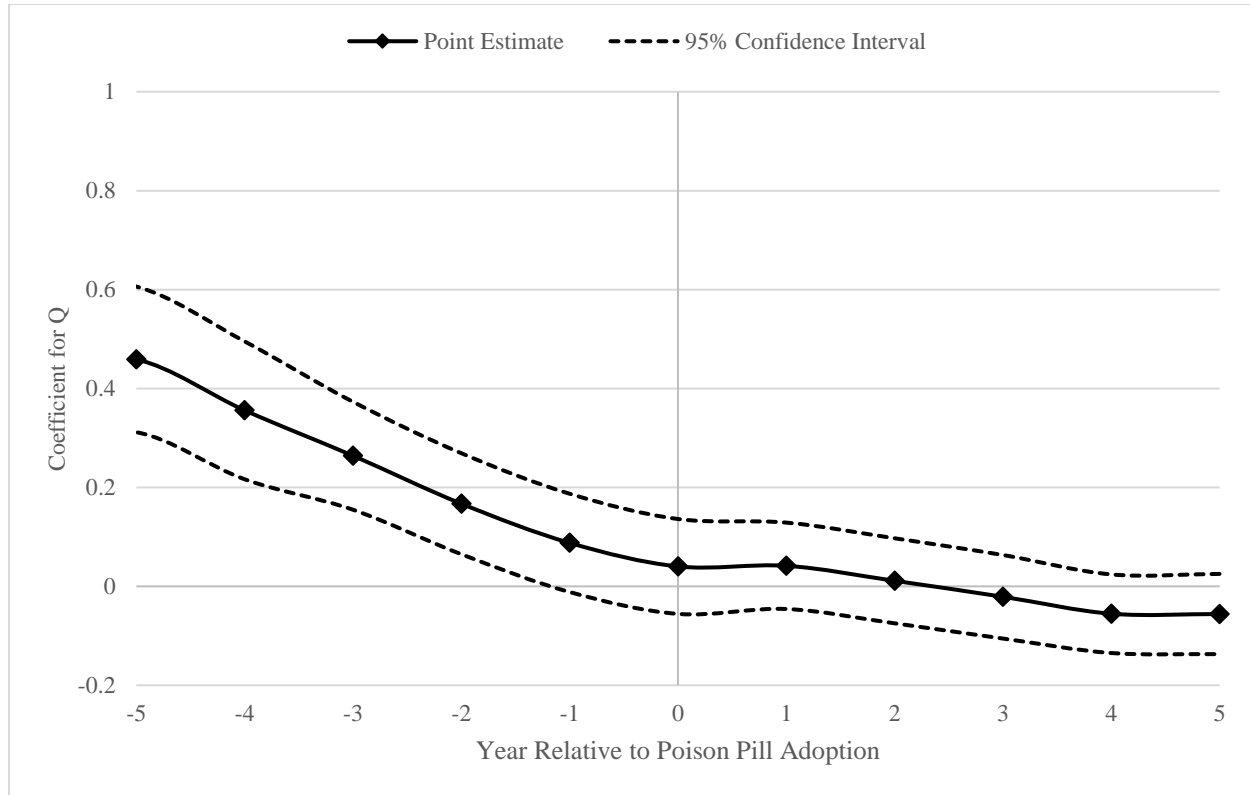


Table A.1: Variable Descriptions

Dependent Variables	Description
<i>Tobin's Q</i>	Market value of assets ($at - \text{book equity} + \text{market equity}$ ($prcc_f * csho$)) divided by the book value of assets (at). Book equity and this measure, in general, follows Fama and French (1992).
<i>Poison Pill Firm-Level</i>	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; also included as a control variable in the Q regressions.
<i>Monthly Stock Returns</i>	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.
<i>Takeover Bid (Bid)</i>	<i>Bid</i> 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.
<i>Takeover Complete (Acquired)</i>	<i>Acquired</i> 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.
<i>1-Day Premium</i>	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.
<i>1-Week Premium</i>	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.
<i>4-Week Premium</i>	Premium of offer price to target closing stock price 4-weeks prior to the original announcement date, expressed as a percentage. Data comes from the SDC M&A database.
<i>Return on Assets (ROA)</i>	Operating income before depreciation and amortization ($oibdp$) divided by the book value of assets (at).
<i>Net Profit Margin (NPM)</i>	Net income (ni) divided by the value of sales ($sale$).
<i>Operating Margin (OM)</i>	Operating income after depreciation and amortization ($oiadp$) divided by the value of sales ($sale$).
<i>Sales Growth (SG)</i>	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 <i>Tobin's Q</i> regressions.
<i>Total Tobin's Q</i>	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
<i>Poison Pill Law</i>	An indicator variable equal to one if a firm is incorporated in a state that has adopted a poison pill law, and zero otherwise. We use adoption dates provided by Cain, McKeon and Solomon (2017) and Karpoff and Wittry (2018).
<i>Poison Pill Law First Wave</i>	An indicator variable equal to one if a firm is incorporated in a state that passes a poison pill law during the period 1986 to 1990, and zero otherwise. We use adoption dates provided by Cain, McKeon and Solomon (2017) and Karpoff and Wittry (2018).
<i>Poison Pill Law Second Wave</i>	An indicator variable equal to one if a firm is incorporated in a state that passes a poison pill law during the period 1995 to 2009, and zero otherwise. We use adoption dates provided by Cain, McKeon and Solomon (2017) and Karpoff and Wittry (2018).
<i>Alpha</i>	Monthly portfolio abnormal returns, estimated using either the four-factor Carhart (1997) or three-factor Fama-French (1993) models, respectively.
<i>Poison Pill Law First Wave Adjusted</i>	An indicator variable equal to one if a firm is incorporated in a state that passes a poison pill law during the period 1986 to 1988, and zero otherwise. We use adoption dates provided by Cain, McKeon and Solomon (2017) and Karpoff and Wittry (2018).
<i>Poison Pill Law Second Wave Adjusted</i>	An indicator variable equal to one if a firm is incorporated in a state that passes a poison pill law during the period 1989 to 2009, and zero otherwise. We use adoption dates provided by Cain, McKeon and Solomon (2017) and Karpoff and Wittry (2018).
<i>PPV-Index</i>	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 17.
Main Interaction Variables	Description
<i>Inc-HQ State</i>	An indicator variable set equal to one if a firm's state of incorporation is also its state of location, and zero otherwise.
<i>Incorp State-Year M&A Volume</i>	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.
<i>Industry-Year M&A Volume</i>	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.
<i>Large Customer</i>	An indicator variable equal to one if a firm has at least one large customer based on the Compustat Customer Segments database.
<i>Strategic Alliance</i>	An indicator variable equal to one if the firm is in an active strategic alliance based on the SDC Strategic Alliances database.

<i>Labor Capital</i>	Selling, general and administrative expenses (<i>xsga</i>) scaled by the book value of assets (<i>at</i>).
<i>R&D/Sales</i>	Research and development expense (<i>xrd</i>) divided by the value of sales (<i>sale</i>).
<i>Intangible Capital/Assets</i>	Firm's intangible capital estimated replacement cost scaled by the book value of assets (<i>at</i>). The measure (<i>K_int</i>) is available on WRDS from 1950 to 2015, from Peters and Taylor (2017).
<i>Knowledge Capital/Assets</i>	Firm's knowledge capital replacement cost scaled by the book value of assets (<i>at</i>). The measure (<i>K_int_Know</i>) is available on WRDS from 1950 to 2015, from Peters and Taylor (2017).
<i>Staggered Board</i>	An indicator variable equal to one if the board is staggered in year <i>t</i> , and zero otherwise. Data come from Cremers, Litov, and Sepe (2017).
Control Variables	Description
<i>Ln(Assets)</i>	The natural logarithm of the value of total book assets (<i>at</i>) in millions, where assets are adjusted using 2015 dollars.
<i>Ln(Age)</i>	The natural logarithm of one plus the number of firm-year observations since the firm's first appearance in Compustat.
<i>HHI</i>	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>i</i> is defined as the value of sales (<i>sale</i>) of firm <i>i</i> divided by the total value of sales (<i>sale</i>) in the industry of firm <i>i</i> .
<i>Loss</i>	An indicator variable set to one if a firm has negative net income (<i>ni</i>) during a fiscal year, and zero otherwise.
<i>Debt-to-Equity</i>	Long-term debt (<i>dltt</i>) divided by book equity, where book equity is calculated as in Fama and French (1992).
<i>Firm Liquidity</i>	Current assets (<i>act</i>) minus current liabilities (<i>lct</i>) divided by the value of total book assets (<i>at</i>).
<i>CAPX/Assets</i>	Capital expenditures (<i>capx</i>) divided by the value of total book assets (<i>at</i>).
<i>Institutional Ownership</i>	The percent ownership of a firm by its institutional owners, measured by their equity ownership in their 13F holdings reports from Thomson Reuters, weighted by the firm's market capitalization.
<i>Business Combination Law</i>	An indicator variable equal to one if a firm is incorporated in a state that has adopted a business combination law, and zero otherwise. We use adoption dates provided by Cain, McKeon and Solomon (2017) and Karpoff and Wittry (2018).
<i>Control Share Law</i>	An indicator variable equal to one if a firm is incorporated in a state that has adopted a control share law, and zero otherwise. We use adoption dates provided by Cain, McKeon and Solomon (2017) and Karpoff and Wittry (2018).
<i>Directors' Duties Law</i>	An indicator variable equal to one if a firm is incorporated in a state that has adopted a directors' duties law, and zero otherwise. We use adoption dates provided by Karpoff and Wittry (2018).
<i>Fair Price Law</i>	An indicator variable equal to one if a firm is incorporated in a state that has adopted a fair price law, and zero otherwise. We use adoption

	dates provided by Cain, McKeon and Solomon (2017) and Karpoff and Wittry (2018).
<i>Incorp State-Year Q</i>	The average Tobin's Q of all firms incorporated within a state, in a given year.
<i>Incorp State-Year Poison Pill Firm Level</i>	The average percent of all firms incorporated within a state with an existing poison pill in-place, in a given year.
<i>Incorp State-Year Ln(Assets)</i>	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.
<i>Incorp State-Year Ln(Age)</i>	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.
<i>Incorp State-Year HHI</i>	The average Herfindahl-Hirschman Index of all firms incorporated within a state, in a given year.
<i>Incorp State-Year Sales Growth</i>	The average sales growth of all firms incorporated within a state, in a given year.
<i>Incorp State-Year Loss</i>	The average percent of all firms incorporated within a state experiencing negative net income, in a given year.
<i>Incorp State-Year Debt-to-Equity</i>	The average debt-to-equity of all firms incorporated within a state, in a given year.
<i>Incorp State-Year Firm Liquidity</i>	The average firm liquidity of all firms incorporated within a state, in a given year.
<i>Incorp State-Year CAPX/Assets</i>	The average ratio of capital expenditure to total assets of all firms incorporated within a state, in a given year.
<i>Incorp State-Year R&D/Sales</i>	The average ratio of research and development expenditure to sales of all firms incorporated within a state, in a given year.
<i>Incorp State-Year Institutional Ownership</i>	The average percentage of institutional ownership of all firms incorporated within a state, in a given year.
<i>R&D Tax Credit</i>	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).
<i>Percent Incorp State Republican</i>	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.
<i>Ln(Incorp State Per Capita GDP)</i>	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.
<i>Incorp State GDP Growth</i>	The incorporated state-level GDP growth rate over the fiscal year. We use data from the U.S. Bureau of Economic Analysis.

Table A.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 2012. 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.

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 ⁴⁹		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 ⁵⁰	4/2002	4	Yes	No	No	Yes	Yes	No
Maryland	5/1999	73	Yes	No	No	Yes	Yes	No
Massachusetts	7/1989	77	Yes	No	Yes	No	No	No
Michigan	7/2001	72	Yes	No	No	Yes	Yes	No

⁴⁹ The *Moran v. Household* court decision in Delaware in 1985 provides some legitimacy to poison pills. However, Delaware never issued a poison pill law, thus we treat Delaware as a control state or exclude it from the analysis all together.

⁵⁰ 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.

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 A.3: Summary Statistics

This table reports summary statistics for the main dependent and explanatory variables used in the pooled panel regressions. Panel A shows the summary statistics by first wave (1983 to 1994) and second wave (1995 to 2012) periods. Panel C reports the summary statistics by treated and control grouping. If a firm is incorporated in a state that has adopted poison pill legislation it is included in the treated group, and in the control group otherwise. The sample is composed of Compustat industrial firms over the period 1983 to 2012. All continuous variables are winsorized at the 1st and 99th percentiles and the dollar values are expressed in 2015 dollars. Table A.1 provides variable definitions. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Panel A: Full Sample by Wave

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

Independent Variables:	First Wave (1983 to 1994)			Second Wave (1995 to 2012)			Diff.	t-stat
	Mean	St. Dev.	Obs.	Mean	St. Dev.	Obs.		
<i>Poison Pill Law</i> _[t]	0.191	0.393	10,242	0.324	0.468	23,584	-0.133***	-25.23
<i>Poison Pill Firm-Level</i> _[t]	0.410	0.492	10,242	0.383	0.486	23,584	0.027***	4.64
<i>Business Combination Law</i>	0.552	0.497	10,242	0.878	0.328	23,584	-0.325***	-71.06
<i>Control Share Law</i> _[t]	0.190	0.393	10,242	0.260	0.439	23,584	-0.070***	-13.88
<i>Directors' Duties Law</i> _[t]	0.227	0.419	10,242	0.308	0.462	23,584	-0.081***	-15.28
<i>Fair Price Law</i> _[t]	0.247	0.431	10,242	0.307	0.461	23,584	-0.060***	-11.17
<i>Ln(Assets)</i> _[t]	7.079	1.536	10,242	7.002	1.838	23,584	0.077***	3.72
<i>Ln(Age)</i> _[t]	3.044	0.423	10,242	3.024	0.605	23,584	0.021***	3.12
<i>HHI</i> _[t]	0.261	0.173	10,242	0.229	0.182	23,584	0.033***	15.32
<i>Sales Growth</i> _[t]	0.031	0.199	10,242	0.051	0.243	23,584	-0.020***	-7.31
<i>Loss</i> _[t]	0.178	0.383	10,242	0.231	0.421	23,584	-0.053***	-10.85
<i>Debt- to- Equity</i> _[t]	0.580	1.276	10,242	0.539	1.400	23,584	0.041**	2.54
<i>Firm Liquidity</i> _[t]	0.241	0.185	10,242	0.242	0.214	23,584	-0.001	-0.51
<i>CAPX/Assets</i> _[t]	0.071	0.051	10,242	0.057	0.057	23,584	0.014***	20.64
<i>R&D/Sales</i> _[t]	0.024	0.045	10,242	0.039	0.086	23,584	-0.015***	-16.34
<i>Institutional Ownserhip</i> _[t]	0.303	0.255	10,242	0.514	0.343	23,584	-0.211***	-55.86

Table A.3 – (Continued)

Panel B: Full Sample by Treated Status

Dependent Variable:	Treated (Poison Pill Law = 1)			Control (Poison Pill Law = 0)			Diff.	t-stat
	Mean	St. Dev.	Obs.	Mean	St. Dev.	Obs.		
$Q_{[t]}$	1.844	1.159	9,602	1.865	1.278	24,224	-0.021	1.38
Independent Variables:	Treated (Poison Pill Law = 1)			Control (Poison Pill Law = 0)			Diff.	t-stat
	Mean	St. Dev.	Obs.	Mean	St. Dev.	Obs.		
<i>Poison Pill Firm-Level</i> _[t]	0.429	0.495	9,602	0.376	0.484	24,224	0.053***	9.05
<i>Business Combination Law</i>	0.856	0.351	9,602	0.749	0.434	24,224	0.107***	21.52
<i>Control Share Law</i> _[t]	0.646	0.478	9,602	0.078	0.268	24,224	0.568***	1,400
<i>Directors' Duties Law</i> _[t]	0.890	0.313	9,602	0.043	0.203	24,224	0.847***	2,900
<i>Fair Price Law</i> _[t]	0.806	0.395	9,602	0.083	0.276	24,224	0.723***	1,900
<i>Ln(Assets)</i> _[t]	7.031	1.724	9,602	7.023	1.764	24,224	0.008	0.36
<i>Ln(Age)</i> _[t]	3.191	0.531	9,602	2.966	0.554	24,224	0.225***	34.02
<i>HHI</i> _[t]	0.254	0.182	9,602	0.232	0.179	24,224	0.021***	9.77
<i>Sales Growth</i> _[t]	0.037	0.206	9,602	0.047	0.240	24,224	-0.010***	-3.59
<i>Loss</i> _[t]	0.197	0.398	9,602	0.222	0.416	24,224	-0.025***	-5.07
<i>Debt- to- Equity</i> _[t]	0.529	1.247	9,602	0.560	1.407	24,224	-0.030*	-1.85
<i>Firm Liquidity</i> _[t]	0.242	0.204	9,602	0.242	0.207	24,224	0.000	0.16
<i>CAPX/Assets</i> _[t]	0.056	0.051	9,602	0.063	0.057	24,224	-0.007***	-10.43
<i>R&D/Sales</i> _[t]	0.030	0.070	9,602	0.036	0.078	24,224	-0.007***	-7.34
<i>Institutional Ownserhip</i> _[t]	0.480	0.324	9,602	0.438	0.336	24,224	0.041***	10.31

Table A.4: Explaining the Adoption of Poison Pill Statutes

This table presents the complete 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. Table A.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 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: <i>Poison Pill Law</i> _[<i>t</i>]						
	1983 – 2012		First Wave (1983 – 1994)		Second Wave (1995 – 2012)	
Variables	(1)	(2)	(3)	(4)	(5)	(6)
<i>Incorp State-Year Q</i> _[<i>t</i>-1]	-0.009 (-0.09)	0.009 (0.09)	-0.008 (-0.06)	0.106 (0.90)	0.080 (0.57)	0.113 (0.86)
<i>Incorp State-Year M&A Volume</i> _[<i>t</i>-1]	-0.008 (-0.57)	-0.004 (-0.27)	0.044 (1.04)	0.047 (1.35)	0.002 (0.12)	-0.003 (-0.26)
<i>Industry-Year M&A Volume</i> _[<i>t</i>-1]	-0.001 (-0.83)	-0.001 (-0.70)	-0.002 (-1.20)	-0.003 (-0.84)	-0.001 (-0.65)	-0.002 (-0.96)
<i>Incorp State-Year Poison Pill Firm-Level</i> _[<i>t</i>-1]	-0.218 (-1.04)	-0.180 (-0.82)	-0.093 (-0.27)	-0.311 (-1.06)	0.010 (0.04)	-0.177 (-0.58)
<i>Incorp State-Year Ln(Assets)</i> _[<i>t</i>-1]	0.027 (0.31)	0.005 (0.06)	0.238 (0.88)	0.164 (0.59)	-0.076 (-0.53)	-0.077 (-0.64)
<i>Incorp State-Year Ln(Age)</i> _[<i>t</i>-1]	0.157 (0.78)	0.193 (0.68)	0.518 (1.07)	0.563 (1.06)	0.115 (0.38)	0.112 (0.27)
<i>Incorp State-Year HHI</i> _[<i>t</i>-1]	-0.184 (-0.40)	0.068 (0.15)	-0.476 (-0.69)	0.133 (0.17)	0.402 (0.67)	0.204 (0.46)
<i>Incorp State-Year Sales Growth</i> _[<i>t</i>-1]	-0.071 (-0.22)	-0.050 (-0.16)	0.105 (0.28)	0.131 (0.45)	-0.219 (-0.46)	-0.316 (-0.66)
<i>Incorp State-Year Loss</i> _[<i>t</i>-1]	0.022 (0.15)	0.031 (0.22)	0.110 (0.53)	0.067 (0.37)	-0.055 (-0.36)	-0.087 (-0.49)
<i>Incorp State-Year Debt-to-Equity</i> _[<i>t</i>-1]	-0.099 (-0.82)	-0.043 (-0.41)	-0.223* (-1.70)	-0.089 (-0.81)	0.133 (0.64)	0.129 (0.69)
<i>Incorp State-Year Firm Liquidity</i> _[<i>t</i>-1]	-0.488 (-0.95)	-0.582 (-1.19)	1.038 (1.29)	0.140 (0.26)	-0.515 (-0.57)	-0.141 (-0.16)
<i>Incorp State-Year CAPX/Assets</i> _[<i>t</i>-1]	-0.930 (-0.90)	-0.794 (-0.73)	2.094 (1.50)	1.079 (0.91)	-1.807 (-1.10)	-1.742 (-1.06)
<i>Incorp State-Year R&D/Sales</i> _[<i>t</i>-1]	0.074 (0.13)	0.156 (0.28)	0.872 (0.62)	1.578 (0.91)	-0.654 (-1.01)	-0.736 (-1.29)
<i>Incorp State-Year Institutional Ownership</i> _[<i>t</i>-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)
<i>Business Combination Law</i> _[t-1]		-0.078		-0.268**		0.150
		(-0.58)		(-2.23)		(1.02)
<i>Control Share Law</i> _[t-1]		0.064		0.088		-0.318
		(0.54)		(0.65)		(-1.04)
<i>Directors' Duties Law</i> _[t-1]		0.388**		0.429**		0.471***
		(2.28)		(2.43)		(2.61)
<i>Fair Price Law</i> _[t-1]		0.033		0.136		-0.568**
		(0.27)		(1.13)		(-2.35)
<i>R&D Tax Credit</i> _[t-1]		0.003		0.109		-0.021
		(0.03)		(0.70)		(-0.17)
<i>Percent Incorp State Republican</i> _[t-1]		-0.022		0.008		0.064
		(-0.83)		(0.40)		(1.21)
<i>Ln(Incorp State Per Capita GDP)</i>		0.140		0.307**		-0.254*
		(1.26)		(2.01)		(-1.72)
<i>Incorp State GDP Growth</i> _[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 A.5: 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 A.1 provides variable definitions. The included controls are: $\ln(\text{Assets})$, $\ln(\text{Age})$, HHI , Loss , Debt-to-Equity , Firm Liquidity , CAPX/Assets , R\&D/Sales , $\text{Institutional Ownership}$, *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: $Q_{[t]}$				
	1983 – 1991		1994 – 2012	
Variables	(1)	(2)	(3)	(4)
<i>Poison Pill Law</i> $_{[t]}$	-0.016 (-0.41)	-0.024 (-0.61)	0.316*** (5.14)	0.237*** (3.18)
<i>Poison Pill Firm-Level</i> $_{[t-1]}$	0.007 (0.22)	0.008 (0.24)	-0.086** (-2.42)	-0.083** (-2.32)
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	0.754	0.755	0.607	0.607

Table A.6: 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: $\ln(\text{Assets})$, $\ln(\text{Age})$, HHI , *Sales Growth*, *Loss*, *Debt-to-Equity*, *Firm Liquidity*, CAPX/Assets , R\&D/Sales and *Institutional Ownership*. Further, columns (2), and (4) specify: *Business Combination Law*, *Control Share Law*, *Directors' Duties Law*, and *Fair Price Law* dummies. Table A.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)
<i>Poison Pill Law</i> $_{[t]}$	0.111** (2.41)	0.088* (1.73)		
<i>Poison Pill Law</i> $_{[t]} \times \textit{Poison Pill Firm-Level}_{[t-1]}$	0.021 (0.44)	0.020 (0.45)		
<i>Poison Pill Law First Wave</i> $_{[t]}$			-0.005 (-0.10)	-0.078 (-1.58)
<i>Poison Pill Law First Wave</i> $_{[t]} \times \textit{Poison Pill Firm-Level}_{[t-1]}$			0.054 (1.05)	0.050 (0.98)
<i>Poison Pill Law Second Wave</i> $_{[t]}$			0.282*** (3.37)	0.237*** (2.78)
<i>Poison Pill Law Second Wave</i> $_{[t]} \times \textit{Poison Pill Firm-Level}_{[t-1]}$			-0.094 (-1.01)	-0.094 (-1.02)
<i>Poison Pill Firm-Level</i> $_{[t-1]}$	-0.111*** (-3.34)	-0.112*** (-3.54)	-0.116*** (-3.45)	-0.116*** (-3.45)
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	0.597	0.597	0.597	0.598

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

Table A.7 – (Continued)

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

	Full Period		First Wave Period		Second Wave Period	
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
$Acquired_{[t]}$	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.
$Treated_{[t]} \times Post_{[t]}$	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
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 A.8: 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), 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. Table A.1 provides variable definitions. 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 Period

Portfolio “6m24”	Four-factor model			Three-factor model		
	Long	Short	Long - Short	Long	Short	Long - Short
Alpha (monthly)	0.629* (1.67)	-0.145 (-0.56)	0.671* (1.79)	0.751* (1.97)	0.008 (0.03)	0.634 (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

Portfolio “6m36”	Four-factor model			Three-factor model		
	Long	Short	Long - Short	Long	Short	Long - Short
Alpha (monthly)	0.520 (1.29)	-0.313 (-1.17)	0.707* (1.80)	0.639 (1.61)	-0.151 (-0.54)	0.675* (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 A.8 – (Continued)

Panel B: First Wave Period

Portfolio “6m24”	Four-factor model			Three-factor model		
	Long	Short	Long - Short	Long	Short	Long - Short
Alpha (monthly)	-0.047 (-0.18)	0.098 (0.31)	-0.145 (-0.61)	-0.080 (-0.31)	0.101 (0.33)	-0.181 (-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

Portfolio “6m36”	Four-factor model			Three-factor model		
	Long	Short	Long - Short	Long	Short	Long - Short
Alpha (monthly)	0.231 (0.86)	0.035 (0.10)	0.196 (0.55)	0.206 (0.80)	-0.021 (-0.06)	0.227 (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 Period

Portfolio “6m24”	Four-factor model			Three-factor model		
	Long	Short	Long - Short	Long	Short	Long - Short
Alpha (monthly)	0.899* (1.66)	-0.341 (-0.93)	1.093** (2.00)	1.057* (1.91)	-0.159 (-0.42)	1.060* (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

Portfolio “6m36”	Four-factor model			Three-factor model		
	Long	Short	Long - Short	Long	Short	Long - Short
Alpha (monthly)	0.614 (1.07)	-0.514 (-1.38)	0.937 (1.64)	0.766 (1.35)	-0.303 (-0.79)	0.900 (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 A.9: 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. 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 *Treated* \times *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* and *Institutional Ownership*. Further, columns (2), (4), (6), and (8) specify: *Business Combination Law*, *Control Share Law*, *Directors' Duties Law*, and *Fair Price Law* dummies. Table A.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: <i>Total Q</i> _[t]						
1983 – 2012						
Variables	(1)	(2)	(3)	(4)	(5)	(6)
<i>Poison Pill Law</i> _[t]	0.154** (2.30)	0.140* (1.71)				
<i>Poison Pill Law First Wave</i> _[t]			-0.025 (-0.46)	-0.189 (-1.58)	0.026 (0.44)	-0.188 (-1.57)
<i>Poison Pill Law Second Wave</i> _[t]			0.371*** (2.85)	0.329** (2.46)	0.360*** (2.78)	0.305** (2.30)
<i>Post 94</i> _[t] \times <i>Poison Pill Law First Wave</i> _[t]					-0.094 (-1.18)	-0.126 (-1.47)
<i>Poison Pill Firm-Level</i> _[t-1]	-0.160*** (-2.88)	-0.153*** (-2.91)	-0.163*** (-2.91)	-0.157*** (-2.96)	-0.163*** (-2.91)	-0.157*** (-2.96)
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	0.352	0.353	0.353	0.353	0.353	0.353

Table A.9 – (Continued)

Panel B: Matched Sample Regressions

Dep. Variable: <i>Total Q_[t]</i>								
(t-3) to (t+3)								
	Full Period		First Wave Period (law adopted: 1986-1990)		Second Wave Period (law adopted: 1995-2009)		Full Period with First Wave Dummy	
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Treated_[t] × Post_[t]</i>	0.100*	0.130*	-0.017	0.005	0.276**	0.280*	0.251**	0.257**
	(1.71)	(1.65)	(-0.30)	(0.08)	(2.21)	(1.66)	(2.17)	(2.04)
<i>Treated_[t] × Post_[t] × Poison Pill Law First Wave_[t]</i>							-0.356	-0.322
							(-1.57)	(-1.46)
<i>Post_[t]</i>	0.015	0.003	0.030	0.018	0.025	0.030	0.005	-0.000
	(0.32)	(0.06)	(0.84)	(0.52)	(0.28)	(0.31)	(0.11)	(-0.01)
<i>Poison Pill Firm-Level_[t-1]</i>	-0.019	-0.019	0.010	0.008	-0.080	-0.083	-0.024	-0.024
	(-0.38)	(-0.38)	(0.20)	(0.18)	(-0.61)	(-0.63)	(-0.49)	(-0.49)
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	0.706	0.706	0.689	0.689	0.699	0.699	0.707	0.706

Table A.10: Poison Pill Laws and M&A Activity

This table reports the results for matched sample regressions of *M&A Activity* on a *Treated* \times *Post* interaction term. *M&A Activity* dependent variables include the following: *Bid* and *Acquired*. *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. *Acquired* 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. *Treated* 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*, *Acquired*, *Treated* \times *Post*, and *Post* are measured contemporaneously, and the controls are lagged one period. *Treated* 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 A.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*, *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.

Panel A: Full Period

		Full Sample: (<i>t</i> -3) to (<i>t</i> +3)			
Dep. Variables:		<i>Bid</i> _[<i>t</i>]		<i>Acquired</i> _[<i>t</i>]	
Variables		(1)	(2)	(3)	(4)
<i>Treated</i> _[<i>t</i>] \times <i>Post</i> _[<i>t</i>]		-0.008 (-0.78)	-0.015 (-1.35)	0.001 (0.15)	-0.002 (-0.26)
<i>Post</i> _[<i>t</i>]		0.009 (0.95)	0.015 (1.44)	-0.004 (-0.53)	-0.000 (-0.03)
<i>Poison Pill Firm-Level</i> _[<i>t</i>-1]		0.001 (0.17)	0.001 (0.17)	-0.003 (-0.75)	-0.003 (-0.71)
Control variables	Yes	Yes	Yes	Yes	Yes
Other law controls	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	No	Yes	Yes	No	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
# of firms in regression	873	873	873	873	873
N	6,117	6,117	6,117	6,117	6,117
Adjusted R ²	0.019	0.022	0.022	0.011	0.017

Table A.10 – (Continued)

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

First Wave: ($t-3$) to ($t+3$)				
Dep. Variables:	$Bid_{[t]}$		$Acquired_{[t]}$	
Variables	(1)	(2)	(3)	(4)
$Treated_{[t]} \times Post_{[t]}$	-0.010 (-0.81)	-0.012 (-0.94)	-0.006 (-0.60)	-0.010 (-0.90)
$Post_{[t]}$	-0.003 (-0.25)	-0.004 (-0.31)	-0.009 (-1.09)	-0.008 (-0.85)
$Poison\ Pill\ Firm-Level_{[t-1]}$	0.002 (0.32)	-0.003 (-0.38)	0.001 (0.26)	-0.002 (-0.34)
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	0.010	0.029	0.006	0.014

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

Second Wave: ($t-3$) to ($t+3$)				
Dep. Variables:	$Bid_{[t]}$		$Acquired_{[t]}$	
Variables:	(1)	(2)	(3)	(4)
$Treated_{[t]} \times Post_{[t]}$	-0.009 (-0.44)	-0.017 (-0.74)	0.005 (0.33)	0.006 (0.34)
$Post_{[t]}$	0.020 (1.18)	0.029 (1.56)	0.002 (0.15)	0.007 (0.57)
$Poison\ Pill\ Firm-Level_{[t-1]}$	-0.000 (-0.04)	0.004 (0.33)	-0.009 (-1.19)	-0.008 (-0.91)
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	0.013	0.027	0.005	0.021

Table A.11: 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 *Treated* \times *Post* \times *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. *Treated* 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 *Treated* \times *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 measure the premium of the offer price to the target closing price 1-day, 1-week, or 4-weeks prior to the announcement date, respectively. *Treated* is omitted in the regression due to collinearity with its firm fixed effect. Table A.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*, *Business Combination Law*, *Control Share Law*, *Directors' Duties Law*, and *Fair Price Law*. 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 (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 Period		First Wave Period (law adopted: 1986-1990)		Second Wave Period (law adopted: 1995-2009)	
Variables	(1)	(2)	(3)	(4)	(5)	(6)
$Treated_{[t]} \times Post_{[t]} \times Incorp\ State-Year\ M\&A\ Volume_{[t]}$	-5.104 (-1.38)		0.998 (0.32)		-4.759 (-1.02)	
$Treated_{[t]} \times Post_{[t]} \times Industry-Year\ M\&A\ Volume_{[t]}$		-0.069 (-0.11)		-0.899* (-1.77)		0.624 (0.52)
$Treated_{[t]} \times Post_{[t]}$	0.115* (1.67)	0.102 (1.57)	0.029 (0.39)	0.054 (0.90)	0.243* (1.66)	0.213* (1.68)
$Incorp\ State-Year\ M\&A\ Volume_{[t]}$	-0.464 (-0.36)		-0.691 (-0.39)		-0.221 (-0.13)	
$Industry-Year\ M\&A\ Volume_{[t]}$		-0.035 (-0.10)		-0.728*** (-2.62)		0.846 (1.23)
$Post_{[t]}$	0.030 (0.49)	0.004 (0.10)	-0.020 (-0.34)	-0.018 (-0.47)	0.031 (0.25)	0.001 (0.01)
$Poison\ Pill\ Firm-Level_{[t-1]}$	0.012 (0.28)	0.011 (0.25)	0.010 (0.26)	0.009 (0.22)	0.014 (0.11)	0.014 (0.12)
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	0.661	0.703	0.704	0.638	0.637

Table A.11 – (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)
$Treated_{[t]} \times Post_{[t]}$	-0.004 (-0.02)	-0.230 (-0.80)	-0.041 (-0.20)	-0.151 (-0.54)	-0.008 (-0.03)	-0.332 (-0.87)
$Post_{[t]}$	-0.112 (-0.78)	-0.032 (-0.23)	-0.008 (-0.05)	0.077 (0.60)	-0.105 (-0.56)	0.003 (0.02)
$Poison\ Pill\ Firm-Level_{[t-1]}$	0.138 (1.02)	0.093 (0.67)	0.152 (1.13)	0.124 (0.94)	0.043 (0.28)	-0.037 (-0.23)
Dep. Variable average (standard deviation)	0.364 (0.299)		0.405 (0.313)		0.484 (0.380)	
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Other law controls	No	Yes	No	Yes	No	Yes
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
# of firms in regression	129	129	129	129	129	129
N	129	129	129	129	129	129
Adjusted R ²	0.037	0.120	0.129	0.227	-0.099	-0.043

Table A.12: 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 two-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 A.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.

	Treated			Control		
	(1)	(2)	(3)	(4)	(5)	(6)
Matched Variables:	First Wave	Second Wave	Difference	First Wave	Second Wave	Difference
$Q_{[t]}$	1.418 (0.555)	1.752 (1.343)	-0.334*** (-3.83)	1.396 (0.468)	1.753 (1.269)	-0.357*** (-4.41)
$Poison\ Pill\ Firm-Level_{[t]}$	0.332 (0.472)	0.363 (0.482)	-0.031 (-0.73)	0.332 (0.472)	0.363 (0.482)	-0.031 (-0.73)
$\ln(Assets)_{[t]}$	7.075 (1.642)	5.505 (1.844)	1.570*** (10.17)	6.941 (1.489)	5.784 (1.883)	1.157*** (7.76)
Other Control Variables:						
$\ln(Age)_{[t]}$	3.105 (0.302)	2.760 (0.659)	0.345*** (7.89)	3.066 (0.357)	2.767 (0.621)	0.299*** (6.85)
$HHI_{[t]}$	0.261 (0.161)	0.237 (0.193)	0.024 (1.53)	0.269 (0.169)	0.233 (0.202)	0.036** (2.19)
$Sales\ Growth_{[t]}$	0.044 (0.212)	0.035 (0.265)	0.009 (0.43)	0.056 (0.238)	0.007 (0.322)	0.049** (1.98)
$Loss_{[t]}$	0.128 (0.335)	0.323 (0.469)	-0.195*** (-5.48)	0.159 (0.366)	0.390 (0.489)	-0.231*** (-6.11)
$Firm\ Liquidity_{[t]}$	0.271 (0.184)	0.266 (0.220)	0.005 (0.28)	0.249 (0.190)	0.284 (0.261)	-0.035* (-1.76)
$CAPX/Assets_{[t]}$	0.068 (0.051)	0.066 (0.063)	0.002 (0.40)	0.066 (0.048)	0.057 (0.055)	0.009** (1.97)
$Institutional\ Ownership_{[t]}$	0.315 (0.244)	0.291 (0.277)	0.024 (1.04)	0.267 (0.238)	0.330 (0.304)	-0.063*** (-2.63)
Interacted Variables:						
$Large\ Customer_{[t]}$	0.356 (0.480)	0.511 (0.501)	-0.155*** (-3.55)	0.439 (0.497)	0.565 (0.497)	-0.126*** (-2.84)
$Strategic\ Alliance_{[t]}$	0.149 (0.356)	0.511 (0.501)	-0.362*** (-9.55)	0.107 (0.310)	0.489 (0.501)	-0.382*** (-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)
<i>R&D/Sales</i> _[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)
<i>Intangible Capital/Assets</i> _[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)
<i>Knowledge Capital/Assets</i> _[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 A.13: Poison Pill Laws, Innovation, and Firm Value by Wave

This table reports matched sample regressions of Tobin's Q on $Treated \times Post \times Innovation$. *Treated* 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. *Innovation* measures include: *R&D/Sales*, *Intangible Capital/Assets*, and *Knowledge Capital/Assets*. *Q*, $Treated \times Post$, $Treated \times Post \times Innovation$, and *Post* are measured contemporaneously, and the controls are lagged one year. *Treated* is omitted in the regression because of collinearity with its firm fixed effect. The fourth interacted variable is *Poison Pill Law First Wave*. Table A.1 provides variable definitions. Included controls: *Firm-Level Poison Pill*, $Ln(Assets)$, $Ln(Age)$, *HHI*, *Sales Growth*, *Loss*, *Debt-to-Equity*, *Firm Liquidity*, *CAPX/Assets*, *R&D/Sales*, *Institutional Ownership*, *Business Combination Law*, *Control Share Law*, *Directors' Duties Law*, and *Fair Price Law*. 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)
$Treated_{[t]} \times Post_{[t]} \times \frac{R\&D}{Sales_{[t]}} \times$ $Poison\ Pill\ Law\ First\ Wave_{[t]}$	0.927 (0.15)		
$Treated_{[t]} \times Post_{[t]} \times \frac{Intangible\ Capital}{Assets_{[t]}} \times$ $Poison\ Pill\ Law\ First\ Wave_{[t]}$		-1.958 (-1.44)	
$Treated_{[t]} \times Post_{[t]} \times \frac{Knowledge\ Capital}{Assets_{[t]}} \times$ $Poison\ Pill\ Law\ First\ Wave_{[t]}$			1.867 (0.34)
$Treated_{[t]} \times Post_{[t]} \times \frac{R\&D}{Sales_{[t]}}$	1.177* (1.78)		
$Treated_{[t]} \times Post_{[t]} \times \frac{Intangible\ Capital}{Assets_{[t]}}$		0.375* (1.90)	
$Treated_{[t]} \times Post_{[t]} \times \frac{Knowledge\ Capital}{Assets_{[t]}}$			0.777** (2.34)
$\frac{R\&D}{Sales_{[t]}}$	0.854*** (2.84)		
$\frac{Intangible\ Capital}{Assets_{[t]}}$		-0.002 (-0.01)	
$\frac{Knowledge\ Capital}{Assets_{[t]}}$			0.636 (1.63)
$Treated_{[t]} \times Post_{[t]}$	0.071 (1.65)	-0.027 (-0.21)	0.118 (1.26)
$Post_{[t]}$	0.013 (0.76)	0.211*** (2.76)	0.101** (2.03)
$Poison\ Pill\ Law\ First\ Wave_{[t]}$	-0.041 (-0.30)	1.388 (1.07)	0.316 (0.84)
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	0.668

Table A.14: Poison Pill Laws, Stakeholder Relationships, and Firm Value by Wave

This table reports matched sample regressions of Tobin's Q on $Treated \times Post \times Stakeholder Relationship$ proxy. *Treated* 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*, $Treated \times Post$, $Treated \times Post \times Stakeholder Relationship$ Proxy, and *Post* are measured contemporaneously, whereas the remaining controls are lagged one period. *Treated* 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 A.1 provides variable definitions. Included controls: *Firm-Level Poison Pill*, $Ln(Assets)$, *Poison Pill Firm-Level*, $Ln(Age)$, *HHI*, *Sales Growth*, *Loss*, *Debt-to-Equity*, *Firm Liquidity*, *CAPX/Assets*, *R&D/Sales*, *Institutional Ownership*, *Business Combination Law*, *Control Share Law*, *Directors' Duties Law*, and *Fair Price Law*. 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)
$Treated_{[t]} \times Post_{[t]} \times Large\ Customer_{[t]} \times$ $Poison\ Pill\ Law\ First\ Wave_{[t]}$	-0.155 (-0.51)		
$Treated_{[t]} \times Post_{[t]} \times Strategic\ Alliance_{[t]} \times$ $Poison\ Pill\ Law\ First\ Wave_{[t]}$		-0.078 (-0.16)	
$Treated_{[t]} \times Post_{[t]} \times Labor\ Capital_{[t]} \times$ $Poison\ Pill\ Law\ First\ Wave_{[t]}$			-1.072 (-1.28)
$Treated_{[t]} \times Post_{[t]} \times Large\ Customer_{[t]}$	0.161** (2.27)		
$Treated_{[t]} \times Post_{[t]} \times Strategic\ Alliance_{[t]}$		0.190* (1.70)	
$Treated_{[t]} \times Post_{[t]} \times Labor\ Capital_{[t]}$			0.506** (2.10)
$Large\ Customer_{[t]}$	0.015 (0.48)		
$Strategic\ Alliance_{[t]}$		-0.009 (-0.11)	
$Labor\ Capital_{[t]}$			0.334** (1.21)
$Treated_{[t]} \times Post_{[t]}$	0.021 (0.37)	0.060 (0.71)	-0.033 (-0.34)
$Post_{[t]}$	0.029 (0.56)	0.020 (0.58)	0.041 (0.76)
$Poison\ Pill\ Law\ First\ Wave_{[t]}$	0.415 (1.16)	0.639 (1.47)	0.702 (1.16)
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	0.706	0.708	0.704

Table A.15: Poison Pill Laws, Wave Adjustments, and Firm Value excluding Delaware

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*, *Treated* \times *Post*, *Treated First Wave Adjusted* \times *Post*, and *Treated 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* and *Pillsbury* 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. Below we show 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*, *Business Combination Law*, *Control Share Law*, *Directors' Duties Law*, and *Fair Price Law* indicators. Table A.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: $Q_{[t]}$					
Variables	Pooled Panel: 1983 to 2012		Matched Sample: ($t-3$) to ($t+3$)		
	(1)	(2)	(3)	(4)	(5)
<i>Poison Pill Law</i> $_{[t]}$	0.086*				
	(1.70)				
<i>Poison Pill Law First Wave Adjusted</i> $_{[t]}$		-0.058			
		(-0.94)			
<i>Poison Pill Law Second Wave Adjusted</i> $_{[t]}$		0.108*			
		(1.91)			
<i>Treated</i> $_{[t]} \times$ <i>Post</i> $_{[t]}$			0.119*		
			(1.85)		
<i>Treated First Wave Adjusted</i> $_{[t]} \times$ <i>Post</i> $_{[t]}$				-0.029	
				(-0.53)	
<i>Treated Second Wave Adjusted</i> $_{[t]} \times$ <i>Post</i> $_{[t]}$					0.230**
					(2.52)
<i>Poison Pill Firm-Level</i> $_{[t-1]}$	-0.128***	-0.130***	0.005	0.034	-0.042
	(-3.49)	(-3.53)	(0.08)	(0.70)	(-0.48)
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	0.597	0.597	0.653	0.744	0.639

Table A.16: PPV-Index and Firm Value

This table describes the construction of the poison pill validity index (*PPV-Index*). 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.

Poison Pill Validity Event	Code	Explanation
<i>Moran v. Household</i> (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 <i>de facto</i> 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.
<i>Georgia-Pacific v. Great Northern</i> (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 <i>Moran</i> and <i>Georgia-Pacific</i>)	= 0 or 1	If a state has a court case, before or after <i>Moran</i> or <i>Georgia-Pacific</i> , 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	This measure ranges from 0 to 3 and captures the change or relative strength of poison pill validity over time by state of incorporation.

B. Robustness Analysis

B.1 *Poison Pill Laws and Firm Value Without Same Year, Multi-Law Adopters*

The main focus of 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 three different state statutes: 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.⁵¹

Table B.1 presents the results. In columns (1) and (2), we report the findings from pooled panel regressions of Q on *Poison Pill Law* using the full sample. We find that our main results are robust to the exclusion of firms incorporated in states that adopt these other statutes in the same year as their poison pill law, where we include Delaware firms in column (1) and exclude Delaware firms in column (2) (in order to capture the unique position of Delaware as a state whose courts have long endorsed the poison pill, but which never issued a poison pill law). Columns (3) and (4) present the matched sample results, which are similar.⁵²

B.2 *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. However, we make an exception for the interaction between visible poison pills and having a staggered board, as a substantial literature has argued that 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).

⁵¹ Nine states meet this criterion and are excluded from the analysis in Table B.2: Georgia, Idaho, Illinois, Indiana, Massachusetts, Pennsylvania, Rhode Island, South Dakota, and Wisconsin. 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). Directors' duties laws do not per se provide an antitakeover defense, but rather offer directors more leeway to justify the adoption of antitakeover measures by enabling them to justify the adoption of such measures based on the best interests of all stakeholders rather than just shareholders.

⁵² Table B.2 of the internet appendix distinguishes heterogeneous provisions in poison pill laws.

Our conjecture is that, unlike visible poison pills, the shadow pill might act more as a substitute than a complementary antitakeover measure. This conjecture is based on the bonding hypothesis of takeover defenses, under which both 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, as for example argued in Bebchuk, Coates, and Subramanian (2002) or Cohen and Wang (2013), *both* these measures would be necessary when they are used for entrenchment purposes.

Table B.3 examines these empirical predictions. In columns (1) and (2), we explore the association of both poison pill laws and staggered boards with firm value, without interacting them. Once we control for the presence of a staggered board, the adoption of a poison pill law remains positively and significantly associated with Q . The positive coefficient on *Staggered Board* is consistent with Cremers, Litov, and Sepe (2017), indicating that the adoption (removal) of a staggered board is associated with higher (lower) firm value, with an economic magnitude of the increase equal to 4.7% ($=0.087/1.859$) relative to the sample mean of Q .

The specifications in columns (3) and (4) add the interaction between having both a stronger right to adopt a pill (via the enactment of a poison pill law) and a staggered board, i.e., *Poison Pill Law* \times *Staggered Board*, as well of having both a visible pill and a staggered board, i.e., *Poison Pill Firm-Level* \times *Staggered Board*. Both interactions have an insignificant coefficient in column (3) where we don't control for other state antitakeover laws and in column (4) where we do. The lack of statistical significance of the interactions supports 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 substitutes, rather than complements).

B.3 Additional Robustness

We include five additional tables in Section B of this internet appendix (Tables B4 – B8) verifying the robustness of our main results. In Tables B4 – B6 we document that the inclusion of firms incorporated in the state of Delaware as control firms (for which *Poison Pill Law* = 0) does not drive our results in either the full sample or the matched sample, as our results are robust to the exclusion of Delaware firms. In Tables B7 – B8, we report the results for a placebo test in the matched sample, where we move back the actual adoption date of poison pill laws by five years. That is, the pseudo-adoption date equals the actual adoption date minus five years. We estimate the matched sample regressions over plus and minus three-year windows around the pseudo-

adoption date and find insignificant coefficients for $Treated \times Post$, consistent with our identification assumption.

Table B.1: 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 $Treated \times Post$ 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 period. 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*, *Business Combination Law*, *Control Share Law*, *Directors' Duties Law*, and *Fair Price Law* indicators. Table A.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: $Q_{[t]}$				
Variables	Pooled Panel: 1983 to 2012		Matched Sample: $(t-3)$ to $(t+3)$	
	(1)	(2)	(3)	(4)
<i>Poison Pill Law</i> $_{[t]}$	0.105** (2.00)	0.085* (1.81)		
$Treated_{[t]} \times Post_{[t]}$			0.117* (1.94)	0.146** (2.08)
<i>Poison Pill Firm-Level</i> $_{[t-1]}$	-0.110*** (-3.89)	-0.154*** (-3.49)	-0.055 (-1.00)	0.003 (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	0.596	0.592	0.657	0.651

Table B.2: 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-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 full period. *Dead-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.⁵³ *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.⁵⁴ Control variables: *Ln(Assets)*, *Ln(Age)*, *HHI*, *Sales Growth*, *Loss*, *Debt-to-Equity*, *Firm Liquidity*, *CAPX/Assets*, *R&D/Sales*, *Institutional Ownership*, *Business Combination Law*, *Control Share Law*, *Directors' Duties Law*, and *Fair Price Law*. Table A.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 Period		
Variables	(1)	(2)	(3)	(4)	(5)	(6)
<i>Poison Pill Law</i> _[t]	0.101** (2.07)	0.102** (2.08)	0.110** (2.18)			
<i>Treated</i> _[t] × <i>Post</i> _[t]				0.104* (1.69)	0.103* (1.67)	0.104* (1.68)
<i>Dead-Hand Provision</i> _[t]	-0.079 (-0.94)		-0.091 (-1.09)	-0.089 (-0.88)		-0.090 (-0.88)
<i>Weak- Pill Provision</i> _[t]		-0.119 (-1.29)	-0.129 (-1.38)		-0.005 (-0.06)	-0.008 (-0.10)
<i>Poison Pill Firm-Level</i> _[t-1]	-0.105*** (-3.82)	-0.105*** (-3.83)	-0.105*** (-3.82)	0.013 (0.31)	0.014 (0.31)	0.014 (0.31)
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	0.597	0.597	0.597	0.662	0.662	0.661

⁵³ There are three states with dead-hand pill provisions: Georgia, after it amended its earlier statute in 2000, as well as Maryland and Virginia.

⁵⁴ There are two states with weak-pill provisions: Both New York and North Carolina explicitly admit judicial review of poison pills.

Table B.3: 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-Level*, and the remaining controls, are lagged one period. We also interact *Poison Pill Law* \times *Staggered Board* and *Poison Pill Firm-Level* \times *Staggered Board* in the last two columns. Each of the four columns include the following control variables: $\ln(\text{Assets})$, $\ln(\text{Age})$, HHI , Sales Growth , Loss , Debt-to-Equity , Firm Liquidity , CAPX/Assets , R\&D/Sales and $\text{Institutional Ownership}$. The even-numbered columns further specify *Business Combination Law*, *Control Share Law*, *Directors' Duties Law*, and *Fair Price Law* indicators. Table A.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)
<i>Poison Pill Law</i> $_{[t]}$	0.116*** (3.21)	0.093** (2.07)	0.117*** (2.82)	0.094* (1.87)
<i>Poison Pill Firm-Level</i> $_{[t-1]}$	-0.111*** (-4.24)	-0.111*** (-4.27)	-0.137*** (-3.88)	-0.138*** (-3.88)
<i>Staggered Board</i> $_{[t]}$	0.087*** (2.74)	0.087*** (2.72)	0.067* (1.81)	0.066* (1.78)
<i>Poison Pill Law</i> $_{[t]} \times \textit{Staggered Board}_{[t]}$			-0.004 (-0.09)	-0.002 (-0.04)
<i>Poison Pill Firm-Level</i> $_{[t-1]} \times \textit{Staggered Board}_{[t]}$			0.056 (1.38)	0.055 (1.35)
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	0.597	0.597	0.597	0.597

Table B.4: 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* and *Institutional Ownership*. Column's (2) and (4) further specify: *Business Combination Law*, *Control Share Law*, *Directors' Duties Law*, and *Fair Price Law* indicators. Table A.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)
<i>Poison Pill Law</i> $_{[t]}$	0.136*** (2.91)	0.086* (1.70)		
<i>Poison Pill Law First Wave</i> $_{[t]}$			0.022 (0.44)	-0.064 (-1.25)
<i>Poison Pill Law Second Wave</i> $_{[t]}$			0.196*** (2.86)	0.151** (2.15)
<i>Poison Pill Firm-Level</i> $_{[t-1]}$	-0.126*** (-3.43)	-0.128*** (-3.49)	-0.129*** (-3.50)	-0.132*** (-3.59)
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	0.602	0.602	0.602	0.603

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

Table B.6: 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 $Treated \times Post$ interaction term, in which we exclude firms incorporated in Delaware from the pool of potential controls. $Treated$ 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 , $Treated \times Post$, and $Post$ are measured contemporaneously, whereas the remaining controls are lagged one period. $Treated$ is omitted in the regression because of collinearity with its firm fixed effect. Columns (1) – (2) regresses Tobin's Q on $Treated \times Post$ 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 $Treated \times Post$ 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 A.1 provides variable definitions. The included controls are: $Ln(Assets)$, $Ln(Age)$, HHI , $Sales\ Growth$, $Loss$, $Debt\text{-}to\text{-}Equity$, $Firm\ Liquidity$, $CAPX/Assets$, $R\&D/Sales$ and *Institutional Ownership*. Further, columns (2), (4), (6), and (8) 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 Period		First Wave Period (law adopted: 1986-1990)		Second Wave Period (law adopted: 1995-2009)		Full Period with First Wave Dummy	
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Treated_{[t]} \times Post_{[t]}$	0.131** (2.16)	0.119* (1.85)	-0.044 (-0.55)	-0.052 (-0.66)	0.321*** (2.89)	0.329** (2.64)	0.240** (2.21)	0.247** (2.30)
$Treated_{[t]} \times Post_{[t]} \times$ $Poison\ Pill\ Law\ First\ Wave_{[t]}$							-0.337 (-1.42)	-0.342 (-1.50)
$Post_{[t]}$	0.046 (0.79)	0.050 (0.82)	0.040 (0.81)	0.040 (0.89)	-0.078 (-0.63)	-0.077 (-0.62)	0.037 (0.63)	0.047 (0.77)
$Poison\ Pill\ Firm\text{-}Level_{[t-1]}$	0.007 (0.11)	0.005 (0.08)	0.026 (0.44)	0.027 (0.46)	-0.189 (-1.61)	-0.190 (-1.61)	-0.003 (-0.04)	0.001 (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	0.653	0.653	0.725	0.726	0.644	0.644	0.653	0.655

Table B.7: 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 Treated 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 A.1 provides variable definitions. All continuous variables are winsorized at the 1st and 99th percentiles and dollar values are expressed in 2015 dollars.

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

Table B.8: Placebo Test

This table reports results from matched sample regressions of Tobin's Q on a $Treated \times Post$ interaction term. $Treated$ 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 , $Treated \times Post$, 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 A.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*, $CAPX/Assets$, $R\&D/Sales$ and *Institutional Ownership*. 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 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.

the 10%, 5%, and 1% level, respectively

Dep. Variable: $Q_{[t]}$		$(t-3)$ to $(t+3)$				
Full Period		First Wave Period (law pseudo adopted: 1981-1985)		Second Wave Period (law pseudo adopted: 1990-2004)		
Variables	(1)	(2)	(3)	(4)	(5)	(6)
$Treated_{[t]} \times Post_{[t]}$	0.022 (0.59)	0.016 (0.43)	0.038 (1.19)	0.031 (0.95)	-0.058 (-0.61)	-0.063 (-0.64)
$Post_{[t]}$	0.015 (0.46)	0.019 (0.56)	-0.030 (-1.34)	-0.027 (-1.21)	0.064 (0.80)	0.065 (0.81)
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	0.651	0.650	0.707	0.707	0.597	0.597

END OF THE INTERNET APPENDIX