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Shadow Pills, Pill Policy, and Firm Value

Finance Working Paper N° 595/2019
July 2020

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Abstract

We analyze the impact of the right to adopt a poison pill – a "shadow pill" – on pill policy and firm value by exploiting the quasi-natural experiment provided by U.S. states' staggered adoption of poison pill laws that validate the pill. We document that a strengthened shadow pill promotes the use of actual poison pills and increases firm value – especially for more innovative firms or firms with stronger stakeholder relationships, and for hostile acquisition targets. Our findings suggest shadow pills create value for some firms by reducing their contracting costs with stakeholders and increasing their bargaining power in takeovers.

Keywords: Poison pill, antitakeover statutes, shadow pill, firm value, bonding, bargaining power

JEL Classifications: G32, G34, K22, O32

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Shadow Pills, Pill Policy, and Firm Value

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July 8, 2020

Abstract

We analyze the impact of the *right to adopt* a poison pill – a "shadow pill" – on pill policy and firm value by exploiting the quasi-natural experiment provided by U.S. states' staggered adoption of poison pill laws that validate the pill. We document that a strengthened shadow pill promotes the use of actual poison pills and increases firm value – especially for more innovative firms or firms with stronger stakeholder relationships, and for hostile acquisition targets. Our findings suggest shadow pills create value for some firms by reducing their contracting costs with stakeholders and increasing their bargaining power in takeovers.

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Law and finance scholars generally agree that the poison pill (formally known as a "shareholder rights plan") is among the most powerful antitakeover defenses (e.g., Malatesta and Walkling 1988; Ryngaert 1988; Comment and Schwert 1995; Coates 2000; 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, while withholding such rights from the hostile bidder. As a result, poison pills grant the board of directors the ability to dilute the ownership stake of a hostile bidder substantially, giving the board de facto veto power over any hostile acquisition.

After the Delaware Supreme Court validated the use of the pill in 1985, a significant literature investigated whether the adoption of a poison pill is beneficial or detrimental to shareholder interests. While earlier findings were mixed,[1] over the past decade, empirical studies have found that the adoption of a pill is negatively associated with firm value (e.g., Bebchuk, Cohen and Ferrell 2009; Cuñat, Gine, and Guadalupe 2012; Cremers and Ferrell 2014). However, this result is difficult to interpret, as the decision to employ 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 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 (Comment and Schwert 1995; Catan 2019).

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 *right to adopt* a poison pill (which right constitutes the shadow pill). To this end, we consider the implications of state-level poison pill laws (PPLs) on a firm's pill policy and financial value, consistent with a large body of studies that exploit variation from state antitakeover laws as quasi-natural experiments (e.g., Karpoff and Malatesta 1989; Giroud and Mueller 2010; Gormley and Matsa

[1] Some prior studies find a *negative* association between the adoption of a poison pill and, respectively, abnormal stock returns (Malatesta and Walkling 1988; Ryngaert 1988; Brickley, Coles, and Terry 1994; Bizjak and Marquette 1998; Gillan and Starks 2000), bond returns (Datta and Iskandar-Datta 1996), takeover propensities (Field and Karpoff 2002), and Tobin's Q (Gompers, Ishii, and Metrick 2003). Other studies, instead, find a *positive* association between the adoption of a poison pill and, respectively, stock returns (Caton and Goh 2008), takeover premiums (Comment and Schwert 1995; Cotter, Shivdasani and Zenner 1997; Heron and Lie 2006, 2015), and operating performance (Danielson and Karpoff 2006), while also finding that the poison pill does not deter takeovers (Ambrose and Megginson 1992).

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2016; Karpoff and Wittry 2018). Among these state laws, PPLs explicitly sanction the validity of the right to adopt a poison pill, thereby (as we will show) strengthening the relevance of the shadow pill.

Our main findings are twofold. First, we document that the passage of PPLs result in significant increases in the likelihood that PPL-affected firms with lower valuations (before the laws' adoption) increase their use of visible poison pills. Second, we show that the Tobin's Q of the companies incorporated in states that adopt a PPL increases significantly relative to similar firms incorporated elsewhere; relatedly, we confirm that the association between visible pills and Tobin's Q is negative and due to reverse causality (Catan 2019). These combined results underline the relevance of disentangling the value implications of the shadow pill from the ex-post endogenous decision to put an actual pill in-place.

The finding that some firms incorporated in states with PPLs are more likely to employ visible pills may be somewhat surprising. A reasonable expectation could be that firms with a strong shadow pill might be less likely to use actual pills, under the assumption that the *threat* to be able to swiftly adopt a pill (and without fear of a subsequent judicial challenge) is enough of a deterrent to thwart a hostile takeover bid. Our evidence, however, implies that firms continue to experience unsolicited takeover bids even with a strong shadow pill, which warrants the need for an actual pill. Moreover, it suggests that with a strong shadow pill, corporate boards may be less hesitant in adopting a pill when the firm becomes a target of a hostile acquisition or an activist investor, because directors are less likely to worry about subsequent legal challenges and/or about the reputational harm of being viewed as a "pro-pill" director (Johnson, Karpoff, and Wittry 2019). Therefore, our evidence suggests that a stronger shadow pill increases the use of visible pills by reducing the costs associated with the pill's implementation – costs that tend to be higher for lower-valued firms, more at risk of takeover (Edmans, Goldstein, and Jiang 2012).

As to the finding that firm value tends to increase after PPL adoption, we explore two economic mechanisms as explanation. First, we show that the increases in Tobin's Q for firms incorporated in a PPL-adopting state are more pronounced for more innovative firms and firms where firm-specific stakeholder investments are more relevant (e.g., with a large customer or in a strategic alliance). These findings support the view that the shadow pill promotes value enhancement for some firms by reducing a firm's contracting costs with its stakeholders (the "bonding hypothesis"). Second, we find more limited support for the "bargaining power hypothesis" that the strengthened legal validity granted to poison pills through a PPL helps

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boards of directors to bargain for a higher purchasing price after being targeted in a hostile takeover contest.

Because our study relies on PPLs to identify how a strengthened shadow pill affects visible pill policy and firm value, it is useful to provide some background on the legal environment relevant to these laws (which also informs our empirical strategy, following recent work such as, e.g., Catan and Kahan 2016; Cain, McKeon, and Solomon 2017; Karpoff and Wittry 2018). To start, we assume that the legal validity of the pill was arguably fairly certain after the Delaware Supreme Court's validation of the pill in the 1985 *Moran* v. *Household International, Inc.* decision due to the pervasive influence of Delaware case law over other jurisdictions (e.g., Ryngaert 1988; Cremers and Ferrell 2014). In 1988, however, two subsequent Delaware decisions – *City Capital Associates* v. *Interco Inc.* and *Grand Metropolitan PLC* v. *Pillsbury Co.* – constrained those boards' ability to use the pill, thereby limiting its general application (Catan and Kahan 2016). Indeed, in a memo sent to the clients of the law firm Wachtell, Lipton, Rosen & Katz, Martin Lipton (the corporate lawyer who "invented" the poison pill) wrote: "The Pillsbury decision yesterday fulfills the threat to Delaware corporations presaged by the Interco decision…The effect of the Pillsbury decision will be disastrous for American business…[threatening] the effective use of the poison pill …" (Martin Lipton Memos, p. 146).

Therefore, we pose that during the period 1985 to 1988 (covering most of the PPLs considered in prior studies), all firms had an effective shadow pill, irrespective of whether their incorporating state had adopted a PPL. Moreover, during this period a majority of firms incorporated in both Delaware and elsewhere actually adopted a visible pill (see Figure 1). These two circumstances likely reduced the importance of PPLs in this period. Conversely, because the *Interco* and *Pillsbury* decisions in 1988 rendered the general validity of the pill in Delaware and elsewhere relatively less certain,[2] we pose that the relevance of PPLs introduced in other states for the pill's validation increased starting from 1988. This topsy-turvy chain of judicial events could explain why most states (27-out-of-35) decided to adopt PPLs post-*Interco* and *Pillsbury*.

These changes pertaining to the legal validity of the right to adopt a poison pill motivate our focus on PPLs adopted during the period 1995 to 2009 – which we term the "second wave"

[2] While subsequent decisions in Delaware have ruled in favor of the pill (e.g., *Paramount Communications, Inc.* v. *Time, Inc.*; *Air Products and Chemicals, Inc.* v. *Airgas, Inc.*), we interpret the fact that similar cases are continually tried as indication that the pill's status in Delaware is *less* certain than in states with PPLs, where this uncertainty began with the rulings in *Interco* and *Pillsbury*. The fact that Delaware is among the few states that have maintained a separate court of equity, where the "stare decisis principle" does not apply (i.e., precedents have no binding authority) irrespective of the presiding court, could explain this continued *relative* uncertainty.

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(SW) of adoptions – rather than on PPLs that were passed between 1986 and 1990 (i.e., the "first wave" (FW) of adoptions) considered in prior studies. Starting in 1995 ensures that we have a relatively stable pre-treatment period (i.e., not confounded by the aforementioned Delaware court decisions or the hostile takeover wave of the 1980s), which helps address identification concerns. Additionally, the value implications of SW-PPLs have never been studied and, given the above changes in the legal environment, a priori it is reasonable to expect that results for this later set of PPLs might differ from results obtained by prior studies using FW-PPLs.[3]

We first investigate how the likelihood of states adopting a SW-PPL is associated with state-level characteristics (e.g., prior adoption of other major antitakeover laws, the incorporating state's M&A volume, GDP per capita and growth rate, and state business entry and exit rates) and year fixed effects. We find that the only significant predictor for the adoption of SW-PPLs is whether the adopting states had previously enacted other forms of antitakeover legislation, suggesting that the laws' passage is largely exogenous to the economic and political environment in which they were introduced.

Our principal findings on the effect of SW-PPLs on pill policy and long-term value are estimated using difference-in-differences regressions that include firm, U.S. Census division-by-year, and industry-by-year fixed effects. These findings show, first, that SW-PPLs significantly increased the propensity of affected firms with lower ex ante valuations to use visible poison pills. Second, they also document that a strengthened shadow pill, as enabled by the passage of PPLs, results in an economically and statistically significant increase of 5.9% in the relevant firms' Tobin's Q.

These findings are robust to the incorporation of possible selection effects (e.g., reincorporation) through the creation of a matched sample. We match the "treated" firms incorporated in SW-PPL-adopting states in the year prior to the laws' passage to "control" firms with similar observable ex-ante characteristics and headquartered in the same Census division and in the same industry, but incorporated in a state that has not adopted a PPL. The difference in the Tobin's Q between treated and control firms in the matched sample – as well

[3] Our results for FW-PPLs are in line with the prior literature. In particular, Karpoff and Wittry (2018) document that prior studies using business combination laws (BCLs) are potentially plagued by an omitted variable problem if they do not account for legal context – including the passage of PPLs. They show that PPLs adopted during their sample period (i.e., 1976-1995) are negatively associated with return on assets (ROA), although this negative association becomes insignificant in their subsequent tests that control for firm-level defenses, such as visible poison pills (similar to prior work, e.g., Karpoff and Malatesta 1989). Consistent with their results, we show that firms incorporated in states adopting FW-PPLs did not experience significant changes in Tobin's Q, excess stock returns, or ROA (see Online Appendix Tables OA1 and OA2), after controlling for firm-level pills, supporting our conjecture that FW-PPLs were not material due to arguably all firms having strong shadow pills in 1985-1988.

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as pre-event trends of other important firm characteristics – is insignificant in the three-year period preceding the laws' passage, while the difference in Tobin's Q is significantly positive in the three-year period following their adoptions.

Further, we find analogous results of increased value after SW-PPL adoptions using other metrics for firm value than Tobin's Q, such as Total Q (Peters and Taylor 2017), excess stock returns, and profitability. We also conduct a long-term event study surrounding the adoption of SW-PPLs, employing long (short) portfolios that buy (sell) treated (control) stocks from our matched sample around the time their (matched sample counterpart's) state of incorporation adopts a law. The resulting long-short portfolio has a positive and significant alpha of about 0.75% per month in the period surrounding the adoption of the SW-PPLs.

Finally, we examine two possible economic explanations for our finding that the shadow pill seems to positively contribute to firm value; namely the "bonding hypothesis" (e.g., Laffont and Tirole 1988; Shleifer and Summers 1988; Johnson, Karpoff, and Yi 2015) and the "bargaining power hypothesis" (e.g., Stulz 1988; Berkovitch and Khanna 1990; Kadyrzhanova and Rhodes-Kropf 2011). Under the bonding hypothesis, limiting the ability of shareholders to disrupt a firm's long-term strategy – including by strengthening a firm's shadow pill – serves as a commitment device that binds the shareholders to its current long-term strategy and a cooperative relationship with the board. Such bonding can decrease a firm's cost of contracting with its stakeholders and, thereby, improve long-term firm value. Under the bargaining power hypothesis, having the right to adopt a poison pill strengthens the negotiating position of the board vis-à-vis any potential bidder, allowing directors to obtain a higher offer price for the target's shareholders.

In support of the bonding hypothesis, we find that firms incorporated in a state that adopts a PPL and for which stakeholder relationships are likely more relevant – such as firms that are more engaged in long-term investments in innovation, have a large customer, are in a strategic alliance, or are more labor intensive – experience a higher increase in Tobin's Q. Consistent with recent work showing that investments in intangibles (such as R&D, patenting, human capital, etc.) have been steadily increasing over time (e.g., Corrado and Hulten 2010; Peters and Taylor 2017), this finding suggests that the bonding hypothesis would have become increasingly more important during the SW-PPL period.

We also find some, though more limited, evidence in support of the bargaining power hypothesis. Specifically, firms with a pill in place receive a higher premium in a hostile takeover after their state adopts a PPL, which might partly explain why firms with a strong shadow pill increase their use of visible pills (as their shareholders would profit from this).

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This finding also lends support for the argument that potential frictions in the adoption of a pill (such as board coordination costs or the anticipated transaction costs arising from future legal disputes; see also Johnson, Karpoff, and Wittry 2019) warrants the need for an actual pill, and that PPLs help mitigate these frictions. However, the bargaining power hypothesis does not explain the full range of our results and in our view acts as a supplementary mechanism to the bonding channel.

Overall, our study contributes to the literature on the poison pill and, more generally, takeover defenses, in the following ways. First, our analysis of the adoption of both FW- and SW-PPLs extends prior work only considering FW-PPLs (e.g., Karpoff and Malatesta 1989; Karpoff and Wittry 2018).[4] Second, we confirm the insignificant results obtained by earlier studies on the association between FW-PPLs and firm value, and show that SW-PPLs are positively related to these same measures. We explain this difference through the changed legal context surrounding poison pills from the FW- to the SW-period. Third, we assemble a comprehensive panel dataset on firm-level pills to test (for the first-time) the impact of PPLs on pill policy. Finally, we contribute to the literature examining the relationship between takeover defenses and firm value (for a review see, e.g., Straska and Waller 2014), finding support particularly for the bonding hypothesis of takeover defenses (e.g., Cen, Dasgupta, and Sen 2015; Johnson, Karpoff, and Yi 2015, 2018; Cremers, Litov, and Sepe 2017), as well as some supplementary support for the bargaining power hypothesis (e.g., Comment and Schwert 1995; Heron and Lie 2006, 2015; Kadyrzhanova and Rhodes-Kropf 2011).

1. Data and Empirical Framework

1.1. Sample selection, definition of variables and descriptive statistics

We start the construction of our primary dataset by combining information on firm-level poison pills from two institutional providers, four prior academic studies, and our own hand-collected sample. The institutional data providers include the Securities Data Companies (SDC) Corporate Governance and the Institutional Shareholder Services (ISS) Governance databases. We supplement these observations with poison pill data from Comment and Schwert (1995), Caton and Goh (2008), Cremers and Ferrell (2014), and Cremers, Litov and Sepe (2017). Additionally, we add our own hand-collected data from Factiva searches on firms with missing pill information from the sources above over the period 1992–2012.

[4] To the best of our knowledge, only one published study – Cain, McKeon, and Solomon (2017) – considers both FW- and SW-PPLs. However, Cain, McKeon, and Solomon's main focus in using PPLs is to combine them together with 16 other takeover laws and court decisions to construct a firm-level "takeover susceptibility index." In constructing this index, they find that PPLs do not impact hostile takeover activity.

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The resulting sample contains firm-level poison pill ($PPill$) information on 3,423 unique firms between 1983 and 2012, which we merge with the industrial firms (excluding utilities and financials) in the CRSP-Compustat database. To be included in the sample, we require that firms are incorporated and headquartered in the U.S. with non-missing or non-negative book value of assets or net sales and without missing observations for the dependent and independent variables used in our baseline regression models. This selection criterion results in a panel with 34,888 firm-year observations covering the period 1983–2012, which begins and ends three-years before and after the first and last state adopts a PPL. Per our discussion in the introduction, we then partition this dataset into two separate samples encompassing the first (1983 to 1993) and second (1992 to 2012) wave of PPL adoptions.

Our study's key independent variable, PPL, is an indicator capturing whether a firm is incorporated in a state that has passed a PPL at any point between 1986 and 2009. We obtain information on whether states have passed one of these laws from Cain, McKeon, and Solomon (2017) and Karpoff and Wittry (2018) and report each state's adoption date in Online Appendix Table OA3. We use historical incorporation and location information from Compact Disclosure covering the period 1986 to 2006 and the CRSP Historical U.S. Stock database (available directly from the University of Chicago, though currently not included in WRDS) between 1990 and 2012.[5] Combining law adoption dates and historical incorporation data, we construct the indicator variable, PPL, which is set equal to one in the adoption year and afterwards for all firms incorporated in the enacting states and set to zero in the years prior to adoption of the law. PPL always equals zero for firms in states that never passed a PPL, including firms incorporated in Delaware.[6]

Along with $PPill$, which measures the adoption and maintenance of poison pills, we study the separate implications of PPLs for new adoptions of pills ($New\ PPill$) and the duration of existing pills ($Ln(PPill\ Duration)$). $New\ PPill$ is defined as an indicator equal to one if a firm adopts a poison pill for the first-time in the current year, and zero otherwise. $Ln(PPill\ Duration)$ is measured as the natural logarithm of one plus the number of years a firm has had an existing pill in-place as of the current year.

[5] We backfill states of incorporation (and location) for firm-years prior to 1986 using the oldest observation from either the Compact Disclosure or CRSP Historical database.

[6] Given Delaware's prominence, its history of poison pill case law, and the empirical uncertainty that Delaware's unique regime creates for the validity of pill adoption and redemption, we verify that our main findings are robust to: (i) setting *PPL* equal to one for Delaware firms after *Moran*, (ii) excluding firms incorporated in Delaware entirely, and (iii) creating a "poison pill validity-index" ($PPV\text{-}Index$) that captures relative certainty about the legality of the pill as a takeover defense based on both state-level PPLs and poison pill-related court decisions (such as, e.g., *Moran*).

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We employ Tobin's Q (Q) as our main measure of firm value, consistent with prior work examining the value relevancy of corporate governance arrangements (e.g., Morck, Shleifer, and Vishny 1988; Gompers, Ishii, and Metrick 2003; Cremers, Litov and Sepe 2017). We follow Fama and French (1992) and define Q as the ratio of market to book value of assets using financial data from Compustat. Using this definition of Tobin's Q, our dependent variable in most regressions is its natural logarithm ($Ln(Q)$) as in, for example, Bebchuk, Cohen, and Ferrell (2009) and Atanassov (2013).

We recognize, however, that Tobin's Q is an imperfect measure of value; for example, because it can also proxy for a firm's growth opportunities (Jung, Kim, and Stulz 1996) and is subject to potential measurement error (Erickson and Whited 2012). Therefore, in robustness tests, we analyze the implications of PPLs for the following alternative metrics of firm value: Total Tobin's Q ($Total\ Q$), which is a modified version of Q that includes intangible capital in the denominator (Peters and Taylor 2017) (data comes from the WRDS database: Peters and Taylor Total Q); excess stock returns in both an annual regression setting ($Excess\ Return$) and using a monthly portfolio approach ($Alpha$), measured using either the Fama-French four-factor (Carhart 1997), three-factor (Fama and French 1993), or market models (returns data comes from the CRSP database); return on assets (ROA), measured as operating income before depreciation and amortization scaled by total assets (Giroud and Mueller 2010) (data comes from Compustat).

Following Karpoff and Wittry (2018), we include controls for the other most common state antitakeover statutes: business combination laws (BCLs), control share laws ($CSLs$), directors' duties laws ($DDLs$), and fair price laws ($FPLs$). We further exclude firms with observed lobbying activity for specific antitakeover statutes (Karpoff and Wittry 2018, Table III, p. 662); winsorize all of the continuous variables at the 5% level in both tails to mitigate the influence of extreme outliers;[7] and adjust dollar values for inflation using 2015 dollars. As we generally use three-digit SIC group-by-year fixed effects, we drop firm-years with a unique three-digit SIC code (i.e., "singleton groups"). Appendix Table A1 provides variable definitions.

Table 1 reports the mean, standard deviation, 25th, 50th and 75th percentiles, and the total number of observations for the main variables in our dataset for the period, 1992–2012, which begins and ends three years before and after the first and last SW-PPL states adopted their laws. Our main sample is comprised of 25,465 firm-year observations. The average percentage of

[7] Our findings are unchanged if, instead, we winsorize continuous variables at the 1% or 2.5% level in both tails.

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firm-years in our main sample in which a company has a $PPill$ in-place is 40.7% and has a standard deviation of 0.49. The respective average Q in our focal SW-sample is 1.9 with a standard deviation of 0.92, while 32.2% of the observations during this period are affected by a PPL. Online Appendix Figure OA1 plots the percentage of firms in our sample that are incorporated in FW-PPL and SW-PPL states (and in Delaware) each year from 1992 to 2012, and we provide supplementary descriptive statistics in Online Appendix Tables OA4 and OA5.

1.2. Identification strategy

We investigate the relevancy of the shadow pill for firm-level pill adoptions and value by exploiting the quasi-natural experiment created by the staggered enactment of PPLs by firms' state of incorporation. The key assumption underlying this strategy is that the enactment of these laws provides an exogenous "shock" to the takeover protection of firms incorporated in the adopting states through the strengthening of the shadow pill. An essential step in verifying the plausibility of this assumption is to assess the likelihood that state adoptions of PPLs are related to certain local characteristics (e.g., state macroeconomic factors) that might also correlate with individual firms' pill decisions and value and, thereby, invalidate the exclusion restriction of the identification strategy.

To examine this concern, we follow a similar approach as Acharya, Baghai, and Subramanian (2014) and Serfling (2016) and analyze the predictability of PPLs. We estimate a Cox proportional hazard model, where the dependent variable is PPL. As predictor variables, we consider state-level firm, macroeconomic, political economy, and corporate law factors that a priori could determine these laws' enactment, along with year fixed effects. We explore the possibility of a reverse causality problem by constructing the state-year ('SY') propensity of firms incorporated in the state ('$Inc.$') to have a poison pill in place ($Inc.SY\ PPill$), and through using the medians across all sample firms incorporated in a given state of three separate measures of firm value ($Inc.SY\ Q$, $Inc.SY\ Return$, and $Inc.SY\ ROA$). In addition, we include predictors for whether the state has already adopted another common antitakeover law (BCL, CSL, DDL, and FPL).

Other predictors include the state's level of M&A activity ($Inc.SY\ M\&A\ Volume$), log GDP per capita ($Ln(Inc.SY\ GDPPC)$) and growth rate ($Inc.SY\ GDP\ Growth$), a dummy for whether the majority of a state's U.S. House of Representatives belongs to the Republican Party ($Political\ Balance$), a state's level of population ($Ln(Inc.SY\ Pop)$), rates of unemployment ($Inc.SY\ Unemploy$) and state business entry and exit ($Inc.SY\ Entry$ and $Inc.SY\ Exit$). We include year fixed effects to account for transitory U.S.-wide factors (e.g., macroeconomic

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conditions). In the main analysis, we focus on SW-PPLs, which are unexplored by prior literature, using the sample period 1992 to 2012. The predictor variables are measured in the year prior to the law's passage and we drop states from the analysis once they adopt a PPL. We standardize the continuous variables to have a mean of zero and unit variance in order to ease comparisons across coefficients and estimate standard errors clustered at the state of incorporation level. Table 2 presents our findings.

The evidence from each of the four columns in Table 2 suggests that only the prior enactment of other antitakeover laws consistently predicts the passage of SW-PPLs. In particular, states with pre-existing BCLs and FPLs are more likely to adopt PPLs during the SW-period than states without this legislation. The coefficients pertaining to a states' median level of poison pills, Tobin's Q, stock returns, and ROA are insignificant (Columns (2) to (4)), so that reverse causality is unlikely to be a concern for our identification. The coefficients on $Ln(Inc.SY\ GDPPC)$ and all other state-level macro-economic and political factors are also always statistically insignificant, suggesting that the passage of SW-PPLs is not driven by local economic conditions. We conclude that the findings in Table 2 are consistent with the assumption that states' firms' characteristics and economic and political factors do not significantly influence whether their legislators adopt SW-PPLs.

1.3. Empirical specification

Our baseline investigation of the implications of the shadow pill employs a difference-in-differences regression model, comparing changes in either poison pill status or firm value amongst firms incorporated in states with PPLs relative to those of firms incorporated elsewhere. Specifically, we estimate

$$y_{[ijlst]} = \beta PPL_{[st]} + \alpha' \mathrm{ATS}_{[st]} + \delta PPL_{[st]} \times X_{[i\tau(s)-1]} + \gamma_{[i]} + \omega_{[lt]} + \lambda_{[jt]} + \varepsilon_{[ijlst]} \quad (1)$$

where y denotes either a poison pill- or value-based measure of firm i, operating in industry j, headquartered in U.S. Census division l, incorporated in state s, in year t. Our main independent variable, $PPL_{[st]}$, is an indicator for whether a firm's incorporation state s has adopted a PPL as of the current year t, while $\mathrm{ATS}_{[st]}$ represents a vector of dummy variables to control for the four other most common anti-takeover statutes (BCL, CSL, DDL, FPL).

In most of our specifications evaluating the effect of PPLs on visible pill policy (firm value), we include $X_{[i\tau(s)-1]}$ to control for a PPL-firm's Tobin's Q (poison pill status) in the specific year before the adoption of its state's respective law – denoted with the subscript

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$\tau(s) - 1$, where $\tau(s)$ denotes the year that state s adopts a PPL. Therefore, $X_{[i\tau(s)-1]}$ is not time-varying. We interact $X_{[i\tau(s)-1]}$ with the PPL dummy to control for PPL-affected-firms' pre-law X characteristic in the post-law adoption period.

We undertake this approach to avoid the problem of specifying "bad controls" (Angrist and Pischke 2009). For example, if we included a time-varying control for firm value in the poison pill regression, then this could bias the coefficient on PPL and render any causal inference invalid if the control itself may be affected by the PPL (which we provide evidence for in this case). Our models also include firm fixed effects, γ, to control for unobserved, time-invariant heterogeneity within firms, and U.S. Census division-by-year, ω, and industry-by-year interacted fixed effects, λ, to control for unobserved, time-varying heterogeneity within divisions of location and industries, respectively. Finally, we cluster our standard errors by states of incorporation, which results in the most conservative *t*-statistics.

The U.S. Census division dummies are defined using the U.S. Census Bureau's nine geographical subdivisions (New England, Middle Atlantic, East North Central, West North Central, South Atlantic, East South Central, West South Central, Mountain, and Pacific). Importantly, this specification ensures that our inference is robust to many sources of unobserved, time-variant heterogeneity that could bias our estimates, including local macroeconomic factors that are likely shared by states within close geographic proximity (Heider and Ljungqvist 2015). We assign a firm's division-of-location based on its (historical) state of headquarters because this is generally where its major plants and operations are located (Henderson and Ono 2008).

The three-digit SIC industry-by-year fixed effects control for potential unobserved time-varying industry trends. Prior work shows that merger waves tend to occur within industries (e.g., Mitchell and Mulherin 1996; Rhodes-Kropf, Robinson, and Viswanathan 2005). If the staggered adoption of PPLs across states is correlated with M&A activity – though Table 2 suggests this is not the case – or with other unobservable characteristics that also impact firms' visible pill policy and firm value, our use of industry-by-year fixed effects account for this source of confounding variation. The division-by-year fixed effects also control for some of this variation since most industries cluster by geography (Ellison, Glaeser and Kerr 2010).

A common alternative strategy developed in the prior literature to deal with local sources of unobserved confounding variation is to use fixed effects at the level of the state where the corporate headquarter is located (Gormley and Matsa 2014, 2016). Our results are robust to using this approach. A limitation of this strategy, however, is that it relies on the assumption

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that most firms are incorporated and headquartered in different states. For example, Gormley and Matsa (2016, p. 437) "are able to obtain estimates for the BC laws' effect even after including state-by-year fixed effects because more than 60% of [their sample] firms are incorporated and located in different states." In contrast, only 28% of the firms in our sample that are incorporated in a PPL-adopting state are headquartered somewhere else (similarly, fewer than 29% of the non-Delaware-incorporated firms that are in states without these laws are headquartered outside of their incorporation state). In contrast, more than 99% of Delaware-incorporated firms are headquartered in a different state. Therefore, the use of headquarter-state-by-year fixed effects in our setting leaves only a relatively small amount of variation to estimate the coefficient on PPL. This limits our tests' statistical power and restricts our controls to almost exclusively Delaware-incorporated firms. This latter point is especially relevant, as it increases the likelihood that some other confounding events in Delaware (e.g., other poison pill case law) might bias our point estimates. Therefore, we use U.S. Census division-by-year fixed effects as an alternative approach to address these econometric issues.

2. Legal Background

Our study relies on PPLs to identify how a strengthened shadow pill affects pill policy and firm value. This section provides background on the historical and legal environment relevant to the PPLs that form the basis of our identification strategy.

2.1. First wave-PPLs in the shadow of Delaware

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. Whether or not this decision also affirmed the validity of the poison pill for firms incorporated outside of Delaware has been the subject of debate. Some law and finance scholars describe the legal status of the pill for non-Delaware firms as uncertain until these firms' states of incorporation adopted a PPL (Catan and Kahan 2016; Cain, McKeon, and Solomon 2017; Karpoff and Wittry 2018). The argument commonly given to defend this view is that, while court decisions in some of the other U.S. states upheld the validity of the pill in the years immediately following *Moran*, the states of New York, New Jersey, Georgia, Wisconsin, Colorado, Virginia, and Indiana all had court decisions that invalidated the use of the poison pill between 1986 and 1989 (Catan and Kahan 2016; Cain, McKeon, and Solomon 2017). Therefore, the uncertainty created by these decisions would only have been cleared when legislators in those states (and elsewhere) decided to pass a PPL.

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Other scholars, instead, claim that the above argument discounts evidence on the pervasive authority of Delaware judicial decisions over non-Delaware corporations (Ryngaert 1988; Cremers and Ferrell 2014). The alternative argument made by these scholars – which is also our interpretation – is that because of the influence of Delaware's case law, the validity of the poison pill was, in fact, fairly certain in the immediate aftermath of *Moran* for firms incorporated both in Delaware and outside of Delaware. As shown by Figure 1, the widespread adoption of visible poison pills, even for non-Delaware firms (incorporated in states without PPLs) in the years immediately following *Moran,* is consistent with the view that this ruling was understood to apply to non-Delaware firms as well (Helman and Junewicz 1986; Fleischer, Hazard, and Klipper 1988). This interpretation is also supported by the evidence that many of the court decisions that did uphold the pill in non-Delaware states referenced *Moran* in their poison pill rulings. Further, even in states where court decisions intervened to invalidate the pill, the uncertainty on the status of the pill did not last long. 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.*), the state of New York passed a PPL in December of the same year.

Under the view that Delaware case law helps to shape corporate law in all other states, it is also reasonable to assume that subsequent Delaware decisions that partially amended *Moran* also increased the uncertainty of the pill's validity for firms incorporated in states that had not adopted a PPL. In particular, in the fall of 1988, the Delaware courts issued two decisions – *City Capital Associates* v. *Interco Inc.* and *Grand Metropolitan PLC* v. *Pillsbury Co.* – that unexpectedly limited the general applicability of the poison pill.[8] As described by Catan and Kahan (2016), *Interco* and *Pillsbury* were among "the most important legal developments for Delaware in 1988," as they "imposed severe constraints on the use of poison pills" (p. 645).[9] These decisions prompted considerable comment at the time, with corporate lawyers predicting that the effect of *Interco* and *Pillsbury* on American business would be "disastrous" and some of them recommending firms to move out of Delaware (Fleischer and Sussman 2013).[10]

[8] In both of these decisions, the Delaware court halted the continued use of a visible poison pill that prevented an unsolicited tender offer.

[9] Here, we think it is useful to distinguish between two dimensions of uncertainty affecting pill policy. The first – on which we focus – concerns the legal validity of the pill. Under this dimension, *Moran* validated the use of the pill as an antitakeover defense, making its legal validity more certain. Conversely, by introducing restrictions to the use of the pill, *Interco* and *Pillsbury* injected uncertainty in the pill's validity regime. The second dimension pertains to the scope of judicial scrutiny over the pill. Under this second dimension, *Moran* left broad discretion to the court in the exercise of judicial scrutiny, therefore also leaving (more) room for uncertainty. To the contrary, by setting specific parameters, *Interco* and *Pillsbury* reduced the uncertainty over the scope of judicial scrutiny.

[10] For example, Martin Lipton wrote to his clients that: "Unless Delaware acts quickly to correct the [Interco and] Pillsbury decision[s], the only avenues open to the half of major American companies incorporated in Delaware

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Consistent with the above assumption about the limitations imposed by *Interco* and *Pillsbury*, Figure 1 indicates that the average percentage of "All Firms" in our sample with a pill in-place began to decrease soon thereafter in the early 1990s (after having steadily increased in the preceding takeover-intense years).[11] This figure also suggests that the decrease in the percentage of firms with a pill is less pronounced for firms incorporated in states that had previously enacted PPLs ("FW-PPL Firms") relative to firms incorporated in states without similar laws sanctioning an explicit right to adopt poison pills ("Delaware Firms" and "Other-No-PPL Firms"). This seems consistent with our interpretation that FW-PPL firms were the least affected by the *Interco* and *Pillsbury* decisions and, thus, more likely to retain their existing pills as the validity of the pill was arguably more certain for them.

This interpretation might only provide a partial explanation for the behavior of the curve in Figure 1, as other events (outside of the court decisions) likely impact the percentage of firms in our sample with pills. For example, new firms enter the sample due to an IPO or spinoff, exit the sample due to being acquired or going bankrupt, and, for firms incorporated in SW-PPL states, changes from the "Other-No-PPL Firms" to the "SW-PPL Firms" group as their respective states adopt a law.

Online Appendix Figure OA2 addresses some of these other events by plotting the dynamics of maintaining a pill within six different cohorts of firms through time (until the end of our sample, 2012), where no new firms are allowed to enter each cohort subsequently.[12] In Panel A of Figure OA2, we observe a clear and sustained separation between the maintenance of pre-existing pills by the PPL-firms relative to the No-PPL-firms starting in 1999.[13] This

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will be federal legislation…or leaving Delaware for a more hospitable state of incorporation" (Martin Lipton Memos, p. 146).

[11] Figure 2 in Cremers and Ferrell (2014) suggests, similar to our Figure 1, that firms began adopting pills meaningfully in 1985 and continued to do so until roughly 1988. However, their percentage of sample firms with a pill plateaus between 1989 and 2006, while our Figure 1 indicates a steady decline in the percentage of firms with pills until reaching a plateau between 1997 and 2006. This difference might be explained by our sample, including a more diverse set of firms than those used in Cremers and Ferrell (S&P 1500 firms), that were likely more at-risk of takeover during the corporate raiders' era and, thus, adopted pills more often. Over the period 1996 to 2012, in which the hostile takeover era of the 1980s had subsided, the overall percentages in our Figure 1 are quite similar with Figure 1 in Catan (2019). For example, both figures suggest a percentage of firms with a poison pill of about 42% to 46% in 1996, with the overall trend increasing until about 2003 or 2004, followed by a precipitous drop down to about 12% by 2012.

[12] The six cohorts of firms are constructed as follows: (1) firms incorporated in a PPL state ("PPL Firms") and with a pill in 1986 (i.e., right after *Moran* and the start of the FW of PPL adoptions), (2) firms incorporated in a state without a PPL ("No-PPL Firms") and with a pill in 1986, (3) PPL-firms with a pill in 1989 (i.e., right after the *Interco* and *Pillsbury* decisions), (4) No-PPL firms with a pill in 1989, (5) PPL-firms with a pill in 1995 (i.e., the start of the SW of PPL adoptions), and (6) No-PPL firms with a pill in 1995.

[13] Before 1994, both firms incorporated in PPL- and non-PPL-adopting states that had a pill in 1986 maintain those pills at the same rate until 1993. From 1994 to 1996, PPL-firms keep their pre-existing pills at a slightly higher rate before both cohorts of firms exhibit a roughly 60% drop in their use of the pill in 1997. This decrease

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evidence is supportive of our claim that firms in states without PPLs were more likely to let their pills expire due to the greater uncertainty surrounding the general validity of the pill in the post-*Interco* and *Pillsbury* era. Under this uncertainty, the decision not to renew a pill might well have been motivated by the expectation that a pill renewal would have likely faced frictions, such as a challenge in courts by disgruntled shareholders.[14]

We lend further evidence to this conjecture (and improve on our univariate analyses in the figures) in Online Appendix Table OA6 by regressing a poison pill indicator variable ($PPill$) on dummies for whether a firm's state of incorporation adopts a FW-PPL ($FW\ PPL$), or eventually adopts one of these laws during the SW ($Eventual\ SW\ PPL$). Our sample periods encompass the "Entire FW-Period" of 1983 to 1993 (Columns (1)-(2)), the "Post-*Moran*" period of 1986 to 1993 (Columns (3)-(4)), and the "Post-*Interco* & *Pillsbury*" period of 1989 to 1993 (Columns (5)-(6)). Following prior studies (e.g., Comment and Schwert 1995), we include dummies for other common antitakeover laws (BCL, CSL, DDL, and FPL) and specify firm, division-by-year, and industry-by-year fixed effects. The standard errors are adjusted for clustering at the state of incorporation level.

Consistent with the argument that *Moran* validated the use of the pill for both Delaware and non-Delaware incorporated firms at least until November 1988, we document that the point estimates on $FW\ PPL$ are always statistically insignificant in Columns (1)–(4) of Table OA6. Furthermore, when we consider the use of the pill by firms incorporated in states that eventually adopted PPLs during the SW-period ($Eventually\ SW\ PPL$), we show that these firms are equally likely to adopt pills as firms incorporated in Delaware and in other non-Delaware states that never enacted a PPL during the full FW-period (Column (2)) and the Post-*Moran*-period (Column (4)). The last two columns of Online Appendix Table OA6 show that firms incorporated in states with FW-PPLs were 7.2% to 10.5% more likely to have a pill in-place in the "Post-*Interco* & *Pillsbury*" period (1989–1993). The results in Column (6) indicate that firms incorporated in states that eventually adopted an SW-PPL, but at the time of the analysis are not covered by a law, are 23.9% less likely to employ the use of a poison pill after 1988. This could plausibly explain why most states (27-of-the-35) adopted PPLs post-November of

in pills is likely attributable to the fact that the typical shareholder rights plan (in the late-1980s) is issued with a 10-year expiration date and that most firms began meaningfully adopting pills in 1987 (after the *Moran* decision and at the start of the hostile takeover wave).

[14] Our inference is similar for the cohorts of firms assembled after the *Interco* and *Pillsbury* decisions in 1989 (Panel B) or at the start of the SW of PPL adoptions in 1995 (Panel C).

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1988, as the general viability of the pill as a strong defense was no longer assured after *Interco* and *Pillsbury*.

Considering this legal context and consistent with the prior literature's argument that "the institutional, political-economy, and historical context in which a law is enacted has a large effect on the appropriate specification and interpretation of tests that use legal changes for identification" (Karpoff and Wittry 2018, page 658), our analysis focuses on SW-PPLs that were passed during the 1995 to 2009 period. This also ensures that we have a relatively stable pre-treatment period – i.e., unconfounded by the passage of Delaware court decisions related to the use of the pill or the hostile takeover wave of the 1980s – and, thus, mitigates the likelihood of measurement error that could bias our estimates.

3. Main Results

Our main research question considers how a "strengthened" shadow pill – as measured by the adoption of a PPL – impacts actual pill policy and firm value. We first analyze the relation between PPLs – focusing on SW-PPLs for the reasons explained in Section 2 – and firm decisions to adopt and maintain poison pills, as well as new pill adoptions and existing pill redemptions. Second, we estimate the value implications of PPLs using Tobin's Q regressions. We also show that our main specification using Tobin's Q is robust to additional tests, such as a matched sample analysis, and to using alternative measures of firm value.

3.1. Shadow pills and visible pill policy

We begin our empirical analysis of PPLs by examining their relationship with firm-level poison pills. We hypothesize that there are at least two potentially competing effects governing a firm's decision to implement a pill when its shadow pill is strengthened by the enactment of PPLs. On the one hand, if visible poison pills do not provide incremental protection beyond the shadow pill, we might anticipate that firms do not alter their use of actual pills or even decrease their reliance on them (i.e., a "substitution effect"). Under this view, the threat to be able to adopt a pill on short notice and without fearing a subsequent challenge in court (i.e., a stronger shadow pill) should be enough of a deterrent to thwart a hostile takeover bid.

On the other hand, if there are frictions to pill adoption – e.g., the risk of subsequent judicial challenges and/or the reputational costs that are associated with being pro-pill for directors (Johnson, Karpoff, and Wittry 2019) – then we might expect firms to still need an

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actual pill and PPLs to help ease these related frictions.[15] Moreover, if the threat of a shadow pill is not enough and corporate raiders or activist investors pursue a target in spite of its state's PPL, a visible pill would still be necessary. Under these circumstances, we would then expect firms incorporated in PPL-adopting states to have pills in-place more frequently (i.e., a "validation effect"). We start by testing these predictions in Table 3 by regressing poison pill-based measures on PPL plus other controls and firm, division-by-year, and industry-by-year fixed effects.

In Column (1), we find evidence that seems to point toward the validation effect under which PPLs would ease the frictions involved with a pill's adoption and, hence, increase the number of visible pills. This evidence, however, is not conclusive because, although the point estimate on PPL is positive, it lacks statistical significance. A follow-on consideration is whether a stronger shadow pill might affect the visible pill policy for some PPL-firms differently. In particular, prior work has found that a lower valuation increases a firm's susceptibility to takeover (Cremers, Nair, and John 2009; Edmans, Goldstein, and Jiang 2012). Motivated by this finding, we first test whether lower-valued firms, irrespective of PPLs, are more likely to adopt pills.

Column (2) adds lagged firm value-based regressors to the model. Specifically, we create the following four dummies, $Q(Lowest)_{[t-1]}$, $Q(Low)_{[t-1]}$, $Q(High)_{[t-1]}$, and $Q(Highest)_{[t-1]}$, by forming quartiles based on our sample's empirical distribution of Tobin's Q. The respective dummies are set to one if the firm's level of Q lies in the bottom, middle-to-bottom, middle-to-top, or top quartile, respectively. The specifications using these "bad controls" (Angrist and Pischke 2009) are a robustness check, and the subsequent columns instead use only controls that are not affected by the PPL. The positive coefficients on $Q(Lowest)$ and $Q(Low)$, and negative coefficient on $Q(Highest)$ suggest a potential reverse causality problem (consistent with, e.g., Cremers and Ferrell 2014; Catan 2019), where the term $Q(High)$ is dropped due to multicollinearity. This finding indicates that lower- (higher-) valued firms – and, thus, firms more (less) at risk of receiving a takeover bid – are, respectively, 2.9% to 4.6% (3.9%) more (less) likely to have a pill in-place.

[15] Some boards may opt to adopt a pill on a "clear day" (i.e., at a time that they are not a takeover target) or, as is standard practice nowadays, draft a rights plan on a clear day to be kept "on the shelf and ready to go" in the event that it becomes necessary to activate. Among other benefits, a pill "on the shelf" could ease some of the reputational costs that are associated with being pro-pill (i.e., frictions), as directors would only bear those costs if the pill is eventually activated. A recent study by Johnson, Karpoff, and Yi (2019), however, suggests that directors who are associated with pill adoption experience negative labor market consequences. They find no significant difference for directors whose first pill was a clear day pill and those that adopted a pill in the midst of a takeover.

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We supplement this finding by estimating regressions of $Ln(Q)$ on "relative year" dummy variables that indicate the number of years before and after the year in which a firm adopts a poison pill, along with firm, division-by-year, and industry-by-year fixed effects (following a similar approach as in Catan 2019). We include relative year dummies for up to 10 years before and after a pill's adoption and we estimate robust standard errors adjusted for clustering by firm since both of these variables are measured at this level. The resulting point estimates and 95% confidence intervals of the relative year dummies are plotted in Online Appendix Figure OA3. The figure provides suggestive evidence that firm value significantly declines in the five years before a firm decides to deploy a poison pill. Meanwhile, the association with Tobin's Q is insignificant in the year of, and each of the five years after, the pill's adoption. This finding thus supports 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 (Cremers and Ferrell 2014; Catan 2019).

We next test whether the validation effect of a stronger shadow pill might be more pronounced for lower-valued PPL-firms, which are both more likely to be at risk of a future takeover and more likely to face significant frictions in adopting a pill (e.g., the expected reputational costs of adopting a pill are likely to be higher for the directors of a low-valued firm). Rather than using "bad" controls that may be affected by the PPL, we construct a control variable based on the distribution of firm-level Q in the state in the year before that state's PPL is adopted (denoted by $\tau(s) - 1$) and then form quartiles based on the empirical distribution. We then interact PPL with each of these measures – $Q(Lowest)_{[\tau(s)-1]}$, $Q(Low)_{[\tau(s)-1]}$, $Q(High)_{[\tau(s)-1]}$, and $Q(Highest)_{[\tau(s)-1]}$. In Column (3), we confirm that PPLs validate the adoption and maintenance of $PPill$ for firms with the lowest values of Tobin's Q in the year before the PPL-adoption. In particular, this subset of firms are 9.7% more likely to have a poison pill relative to PPL-firms with higher values of Q.

We then separately consider the decision to adopt a new poison pill ($New\ PPill$) and how long pills are kept in place ($Ln(PPill\ Duration)$) to distinguish how PPLs affect adoptions of new pills relative to the maintenance of existing pills. Columns (4)-(6) use $New\ PPill$ as the dependent variable and include our full set of fixed effects. We find that the average firm incorporated in a PPL-adopting state does not alter their frequency of new poison pill adoptions (Columns (4) and (5)); rather, only firms with Tobin's Q in $\tau(s) - 1$ (i.e., in the year before the firm's state of incorporation adopts a PPL) in the lowest two quartiles significantly increase their use of new pills at a rate of 14.5% and 15.9%, respectively (Column (6)). Using

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$Ln(PPill\ Duration)$ in Columns (7)-(8), we find that the average PPL-affected firm does not alter the duration of its existing pills. However, in Column (9), we find that firms with both the lowest and highest levels of Q in the year before PPL adoption significantly increase the duration of their pills in-place relative to the other PPL-firms.

We interpret the non-monotonic effect of PPLs on the duration of pills in the following way. The lowest-valued PPL firms – which are both most at risk of takeover and more likely to face significant implementation frictions – do not just adopt, but also maintain pills longer than the other intermediately-valued PPL firms. Conversely, the higher-valued firms – which are less exposed to the risk of a takeover bid and face lower frictions costs – would be less likely to be confronted with the need to adopt a new pill, but also with situations that would warrant the need to make a decision about rescinding an existing pill (likely adopted during the takeover intense years). This, in turn, would explain why the validation effect of a stronger shadow pill would be limited to the duration of the pill for higher-valued firms incorporated in PPL-enacting states (and not new adoptions). Overall, these findings are also consistent with the general, unconditional, level in Figure 1 of pill usage by "SW PPL Firms." Indeed, while the overall trend in the poison pills used by all firms in our sample during most of the SW-period is negative, the average level of pill usage is greater for firms in SW-PPL states starting in 1995 and continuing for much of the next decade.

3.2. Shadow pills and long-term firm value

In this section, we investigate the value implications of a strengthened right to adopt a poison pill, focusing on Tobin's Q as our primary measure of firm value. We address the concern that a potential selection bias (e.g., reincorporation) might drive our results by constructing a matched sample, where all "treated" firms are incorporated in PPL-adopting states at least one year before its passage. We also check the robustness of our findings by examining the effect of PPLs on alternative measures of value such as stock returns and profitability. Finally, supplementary robustness tests are included in the Online Appendix.

3.2.1. Full sample

Table 4 reports the difference-in-differences estimates of the impact of the adoption of PPLs by state legislatures on the Tobin's Q of firms in enacting states over the period 1992 to 2012. Each of the five columns employs $Ln(Q)$ as the dependent variable and includes controls for each of the other four antitakeover laws (BCL, CSL, DDL, and FPL). Columns (1)–(3) include our default set of fixed effects – firm, division-by-year, and industry-by-year –

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whereas, the last two columns check the robustness of our results to controlling for local "shocks" using regions or headquarter states instead of divisions. The standard errors are adjusted for clustering at the state of incorporation level.

We find that the adoption of PPLs has a positive and statistically significant impact on the Tobin's Q of firms in enacting states. In Column (1), without including any firm-level controls, we find that firms incorporated in a state that adopts a PPL experience an increase in firm value of 5.9% relative to firms incorporated elsewhere, but operating in the same U.S. Census Division and sharing a similar industry trend.[16] The estimated coefficient on PPL in Column (2) is similar, showing robustness for controlling for visible poison pills ($PPill_{[t-1]}$). The estimated coefficient on $PPill_{[t-1]}$ confirms the results in the prior literature of a negative correlation between actual firm-level pills and Tobin's Q (e.g., Bebchuk, Cohen and Ferrell 2009; Cremers and Ferrell 2014). However, in light of our results in Table 3 and Online Appendix Figure OA3, the negative association between visible pills and Tobin's Q seems endogenous and due to reverse causality.

Further, the model in Column (2) suffers from an endogeneity problem because PPLs also affect visible pill policy, rendering $PPill_{[t-1]}$ a "bad control." Therefore, in the remaining columns we instead interact PPL with $PPill_{[\tau(s)-1]}$, i.e., with an indicator variable for whether the firm has a visible poison pill in the year before the adoption of the firm's respective state's PPL. We find that the point estimate (-0.008) on the interaction is both economically and statistically insignificant (t-stat=-0.19), while the standalone coefficient on PPL (point estimate=0.062) remains significant at the 1% level (t-stat=2.76), which suggests that shadow pills create long-term value for shareholders independent of the presence of actual pills.

The last two columns of Table 4 serve as robustness checks. Rather than using division-by-year fixed effects, we alternatively employ fixed effects based on U.S. Census Regions (i.e., Northeast, Midwest, South, and West) (Acharya, Baghai, and Subramanian 2014) or headquarter states (Gormley and Matsa 2014, 2016) to control for potential local confounding factors. The coefficient on PPL remains similar using either of these alternative specifications. We prefer the use of fixed effects based on U.S. Census Divisions, as these provide a more granular geographical measurement than regions and are not susceptible to the econometric

[16] We show that our baseline point estimate in Column (1) is robust to the omission of any SW-PPL-passing state in Online Appendix Figure OA4. On the y-axis, we plot each of the coefficients we estimate from separate regressions that exclude SW-PPL states one-by-one – with the excluded state shown on the x-axis – along with their corresponding 95% confidence intervals. The results indicate that these coefficients are similar to the coefficient estimate in Table 4, with magnitudes that fall between 0.049 and 0.064 and t-statistics ranging from 2.64 to 3.96. This mitigates concern that an unobserved, state-specific factor (or outlier) drives our results.

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issues (specific to our setting) engendered by the use of headquarter states that we outlined in Section 1.3.

Next, we move to study the timing of changes in Tobin's Q relative to the timing of PPL adoptions in order to check the validity of our difference-in-differences estimation of PPL. As underscored in Angrist and Pischke (2009), the fundamental assumption of this identification strategy is that of parallel trends in the outcome variable (in our case, $Ln(Q)$) between firms "treated" by PPLs and those "un-treated" by the laws in the period before their passage.

To test the parallel trends assumption, we follow Acharya, Baghai, and Subramanian (2014) and Gormley and Matsa (2016), and regress $Ln(Q)$ on dummy variables that indicate a firm's incorporating state's relative year to PPL enactment, along with firm, division-by-year, and industry-by-year fixed effects, and controls for the other antitakeover laws. We create a dummy for the five years before and for the five years after the passage of a PPL, where the first (last) dummy is set to one if it is five or more years before (after) the laws' passage and zero otherwise. The year before the adoption of the respective PPL is excluded and serves as the reference year (e.g., Cornaggia, Mao, Tian, and Wolfe 2015).

We plot the point estimates in Figure 2 with relative year indicators on the *y*-axis and indicators for each year (except the "minus one" reference year) in a plus or minus four-year window surrounding PPL adoption on the *x*-axis. The 95% confidence interval for the coefficient estimates is based on robust standard errors with state of incorporation clustering. Consistent with the parallel trends assumption, we find that firms incorporated in PPL adopting states and non-PPL adopting states have insignificantly different levels of Tobin's Q in the four-year period before the laws are passed. The difference in $Ln(Q)$ starts to broaden only in the year of adoption and becomes statistically different in the one- through four years post-adoption.

3.2.2. Matched sample

We turn to addressing the concern that selection effects might bias our inference that PPLs positively impact the Tobin's Q of firms incorporated in adopting states. One particular concern is that firms may reincorporate, such that a strengthened shadow pill is non-random for firms reincorporating (or doing an IPO) into a PPL-adopting state.

We account for this by constructing a propensity score-matched sample, where we match each "treated" firm in the SW-PPL adopting states in the year before passage ($\tau(s) - 1$) to a "control" firm incorporated in a state without a PPL in the three years following its matched counterparts' adoption year. The basic idea behind this research design is that by matching

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firms in the year prior to treatment, we ensure that our matched sample is restricted to firms that were already incorporated in the state before the PPL was passed, disallowing the possibility that firms selected into treatment (i.e., a stronger shadow pill) via (re)incorporation.

Our matching procedure requires that treated and control firms are identical on firm-level poison pill status and, whenever possible, that they headquarter in the same U.S. Census Division and operate in the same two-digit SIC industry. When it is infeasible to match exactly on divisions (two-digit SIC industries), we allow matches to be in the next closest division (the same one-digit SIC industry). We construct propensity scores for matching based on these conditions along with pre-treatment year levels of Q and $Total\ Assets$.

We present the summary statistics for the two groups of firms in the year before treatment (for the entire period) in Panel A of Table 5 (Online Appendix Table OA7). None of the matching variables has a statistically significant difference between treated and control firms. For instance, the Q of treated firms in year $\tau(s) - 1$ is 1.74, while the Q of the control firms is 1.81 where the difference is statistically indifferent (t-stat=0.85).

In Panel B of Table 5, we present the results from regressions of $Ln(Q)$ on $Treated \times Post$ (we use this variable name instead of PPL for the matched sample only) interaction term over a $t\pm3$ estimation window. The first two columns use firm, division-by-year, and industry-by-year fixed effects, while the last column only uses firm and year fixed effects for robustness. Each of the three columns include dummies for the other antitakeover laws and the last two columns control for the firms' visible poison pills at time $\tau(s) - 1$ in the post-adoption period. For example, in Column (2), we find that treated firms experience significant increases in their Tobin's Q of 4.9% when compared to the control group, after their state of incorporation adopts a PPL. These results mitigate concerns that a selection effect drives our findings in the full sample.

3.2.3. Alternative value measures

We investigate the robustness of our firm value results using alternative metrics of value. In Panel A of Table 6, we employ the same baseline specifications we use in the full sample (in the odd-numbered columns) and the matched sample (in the even-numbered columns), but replace $Ln(Q)$ as the dependent variable with the following four measures:

1. The level of Tobin's Q (Q);
2. Total Tobin's Q ($Total\ Q$), proposed by Peters and Taylor (2017), which modifies Q by explicitly accounting for intangible capital in the firm's replacement cost of total capital;

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3. $Excess\ Return$ (Cohen and Wang 2013), estimated as the residual from regressions of annual stock returns on the Fama-French four (i.e., Market, SMB, HML, and MOM) factors (Fama and French 1993; Carhart 1997);
4. Return on assets (ROA), defined as operating income before depreciation and amortization divided by the book value of assets (Giroud and Mueller 2010).

The takeaway from Panel A of Table 6 is that a strengthened shadow pill is valuable for shareholders, as our main result that firm value increases after the firm's state of incorporation adopts a PPL is robust to using these four alternative measures of firm value. That is, whether we employ Q, $Total\ Q$, $Excess\ Return$, or ROA as dependent variables and whether we use our full sample or a matched sample, our inference remains unchanged: PPLs are positively and significantly related to firm value. For example, Column (5) suggests that shareholders of firms incorporated in states with PPLs experience a significant 2.9% increase in the $Excess\ Return$ on their shares. In Columns (7) and (8), we find that ROA improves for firms incorporated in states that strengthen the right to adopt a poison pill by 6.9% (=0.009/0.131) in the full sample (Column 7) and 5.7% (0.007/0.123) in the matched sample (Column 8), relative to the respective sample means.

We also consider a monthly portfolio return approach, which can be viewed as a long-term stock event study, consistent with prior corporate governance studies (Gompers, Ishii, and Metrick 2003; Bebchuk, Cohen, and Ferrell 2009; Giroud and Mueller 2011). In this approach, we focus on our matched sample of firms and construct long (short) portfolios of stocks from treated (control) firms around the time their (matched counterparts') state of incorporation adopts a PPL. The central premise is that if a strengthened shadow pill matters for a firm's long-term performance, but its impact is not immediately incorporated into stock prices because of, for example, inefficiencies in information across states and time, then realized returns for a treated firm are systematically higher than those for a control firm. The long (short) portfolios are constructed as follows. For portfolios "6m36" and "12m36," we include all stocks of treated (control) firms starting either 6 or 12 months before the fiscal year-end of the year in which the matched treated incorporating state adopts a PPL, and hold (short) these stocks for 36 months post-adoption. The long-short portfolios are then created by differencing the portfolio returns of the long and short portfolios for each respective month.

In Panel B of Table 6, we find that the long-short portfolios of treated and control firms have a positive and significant $Alpha$ over both "6m36" and "12m36" holding periods, using

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an equally-weighted market factor[17] and estimating the risk-adjusted excess returns with either the four-factor (Carhart 1997), three-factor (Fama and French 1993), or market-factor models. For instance, when we buy stocks of treated firms and short stocks of control firms 12-months before the adoption date of their (matched firms') respective PPL and continue such strategy until 36-months after, we find an overall average annualized abnormal return of 9% using the four-factor model.[18]

3.2.4. Additional robustness

We conduct several additional robustness tests of our main finding that having a stronger shadow pill (via the enactment of PPLs) is value-enhancing for shareholders. To conserve space, we include these supplemental analyses in the Online Appendix (Tables OA9 to OA19). As a roadmap for interested readers, we include a synopsis of these tests below:

(i) Sample adjusted for Delaware case law

Our research design assumes that firms incorporated in states that adopt a PPL have the greatest level of certainty in their right to adopt a poison pill. Prior research, however, has also considered firms incorporated in Delaware (which does not have a PPL) as having an equivalently strong shadow pill because of the 1985 court ruling in *Moran*. In our interpretation, the subsequent Delaware courts' rulings in *Interco* and *Pillsbury* disrupted this certainty in 1988 and thereafter. A counter argument, however, could be made that the shadow pill in Delaware was reinstated in 1989 with the ruling in *Paramount* and, further still, that subsequent rulings (see, e.g., in the past decade, *Air Products* v. *Airgas* in 2010) continued to uphold the certainty of pills in Delaware.[19]

We check the robustness of our results to coding Delaware firms' PPL as a "0" in Tables OA9 and OA10. We use three separate approaches. First, we show that coding Delaware firms' PPL indicator as equal "1" starting in 1985 and leaving it at this value throughout the sample does not change our main result. Second, we document that our full and matched sample

[17] Our results are also robust to using a value-weighted market factor.

[18] We unpack the dynamics of the buildup of shareholder returns around the date of PPL adoption by conducting a short-run event study surrounding important dates in the life of the legislation (e.g., introduction on the House or Senate floor, final passage by the House or Senate, and the Governor's approval). Online Appendix Table OA8 reports the respective cumulative abnormal returns (CARs) as showing a small positive reaction by the market for firms incorporated in SW-PPL states during the first trading week that the bill is introduced, suggesting that the market responds favorably to the prospects of a PPL being passed. And then a much stronger positive market response during the three-to-five-trading weeks before the final passage of the PPL, indicating that the market anticipated its successful ratification and perceived it as valuable for the relevant firms.

[19] The counter to this counter argument is that the continued need for judges to rule in Delaware on the validity of the pill is indicative of its status as being *less* certain than for firms covered by an actual PPL.

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Tobin's Q results are robust to excluding firms incorporated in Delaware entirely. Third, instead of relying solely on the variation stemming from PPLs, we consider an alternative proxy for the strength of the shadow pill. Using PPLs and state-level court decisions (including *Moran* in Delaware) on pills from Cain, McKeon, and Solomon (2017), we construct the $PPV\text{-}Index$ that captures changes across states and time on the relative strength of the shadow pill. Substituting this measure for PPL in our full sample $Ln(Q)$ regressions, we continue to find that shadow pills are valuable.

(ii) An expanded alternative sample

As our study aims at understanding how shadow pills affect both pill policy and firm value, a requirement in constructing our sample is that we have non-missing information on the variable $PPill$. A drawback of this constraint is that it reduces our sample size significantly (and perhaps, non-randomly). As a robustness check, we relax this requirement and verify that our main Tobin's Q results still hold (Table OA11).

(iii) Controlling for PPL-firm characteristics

As discussed above, in order to avoid misspecification by including bad controls in our regression models, we do not include time-varying firm-level controls used by prior corporate governance studies (e.g., $Ln(Assets)$, $CAPX/Assets$). We show that our findings on firm value are not dependent on the exclusion of these controls in Table OA12, where we include time-invariant firm-level controls that are measured in the year prior to the adoption of a firm's state's respective PPL ($\tau(s) - 1$) and are interacted with PPL. We continue to find a positive relation between PPLs and firm value.

(iv) The timing of changes in outcomes

Figure 2 provides evidence that firms incorporated in PPL-adopting states had similar levels of $Ln(Q)$ in the years before the respective laws were passed (i.e., parallel trends). We supplement this finding in Table OA13 by investigating the dynamics for our other outcome variables (both visible pill- and value-based measures). We find reassuring evidence that the changes we document in these outcomes occur after PPLs are adopted and not before.

(v) State-by-year fixed effects

a. Full sample

In Table 4, we verify the strength of our main finding to the inclusion of state-by-year fixed effects. Following Gormley and Matsa (2016), we take this analysis a step further and in Table OA14 we decompose the effect of PPLs into cohorts of firms incorporated and headquartered in the same state ($Same\ Inc\text{-}HQ\ State$) versus that of firms incorporated and

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headquartered in different states ($Diff.Inc\text{-}HQ\ State$). Consistent with our discussion of the econometric issues about the use of state-by-year fixed effects in the PPL-setting, we find that our results are driven by the 72% of $Same\ Inc\text{-}HQ\ State$ firms. The coefficient on $PPL \times Diff.Inc\text{-}HQ\ State$ is positive, but insignificant, which we argue is due to a lack of variation (i.e., low statistical power).

b. Matched sample

As a further robustness check on the concern that unobserved, time-varying headquarter state factors are driving our results, Table OA14 considers an alternative matching procedure, where we no longer exactly match on U.S. Census divisions, but instead, match on the state of the firm's headquarter location. We then regress $Ln(Q)$ on $Treated \times Post$ in this alternative sample and find our results hold, albeit with a lower statistical significance level.

(vi) Placebo tests

a. Full sample

We construct a placebo test by randomly assigning states (without replacement) a PPL, where these assignments follow the laws' actual empirical distribution across time – thus, if our main results are driven by confounding factors that occur around the same time as PPL adoptions, they should remain present in the data and could continue to bias our findings. We repeat the simulation 1,000 times and then estimate the regression model in Column (1) of Table 4 on the simulated data. Figure OA5 plots the distribution of the coefficients and *t*-statistics. The vertical red lines represent the actual respective regression coefficient and *t*-statistic based on the actual data. We find that the actual regression coefficient and *t*-statistic lie at the tails of the distributions (i.e., slightly more than two standard deviations from the mean), suggesting that the effects we find on Tobin's Q are attributable to the actual PPLs.

b. Matched sample

We provide further evidence for the parallel trends assumption in our matched sample in Table OA15 by moving back actual adoption dates of PPLs by four years. We then estimate the value regressions with an ($t \pm 3$) estimation window (i.e., outside the period that includes the actual PPL adoption) and find that the coefficients on $Pseudo\ Treated \times Post$ are always insignificant.

(vii) Sample period adjusted for Delaware case law

In our interpretation, the Delaware rulings in *Interco* and *Pillsbury* injected legal uncertainty in the general validity of the pill both in Delaware and elsewhere after 1988. Accordingly, to have a pre-treatment period unconfounded by these court rulings, our main

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results focus on PPL adoptions beginning with Minnesota in 1995. For robustness, we move back the sample to include PPL adoptions beginning in 1989 in Table OA16. We find that our full and matched sample results persist.

(viii) Subperiods within the second wave

Table OA17 breaks down our SW-sample period into three subperiods, 1992-1998, 1999-2005, and 2006-2012, to test whether the adopted PPLs had comparable effects across these subperiods. We find that the subperiods in which most of the SW-PPLs were adopted have the largest and most significant effects. Meanwhile, we find a positive, but insignificant, coefficient on *PPL* in the last period, 2006-2012. This could be explained by limited power, as only two states – Vermont and Wyoming (with very few firms) – adopt PPLs during this period.

(ix) First and second wave PPL sample periods combined

We consider the combined average effect of first (1986 to 1990) and second (1995 to 2009) wave PPLs by using the period 1983–2012 in the full sample and by matching firms in all 35 law-adopting states in the matched sample and show that our key inference is unchanged (Table OA18).

(x) Excluding multi-law adopting states

We show in Table OA19 that our full and matched sample Tobin's Q results are robust to excluding states that enact other antitakeover laws in the same year they pass PPLs.

4. Economic Channels

This section considers what economic channels can explain our finding that a strengthened shadow pill, as sanctioned by the enactment of a PPL, adds to firm value. We draw on the existing theoretical literature and examine two potential hypotheses, namely, the "bonding hypothesis" and the "bargaining power hypothesis."

4.1. Bonding hypothesis

We begin by examining the bonding hypothesis as a primary economic channel underlying the positive relation between shadow pills and firm value. Under the bonding hypothesis, empowering a board to commit the firm to a business strategy that cannot easily be reversed in the short-term – by strengthening a board's ability to contest the disruption caused by takeovers – is value-enhancing as it decreases a firm's cost of contracting with other stakeholders, promoting long-term projects and stronger stakeholder relationships (Laffont and Tirole 1988; Shleifer and Summers 1988; Stein 1988). In order to test this channel, we follow prior studies (Johnson, Karpoff, and Yi 2015; Cen, Dasgupta, and Sen 2015; Cremers, Litov, and Sepe 2017;

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Chemmanur and Tian 2018) and analyze the heterogeneous effects of PPLs for firms with differences in the importance of innovation and stakeholder relationships.

4.1.1. Innovative firms

We first test the bonding hypothesis by considering the heterogeneous effect of PPLs for firms that are more dependent on investments in innovation. Since companies more engaged in innovation often require more significant firm-specific investments from stakeholders (e.g., employees, strategic alliance partners, suppliers, and customers), a stronger shadow pill could prove useful in preventing the ex-post expropriation of these stakeholders' investments and more credibly committing innovative firms toward long-term projects.

We use the following four measures for the level of a firm's investments in innovation:

1. $R\&D/Sales$, which is a measure for the importance of corporate expenditures on research and development activities (Chan, Lakonishok, and Sougiannis 2001), based on data from Compustat;
2. $Intangible\ Capital$, which is a "catch-all" measure for the importance of intangible capital (Eisfeldt and Papanikolaou 2014) and defined as a firm's intangible capital estimated replacement cost (as proposed by Peters and Taylor 2017), based on data from WRDS in the Peters and Taylor Total Q database;
3. $CW\ Patents$, which captures the novelty or quality of a firm's innovative output by weighting its patents based on the number of citations they receive (Atanassov 2013), based on data from the KPSS Google patents dataset;[20]
4. RQ, or research quotient, which measures the output elasticity of R&D (as proposed in Knott 2008), based on data from WRDS in the Research Quotient database, which is only available for a smaller sample.

We standardize each of these variables to have a mean of zero and a standard deviation of one to ease the interpretation of the coefficient estimates and each is measured in the year before the respective PPL is passed ($\tau(s) - 1$).

Panel A of Table 7 presents our results. In each of the four columns, we specify the natural logarithm of Tobin's Q as the dependent variable and include our fixed $PPill_{[\tau(s)-1]}$ control and the full set of fixed effects. Consistent with the theoretical predictions of the bonding hypothesis, we find that when boards are better equipped to contest the potential disruption caused by a takeover – via the adoption of a PPL that strengthens the shadow pill – firms that

[20] Our inference is unchanged if we use patent counts or the stock market-value of patents (Kogan et al. 2017), instead of citation-weighted patents.

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(in the year before the PPL is adopted) are more engaged in research and development (Column (1)) have more intangible capital (Column (2)) and patent citations (Column (3)), or are better at converting R&D into sales (Column (4)), experience more pronounced gains in firm value. More specifically, Panel A of this table shows that, compared to the Tobin's Qs of PPL-firms less involved in investments in innovation, a strengthened shadow pill is associated with an additional 2.3% to 6.5% higher Tobin's Q for PPL-firms more engaged in investments in innovation, depending on the measure that we use.

4.1.2. Stakeholder relationships

We next test the bonding hypothesis by considering heterogeneous effects based on the importance of the firms' stakeholder relationships. We employ four different measures to capture the importance of these relationships directly:

1. $Organizational\ Capital$, which is a "catch-all" measure for the importance of organizational capital (Eisfeldt and Papanikolaou 2013) that is defined as a firm's organizational capital replacement cost (as proposed by Peters and Taylor 2017), based on data that comes from WRDS in the Peters and Taylor Total Q database;
2. $Labor\ Intensity$, which measures how intensely businesses rely on their human capital and is defined as the total number of employees divided by real sales revenue (Dewenter and Malatesta 2001), where we adjust sales using 2015 dollars;
3. $Large\ Customer$, which captures the significance of customers who are likely to have a longer-term association with the firm (Cen, Dasgupta, and Sen 2015) and is measured using an indicator equal to one if a firm has at least one customer accounting for at least 10% of its consolidated sales in that fiscal year;
4. $Strategic\ Alliance$, which is an indicator of whether the business has a long-term partnership with another company (Bodnaruk, Massa, and Simonov 2013).

Panel B of Table 7 shows our results. Columns (1)–(4) maintain the natural logarithm of Tobin's Q as the dependent variable and include the fixed $PPill_{[\tau(s)-1]}$ control and the full set of baseline fixed effects. We also standardize $Organizational\ Capital$ and $Labor\ Intensity$ to have a mean of zero and unit variance in order to ease the interpretations of the interaction terms and, again, define these interaction terms in the year before the state of incorporation adopts a PPL ($\tau(s) - 1$).

Consistent with the bonding hypothesis, Panel B of Table 7 finds that, compared to the Tobin's Qs of PPL-firms that are less reliant on stakeholder relationships, an enhanced right to adopt a poison pill is associated with a differential 3% to 6.7% increase in the Tobin's Q of

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PPL-firms that are more dependent on relationships with stakeholders, depending on the measure that we use.

In sum, the evidence in Table 7 indicates that the strengthened right to adopt a poison pill, as enabled by PPLs, is more value enhancing for firms where long-term investments in innovation are more critical, that have more organizational capital or a large customer, are in a strategic alliance, or are more labor-intensive. These results support the idea that PPLs enable firms to more closely bond their stakeholders to long-term strategies and, in so doing, decrease a firm's costs of contracting and increase the value of long-term investments in innovation and stakeholder relationships. Consistent with prior work showing a shift in the importance of tangible towards intangible assets, such as R&D, patents, and human capital (Corrado and Hulten 2010; Peters and Taylor 2017), overall the evidence from Table 7 suggests that the need for a bond between stakeholders and firms has become increasingly more important during the era of the SW-PPLs.

4.2. Bargaining power hypothesis

We test the bargaining power hypothesis by analyzing both target acquisition propensities and premiums, following prior empirical studies (Comment and Schwert 1995; Heron and Lie 2006, 2015; Kadyrzhanova and Rhodes-Kropf 2011). The data on acquisitions are from the SDC M&A database and comprise 141 unsolicited acquisition attempts announced over the period 1992–2012. We define a takeover as unsolicited if the SDC database classifies the bid as hostile or otherwise unsolicited (Heron and Lie 2006, 2015).

Panel A of Table 8 examines the impact of PPLs on the likelihood that firms receive a takeover bid (Columns (1)–(3)), as well as on the probability that a deal is successfully completed (Columns (4)–(6)). The dependent variable, Bid ($Acquired$), is an indicator variable equal to one if a target firm announces that it has received a bid (is acquired in a completed takeover, either through a merger or an acquisition) in the SDC M&A database, and zero otherwise. Each of the six columns includes division-by-year and industry-by-year fixed effects. We also interchange controls for firm value and a PPL-firm's poison pill status (in the year before the firm's state of incorporation adopts a PPL).

We find that firms with strengthened shadow pills are equally likely to receive a takeover bid or be successfully acquired as firms incorporated in states without a PPL.[21] Additionally,

[21] There are, however, empirical challenges with this analysis. In particular, we are unable to test how many ex-ante target firms became too expensive to acquire following the enactment of a PPL because, as we document, these laws significantly increased affected firms' market values.

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our analysis of the heterogeneous effect of PPLs for firms that had pills in-place in $\tau(s) - 1$ indicates that the enhanced validity of the pill, as enabled by the laws, does not significantly alter the probability of being a target or acquired in a takeover. This is consistent with prior studies that do not find evidence that actual poison pills materially deter takeovers (Ambrose and Megginson 1992; Comment and Schwert 1995; Heron and Lie 2006, 2015). Lastly, we find associative evidence again consistent with Cremers, Nair, and John (2009) and Edmans, Goldstein, and Jiang (2012) that firms with lower (higher) valuations are more (less) likely to be acquisition targets.

Next, in Panel B of Table 8, we investigate whether takeover premiums are positively related to the adoption of PPLs, as the bargaining power hypothesis would suggest. In these tests, we employ the following two dependent variables: $Premium\ Increase$, defined as the percentage increase in the bid price scaled by the target's stock price 20 days prior to the initial offer, and $Total\ Premium$, measured as the sum of the initial premium and the premium increase, where the summed components are relative to the target's stock price 20 days prior to the initial offer. We include the full set of fixed effects in each of the six columns. Lastly, our specifications use division and industry fixed effects, but not firm or interacted fixed effects, since we are focusing exclusively on the cross-section of successful hostile bids, such that our sample size is limited to 141 observations.

In Columns (1) and (4) of Panel B, we find that the adoption of a PPL is not associated with an increase in the total premium nor the initial premium offered to successfully acquired firms. On the other hand, Columns (2) and (5) show that the coefficients on $PPill_{[t-1]}$ are positive and statistically significant, suggesting that visible poison pills can benefit a target's shareholders via increased bargaining power resulting in a higher acquisition price (consistent with, e.g., Brickley, Coles and Terry 1994; Comment and Schwert 1995; Cotter, Shivdasani, and Zenner 1997; Heron and Lie 2006, 2015). However, $PPill_{[t-1]}$ in this model is a "bad control" since it is also affected by PPL adoption. Therefore, in Columns (3) and (6) we interact PPL with $PPill_{[\tau(s)-1]}$ instead, and find that PPL-firms with an existing pill in the year prior to their respective laws' adoption earn higher takeover premiums relative to PPL-firms without a pill (in $\tau(s) - 1$). For instance, in the third column, shareholders of target firms incorporated in states with one of these laws and that had an actual pill in-place experience a 10.4% increase in the total premium received.

Hence, the evidence from Table 8 indicates that PPLs can be value-enhancing for shareholders of acquisition targets. In particular, the results suggest that the strengthened right

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to adopt a poison pill enhances the negotiating position of firms with pills in-place, also explaining why the adoption of pills for firms with lower levels of Q and, thus, more likely at risk of a takeover, can be valuable.

However, three findings discussed above limit the explanatory power of the bargaining power hypothesis, jointly implying that this economic mechanism cannot fully explain our main result that a stronger shadow pill increases long-term firm value. First, we find that PPLs are value relevant for the *average* firm – and for both firms with and without visible poison pills – while the increase in takeover premiums only applies to the subset of PPL-firms with visible poison pills. For instance, when we interacted PPL with $PPill_{[\tau(s)-1]}$ in Columns (3)-(5) of Table 4, we did not find evidence for a differential effect on Tobin's Q. Second, and more generally, the bargaining power hypothesis does not explain the full range of our results. In particular, while it could be consistent with an increase in $Excess\ Return$ and $Alpha$, it is less consistent with increases in $Ln(Q)$, Q, $Total\ Q$, and ROA. Third, in light of the infrequent occurrence of hostile takeovers during the SW-period (only about 0.55% of the sample's firm-years), the higher takeover premia attributable to PPLs can only explain a small portion of the associated increase in firm value. Therefore, we view the bargaining power hypothesis as a secondary mechanism to the bonding channel.

5. Discussion of our results in light of the recent literature

Our results add to recent studies that carefully examine the legal context underlying the introduction of antitakeover laws (Catan and Kahan 2016; Karpoff and Wittry 2018).

First, and consistent with the prominence and effectiveness of the poison pill as a takeover defense, we interpret these results as suggesting that, among antitakeover laws, PPLs play a more important role than the previous literature has considered.[22] In particular, the finding in Table 2, that the prior enactment of BCLs and FPLs are strong and consistent predictors of SW-PPLs, combined with the evidence that PPLs increase firm value, add to our understanding of the relationship between, and relative importance of, different kinds of antitakeover state laws. One possible interpretation of this finding is that, contrary to the common view in the

[22] For instance, due to the significant costs of intentionally "swallowing" (i.e., triggering) a pill, the adoption of this takeover defense can render other antitakeover laws moot. For example, BCLs only become operative once a bidder has become a major shareholder, which is unlikely to ever happen when a firm has adopted a pill (Catan and Kahan 2016). Speaking to the pill's effectiveness as a takeover mechanism, to date there has only been one case (*Selectica, Inc.* v. *Versata Enterprises, Inc.*) in which a pill was actually triggered and its purpose was not to thwart a takeover bid per se but rather to protect the target's net operating loss carryforward (NOL). For an interesting exposition on the mess created by the actual trigger of this pill (e.g., trading was halted for more than four weeks to sort out all the paperwork) see: https://www.lw.com/upload/pubContent/_pdf/pub2563_1.pdf).

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literature that BCLs (and FPLs) provide substitute takeover protection to PPLs (e.g., Karpoff and Malatesta 1989; Garvey and Hanka 1999), PPLs might complement BCLs and FPLs.[23] PPLs strengthen a firm's right to adopt a pill, which, as we found, leads some PPL-firms to increase their use of visible pills. Therefore, PPLs' ability to prevent a takeover flows through the validity it grants to the actual pill. If a firm's board feels compelled to remove its pill due to institutional investor pressure (e.g., through ISS recommendations), shareholder vote (e.g., Cuñat, Gine, and Guadalupe 2012), or directors' reputational concerns (e.g., Johnson, Karpoff, and Wittry 2019), then the firm will still have a BCL or FPL, which can only be removed by statutory amendment or firm-level opt out. Therefore, BCLs and FPLs provide an added layer of antitakeover protection and bargaining power to the firm, where this added "defensive layer" would complement that provided by PPLs in the first place.

An alternative interpretation is that, consistent with the view of PPLs as the strongest antitakeover law (Karpoff and Wittry 2018; Catan and Kahan 2016), legislators in SW-PPL states might have found BCLs less effective than desired in providing takeover protection, and opted to subsequently introduce PPLs to further enhance this protection. In support of the latter interpretation, Karpoff and Wittry (2018) find that BCLs do not matter as much as commonly reported by previous studies once other institutional and legal context is controlled for. This could explain, for example, why almost all the states that eventually adopted a SW-PPL had already introduced a BCL several years earlier.

Second, our study sheds light on the relative strength of the shadow pill across different states and period of times, providing evidence that, until the Delaware decisions in *Interco* and *Pillsbury* in 1988, the validity granted to the pill in its 1985 ruling in *Moran* likely validated the general use of the pill in other states, whether those states had a PPL or not (see our discussion in Section 2). Our results also indicate that in the post-*Interco* and *Pillsbury* period, the shadow pill's validity (and general applicability) is the most certain (and least constrained) for firms incorporated in states that have adopted a PPL. The next highest level of certainty would stem from court decisions in a firm's state of incorporation that always upholds the use

[23] In practice, the adoption of a poison pill has been frequently accompanied by the adoption of a staggered board (Cohen and Wang 2013). This is because the combination of these defenses substantially reduces the chances that a potential bidder might be able to have the pill removed (i.e., by replacing a majority of directors) through the ballot box, therefore strengthening the antitakeover force of a poison pill. While analyzing the function of the shadow pill vis-à-vis other governance mechanisms is outside the scope of this study, we take an explorative first-look at the combined impact of a shadow pill and staggered board on firm value. We find that PPLs and staggered boards are independently, positively associated with firm value, while their interaction is insignificant (see Table OA20). We are cautious not to make a strong inference from this analysis, however, as a much more rigorous examination of the timing of the adoption of PPLs and staggered boards is necessary; we leave this for future work.

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of the pill as a takeover defense. This would be followed by situations where court decisions in a firm's state of incorporation sometimes uphold the poison pill (e.g., Delaware – *Moran*, *Paramount*, and *Airgas* in favor and *Interco* and *Pillsbury* against). The least certain situation for a firm's right to adopt a visible pill would be *not* having a PPL or any court decision upholding a pill (e.g., Louisiana) or having a court decision or a statutory provision against the pill in the firm's state of incorporation (e.g., California).

Third, and lastly, we reconcile our finding that the presence of a PPL (and thus a strengthened shadow pill) is not significantly associated with a firm's likelihood to be targeted by a hostile bid using an equilibrium argument, i.e., that hostile bids are endogenous. As our main result shows, firm value tends to be higher for firms incorporated in states that have adopted a PPL. Such higher firm value reduces the likelihood of receiving a takeover bid and being acquired (see Table 8), consistent with the findings in Cremers, Nair, and John (2009) and Edmans, Goldstein, and Jiang (2012) that lower market prices have a trigger effect on takeover activity.

6. Conclusion

This paper contributes to the debate on whether poison pills benefit or hurt shareholders by shifting the focus from visible pills to shadow pills – that is, to the *right to adopt* the pill (which right constitutes the shadow pill). We do so by exploiting the quasi-natural experiment provided by the staggered passage by a majority of U.S. states of poison pill laws (PPLs) that validated the use of the pill and, thus, strengthened the relevance of the shadow pill as a takeover defense.

Our paper is the first in the literature to focus on the second wave (SW-) PPLs passed during the period 1995 to 2009, in order to explore the implications of these laws for pill policy and long-term firm value. Given substantial changes in the underlying legal environment since the enactment of first wave PPLs (i.e., adopted from 1986 to 1990), we conjecture that results obtained by the prior literature for these first wave PPLs might well differ from what we can learn from the SW-PPL adoptions. Further, from an identification perspective, focusing on SW-PPLs ensures that we have a pre-treatment period that is unconfounded by the (unprecedented) hostile takeover wave of the 1980s or major Delaware court decisions that could have impacted the importance of PPLs.

We document two main results. First, we show that having a stronger shadow pill via the enactment of a PPL has a validation effect for lower-valued firms, which are more likely to be exposed to future takeover risk and activist investors, and face significant frictions in adopting

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a visible pill (e.g., the risk of subsequent judicial challenges and/or reputational costs affecting directors that are associated with being pro-pill). Therefore, these firms are more likely to benefit from the PPLs' validation of pills as a takeover defense, which in turn leads to an increase in both the adoption and duration of visible pills by these firms.

Second, we find that the availability of a stronger shadow pill is associated with significant improvements in firm value, especially for firms more engaged in innovation or with stronger stakeholder relationships, and for firms with existing pills that are targeted in hostile acquisition attempts. Further, using a comprehensive dataset of firm-level visible pills, we also confirm and expand the findings of the previous literature on the visible pill's negative association with Tobin's Q. Overall, a stronger shadow pill seems beneficial to shareholders, even if the (endogenous) adoption of an actual pill might not be.

We conclude that our results support the view that the shadow pill serves a positive corporate governance function for some firms. In particular, our findings are best explained by the "bonding hypothesis" and, secondarily, by the "bargaining power hypothesis" of takeover defenses. The first hypothesis maintains that the right to adopt a pill increases firm value by bonding the board to the firm's long-term strategy, promoting longer-term investment projects and protecting firm-specific investments by its stakeholder, which reduces the costs of contracting. The second hypothesis posits that strengthening the board's ability to resist a hostile takeover attempt increases the board's power to negotiate with a potential bidder, resulting in a higher purchasing price to the benefit of the target's shareholders.

Appendix

Table A1
Variable definitions

$Bid\ (Acquired)$	An indicator variable equal to one if a firm receives a takeover bid (is successfully acquired) per the SDC M&A database, and zero otherwise.
$CW\ Patents$	The natural logarithm of one plus citation-weighted (CW) patents. We use the KPSS patent data.
$Est.Entry\ (Exit)$	The establishment entry (exit) rate in a firm's state of incorporation. We use data from the U.S. Census Bureau.
$Excess\ Return$	Fama-French 4-factor adjusted excess returns are defined as the residual from annual regressions of raw returns on a value-weighted market factor, small-minus-big factor, high-minus-low factor and momentum factor (Carhart, 1997). Data comes from CRSP and Ken French's website.
$GDP\ Growth$	The incorporated state-level GDP growth rate over the fiscal year. Data comes from the U.S. Bureau of Economic Analysis.
$GDPPC$	An incorporating state's GDP divided by its total population. Data comes from the U.S. Bureau of Economic Analysis. We take the natural logarithm of this variable: $Ln(GDPPC)$.
$Inc.SY$	Denotes that we use the median of the corresponding $[Variable]$ of all firms incorporated within a state, in a given year.
$Intangible\ Capital$	Firm's intangible capital estimated replacement cost scaled by the book value of assets. This measure is available on WRDS and follows Peters and Taylor (2017).

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$Labor\ Intensity$	Number of employees divided by real sales, where sales are adjusted using 2015 dollars. Data comes from Compustat.
$Large\ Customer$	An indicator variable equal to one if there is at least once customer accounting for at least 10% of the consolidated sales of the firm in that fiscal year. Data comes from the Compustat Segments database.
$M\&A\ Volume$	The ratio of M&A 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)). We also only consider transactions that are completed and where the acquirer achieves control of the target.
$New\ PPill$	An indicator variable equal to one if a firm adopts a new poison pill ($PPill$).
Other antitakeover laws: BCL, CSL, DDL, FPL	Four separate indicator variables set equal to one if a firm is incorporated in a state that has adopted a business combination (BC) or control share (CS) or directors' duties (DD) or fair price (FP) law, respectively, and zero otherwise. We use adoption dates from Karpoff and Wittry (2018).
$Organizational\ Capital$	Firm's organizational capital estimated replacement cost scaled by the book value of assets. This measure is available on WRDS and follows Peters and Taylor (2017).
$Political\ Balance$	The proportion of incorporated state-level representatives in the U.S. House of Representatives who are affiliated with the Republican party, in a given year. We use data from the House of Representatives.
$Pop.$	The population in a firm's state of incorporation in a given year. We use data from the U.S. Census Bureau.
$PPill$	An indicator variable equal to one if a firm has adopted a poison pill. We use data from ISS (formerly Riskmetrics), SDC's Corporate Governance and M&A databases, Comment and Schwert (1995), Caton and Goh (2008), Cremers and Ferrell (2014), Cremers, Litov and Sepe (2017), and hand-collected information from Factiva.
$PPill\ Duration$	The number of years a firm has had a poison pill ($PPill$) in-place. We take the natural logarithm of one plus this variable: $Ln(PPill\ Duration)$.
PPL	An indicator variable equal to one if a firm is incorporated in a state that passes a PPL during the period 1986 to 2009, and zero otherwise. We use adoption dates provided by Cain, McKeon, and Solomon (2017) and Karpoff and Wittry (2018). We also partition this variable into first wave (FW) (1986-1990) and second wave (SW) (1995-2009) adoptions.
$Premium\ Increase$	The premium increase in percentage. Data comes from the SDC M&A database.
Q	Market value of assets (total assets – book equity + market equity) divided by the book value of assets. Book equity and this measure, in general, follows Fama and French (1992). We take the natural logarithm: $Ln(Q)$.
$Q(Lowest), Q(Low),$ $Q(High), Q(Highest)$	Four separate indicator variables set to one if a firm's level of $Ln(Q)$ lies in the bottom, middle-to-bottom, middle-to-top, or top quartile, respectively, of its empirical distribution.
$R\&D/Sales$	R&D expenditures divided by the value of sales. Data comes from Compustat.
$Return$	A firm's annual stock return. Measured as the current fiscal end-year price minus last fiscal end-year price all divided by last fiscal end-year price. Data comes from CRSP.
ROA	Return on assets, defined as operating income before depreciation and amortization divided by total assets. Data comes from Compustat.
RQ	Abbreviation for research quotient. Measures the firm-specific output elasticity of R&D, representing the percentage change in revenues for a 1% change in R&D, as proposed by Knott (2008). Source of data is the Research Quotient database on WRDS.
$Strategic\ Alliance$	An indicator variable equal to one if the firm is in an active strategic alliance based on the SDC Strategic Alliances database.
$Total\ Q$	Market value of outstanding equity plus the book value of debt minus the firm's current assets divided by the sum of the book value of property, plant, and equipment, and the replacement cost of intangible capital (the sum of the firm's externally purchased and internally created intangible capital). Calculation follows Peters and Taylor (2017). Measure and source data is available on WRDS.

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Total Premium	The total percentage premium (initial premium plus any increase in the premium) offered scaled by the target firm's stock price 4-weeks prior to the initial offer. Data comes from the SDC M&A database.
Unemploy	The unemployment rate in a firm's state of incorporation in a given year. Data comes from the U.S. Bureau of Labor Statistics.

This table provides the definition and data source, where applicable, for the main variables.

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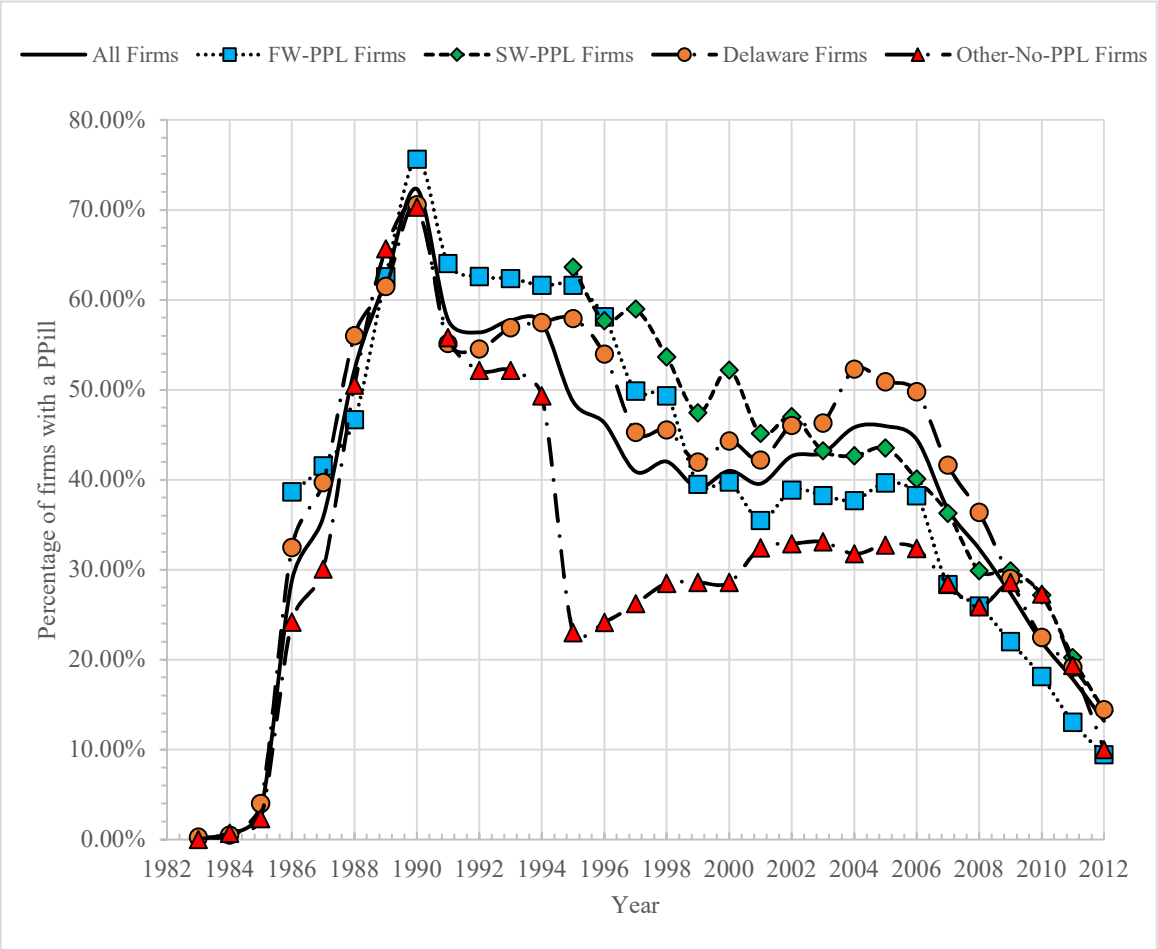

Figure 1
Percentage of firms with a poison pill
The figure plots the percentage of firms with a poison pill in-place ($PPill$) each year from 1982 to 2012, for various partitions of our sample: (i) all firms in the sample (solid black line), (ii) firms incorporated in a state that has adopted a first wave-poison pill law (FW-PPL), enacted between 1986 and 1990 (dashed line with blue squares), (iii) firms incorporated in a state that has adopted a second wave-PPL (SW-PPL), adopted between 1995 and 2009 (dashed line with green diamonds), (iv) firms incorporated in Delaware (dashed line with orange circles), and (v) firms incorporated in other states that have not (or had not yet) adopted a PPL (Other-No-PPL) (dashed line with red triangles).

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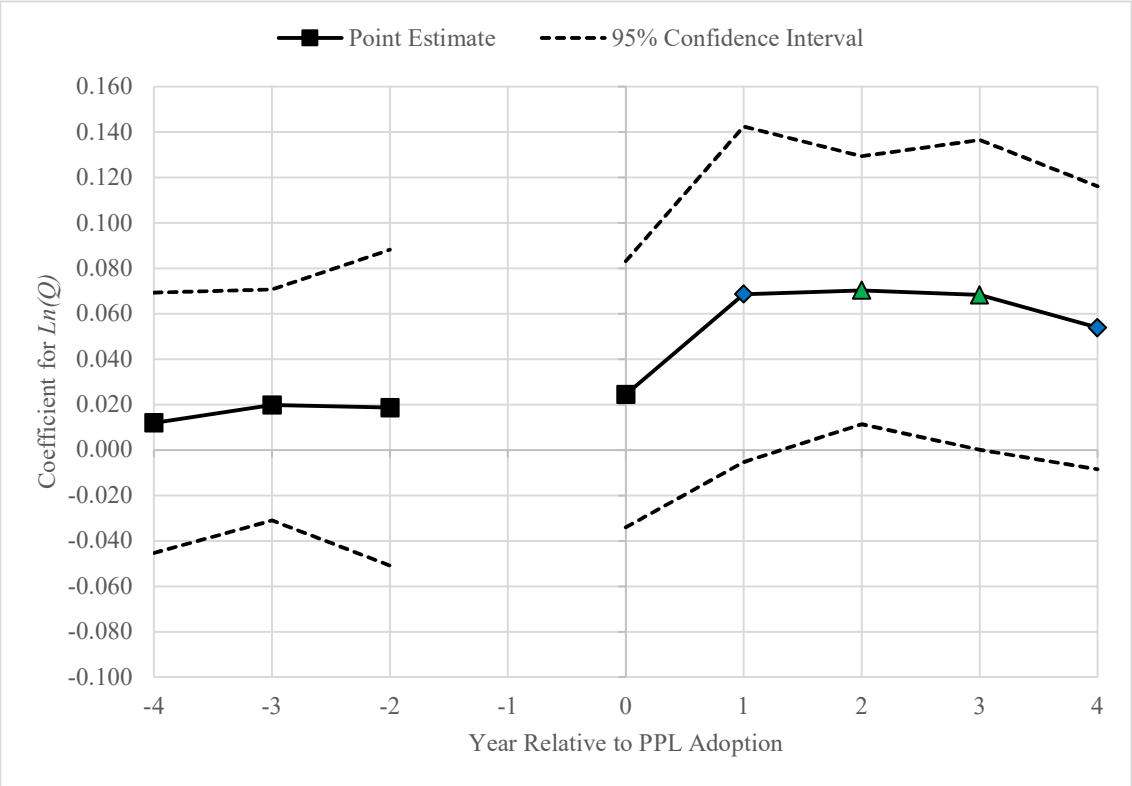

Figure 2
The timing of the PPLs effect on firm value
The figure plots the coefficient estimates (on the *y*-axis) from regressing $Ln(Q)$ on firm, division-by-year, and industry-by-year fixed effects, four other antitakeover law indicators, and dummies denoting the year relative to the adoption date of a firm's incorporating state's PPL (on the *x*-axis) over the period 1992 to 2012. We create dummies for up to five years before and for five years after the respective laws' adoption. The first (last) dummy is set to one of it is (has been) five or more years before (after) the adoption of the PPL and zero otherwise. We exclude the year prior to PPL adoption as it serves as the reference year. The dashed lines correspond to the 95% confidence intervals of the coefficient estimates, calculated using robust standard errors clustered by the state of incorporation. Green triangles (blue diamonds) denote significance at the 5% (10%) level.

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Table 1
Descriptive statistics for the main variables

Panel A: Dependent variables

	Mean	St. Dev.	P25	Median	P75	Obs.
$PPill_{[t]}$	0.407	0.491	0	0	1	25,465
$New\ PPill_{[t]}$	0.071	0.257	0	0	0	5,476
$Ln(PPill\ Duration)_{[t]}$	1.434	1.202	0	2.079	2.485	17,052
$Ln(Q)_{[t]}$	0.516	0.443	0.175	0.443	0.804	25,465
$Q_{[t]}$	1.859	0.916	1.191	1.558	2.234	25,465
$Total\ Q_{[t]}$	1.051	1.028	0.366	0.737	1.359	25,409
$Excess\ Return_{[t]}$	-0.037	0.505	-0.345	-0.106	0.153	25,463
$ROA_{[t]}$	0.131	0.084	0.082	0.133	0.186	25,418
$Bid_{[t]}$	0.040	0.180	0	0	0	25,639
$Acquired_{[t]}$	0.006	0.048	0	0	0	25,639
$Total\ Premium_{[t]}$	0.231	0.178	0.095	0.171	0.325	141
$Premium\ Increase_{[t]}$	0.021	0.223	-0.100	-0.002	0.107	141

Panel B: Independent variables

	Mean	St. Dev.	P25	Median	P75	Obs.
$PPL_{[t]}$	0.322	0.467	0	0	1	25,465
$BCL_{[t]}$	0.880	0.325	1	1	1	25,465
$CSL_{[t]}$	0.259	0.438	0	0	1	25,465
$DDL_{[t]}$	0.311	0.463	0	0	1	25,465
$FPL_{[t]}$	0.309	0.462	0	0	1	25,465
$Q(Lowest)_{[t]}$	0.184	0.388	0	0	0	25,465
$Q(Low)_{[t]}$	0.241	0.428	0	0	0	25,465
$Q(High)_{[t]}$	0.277	0.448	0	0	1	25,465
$Q(Highest)_{[t]}$	0.298	0.457	0	0	1	25,465
$R\&D/Sales_{[t]}$	0.041	0.065	0	0.004	0.055	25,465
$Intangible\ Capital_{[t]}$	0.579	0.354	0.297	0.549	0.810	25,465
$CW\ Patents_{[t]}$	1.235	1.702	0	0	2.406	23,530
$RQ_{[t]}$	0.129	0.057	0.095	0.126	0.162	11,221
$Organizational\ Capital_{[t]}$	0.288	0.232	0.107	0.226	0.416	25,465
$Labor\ Intensity_{[t]}$	0.004	0.003	0.002	0.004	0.005	25,465
$Large\ Customer_{[t]}$	0.182	0.214	0	0.110	0.310	25,465
$Strategic\ Alliance_{[t]}$	0.487	0.500	0	0	1	25,465

The table reports summary statistics for the main dependent (Panel A) and independent (Panel B) variables used in the full sample OLS regressions over the period 1992 to 2012. The continuous variables are winsorized at the 5th and 95th percentiles. Appendix Table A1 provides variable definitions.

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Table 2
Second wave-PPL adoptions

Dependent variable: $PPL_{[t]}$	(1)	(2)	(3)	(4)
$BCL_{[t-1]}$	0.851*	1.169**	1.243**	1.504*
	(1.94)	(2.17)	(2.15)	(1.90)
$CSL_{[t-1]}$	-0.100	-0.149	-0.125	-0.085
	(-0.41)	(-0.49)	(-0.33)	(-0.12)
$DDL_{[t-1]}$	-0.360	-0.142	-0.116	-0.363
	(-0.95)	(-0.30)	(-0.21)	(-0.97)
$FPL_{[t-1]}$	1.243***	1.362***	1.473***	1.890***
	(2.87)	(3.99)	(3.82)	(4.39)
$Inc.SY\ PPill_{[t-1]}$		0.080	0.097	-0.039
		(0.56)	(0.60)	(-0.18)
$Inc.SY\ Ln(Q)_{[t-1]}$		-0.621	-0.701	-0.307
		(-1.07)	(-1.16)	(-0.61)
$Inc.SY\ Return_{[t-1]}$		0.286	0.283	0.312
		(0.94)	(0.76)	(0.62)
$Inc.SY\ ROA_{[t-1]}$		-0.007	0.076	-0.138
		(-0.01)	(0.10)	(-0.31)
$Inc.SY\ M\&A\ Volume_{[t-1]}$			-0.888	-2.372
			(-0.94)	(-1.18)
$Ln(Inc.SY\ GDPPC)_{[t-1]}$				-0.679
				(-0.99)
$Inc.SY\ GDP\ Growth_{[t-1]}$				0.904
				(1.44)
$Political\ Balance_{[t-1]}$				-1.340
				(-1.28)
$Ln(Inc.SY\ Pop)_{[t-1]}$				-1.077
				(-1.29)
$Inc.SY\ Unemploy_{[t-1]}$				-0.860
				(-0.67)
$Inc.SY\ Est\ Entry_{[t-1]}$				0.463
				(0.47)
$Inc.SY\ Est\ Exit_{[t-1]}$				-0.968
				(-0.65)
Year FE	Yes	Yes	Yes	Yes
N	347	347	347	347
Adjusted R^2	0.289	0.325	0.343	0.425

The table presents results from Cox proportional hazard models analyzing the hazard of a state legislature adopting a second wave-poison pill law (SW-PPL) over the period 1992-2012. A "failure event" is the adoption of a SW-PPL in a given state. States are excluded from the sample after they adopt a PPL (hence, FW-PPL states are never included). Independent variables are measured at the state level and lagged one-year (t-1). All continuous variables are winsorized at the 5% level in both tails and then standardized to have zero mean and unit variance. Appendix Table A1 provides variable definitions. t-statistics (clustered by state of incorporation) are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

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Table 3
PPLs and visible pills

Dependent variables:	$PPill_{[t]}$			$New\ PPill_{[t]}$			$Ln(PPill\ Duration)_{[t]}$		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$PPL_{[t]}$	0.026	0.029		0.029	0.024		0.177	0.181	
	(1.05)	(1.20)		(0.77)	(0.65)		(1.47)	(1.50)	
$PPL_{[t]} \times Q(Lowest)_{[\tau(s)-1]}$			0.097**			0.145**			0.611***
			(2.25)			(2.03)			(3.10)
$PPL_{[t]} \times Q(Low)_{[\tau(s)-1]}$			0.026			0.159**			0.108
			(0.73)			(2.08)			(0.72)
$PPL_{[t]} \times Q(High)_{[\tau(s)-1]}$			-0.013			0.027			-0.171
			(-0.24)			(0.25)			(-0.68)
$PPL_{[t]} \times Q(Highest)_{[\tau(s)-1]}$			0.056			-0.006			0.533***
			(1.26)			(-0.07)			(2.77)
$Q(Lowest)_{[t-1]}$		0.046***			0.066***			0.141***	
		(3.95)			(2.82)			(3.15)	
$Q(Low)_{[t-1]}$		0.029***			0.021			0.091***	
		(4.95)			(1.13)			(4.10)	
$Q(Highest)_{[t-1]}$		-0.039***			-0.005			-0.137***	
		(-5.28)			(-0.39)			(-5.19)	
Other antitakeover laws	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Division × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	25,465	25,465	25,465	5,476	5,476	5,476	16,609	16,609	16,609
Adjusted R^2	0.595	0.597	0.595	0.138	0.141	0.138	0.502	0.505	0.503

The table presents results from OLS regressions analyzing the implications of PPLs for firm-level poison pill decisions over the sample period 1992 to 2012. Dependent variables include: $PPill_{[t]}$–an indicator for whether a firm has a poison pill in-place as of the current year; $New\ PPill_{[t]}$– an indicator for the first time a firm adopts a poison pill; $Ln(PPill\ Duration)_{[t]}$–a count variable for the number of years a firm has a pill in-place. $PPL_{[t]}$ is an indicator variable equal to one if the firm is incorporated in a state whose legislature has adopted a PPL. $Q(Lowest)_{[t-1]}$, $Q(Low)_{[t-1]}$, $Q(High)_{[t-1]}$, and $Q(Highest)_{[t-1]}$ ($Q(Lowest)_{[\tau(s)-1]}$, $Q(Low)_{[\tau(s)-1]}$, $Q(High)_{[\tau(s)-1]}$, and $Q(Highest)_{[\tau(s)-1]}$) are indicator variables that equal one if a (PPL-) firm's level of Tobin's Q (in the year before the adoption of its respective PPL ($\tau(s)-1$)) lies in the bottom, middle-to-bottom, middle-to-top, and top quartile, respectively, of its (adopting state's) empirical distribution. Columns 4-9 only include firms that eventually adopt a pill, while columns 4-6 exclude firms after they adopt a new pill. Controls for other antitakeover laws include: BCL, CSL, DDL, and FPL. Division fixed effects are measured using U.S. Census divisions and industry fixed effects are defined by three-digit SIC codes. Continuous variables are winsorized at the 5% level in both tails. Appendix Table A1 provides variable definitions. t-statistics (clustered by state of incorporation) are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

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Table 4
PPLs and firm value

Dependent variable: $Ln(Q)_{[t]}$					
	(1)	(2)	(3)	(4)	(5)
$PPL_{[t]}$	0.059***	0.062***	0.062***	0.057**	0.058**
	(3.34)	(3.52)	(2.76)	(2.53)	(2.01)
$PPL_{[t]} \times PPill_{[\tau(s)-1]}$			-0.008	-0.008	-0.015
			(-0.19)	(-0.21)	(-0.40)
$PPill_{[t-1]}$		-0.062***			
		(-5.55)			
Other antitakeover laws	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Division × Year FE	Yes	Yes	Yes	No	No
Region × Year FE	No	No	No	Yes	No
State × Year FE	No	No	No	No	Yes
Industry × Year FE	Yes	Yes	Yes	Yes	Yes
N	25,465	25,465	25,465	25,465	25,465
Adjusted R^2	0.678	0.680	0.678	0.677	0.678

The table presents results from OLS regressions analyzing the value implications of PPLs over the period 1992 to 2012. The dependent variable is the natural logarithm of Tobin's Q ($Ln(Q)$). The "Other antitakeover laws" include: BCL, CSL, DDL, and FPL. Division (region) fixed effects are measured using U.S. Census divisions (regions), state fixed effects are based on a firm's state of location, and industry fixed effects are defined by three-digit SIC codes. Continuous variables are winsorized at the 5% level in both tails. Appendix Table A1 provides variable definitions. t-statistics (clustered by state of incorporation) are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

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Table 5
PPLs and firm value in a matched sample

Panel A: Pre-treatment year (t-1) summary statistics

	(1)	(2)	(3)
	Treated	Control	Difference
$Q_{[t]}$	1.744	1.814	-0.071
	(0.986)	(0.919)	(-0.85)
$Total\ Assets_{[t]}$	1908.5	1938.2	-29.67
	(5510.9)	(4977.2)	(-0.10)
$PPill_{[t]}$	0.330	0.330	0.000
	(0.471)	(0.471)	(0.00)
$SIC2_{[t]}$	43.63	43.69	-0.057
	(19.63)	(19.75)	(-0.03)
$Division_{[t]}$	5.511	5.652	-0.140
	(2.388)	(2.571)	(-0.65)
N (by group)	264	264	

Panel B: The effect of PPLs on $Ln(Q)_{[t]}$

	(1)	(2)	(3)
$Treated \times Post_{[t]}$	0.049**	0.049**	0.055***
	(2.21)	(2.23)	(3.49)
$Post_{[t]}$	-0.020	-0.016	0.027
	(-0.68)	(-0.55)	(0.78)
$Post_{[t]} \times PPill_{[\tau(s)-1]}$		-0.014	-0.008
		(-0.51)	(-0.35)
Other antitakeover laws	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Division × Year FE	Yes	Yes	No
Industry × Year FE	Yes	Yes	No
Year FE	No	No	Yes
N	2,086	2,086	2,819
Adjusted R^2	0.793	0.793	0.727

The table presents summary statistics and results from OLS regressions for a matched sample. $Treated$ (control) firms are defined as companies incorporated in states that (do not) adopt PPLs (in at least the three years following its matched counterpart's adoption year). We use propensity score matching with replacement in year t-1 to create a sample matched on Q and $Total\ Assets$, and exactly on $PPill$, and when possible, exactly on divisions and two-digit SIC industries – when it is not possible to match exactly on division (SIC2), we match on the next closest division (SIC1). Panel A reports pre-treatment year summary statistics. We also report differences between sample means (t-statistics in parentheses). Panel B shows the matched sample $Ln(Q)$ regression results over a $t \pm 3$ estimation window. The "Other antitakeover laws" include: BCL, CSL, DDL, and FPL. Division fixed effects are measured using U.S. Census divisions, and industry fixed effects are defined by three-digit SIC codes. $Treated$ and $PPill_{[\tau(s)-1]}$ are absorbed by the firm fixed effects. Continuous variables are winsorized at the 5% level in both tails. t-statistics (clustered by state of incorporation) are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

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Table 6
PPLs and alternative measures of firm value

Panel A: The implications of PPLs for:

	$Q_{[t]}$		$Total\ Q_{[t]}$		$Excess\ Return_{[t]}$		$ROA_{[t]}$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$PPL_{[t]}$	0.112***		0.115**		0.029**		0.009**	
	(3.35)		(2.16)		(2.09)		(2.03)	
$Treated \times Post_{[t]}$		0.124**		0.125**		0.047*		0.007*
		(2.14)		(2.12)		(1.73)		(1.81)
$Post_{[t]}$		-0.017		-0.006		-0.037		0.002
		(-0.30)		(-0.06)		(-0.50)		(0.28)
$PPL_{[t]} \times PPill_{[\tau(s)-1]}$	-0.059		-0.034		-0.005		-0.003	
	(-0.75)		(-0.62)		(-0.34)		(-0.53)	
$Post_{[t]} \times PPill_{[\tau(s)-1]}$		-0.020		-0.063		0.010		-0.005
		(-0.29)		(-0.80)		(0.32)		(-1.18)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Division × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	25,465	2,086	25,409	2,061	25,463	2,086	25,418	2,074
Adjusted R^2	0.642	0.744	0.628	0.760	0.059	0.144	0.620	0.764

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Panel B: Portfolio analysis

Portfolio "6m36"	Four-factor model			Three-factor model			Market-factor model		
	Long	Short	Long - Short	Long	Short	Long - Short	Long	Short	Long - Short
Alpha (monthly)	0.820*	-0.046	0.751*	0.933**	0.001	0.801**	0.886*	0.076	0.704*
	(1.80)	(-0.12)	(1.89)	(2.02)	(0.00)	(2.04)	(1.96)	(0.21)	(1.80)
Average # firms	45.11	45.58	-	45.11	45.58	-	45.11	45.58	-
N	252	254	-	252	254	-	252	254	-
Adjusted R^2	0.262	0.351	0.022	0.261	0.353	0.026	0.232	0.340	0.005

Portfolio "12m36"	Four-factor model			Three-factor model			Market-factor model		
	Long	Short	Long - Short	Long	Short	Long - Short	Long	Short	Long - Short
Alpha (monthly)	0.490	-0.313	0.757*	0.569	-0.281	0.790**	0.515	-0.237	0.709*
	(1.07)	(-0.86)	(1.96)	(1.25)	(-0.81)	(2.08)	(1.14)	(-0.68)	(1.87)
Average # firms	49.81	50.07	-	49.81	50.07	-	49.81	50.07	-
N	253	254	-	253	254	-	253	254	-
Adjusted R^2	0.280	0.375	0.014	0.281	0.378	0.018	0.257	0.359	0.004

The table examines the effect of PPLs on alternative measures of firm value. Panel A reports results from OLS regressions on both the full sample over the period 1992 to 2012 and the matched sample with a $t \pm 3$ estimation window. Dependent variables include: Q, $Total\ Q$, $Excess\ Return$, and ROA. The "Other antitakeover laws" include: BCL, CSL, DDL, and FPL. Division fixed effects are measured using U.S. Census divisions, and industry fixed effects are defined by three-digit SIC codes. $Treated$ and $PPill_{[\tau(s)-1]}$ are absorbed by firm fixed effects. t-statistics (clustered by state of incorporation) are reported in parentheses. Continuous variables are winsorized at the 5% level in both tails. Panel B shows results from a portfolio analysis using the matched sample $Treated$ and control firms. The long (short) portfolios are constructed as follows: For portfolios "6m36" and "12m36," we include all stocks of matched $Treated$ (control) firms starting either 6 or 12 months before the fiscal year-end of the year in which the matched treated incorporating state adopts a PPL and hold (short) these stocks for 36 months, post-adoption. The long-short portfolios are then created by differencing the portfolio returns of the long and short portfolios for each respective month. We use the four-factor, three-factor, and market factor models to estimate $Alpha$ (monthly), where each of the models uses an equally-weighted market factor, and we calculate the portfolio return with each stock weighted by its market capitalization immediately preceding its inclusion in the portfolio. t-statistics (based on robust standard errors) are presented in parentheses. The number of stocks in the long and short portfolios are averaged across all months and displayed in the "Average # firms" row. The "N" row shows the total number of firms with useable returns. Portfolio returns are winsorized at the 5% level in both tails. Appendix Table A1 provides variable definitions. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

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Table 7
Testing the bonding hypothesis

Panel A: Heterogeneous effects of PPLs for innovative firms
Dependent variable: $Ln(Q)_{[t]}$

	(1)	(2)	(3)	(4)
$PPL_{[t]} \times R\&D/Sales_{[\tau(s)-1]}$	0.026**			
	(2.32)			
$PPL_{[t]} \times Intangible\ Capital_{[\tau(s)-1]}$		0.065***		
		(2.74)		
$PPL_{[t]} \times CW\ Patents_{[\tau(s)-1]}$			0.033**	
			(1.96)	
$PPL_{[t]} \times RQ_{[\tau(s)-1]}$				0.023*
				(1.73)
$PPL_{[t]}$	0.060*	0.045**	0.049***	0.033
	(1.99)	(2.32)	(3.14)	(0.68)
$PPL_{[t]} \times PPill_{[\tau(s)-1]}$	-0.010	-0.001	-0.027	0.019
	(-0.28)	(-0.02)	(-1.24)	(0.39)
Other antitakeover laws	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Division × Year FE	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes
N	25,465	25,465	23,528	11,221
Adjusted R^2	0.709	0.679	0.654	0.665

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Panel B: Heterogeneous effects of PPLs for stakeholder relationships
Dependent variable: $Ln(Q)_{[t]}$

	(1)	(2)	(3)	(4)
$PPL_{[t]} \times Organizational\ Capital_{[\tau(s)-1]}$	0.067***			
	(4.65)			
$PPL_{[t]} \times Labor\ Intensity_{[\tau(s)-1]}$		0.030***		
		(3.22)		
$PPL_{[t]} \times Large\ Customer_{[\tau(s)-1]}$			0.039*	
			(1.85)	
$PPL_{[t]} \times Strategic\ Alliance_{[\tau(s)-1]}$				0.066**
				(2.27)
$PPL_{[t]}$	0.051**	0.064***	0.047**	0.005
	(2.58)	(2.74)	(2.20)	(0.26)
$PPL_{[t]} \times PPill_{[\tau(s)-1]}$	-0.008	-0.014	-0.012	-0.031
	(-0.27)	(-0.32)	(-0.45)	(-0.96)
Other antitakeover laws	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Division × Year FE	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes
N	25,465	25,465	25,465	25,465
Adjusted R^2	0.679	0.678	0.677	0.642

The table presents results from OLS regressions analyzing the heterogeneous value implications of PPLs for firms that are more innovative or more reliant on stakeholder relationships over the period 1992 to 2012. The dependent variable is the natural logarithm of Tobin's Q ($Ln(Q_{[t]})$). Panel A (B) interacts $PPL_{[t]}$ with the following measures of innovative activity (stakeholder relationships) – $R\&D/Sales_{[\tau(s)-1]}$, $Intangible\ Capital_{[\tau(s)-1]}$, $CW\ Patents_{[\tau(s)-1]}$, and $RQ_{[\tau(s)-1]}$ ($Organizational\ Capital_{[\tau(s)-1]}$, $Labor\ Intensity_{[\tau(s)-1]}$, $Large\ Customer_{[\tau(s)-1]}$, and $Strategic\ ALliance_{[\tau(s)-1]}$) – measured in the year before the adoption of a PPL-firm's respective PPL. $R\&D/Sales$, $Intangible\ Capital$, $CW\ Patents$, and RQ ($Organizational\ Capital$ and $Labor\ Intensity$) are standardized to have a mean of zero and a standard deviation of one. The "Other antitakeover laws" include: BCL, CSL, DDL, and FPL. Division fixed effects are measured using U.S. Census divisions, and industry fixed effects are defined by three-digit SIC codes. The interacting variables are time-invariant and absorbed by the firm fixed effects. Continuous variables are winsorized at the 5% level in both tails. Appendix Table A1 provides variable definitions. *t*-statistics (clustered by state of incorporation) are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively

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Table 8
Testing the bargaining power hypothesis

Panel A: PPLs and takeover propensities

Dependent variables:	$Bid_{[t]}$			$Acquired_{[t]}$		
	(1)	(2)	(3)	(4)	(5)	(6)
$PPL_{[t]}$	-0.023	-0.024	-0.027	-0.001	-0.001	-0.002
	(-0.87)	(-0.88)	(-0.94)	(-0.08)	(-0.07)	(-0.17)
$PPL_{[t]} \times PPill_{[\tau(s)-1]}$			0.012			0.006
			(0.42)			(0.28)
$Q(Lowest)_{[t-1]}$		0.059***			0.005	
		(4.61)			(0.45)	
$Q(Highest)_{[t-1]}$		-0.042***			-0.012	
		(-3.27)			(-1.37)	
Other antitakeover laws	Yes	Yes	Yes	Yes	Yes	Yes
Division × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
N	25,465	25,465	25,465	25,465	25,465	25,465
Adjusted R^2	0.040	0.041	0.040	0.035	0.036	0.035

Panel B: PPLs and takeover premiums

Dependent variables:	$Total\ Premium_{[t]}$			$Premium\ Increase_{[t]}$		
	(1)	(2)	(3)	(4)	(5)	(6)
$PPL_{[t]}$	0.038	0.042	0.018	0.055	0.064	0.054
	(0.59)	(0.66)	(0.25)	(0.84)	(1.00)	(0.78)
$PPL_{[t]} \times PPill_{[\tau(s)-1]}$			0.104*			0.063**
			(1.90)			(2.18)
$PPill_{[t-1]}$		0.052***			0.075**	
		(3.41)			(2.70)	
Other antitakeover laws	Yes	Yes	Yes	Yes	Yes	Yes
Division FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
N	141	141	141	141	141	141
Adjusted R^2	0.046	0.076	0.061	0.015	0.040	0.032

The table presents results from OLS regressions analyzing the takeover implications of PPLs over the period 1992 to 2012. Panel A examines the effect of PPLs on takeover propensities. The dependent variables include: $Bid_{[t]}$ and $Acquired_{[t]}$. Bid ($Acquired$) is an indicator equal to one if a firm receives a takeover bid (acquired) as cataloged by the SDC M&A database. Division fixed effects are measured using U.S. Census divisions, and industry fixed effects are defined by three-digit SIC codes. Panel B explores the effect of PPLs on takeover premiums. Dependent variables include: $Total\ Premium_{[t]}$ and $Premium\ Increase_{[t]}$. $Total\ Premium$ ($Premium\ Increase$) is the total percentage premium (premium increase in percentage) offered relative to the target's price 20 days before the initial offer. The "Other antitakeover laws" include: BCL, CSL, DDL, and FPL. Continuous variables are winsorized at the 5% level in both tails. Appendix Table A1 provides variable definitions. t-statistics (clustered by state of incorporation) are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

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ONLINE APPENDIX OF

SHADOW PILLS, PILL POLICY, AND FIRM VALUE

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Additional References

Kolari, J. W., and S. Pynnönen. 2010. Event study testing with cross-sectional correlation of abnormal returns. *Review of Financial Studies* 23: 3996-4025.

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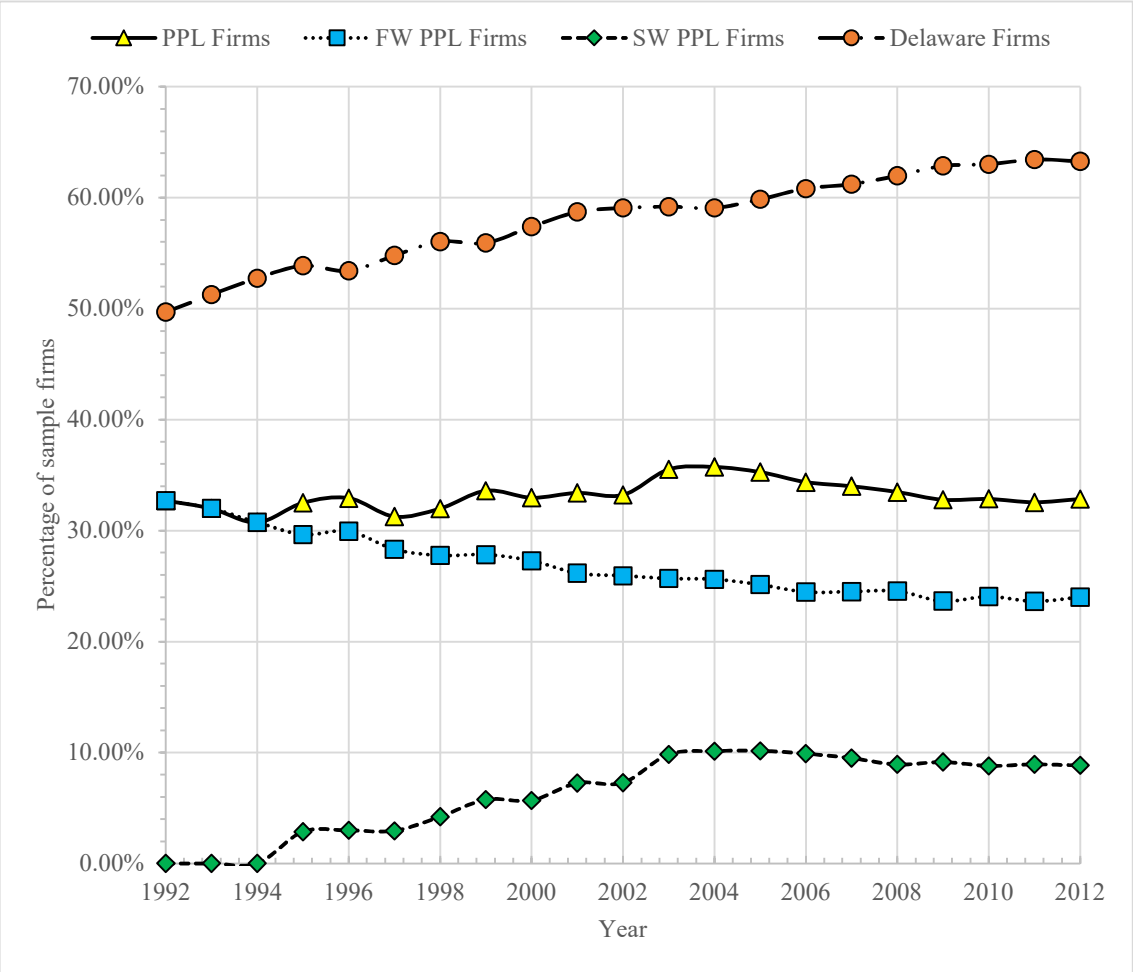

Figure OA1
Percentage of firms incorporated in PPL states, FW-PPL states, SW-PPL states, and Delaware
The figure plots the percentage of firms incorporated in a poison pill law (PPL) state, first wave-PPL (FW-PPL) state, SW-PPL state, or Delaware each year from 1992 to 2012. Sample firms incorporated in a PPL state, passed between 1986 and 2009, are shown with a solid line and yellow triangles, in a FW-PPL state, enacted between 1986 and 1990, with a round dotted line and blue squares, in a SW-PPL state, adopted between 1995 and 2009, with a square dotted line and green diamonds, and those incorporated in Delaware with a long dash dotted line and orange circles.

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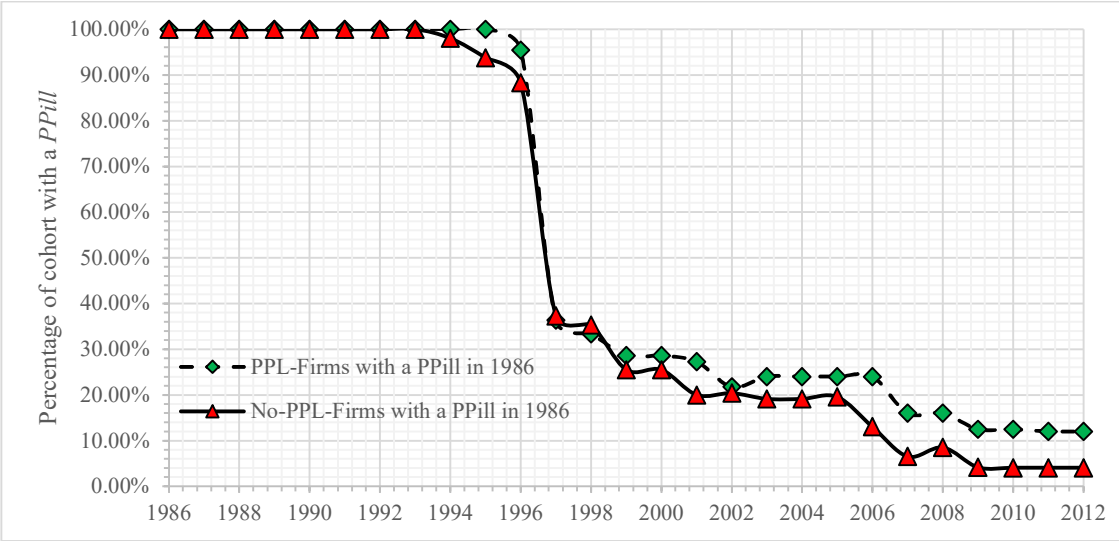
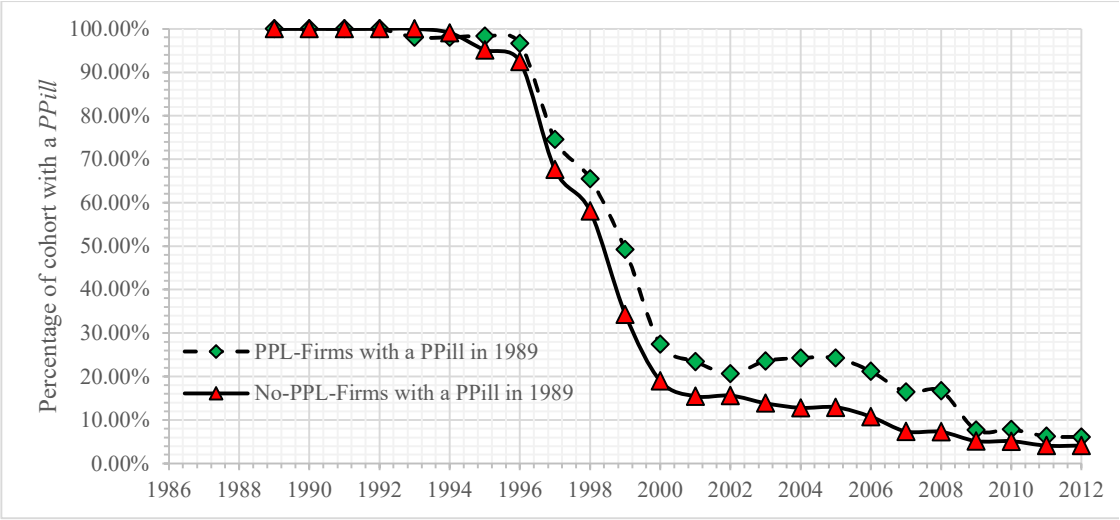
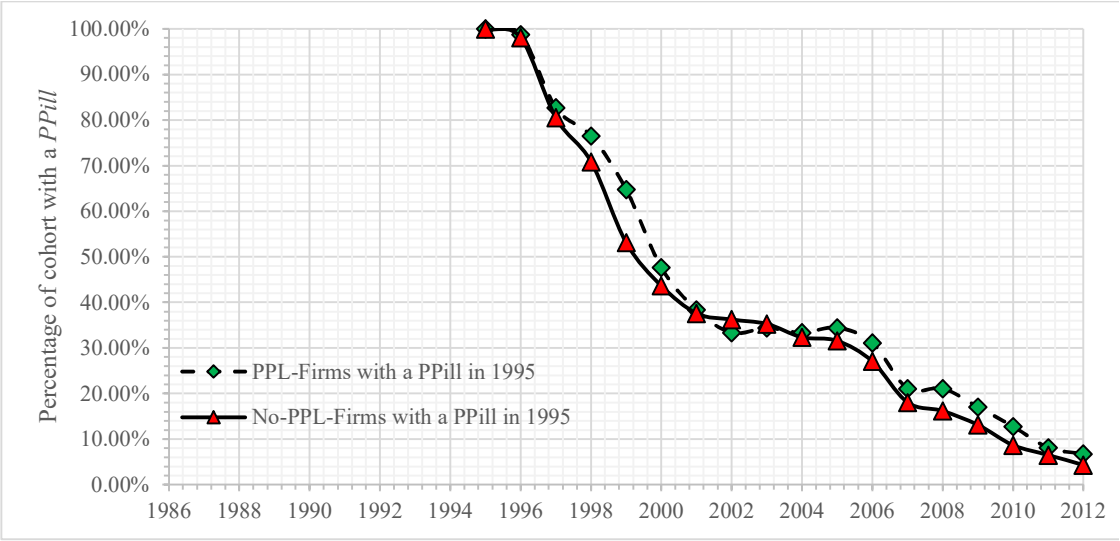


Panel A: Cohorts formed in 1986



Panel B: Cohorts formed in 1989



Panel C: Cohorts formed in 1995

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Figure OA2
Percentage of cohorts with a poison pill
The figure plots the percentage of six different cohorts with a poison pill ($PPill$): (1) firms incorporated in a state that adopts a PPL ("PPL Firms") and with a poison pill in 1986, (2) firms incorporated in a state without a PPL ("No-PPL Firms") and with a poison pill in 1986, (3) PPL-firms with a poison pill in 1989, (4) No-PPL-firms with a poison pill in 1989, (5) PPL-firms with a poison pill in 1995, and (6) No-PPL-firms with a poison pill in 1995. The figure plots the annual percentage of firms with a poison pill within each cohort as a percentage of those firms that remain in the sample from that year until the last year in the sample, 2012.

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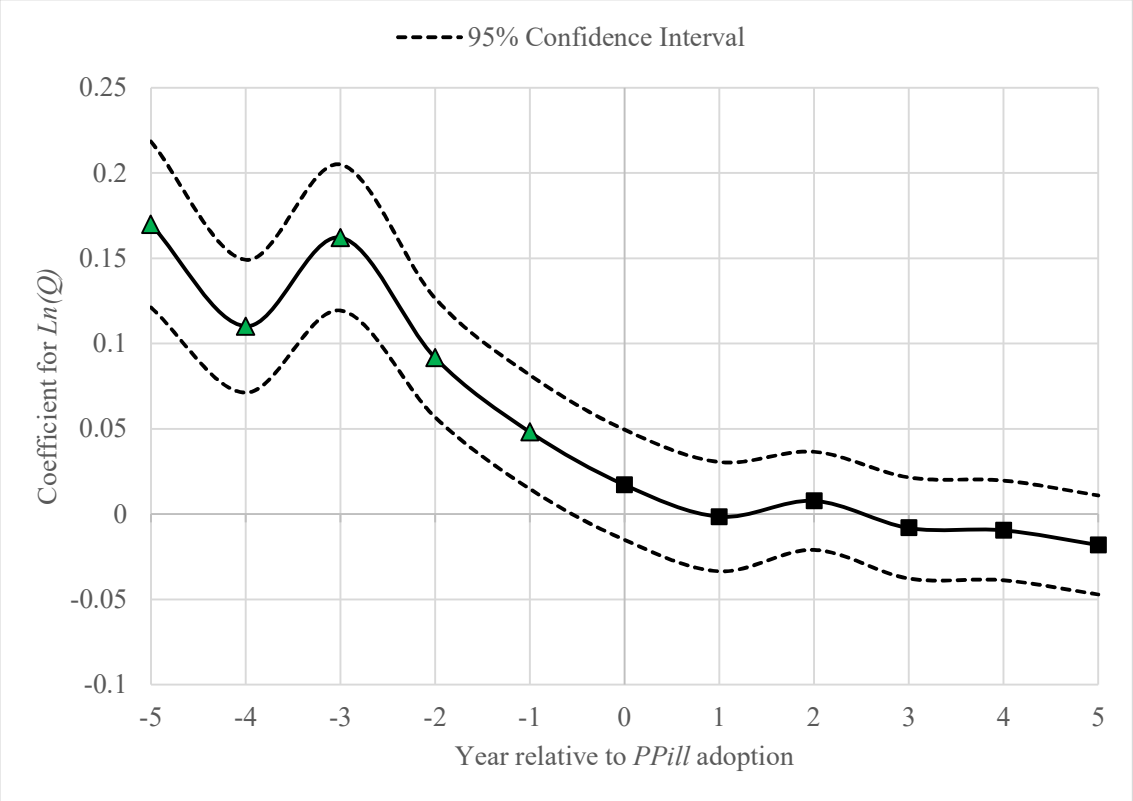

Figure OA3
Reverse causality: Firm value and visible pill adoption
The figure plots the resulting point estimates (y-axis) from regressing $Ln(Q)$ on dummy variables indicating the year relative to the adoption of a $PPill$ (x-axis), as well as on firm, division-by-year, and industry-by-year fixed effects over the period 1992 to 2012. We create dummies for up to 5 years before and after $PPill$ adoption. The dashed lines correspond to 95% confidence intervals – calculated with robust standard errors clustered by firm – and green triangles indicate significance at the 1% level.

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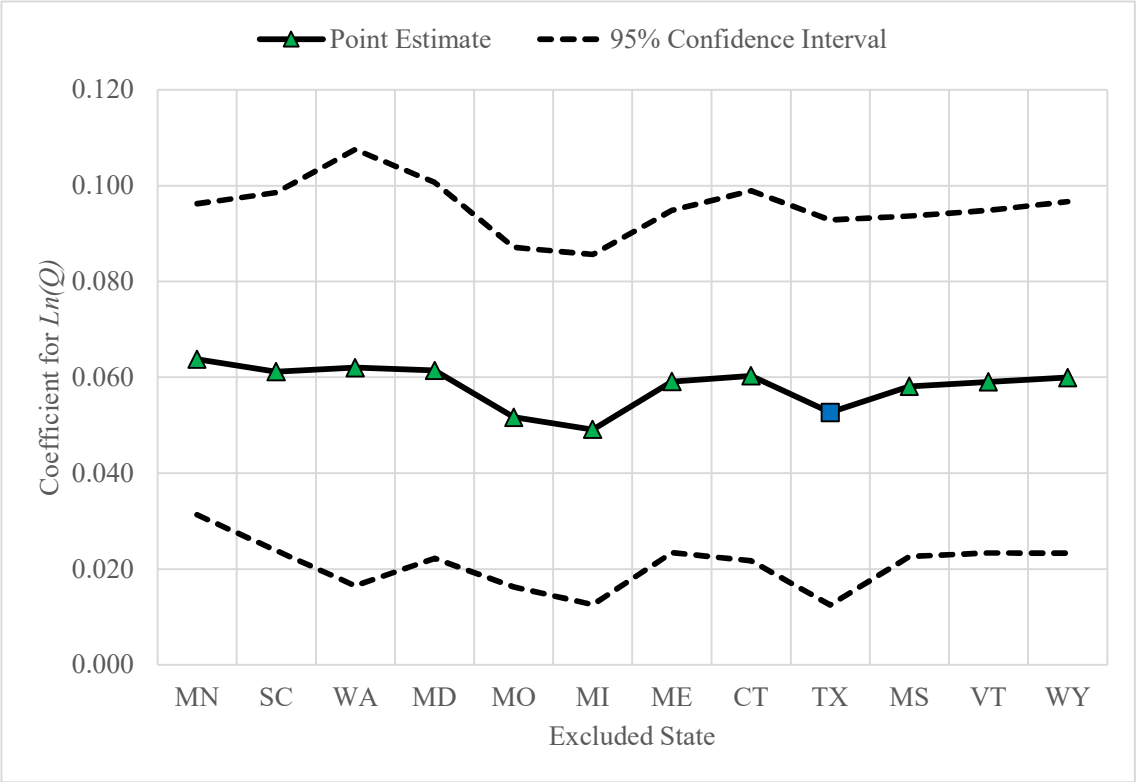

Figure OA4
PPLs and firm value with each law adopting state dropped once
The figure plots the point estimates (y-axis) for our baseline regressions of $Ln(Q)$ on PPL (i.e., Table 4, Column 1), but where we exclude each law adopting state (x-axis) one-by-one over the period 1992 to 2012. The dashed lines correspond to 95% confidence intervals and green triangles (blue squares) indicate significance at the 1% (5%) level.

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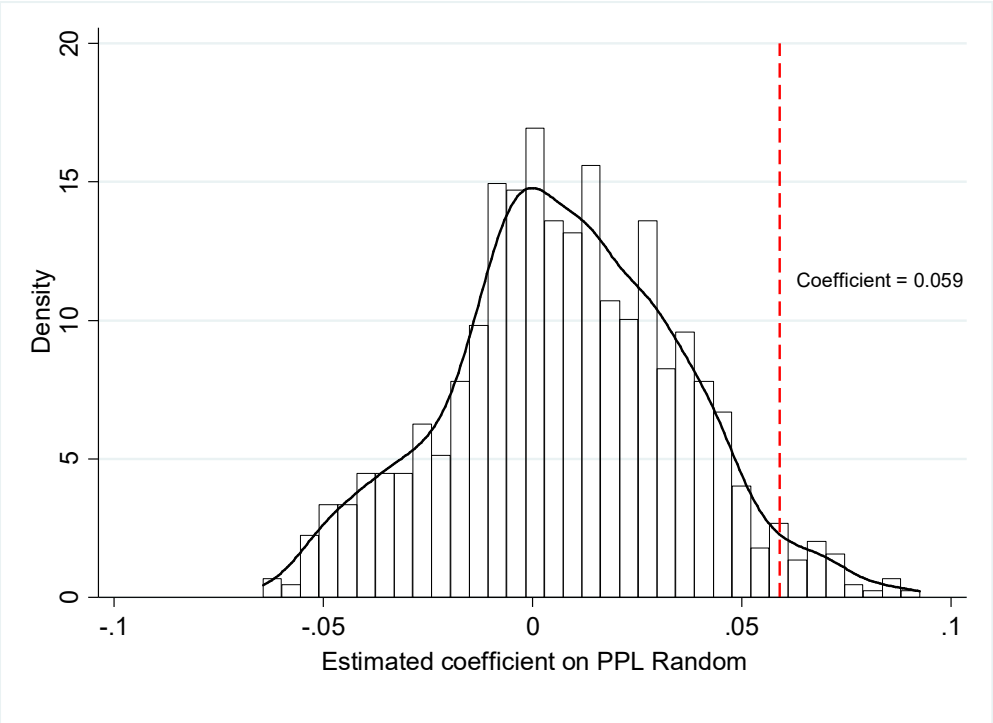

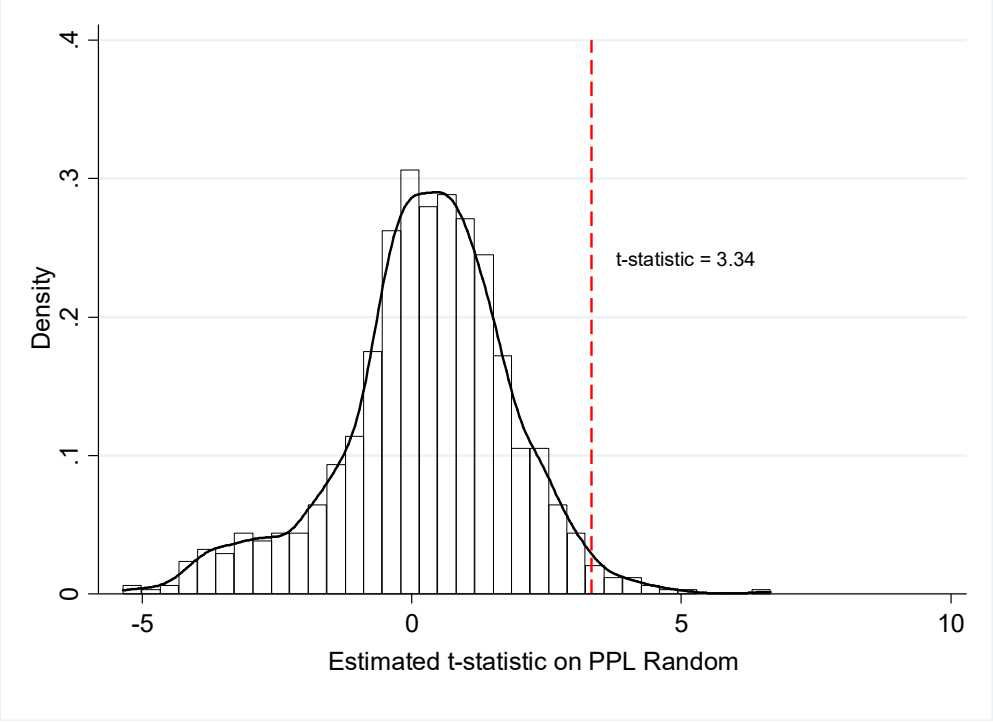

Figure OA5
Full sample placebo test
The figures plot the distribution of coefficient (top) and *t*-statistic (bottom) estimates from randomized PPL adoption dates across different states. We simulate fictitious adoptions by randomly assigning states PPLs but maintain the structure of the empirical distribution of actual adoptions. We repeat the estimation 1,000 times. In each of the pseudo samples, we then run the regression as in Table 4, Column 1, and plot the corresponding coefficients and *t*-statistics. The dashed red vertical lines represent the actual regression coefficient and *t*-statistic based on the actual data.

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Table OA1
PPLs and ROA

Dependent variable: $ROA_{[t]}$	1976 to 1995 w/out $PPill$		1976 to 1995 w/$PPill$		1992 to 2012 w/$PPill$	
	(1)	(2)	(3)	(4)	(5)	(6)
$PPL_{[t]}$	-0.010*	-0.010*	-0.001	-0.002	0.009**	0.009*
	(-1.80)	(-1.84)	(-0.31)	(-0.43)	(2.03)	(1.73)
$PPL_{[t]} \times PPill_{[\tau(s)-1]}$	N/A	N/A	0.002	0.003	-0.003	-0.003
			(0.25)	(0.38)	(-0.53)	(-0.34)
Other antitakeover laws	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Division × Year FE	Yes	No	Yes	No	Yes	No
State × Year FE	No	Yes	No	Yes	No	Yes
Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
N	92,038	92,038	10,529	10,529	25,418	25,418
Adjusted R^2	0.629	0.629	0.681	0.682	0.620	0.620

The table presents results from OLS regressions analyzing the implications of PPLs on ROA over varying sample periods and with and without specifying a control for firm-level poison pills. The first (middle) two columns are specific to the sample period 1976 to 1995 without (with) specifying $PPill_{[\tau(s)-1]}$, while the last two columns pertain to the sample period 1992 to 2012 and include $PPill_{[\tau(s)-1]}$. The dependent variable is return on assets (ROA). $PPL_{[t]} \times PPill_{[\tau(s)-1]}$ interacts an indicator variable set to one if a PPL-firm has a poison pill in-place in the year before the adoption of its respective PPL ($\tau(s)-1$) with PPL. The "Other antitakeover laws" include: BCL, CSL, DDL, and FPL. Division (state) fixed effects are measured using U.S. Census divisions (states of location) and industry fixed effects are defined by three-digit SIC codes. Continuous variables are winsorized at the 5% level in both tails. Appendix Table A1 provides variable definitions. t-statistics (clustered by state of incorporation) are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

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Table OA2
First wave-PPLs and firm value

Dependent variables:	$Ln(Q)_{[t]}$	$Q_{[t]}$	$Total\ Q_{[t]}$	$Excess\ Return_{[t]}$	$ROA_{[t]}$
	(1)	(2)	(3)	(4)	(5)
$FW\ PPL_{[t]}$	-0.020	-0.021	-0.049	0.000	-0.001
	(-1.01)	(-0.65)	(-1.21)	(0.08)	(-0.15)
$FW\ PPL_{[t]} \times PPill_{[\tau(s)-1]}$	0.008	0.010	0.008	-0.005	0.002
	(0.67)	(0.50)	(0.35)	(-1.00)	(0.47)
Other antitakeover laws	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Division × Year FE	Yes	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes	Yes
N	8,506	8,506	8,500	8,506	8,498
Adjusted R^2	0.774	0.770	0.781	0.068	0.689

The table presents results from OLS regressions analyzing the value implications of first wave-poison pill laws (FW-PPLs) over the sample period 1983 to 1993. The dependent variables include: the natural logarithm of Tobin's Q ($Ln(Q)$); Tobin's Q (Q); Total Tobin's Q ($Total\ Q$), which explicitly accounts for intangible assets when estimating a firm's replacement cost of capital; excess stock returns, estimated using the Fama-French four-factor model ($Excess\ Return$), and return on assets (ROA). $FW\ PPL$ is an indicator variable equal to one if a firm's state of incorporation has adopted a FW-PPL (enacted at any point between 1986 and 1990) as of the current year, and zero otherwise. The "Other antitakeover laws" include: BCL, CSL, DDL, and FPL. Division fixed effects are measured using U.S. Census divisions, and industry fixed effects are defined by three-digit SIC codes. Continuous variables are winsorized at the 5% level in both tails. Appendix Table A1 provides variable definitions. t-statistics (clustered by state of incorporation) are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

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Table OA3
PPL adoption dates

Alabama		Montana	
Alaska		Nebraska	
Arizona		Nevada	6/1989
Arkansas		New Hampshire	
California		New Jersey	6/1989
Colorado	3/1989	New Mexico	
Connecticut	6/2003	New York	12/1988
Delaware[a]		North Carolina	6/1989
Florida	6/1989	North Dakota	
Georgia	4/1988	Ohio	11/1986
Hawaii	6/1988	Oklahoma	
Idaho	3/1988	Oregon	3/1989
Illinois	8/1989	Pennsylvania	3/1988
Indiana	3/1986	Rhode Island	7/1990
Iowa	6/1989	South Carolina	6/1998
Kansas		South Dakota	2/1990
Kentucky	7/1988	Tennessee	5/1989
Louisiana		Texas	5/2003
Maine[b]	4/2002	Utah	3/1989
Maryland	5/1999	Vermont	6/2008
Massachusetts	7/1989	Virginia	4/1990
Michigan	7/2001	Washington	3/1998
Minnesota	5/1995	West Virginia	
Mississippi	4/2005	Wisconsin	9/1987
Missouri	7/1999	Wyoming	3/2009

The table reports the month and year in which a state adopts a poison pill law (PPL). The dates listed above come from Cain, McKeon, and Solomon (2017) and Karpoff and Wittry (2018).

[a] The *Moran v. Household* court decision in Delaware in 1985 provides some legitimacy to poison pills. However, Delaware never issued a PPL, thus we treat Delaware as a control state.
[b] The *Georgia-Pacific v. Great Northern Nekoosa Corp.* court decision in Maine in 1990 provides some legitimacy to poison pills, although, 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.

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Table OA4
Summary statistics for the period 1983-2012

	Mean	St. Dev.	P25	Median	P75	Obs.
$PPill_{[t]}$	0.392	0.488	0	0	1	32,220
$Ln(Q)_{[t]}$	0.472	0.434	0.138	0.389	0.742	32,220
$Q_{[t]}$	1.774	0.879	1.147	1.475	2.099	32,220
$Total\ Q_{[t]}$	0.960	0.988	0.307	0.661	1.241	32,186
$Excess\ Return_{[t]}$	-0.041	0.487	-0.337	-0.102	0.148	32,218
$ROA_{[t]}$	0.133	0.082	0.086	0.135	0.187	32,203
$PPL_{[t]}$	0.285	0.451	0	0	1	32,220
$BCL_{[t]}$	0.781	0.413	1	1	1	32,220
$CSL_{[t]}$	0.238	0.426	0	0	0	32,220
$DDL_{[t]}$	0.284	0.451	0	0	1	32,220
$FPL_{[t]}$	0.289	0.453	0	0	1	32,220

The table reports summary statistics for the main variables over the period 1983 to 2012. The continuous variables are winsorized at the 5th and 95th percentiles. Appendix Table A1 provides variable definitions.

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Table OA5
Descriptive statistics: Sample splits

Panel A: Comparing sample means for firms with and without firm-level poison pills

	$PPill_{[t]} = 1$ (Obs. = 10,353)	$PPill_{[t]} = 0$ (Obs. = 15,112)
Main dependent variables		
$Ln(Q)_{[t]}$	0.494***	0.531
	(0.429)	(0.452)
$Q_{[t]}$	1.807***	1.895
	(0.871)	(0.944)
$Total\ Q_{[t]}$	0.959	1.114
	(0.948)	(1.075)
$Excess\ Return_{[t]}$	-0.042	-0.033
	(0.499)	(0.510)
$ROA_{[t]}$	0.128***	0.133
	(0.084)	(0.084)
Main independent variables		
$PPL_{[t]}$	0.324	0.320
	(0.468)	(0.466)
$BCL_{[t]}$	0.901**	0.865
	(0.299)	(0.342)
$CSL_{[t]}$	0.255	0.262
	(0.436)	(0.440)
$DDL_{[t]}$	0.317*	0.307
	(0.465)	(0.461)
$FPL_{[t]}$	0.296***	0.317
	(0.456)	(0.465)

Panel B: Comparing sample means for firms incorporated in states with and without PPLs

	$PPL_{[t]} = 1$ (Obs. = 8,189)	$PPL_{[t]} = 0$ (Obs. = 17,276)
Main dependent variables		
$Ln(Q)_{[t]}$	0.494***	0.527
	(0.431)	(0.448)
$Q_{[t]}$	1.809***	1.883
	(0.876)	(0.933)
$Total\ Q_{[t]}$	0.976***	1.087
	(0.973)	(1.051)
$Excess\ Return_{[t]}$	-0.042	-0.034
	(0.475)	(0.519)
$ROA_{[t]}$	0.134***	0.129
	(0.079)	(0.086)
Main independent variables		
$PPill_{[t]}$	0.427***	0.409
	(0.495)	(0.492)
$BCL_{[t]}$	0.877	0.881
	(0.328)	(0.324)
$CSL_{[t]}$	0.651***	0.074
	(0.477)	(0.261)
$DDL_{[t]}$	0.889***	0.037
	(0.314)	(0.188)
$FPL_{[t]}$	0.820***	0.066
	(0.384)	(0.248)

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Panel C: Comparing sample means between firms incorporated in SW-PPL states and FW-PPL states

	$SW\ PPL_{[t]} = 1$ (Obs. = 2,216)	$FW\ PPL_{[t]} = 1$ (Obs. = 5,973)
Main dependent variables		
$Ln(Q)_{[t]}$	0.534***	0.479
	(0.485)	(0.408)
$Q_{[t]}$	1.929***	1.765
	(1.015)	(0.815)
$Total\ Q_{[t]}$	1.080***	0.937
	(1.118)	(0.911)
$Excess\ Return_{[t]}$	-0.004***	-0.056
	(0.575)	(0.431)
$ROA_{[t]}$	0.120***	0.140
	(0.093)	(0.073)
Main independent variables		
$PPill_{[t]}$	0.405	0.412
	(0.491)	(0.492)
$BCL_{[t]}$	0.999***	0.832
	(0.030)	(0.374)
$CSL_{[t]}$	0.607***	0.667
	(0.488)	(0.471)
$DDL_{[t]}$	0.652***	0.977
	(0.476)	(0.151)
$FPL_{[t]}$	0.864***	0.804
	(0.343)	(0.397)

Panel D: Comparing sample means of firms incorporated in SW-PPL states and firms incorporated elsewhere

	$SW\ PPL_{[t]} = 1$ (Obs. = 2,216)	$SW\ PPL_{[t]} = 0$ (Obs. = 23,249)
Main dependent variables		
$Ln(Q)_{[t]}$	0.533**	0.514
	(0.486)	(0.439)
$Q_{[t]}$	1.928***	1.853
	(1.015)	(0.906)
$Total\ Q_{[t]}$	1.078	1.049
	(1.117)	(1.019)
$Excess\ Return_{[t]}$	-0.006***	-0.040
	(0.575)	(0.498)
$ROA_{[t]}$	0.120***	0.132
	(0.094)	(0.083)
Main independent variables		
$PPill_{[t]}$	0.405	0.407
	(0.491)	(0.491)
$BCL_{[t]}$	0.998***	0.868
	(0.042)	(0.338)
$CSL_{[t]}$	0.608***	0.226
	(0.488)	(0.418)
$DDL_{[t]}$	0.650***	0.278
	(0.477)	(0.448)
$FPL_{[t]}$	0.864***	0.255
	(0.343)	(0.436)

The table reports summary statistics for the main variables used in the regression models over the period 1992 to 2012. Panel A (B) shows sample means (standard deviations in parentheses) between firms with and without firm-level poison pills (firms incorporated in states with and without PPLs). Panel C (D) compares sample means (standard deviations in parentheses) between firms incorporated in SW-PPL states and FW-PPL states (firms incorporated in states that adopt SW-PPLs with firms incorporated elsewhere). *, **, and *** indicates significance at the 10%, 5%, and 1% level, respectively, for a *t*-test of whether the two respective samples have equal means. Continuous variables are winsorized at the 5th and 95th percentiles. Appendix Table A1 provides variable definitions.

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Table OA6
First wave-PPLs and visible poison pills

Dependent variable: $PPill_{[t]}$	Entire FW-period (1983-1993)		Post-*Moran* (1986-1993)		Post-*Interco* & *Pillsbury* (1989-1993)	
	(1)	(2)	(3)	(4)	(5)	(6)
$FW\ PPL_{[t]}$	0.047	0.046	-0.015	-0.016	0.105*	0.072**
	(0.57)	(0.59)	(-0.39)	(-0.41)	(1.98)	(2.03)
$Eventual\ SW\ PPL_{[t]}$		-0.007		0.056		-0.239***
		(-0.08)		(1.00)		(-3.81)
Other antitakeover laws	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Division × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
N	8,506	8,506	6,128	6,128	3,967	3,967
Adjusted R^2	0.721	0.721	0.773	0.773	0.917	0.917

The table presents results from OLS regressions exploring the implications of FW-PPLs for firm-level poison pill decisions over the entire FW-period (1983 to 1993), as well as post-*Moran* (1986 to 1993), and post-*Interco* & *Pillsbury* (1989 to 1993) sample periods. The dependent variable $PPill$ is an indicator for whether a firm has a poison pill in-place as of the current year. $FW\ PPL$ is an indicator for whether a state has adopted a PPL at any point in time between 1986 and 1990. $Eventual\ SW\ PPL$ is a dummy variable equal to one if a firm is incorporated in a state that adopts a PPL during the period 1995 to 2009. The "Other antitakeover laws" include: BCL, CSL, DDL, and FPL. Division fixed effects are measured using U.S. Census divisions, and industry fixed effects are defined by three-digit SIC codes. Continuous variables are winsorized at the 5% level in both tails variables and are standardized to have zero mean and unit variance. Appendix Table A1 and Online Appendix Table OA21 provide variable definitions. *t*-statistics (clustered by state of incorporation) are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

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Table OA7
Summary statistics of the matched sample

Panel A: Pre-treatment year (*t*-1) summary statistics for the firm value measures

	(1)	(2)	(3)
	Treated	Control	Difference
$Ln(Q)_{[t]}$	0.423	0.487	-0.064
	(0.496)	(0.452)	(-1.54)
$Q_{[t]}$	1.744	1.814	-0.071
	(0.986)	(0.919)	(-0.85)
$Total\ Q_{[t]}$	1.001	1.006	-0.005
	(1.182)	(1.012)	(-0.05)
$Excess\ Return_{[t]}$	-0.069	-0.077	0.007
	(0.465)	(0.513)	(0.16)
$ROA_{[t]}$	0.114	0.118	-0.004
	(0.097)	(0.098)	(-0.49)
N (by group)	264	264	

Panel B: Summary statistics for the entire ($t \pm 3$) estimation window

	Mean	St. Dev.	P25	Median	P75	Obs.
$Ln(Q)_{[t]}$	0.497	0.484	0.095	0.417	0.840	2,086
$PPill_{[t]}$	0.349	0.477	0	0	1	2,086
$Q_{[t]}$	1.860	0.989	1.099	1.518	2.316	2,086
$Total\ Q_{[t]}$	1.057	1.121	0.255	0.701	1.405	2,061
$Excess\ Return_{[t]}$	-0.022	0.597	-0.412	-0.145	0.203	2,086
$ROA_{[t]}$	0.123	0.095	0.066	0.128	0.190	2,074

The table reports summary statistics for the variables used in the matched sample OLS regressions. Panel A (B) presents descriptive statistics for the pre-treatment year (entire $t \pm 3$ estimation window). The continuous variables are winsorized at the 5th and 95th percentiles. Appendix Table A1 provides variable definitions.

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Table OA8
Short-run event study

Panel A: Event-study results

	Introduction	Final passage	Signed by Governor
	(1)	(2)	(3)
[-30,-2]	-0.25%	1.66%*	1.15%
	(-0.28)	(1.68)	(1.25)
[-25,-2]	0.45%	1.73%**	1.12%
	(0.60)	(1.97)	(1.35)
[-20,-2]	-0.03%	1.55%**	1.06%
	(-0.05)	(2.01)	(1.42)
[-15,-2]	0.91%	1.00%	0.99%
	(1.61)	(1.44)	(1.53)
[0,0]	0.20%	-0.20%	0.22%
	(1.11)	(-1.14)	(1.08)
[0,1]	0.28%	0.11%	0.43%
	(1.02)	(0.41)	(1.55)
[0,4]	0.69%*	0.46%	0.57%
	(1.77)	(1.09)	(1.10)
[1,10]	0.08%	0.83%	0.36%
	(0.17)	(1.41)	(0.51)

Panel B: Event-study dates

State	Introduction	Final Passage	Signed by Governor
Connecticut	2/20/2003	5/29/2003	6/26/2003
Maine	1/30/2001	4/3/2002	4/8/2002
Maryland	1/28/1999	4/9/1999	5/13/1999
Michigan	2/13/2001	7/10/2001	7/23/2001
Minnesota	2/2/1995	4/26/1995	5/5/1995
Mississippi	1/5/2005	4/3/2005	4/20/2005
Missouri	1/14/1999	4/22/1999	7/13/1999
South Carolina	4/10/1997	6/4/1998	6/9/1998
Texas	2/20/2003	5/19/2003	5/19/2003
Vermont	3/21/2008	5/3/2008	6/6/2008
Washington	1/13/1998	3/9/1998	3/23/1998
Wyoming	1/5/2009	2/27/2009	3/3/2009

The table reports on a short-run event study. Panel A presents the cumulative abnormal returns (CARs) surrounding important events in the eventual ratification of a SW-PPL for firms incorporated in these states; these events include its introduction on the House or Senate floor, final passage by the House or Senate, and Governor's approval.[1] CARs are estimated over the event windows [0,0], [0,+1], [0,+4], and [+1,+10] and pre-event windows [-30,-2], [-25,-2], [-20,-2], [-15,-2]. CARs are estimated using the Fama-French four-factor model where the market factor is based on CRSP value-weighted returns, and the remaining three factors include: small-minus-big (SMB), high-minus-low (HML, and momentum (MOM). The parameters of the four-factor model are estimated over the window [-300,-101] relative to the respective event's date. The estimated *t*-statistics have been corrected for cross-sectional correlation (following Kolari and Pynnönen, 2010) and are shown in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively. Panel B provides the exact date of the three events used in this study for each of the SW-PPL adopting states.

[1] Ideally, we would have the exact date the market determines the bill is likely to be passed. Unfortunately, we were unable to find press releases for the announcement of SW-PPLs, and, therefore, use these key legislative events instead.

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Table OA9
Adjusting the sample for Delaware case law

Dependent variable: $Ln(Q)_{[t]}$	Full Sample (1992 to 2012) Delaware firms' $PPL = 1$, post-*Moran*		Full Sample (1992 to 2012) Delaware firms excluded		Matched Sample ($t \pm 3$) Delaware firms excluded (Equivalent to assuming they have a FW-PPL)	
	(1)	(2)	(3)	(4)	(5)	(6)
$PPL_{[t]}$	0.048**	0.046*	0.066*	0.071**		
	(2.58)	(1.94)	(1.93)	(2.24)		
$Treated \times Post_{[t]}$					0.066*	0.067*
					(1.87)	(1.92)
$Post_{[t]}$					-0.017	-0.017
					(-1.21)	(-1.17)
$PPL_{[t]} \times PPill_{[\tau(s)-1]}$		0.005		-0.013		
		(0.13)		(-0.23)		
$Post_{[t]} \times PPill_{[\tau(s)-1]}$						-0.039
						(-1.17)
Other antitakeover laws	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Division × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
N	25,465	25,465	11,251	11,251	1,902	1,902
Adjusted R^2	0.678	0.678	0.675	0.675	0.747	0.747

The table presents results from OLS regressions analyzing the value implications of PPLs adjusted for Delaware case law. The first (last) four (two) columns are specific to the full (matched) sample over the period 1983 to 2012 (a $t\pm3$ estimation window). The first two columns adjust the sample by re-coding PPL equal to one for firms incorporated in Delaware after the *Moran* court decision in 1985, while the last four columns exclude firms incorporated in Delaware entirely. For the matched sample, we re-do our matching procedure but exclude Delaware firms from the pool of possible controls; this is equivalent to assuming Delaware firms are treated during the first wave and hence are never in the pool of matching firms during the second wave. The dependent variable is the natural logarithm of Tobin's Q ($Ln(Q)$). The "Other antitakeover laws" include: BCL, CSL, DDL, and FPL. Division fixed effects are measured using U.S. Census divisions and industry fixed effects are defined by three-digit SIC codes. $Treated$ is absorbed by the firm fixed effects. Continuous variables are winsorized at the 5% level in both tails. Table A1 provides variable definitions. t-statistics (clustered by state of incorporation) are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

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Table OA10
PPV-Index

Panel A: Describing the $PPV\text{-}Index$

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

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Panel B: The effect of $PPV\text{-}Index$ on $Ln(Q)$

	(1)	(2)	(3)	(4)
$PPV\text{-}Index_{[t]}$	0.023**	0.021**	0.019**	0.019**
	(2.50)	(2.48)	(2.14)	(2.31)
$PPL_{[t]} \times PPill_{[\tau(s)-1]}$		0.013	0.013	0.001
		(0.38)	(0.40)	(0.01)
Other antitakeover laws	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Division × Year FE	Yes	Yes	No	No
Region × Year FE	No	No	Yes	No
State × Year FE	No	No	No	Yes
Industry × Year FE	Yes	Yes	Yes	Yes
N	25,465	25,465	25,465	25,465
Adjusted R^2	0.678	0.678	0.677	0.678

The table describes the poison pill validity index ($PPV\text{-}Index$) and reports results from OLS regressions analyzing its implications for firm value over the sample period 1992 to 2012. Panel A details the construction of the $PPV\text{-}Index$. We create this variable using poison pill statute and poison pill case information provided by Cain, McKeon, and Solomon (2017). Panel B explores the effect of $PPV\text{-}Index$ on $Ln(Q)$. The "Other antitakeover laws" include: BCL, CSL, DDL, and FPL. Division (region) fixed effects are measured using U.S. Census divisions (regions), state fixed effects are defined by a firm's state of location, and industry fixed effects are defined by three-digit SIC codes. Continuous variables are winsorized at the 5% level in both tails. Table A1 and Online Appendix Table OA21 provide variable definitions. t-statistics (clustered by state of incorporation) are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

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Table OA11
Alternative sample: Dropping the firm-level poison pill data requirement

Dependent variables:	$Ln(Q)_{[t]}$		$Q_{[t]}$	
	(1)	(2)	(3)	(4)
$PPL_{[t]}$	0.043***	0.033**	0.064**	0.067**
	(2.68)	(2.50)	(2.22)	(2.49)
Incorporation and HQ data	Current	Historical	Current	Historical
Other antitakeover laws	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Division × Year FE	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes
N	74,105	66,676	74,105	66,676
Adjusted R^2	0.601	0.606	0.582	0.590
Dependent variable: Mean	0.513	0.501	1.956	1.925
(standard deviation)	(0.520)	(0.513)	(1.209)	(1.183)

The table presents results from OLS regressions analyzing the value implications of PPLs over the period 1992 to 2012 on an alternative sample. The sample construction procedure is the same as before except in this table we do not exclude missing $PPill$ firm-year observations. The even(odd)-numbered columns' PPL, other antitakeover laws, division fixed effects, and standard error clustering uses current (historical) incorporation and headquarter (HQ) data. The dependent variables include: the natural logarithm of Tobin's Q ($Ln(Q)$) (columns 1-2); Tobin's Q (Q) (columns 3-4). The "Other antitakeover laws" include: BCL, CSL, DDL, and FPL. Division fixed effects are measured using U.S. Census divisions, and industry fixed effects are defined by three-digit SIC codes. Continuous variables are winsorized at the 5% level in both tails. Appendix Table A1 provides variable definitions. t-statistics (clustered by state of incorporation) are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

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Table OA12
Controlling for PPL-firm characteristics

Dependent variables:	$Ln(Q)_{[t]}$			$Q_{[t]}$	$Total\ Q_{[t]}$	$Excess\ Return_{[t]}$	$ROA_{[t]}$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$PPL_{[t]}$	0.067*	0.067*	0.061*	0.117**	0.124**	0.030*	0.016*
	(1.87)	(1.88)	(1.94)	(2.07)	(2.31)	(1.83)	(1.98)
$PPL_{[t]} \times PPill_{[\tau(s)-1]}$		-0.012	-0.009	-0.055	-0.062	-0.004	0.004
		(-0.27)	(-0.16)	(-0.54)	(-0.95)	(-0.08)	(0.53)
$PPL_{[t]} \times Ln(Assets)_{[\tau(s)-1]}$	0.038	0.042	0.112	0.163	0.130	0.062	0.004
	(0.77)	(0.77)	(1.31)	(1.04)	(0.88)	(0.59)	(0.26)
$PPL_{[t]} \times Ln(Assets)^2_{[\tau(s)-1]}$	-0.047	-0.049	-0.096	-0.172	-0.129	-0.081	-0.003
	(-0.80)	(-0.79)	(-1.40)	(-1.38)	(-1.32)	(-1.27)	(-0.29)
$PPL_{[t]} \times SG_{[\tau(s)-1]}$			0.005	0.019	0.029	-0.022	0.001
			(0.47)	(0.86)	(0.98)	(-1.66)	(0.32)
$PPL_{[t]} \times Loss_{[\tau(s)-1]}$			0.056	0.139	0.127	0.084	0.018
			(1.27)	(1.39)	(1.23)	(1.53)	(1.11)
$PPL_{[t]} \times DEQ_{[\tau(s)-1]}$			0.000	-0.002	-0.003	0.007	0.002
			(0.01)	(-0.17)	(-0.46)	(0.34)	(1.32)
$PPL_{[t]} \times FLIQ_{[\tau(s)-1]}$			-0.014	-0.018	-0.030	0.029	-0.005
			(-0.78)	(-0.56)	(-0.79)	(1.55)	(-1.52)
$PPL_{[t]} \times CAPX/Assets_{[\tau(s)-1]}$			-0.003	-0.004	-0.005	-0.009	-0.004
			(-0.24)	(-0.72)	(-0.80)	(-0.44)	(-1.10)
$PPL_{[t]} \times IO_{[\tau(s)-1]}$			0.001	0.016	-0.022	-0.012	-0.001
			(0.04)	(0.40)	(-0.52)	(-0.75)	(-0.40)
$Ln(Age)_{[t]}$	-0.096***	-0.096***	-0.097***	-0.233***	-0.245***	-0.039*	-0.002
	(-6.58)	(-6.57)	(-6.77)	(-8.14)	(-9.95)	(-1.87)	(-0.38)
Other antitakeover laws	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Division × Year FE	Yes	Yes	No	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	25,465	25,465	25,465	25,465	25,409	25,463	25,418
Adjusted R^2	0.680	0.680	0.680	0.656	0.647	0.060	0.636

The table presents results from OLS regressions analyzing the value implications of PPLs over the period 1992 to 2012, controlling for firm characteristics. The dependent variables include: the natural logarithm of Tobin's Q ($Ln(Q)$); Tobin's Q (Q); Total Tobin's Q ($Total\ Q$); risk-adjusted, excess stock returns ($Excess\ Return$); return on assets (ROA). We control for PPL-firm characteristics by interacting the level of the respective PPL-firm characteristic in the year before the adoption of the respective PPL ($\tau(s)-1$) with PPL. The firm characteristics include: $Ln(Assets)$, sales growth (SG), a dummy indicating if a firm has negative net income ($Loss$), debt-to-equity ratios (DEQ), firm liquidity ($FLIQ$), $CAPX/Assets$, and institutional ownership (IO); we do not make this adjustment for $Ln(Age)$ because it's deterministic. The "Other antitakeover laws" include: BCL, CSL, DDL, and FPL. Division fixed effects are measured using U.S. Census divisions, and industry fixed effects are defined by three-digit SIC codes. The interacting variables are time-invariant and absorbed by the firm fixed effects. Continuous variables are winsorized at the 5% level in both tails and the continuous controls are standardized to have a mean of zero and a standard deviation of one. Appendix Table A1 and Online Appendix Table OA21 provide variable definitions. t-statistics (clustered by state of incorporation) are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

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Table OA13
The timing of changes in visible policy and firm value around PPL adoptions

Panel A: Changes in visible pill policy

Dependent variables:	$PPill_{[t]}$	$New\ PPill_{[t]}$	$Ln(PPill\ Duration)_{[t]}$
	(1)	(2)	(3)
$PPL_{[t]}^{[-2]}$	-0.002	0.002	-0.018
	(-0.11)	(0.03)	(-0.18)
$PPL_{[t]}^{[-1]}$	-0.018	0.009	-0.009
	(-0.46)	(0.08)	(-0.12)
$PPL_{[t]}^{[0]}$	0.036	0.116*	0.011
	(1.42)	(2.00)	(0.09)
$PPL_{[t]}^{[+1]}$	0.030	0.078	0.112
	(1.42)	(1.11)	(1.05)
$PPL_{[t]}^{[2+]}$	0.074**	0.092	0.242*
	(2.11)	(1.45)	(1.70)
Other antitakeover laws	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Division × Year FE	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes
N	25,465	5,476	17,052
Adjusted R^2	0.592	0.144	0.524

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Panel B: Changes in firm value					
Dependent variables:	$Ln(Q)_{[t]}$	$Q_{[t]}$	$Total\ Q_{[t]}$	$Excess\ Return_{[t]}$	$ROA_{[t]}$
	(1)	(2)	(3)	(4)	(5)
$PPL_{[t]}^{[-2]}$	-0.014	-0.048	-0.063	0.025	0.005
	(-0.45)	(-0.80)	(-0.80)	(1.51)	(0.95)
$PPL_{[t]}^{[-1]}$	-0.002	0.009	-0.027	0.012	-0.000
	(-0.11)	(0.21)	(-0.35)	(0.56)	(-0.02)
$PPL_{[t]}^{[0]}$	0.030	0.094	0.068	0.016	0.004
	(1.21)	(1.48)	(0.97)	(0.47)	(0.76)
$PPL_{[t]}^{[+1]}$	0.074**	0.166**	0.153*	0.046**	0.007*
	(2.29)	(2.04)	(1.82)	(2.14)	(1.77)
$PPL_{[t]}^{[2+]}$	0.069**	0.149***	0.133**	0.033	0.010**
	(2.39)	(2.78)	(2.11)	(1.59)	(2.18)
Other antitakeover laws	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Division × Year FE	Yes	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes	Yes
N	25,465	25,465	25,409	25,463	25,418
Adjusted R^2	0.679	0.645	0.643	0.058	0.635

The table presents results from OLS regressions analyzing the visible pill policy and value implications of PPLs over the period 1992 to 2012. Panel A (B) dependent variables include: an indicator of whether a firm has a poison pill in the current year ($PPill$), an indicator of whether a firm adopts a brand new poison pill ($New\ Pill$); the natural logarithm of one plus the number of years a firm has a poison pill $Ln(PPill\ Dutration)$ (the natural logarithm of Tobin's Q ($Ln(Q)$); Tobin's Q (Q); Total Tobin's Q ($Total\ Q$); risk-adjusted, excess stock returns ($Excess\ Return$); return on assets (ROA)). $PPL^{[-2]}$ ($PPL^{[-1]}$) is an indicator variable equal to one if a firm is incorporated in a state that will adopt a PPL in two (one) years and zero otherwise. $PPL^{[0]}$ is an indicator variable equal to one if a firm is incorporated in a state that adopts a PPL in the current year and zero otherwise. $PPL^{[+1]}$ ($PPL^{[2+]}$) is an indicator variable equal to one if a firm is incorporated in a state that has adopted a PPL one (two or more) year ago and zero otherwise. The "Other antitakeover laws" include: BCL, CSL, DDL, and FPL. Division fixed effects are measured using U.S. Census divisions, and industry fixed effects are defined by three-digit SIC codes. Continuous variables are winsorized at the 5% level in both tails. Appendix Table A1 provides variable definitions. t-statistics (clustered by state of incorporation) are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

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Table OA14
State-by-year fixed effects

Panel A: Full sample over the period 1992 to 2012				
Dependent variable: $Ln(Q)_{[t]}$	(1)	(2)	(3)	(4)
$PPL_{[t]}$	0.053**	0.058**		
	(2.31)	(2.01)		
$PPL_{[t]} \times Same\ Inc\text{-}HQ\ State_{[t]}$			0.067**	0.074**
			(2.60)	(2.34)
$PPL_{[t]} \times Diff.Inc\text{-}HQ\ State_{[t]}$			0.025	0.030
			(0.79)	(0.85)
$PPL_{[t]} \times PPill_{[\tau(s)-1]}$		-0.015		-0.019
		(-0.40)		(-0.52)
Other antitakeover laws	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes
N	25,465	25,465	25,465	25,465
Adjusted R^2	0.678	0.678	0.678	0.678

Panel B: Matched sample over a $t \pm 3$ estimation window			
Dependent variable: $Ln(Q)_{[t]}$	(1)	(2)	(3)
$Treated \times Post_{[t]}$	0.065*	0.058*	0.057*
	(1.88)	(1.75)	(1.72)
$Post_{[t]}$	0.053	0.050	0.057
	(0.81)	(0.78)	(0.88)
$Post_{[t]} \times PPill_{[\tau(s)-1]}$			-0.055
			(-0.82)
Other antitakeover laws	No	Yes	Yes
Firm FE	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes
N	1,461	1,461	1,461
Adjusted R^2	0.782	0.783	0.783

The table reports the results for OLS regressions with state-by-year fixed effects. Panel A provides full sample regression estimates of $Ln(Q)$ on PPL indicator variables and their interactions with $Same$ $(Diff.)$ $Inc\text{-}HQ\ State$ indicator variables over the period 1992-2012. $Same$ $(Diff.)$ $Inc\text{-}HQ\ State$ equals one if a firm's state of incorporation is the same (different) as (than) its state of location, and zero otherwise. Panel B shows the matched sample DID results of $Ln(Q)$ on a $Treated \times Post$ indicator variable. We use propensity score matching with replacement in year t-1 to create a sample matched on $Ln(Q)$ and $Total\ Assets$, and exactly on $PPill$ and state of location, and when possible, exactly on two-digit SIC industries – when it is not possible to match exactly on SIC2, we match on SIC1. $Treated$ (control) firms are defined as firms incorporated in states that (never) adopt PPLs. $Treated$ is absorbed by the firm fixed effects. The "Other antitakeover laws" include: BCL, CSL, DDL, and FPL. State fixed effects are defined using a firm's state of location and industry fixed effects are measured using three-digit SIC codes. Continuous variables are winsorized at the 5% level in both tails. Appendix Table A1 and Online Appendix Table OA21 provide variable definitions. t-statistics (clustered by state of incorporation) are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

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Table OA15
Matched sample placebo test

Panel A: Pre-pseudo treatment year (t-1) summary statistics

	(1)	(2)	(3)
	Treated	Control	Difference
$Ln(Q)_{[t]}$	0.474	0.497	-0.024
	(0.425)	(0.396)	(-0.55)
$Total\ Assets_{[t]}$	1388.67	1585.77	-197.10
	(4399.64)	(3839.24)	(-0.81)
$PPill_{[t]}$	0.341	0.341	0.000
	(0.475)	(0.475)	(0.00)
$SIC2_{[t]}$	42.99	41.10	1.889
	(19.12)	(19.16)	(1.11)
$Division_{[t]}$	4.982	5.307	-0.324
	(2.264)	(2.502)	(-1.44)
N (by group)	252	252	

Panel B: The value implications of pseudo treatment

Dependent variables:	$Ln(Q)_{[t]}$	$Q_{[t]}$	$Total\ Q_{[t]}$	$Excess\ Return_{[t]}$	$ROA_{[t]}$
	(1)	(2)	(3)	(4)	(5)
$Pseudo\ Treated \times Post_{[t]}$	-0.012	0.004	0.025	0.019	-0.000
	(0.32)	(0.05)	(0.23)	(0.42)	(-0.03)
$Post_{[t]}$	0.013	0.025	0.015	-0.021	0.005
	(0.27)	(0.24)	(0.17)	(-0.56)	(0.48)
$Post_{[t]} \times PPill_{[\tau(s)-1]}$	-0.022	-0.037	-0.018	-0.011	-0.001
	(-0.53)	(-0.87)	(-0.41)	(-0.61)	(-0.34)
Other antitakeover laws	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Division × Year FE	Yes	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes	Yes
N	1,493	1,493	1,490	1,493	1,490
Adjusted R^2	0.714	0.664	0.615	0.113	0.611

The table reports results from a matched sample placebo test. We use propensity score matching with replacement in year t-1 to create a sample matched on $Ln(Q)$ and $Total\ Assets$, and exactly on $PPill$, and when possible, exactly on divisions, and two-digit SIC industries – when it is not possible to match exactly on division (SIC2), we match on the next closest division (SIC1). However, in this matching procedure, we purposefully move back actual adoption dates by four-years. Thus, when we consider a $(t \pm 3)$ estimation window treatment never actually occurs. $Treated$ (control) firms are defined as companies incorporated in states that (do not) adopt PPLs (in at least the three years following its matched counterpart's pseudo-adoption year). Panel A shows pre-pseudo treatment year summary statistics for the matching variables. We also report differences between sample means (t-statistics in parentheses). Panel B examines the effect of $Pseudo\ Treated \times Post$ on alternative value measures ($Ln(Q)$, Q, $Total\ Q$, $Excess\ Return$, and ROA). The "Other antitakeover laws" include: BCL, CSL, DDL, and FPL. Division fixed effects are measured using U.S. Census divisions and industry fixed effects are defined by three-digit SIC codes. $Treated$ is absorbed by the firm fixed effects. Continuous variables are winsorized at the 5% level in both tails. Appendix Table A1 provides variable definitions. t-statistics (clustered by state of incorporation) are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

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Table OA16
Adjusting the sample period for Delaware case law

Dependent variables:	$Ln(Q)_{[t]}$		$Q_{[t]}$		$Total\ Q_{[t]}$		$Excess\ Return_{[t]}$		$ROA_{[t]}$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$PPL_{[t]}$	0.042**		0.104***		0.095**		0.027**		0.006*	
	(2.65)		(3.26)		(2.12)		(2.02)		(1.75)	
$Treated \times Post_{[t]}$		0.061**		0.119**		0.120***		0.030**		0.010*
		(2.84)		(2.63)		(3.04)		(2.50)		(1.74)
$Post_{[t]}$		-0.019		0.017		0.024		0.020		-0.001
		(-0.64)		(0.19)		(0.20)		(0.57)		(-0.16)
$PPL_{[t]} \times PPill_{[\tau(s)-1]}$	-0.012		-0.056		-0.037		0.011		-0.002	
	(-0.48)		(-0.82)		(-0.76)		(0.40)		(-0.58)	
$Post_{[t]} \times PPill_{[\tau(s)-1]}$		-0.050		-0.106*		0.012		0.054		-0.002
		(-0.97)		(-1.86)		(0.17)		(0.77)		(-0.37)
Other antitakeover laws	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Division × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	27,472	2,681	27,472	2,681	27,438	2,673	27,470	2,681	27,447	2,656
Adjusted R^2	0.664	0.792	0.643	0.747	0.645	0.761	0.063	0.049	0.614	0.764

The table reports results from OLS regressions analyzing the effect of PPLs on firm value using alternative sample periods that adjust for important Delaware case law events. The dependent variables include: $Ln(Q)$, Q, $Total\ Q$, $Excess\ Return$, and ROA. We adjust the SW to begin in 1989 such that the sample spans the period 1989 to 2012. The odd (even)-numbered columns are specific to the full (matched) sample over the period 1989 to 2012 (a $t \pm 3$ estimation window). The matched sample includes $Treated$ firms (and their corresponding matched control firms) incorporated in states that adopt PPLs at any time between 1989 and 2009. $Treated$ is absorbed by the firm fixed effects. The "Other antitakeover laws" include: BCL, CSL, DDL, and FPL. Division fixed effects are measured using U.S. Census divisions and industry fixed effects are defined by three-digit SIC codes. Continuous variables are winsorized at the 5% level in both tails. Appendix Table A1 provides variable definitions. t-statistics (clustered by state of incorporation) are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

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Table OA17
Subperiods within the second wave

Dependent variable: $Ln(Q)_{[t]}$	1992-1998	1999-2005	2006-2012
	(1)	(2)	(3)
$PPL_{[t]}$	0.055**	0.094***	0.034
	(2.40)	(4.48)	(0.42)
$PPL_{[t]} \times PPill_{[\tau(s)-1]}$	-0.035	0.039	N/A
	(-1.07)	(1.28)	
Other antitakeover laws	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Division × Year FE	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes
N	8,233	8,226	7,143
Adjusted R^2	0.743	0.767	0.799

The table presents results from OLS regressions analyzing the value implications of PPLs over three separate subperiods during the second wave, 1992 to 2012. The dependent variable is the natural logarithm of Tobin's Q ($Ln(Q)$). The "Other antitakeover laws" include: BCL, CSL, DDL, and FPL. Division fixed effects are measured using U.S. Census divisions, and industry fixed effects are defined by three-digit SIC codes. Continuous variables are winsorized at the 5% level in both tails. Appendix Table A1 provides variable definitions. t-statistics (clustered by state of incorporation) are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

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Table OA18
First and second wave PPL sample periods combined

Dependent variable: $Ln(Q)_{[t]}$	Full Sample (1983 to 2012)		Matched Sample ($t \pm 3$)	
	(1)	(2)	(3)	(4)
$PPL_{[t]}$	0.023*	0.025*		
	(1.70)	(1.72)		
$Treated \times Post_{[t]}$			0.016*	0.022*
			(1.89)	(1.92)
$Post_{[t]}$			-0.009	-0.009
			(-0.81)	(-0.74)
$PPL_{[t]} \times PPill_{[\tau(s)-1]}$		0.004		
		(0.12)		
$Post_{[t]} \times PPill_{[\tau(s)-1]}$				-0.018
				(-0.62)
Other antitakeover laws	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Division × Year FE	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes
N	32,220	32,220	5,440	5,440
Adjusted R^2	0.674	0.675	0.831	0.831

The table presents results from OLS regressions analyzing the value implications of PPLs over the combined FW and SW periods. The first (last) two columns are specific to the full (matched) sample over the period 1983 to 2012 (a $t\pm3$ estimation window). The dependent variable is the natural logarithm of Tobin's Q ($Ln(Q)$). The "Other antitakeover laws" include: BCL, CSL, DDL, and FPL. Division fixed effects are measured using U.S. Census divisions and industry fixed effects are defined by three-digit SIC codes. $Treated$ is absorbed by the firm fixed effects. Continuous variables are winsorized at the 5% level in both tails. Appendix Table A1 provides variable definitions. t-statistics (clustered by state of incorporation) are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

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Table OA19
Excluding multi-law adopting states

Dependent variable: $Ln(Q)_{[t]}$	Full Sample (1992 to 2012)		Matched Sample ($t \pm 3$)	
	(1)	(2)	(3)	(4)
$PPL_{[t]}$	0.057***	0.056**		
	(3.05)	(2.31)		
$Treated \times Post_{[t]}$			0.043*	0.061**
			(2.01)	(2.80)
$Post_{[t]}$			-0.018	-0.017
			(-0.60)	(-0.57)
$PPL_{[t]} \times PPill_{[\tau(s)-1]}$		0.005		
		(0.12)		
$Post_{[t]} \times PPill_{[\tau(s)-1]}$				-0.052
				(-1.00)
Other antitakeover laws	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Division × Year FE	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes
N	23,284	23,284	2,086	2,086
Adjusted R^2	0.676	0.676	0.792	0.792

The table presents results from OLS regressions analyzing the value implications of PPLs excluding firms incorporated in states that adopt a BCL, CSL, and/or FPL in the same year as its PPL. The first (last) two columns are specific to the full (matched) sample over the period 1992 to 2012 (a $t\pm3$ estimation window). The dependent variable is the natural logarithm of Tobin's Q ($Ln(Q)$). The "Other antitakeover laws" include: BCL, CSL, DDL, and FPL. Division fixed effects are measured using U.S. Census divisions and industry fixed effects are defined by three-digit SIC codes. $Treated$ is absorbed by the firm fixed effects. Continuous variables are winsorized at the 5% level in both tails. Table A1 provides variable definitions. t-statistics (clustered by state of incorporation) are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

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Table OA20
Staggered boards

Dependent variable: $Ln(Q)_{[t]}$	(1)	(2)	(3)	(4)
$PPL_{[t]}$	0.037***	0.032**	0.038**	0.033*
	(2.85)	(2.23)	(2.31)	(1.73)
$Staggered\ Board_{[t-1]}$	0.022**	0.019*		
	(2.05)	(1.83)		
$PPL_{[t]} \times Staggered\ Board_{[t-1]}$		0.004		
		(0.12)		
$PPL_{[t]} \times Staggered\ Board_{[\tau(s)-1]}$			0.013	0.012
			(0.69)	(0.68)
$PPL_{[t]} \times PPill_{[\tau(s)-1]}$				-0.013
				(-0.41)
Other antitakeover laws	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Division × Year FE	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes
N	18,103	18,103	18,103	18,103
Adjusted R^2	0.716	0.716	0.713	0.713

The table presents results from OLS regressions analyzing the value implications of PPLs over the period 1992 to 2012. The dependent variable is the natural logarithm of Tobin's Q ($Ln(Q)$). $Staggered\ Board_{[t-1]}$ is an indicator variable set to one if a firm has a staggered board, and zero otherwise. $PPL_{[t]} \times Staggered\ Board_{[\tau(s)-1]}$ interacts an indicator variable set to one if a PPL-firm has a staggered board in the year before the adoption of its respective PPL ($\tau(s)-1$) with PPL. The "Other antitakeover laws" include: BCL, CSL, DDL, and FPL. Division fixed effects are measured using U.S. Census divisions, and industry fixed effects are defined by three-digit SIC codes. Continuous variables are winsorized at the 5% level in both tails. Appendix Table A1 and Online Appendix Table OA21 provide variable definitions. t-statistics (clustered by state of incorporation) are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

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Table OA21
Variable definitions

Age	The number of firm-year observations since the firm's first appearance in Compustat. We take the natural logarithm of one plus Age: $Ln(Age)$.
$Assets$	The value of total book assets in millions, where assets are adjusted using 2015 dollars. We take the natural logarithm of this variable: $Ln(Assets)$. Data comes from Compustat.
$CAPX/Assets$	Capital expenditures divided by total assets. Data comes from Compustat.
DEQ	Debt-to-equity, defined as long-term debt divided by book equity. Data comes from Compustat.
$Diff.(Same)\ Inc\text{-}HQ\ State$	An indicator variable set equal to one if a firm's state of incorporation is different than (the same as) its state of location, and zero otherwise.
$Eventual\ SW\ PPL$	An indicator variable equal to one if a firm is incorporated in a state that will eventually pass a PPL during the second wave – SW period 1995 to 2009, and zero otherwise. We use adoption dates provided by Cain, McKeon, and Solomon (2017) and Karpoff and Wittry (2018).
$FLIQ$	Current assets minus current liabilities divided by total assets. Data comes from Compustat.
$FW\ PPL$	An indicator variable equal to one if a firm is incorporated in a state that passes a PPL during the first wave (FW) period 1986 to 1990, and zero otherwise. We use adoption dates provided by Cain, McKeon, and Solomon (2017) and Karpoff and Wittry (2018).
IO	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.
$Loss$	An indicator variable set to one if a firm has negative net income during a fiscal year, and zero otherwise. Data comes from Compustat.
$PPV\text{-}Index$	We create a poison pill validity index ($PPV\text{-}Index$) using poison pill statute and poison pill case information provided by Cain, McKeon, and Solomon (2017). The $PPV\text{-}Index$ captures the relative change or strength of poison pill validity over time and by state of incorporation. For a detailed description of the $PPV\text{-}Index$, see Online Appendix Table OA10.
SG	Sales growth, defined as the natural logarithm of the value of sales in in year *t* divided by the value of sales in year *t*-1. Data comes from Compustat.
$Staggered\ Board$	An indicator variable equal to one if a firm has a staggered board. We use data from Cremers, Litov and Sepe (2017).

The table provides definitions and data source, where applicable, for variables used exclusively in the online appendix.

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