Shadow Pills, Actual Pill Policy, and Firm Value

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Abstract

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

JEL classifications: G32, G34, K22, O32
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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; Bebchuk, Coates, and Subramanian 2002; Cremers and Ferrell 2014). While details vary across different implementations, the basic defensive mechanism of the pill provides existing shareholders with stock purchase rights that entitle them to acquire newly issued shares at a substantial discount in the "trigger" event that a hostile bidder obtains more than a pre-specified percentage of the company's outstanding shares, 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 (Bhagat and Jefferis 1993; 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 effect that arises from the *right to adopt* a poison pill (which right constitutes the shadow pill) rather than its realized adoption. To this end, we consider the implications of state-level poison pill laws (PPLs) on a firm's actual pill policy and financial value, consistent with a large body of studies

[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 (Bebchuk, Coates, and Subramanian 2002; Field and Karpoff 2002; Karpoff, Schonlau, and Wehrly 2017), and Tobin's Q (Gompers, Ishii, and Metrick 2003; Chi 2005). Other studies, instead, find a *positive* association between the adoption of a poison pill and, respectively, stock returns (Caton and Goh 2008) – in particular, for firms with outsider-dominated boards (Brickley, Coles and Terry 1994) – 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; Bhagat and Jefferis 1993; Carney and Silverstein 2003).

that exploit variation from state antitakeover laws as quasi-natural experiments (e.g., Karpoff and Malatesta 1989; Bertrand and Mullainathan 1999, 2003; Garvey and Hanka 1999; Qiu and Yu 2009; Francis et al. 2010; Giroud and Mueller 2010; Atanassov 2013; Gormley and Matsa 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 adopt new pills and maintain existing pills. Second, we find that the Tobin's Q of the companies incorporated in states with PPLs increases significantly relative to firms incorporated elsewhere. Further, we also confirm the findings of the previous literature that visible pills are negatively associated with Tobin's Q, which underlines the importance of our study's endeavor at disentangling the value implications of the ex-ante right to adopt a poison pill (i.e., shadow pill) relative to the ex-post endogenous decision to put an actual pill in-place.

Two economic mechanisms can explain these findings. First, we show that the strengthened validity granted to poison pills through a PPL helps boards of directors to bargain for a higher purchasing price after being targeted in a takeover contest (though we find no evidence that PPLs predict takeover activity itself). Second, we find that the increases in Tobin's Q for firms incorporated in a PPL-adopting state are more pronounced for more innovative firms or firms where firm-specific stakeholder investments are more relevant (e.g., with a large customer or in a strategic alliance). Overall, these findings support the view that the shadow pill promotes value enhancement for some firms by improving the negotiating position of a target's board (the "bargaining power hypothesis") and by reducing a firm's contracting costs with its stakeholders (the "bonding hypothesis").

Because our study relies on PPLs to identify how a strengthened shadow pill affects actual pill policy and firm value, we follow the recommendation of recent work (e.g., Catan and Kahan 2016; Cain, McKeon, and Solomon 2017; Karpoff and Wittry 2018) and account for the legal environment into which these laws were passed. In particular, due to the pervasive influence of Delaware case law over other jurisdictions (e.g., Ryngaert 1988; Cremers and Ferrell 2014; Dammann 2019), the validity of the pill was arguably fairly certain from 1985 (when the Delaware Supreme Court validated the poison pill in *Moran* v. *Household International, Inc.*) until at least 1988, when two Delaware decisions (*City Capital Associates* v. *Interco Inc.* and *Grand Metropolitan PLC* v. *Pillsbury Co.*) injected novel uncertainty by restricting a board's ability to maintain a pill indefinitely. Adding to the confusion, a

subsequent decision in *Paramount Communications, Inc.* v. *Time, Inc.* in 1989 overturned the rulings in *Interco* and *Pillsbury*.

Therefore, during the period 1985 to 1988 covering most of the PPLs considered in prior studies, firms incorporated in both Delaware and elsewhere had ready access to the pill (and thus 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 introducing PPLs in this period. The topsy-turvy chain of judicial events from 1985 to 1989 could also plausibly explain why most states (27-out-of-35) decided to adopt PPLs post-*Interco* and *Pillsbury*, as a way to provide legal certainty.

Considering this legal context, our study focuses on PPLs adopted during the period 1995 to 2009 – which we term the "second wave" (SW) of adoptions. Beginning the SW 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 changed underlying legal context, a priori it is reasonable to expect that results for this later set of laws might differ from results obtained by prior studies using the earlier set of PPLs passed between 1986 and 1990 (i.e., the "first wave" (FW) of adoptions).

We first show that our results for FW-PPLs are in line with prior work. In particular, in a recent paper, Karpoff and Wittry (2018) document evidence that prior corporate governance studies using business combination laws (BCLs) as a source of identifying variation for individual firm's takeover protection are potentially plagued by an omitted variable problem if they do not account for pertinent institutional and legal context – including PPLs. Most relevant for our study, they show that PPLs adopted during their sample period (i.e., 1976-1995) appear to negatively associate with return on assets (ROA), although this negative association becomes insignificant in their subsequent tests that control for firm-level defenses, such as poison pills (similar to prior work, e.g., Karpoff and Malatesta 1989).[2] Consistent with their results, we document that firms incorporated in states adopting FW-PPLs did not experience significant changes in their levels of Tobin's Q, excess stock returns, or ROA.[3] Consistent with our

[2] Karpoff and Malatesta (1989) analyze the effect of several state antitakeover statutes on stock prices between 1982 and 1987 and find negative abnormal returns surrounding press announcements of antitakeover legislation, especially in response to PPLs. In supplemental tests, however, they show their results are contingent on firms not having company-level protections (e.g., poison pills) in-place.

[3] See Internet Appendix Table A1 and Table 1.

conjecture, this result suggests that FW-PPLs did not materially impact firms' shadow pills, due to the arguably undisputed legal right to a pill during almost all of this period as explained above.

We then investigate the likelihood of the passage of SW-PPLs conditional on 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 incorporation state and year fixed effects. We find no significant predictors for the adoption of SW-PPLs, suggesting that the adoption of these laws is largely exogenous to the legal and economic environment in which they were introduced.

Our principal findings on the effect of SW-PPLs on actual pill policy and long-term value are estimated using difference-in-differences regressions that include firm, U.S. Census division-by-year,[4] and industry-by-year fixed effects. First, we show that these PPLs significantly increased the propensity of firms incorporated in the enacting states to adopt and maintain existing (e.g., renew expiring) pills relative to Delaware and non-Delaware firms without such legislation. Second, we document that a strengthened shadow pill, as enabled by the passage of PPLs results in an economically and statistically significant increase in the affected firms' Tobin's Q of 5.1%, relative to the sample average.

We obtain results similar to using division-by-year fixed effects by using instead region-by-year (i.e., Northeast, Midwest, South, and West) or headquarter-state-by-year fixed effects, which control for unobserved, time-varying differences across headquarter regions or headquarter states, respectively. However, we primarily focus on the division-by-year fixed effects results because Census divisions capture a more granular geographic locale than regions, and the majority of affected firms and non-Delaware unaffected firms incorporate and locate their headquarter in the same state, such that results using headquarter-state-by-year fixed effects depend on a limited set of firms for identification.

Our findings are further robust to the incorporation of possible selection effects through the creation of a matched sample, where the "treated" firms incorporated in SW-PPL-adopting states are matched 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

[4] The U.S. Census Bureau classifies census divisions by grouping states and the District of Columbia into the following nine geographical subdivisions: New England, Middle Atlantic, East North Central, West North Central, South Atlantic, East South Central, West South Central, Mountain, and Pacific. We interact a firm's headquarter division with year dummies to control for unobserved, time-varying sources of local heterogeneity that could correlate with PPLs, pill adoptions and firm value.

in the matched sample – as well 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.

We find analogous results of increased value after PPLs are adopted using other proxies for firm value than Tobin's Q, such as excess stock returns, profitability, and Total Q (Peters and Taylor 2017). As a final robustness test, we conduct a long-term event study surrounding the adoption of PPLs, which employs 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 0.9% per month in the period surrounding the adoption of the SW-PPLs.

Finally, we examine two possible economic channels through which the shadow pill could contribute to firm value, namely the "bargaining power hypothesis" (e.g., Stulz 1988; Berkovitch and Khanna 1990; Harris 1990; Kadyrzhanova and Rhodes-Kropf 2011) and the "bonding hypothesis" (e.g., Knoeber 1986; Laffont and Tirole 1988; Shleifer and Summers 1988; Johnson, Karpoff, and Yi 2015) of takeover defenses. 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, thereby allowing directors to obtain a higher offer price for the target's shareholders. Alternatively, under the bonding hypothesis, limiting the ability of shareholders to disrupt a firm's long-term strategy – such as through strengthening the shadow pill – serves as a commitment device that binds the shareholders to long-term strategies. Such bonding can decrease a firm's cost of contracting with its stakeholders and, thereby, improve long-term firm value.

Consistent with the bargaining power hypothesis, firms with a pill in-place receive a higher takeover premium after their state adopts a PPL, without evidence of any change in the likelihood of becoming a target. This finding helps to explain why the increase in the use of the actual pill we document for some PPL-affected firms can be valuable. Additionally, 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.

Overall, our study contributes to the literature on the poison pill and, more generally, takeover defenses, in the following four ways. First, we contribute to prior scholarship by analyzing the adoption of both FW- and SW-PPLs (i.e., considering all 35 enacting states), extending prior work that only employs FW-PPLs (e.g., Karpoff and Malatesta 1989; Karpoff

and Wittry 2018).[5] Second, we confirm the insignificant results obtained by earlier studies on the association between FW-PPLs and proxies of firm value but show that SW-PPLs are positively related to these same proxies. We explain the difference through the changed legal context pertaining to the validity of the (*right to adopt* a) poison pill from the FW-period to the SW-period. Third, we assemble a comprehensive panel dataset on firm-level poison pills. This enables us to test the impact of PPLs on actual pill policy, as well as to confirm the findings of prior literature on other determinants of pill adoption. Fourth, we contribute to the literature examining the relationship between takeover defenses and firm value,[6] finding support for both the bargaining power hypothesis (e.g., Stulz 1988; Berkovitch and Khanna 1990; Harris 1990; Comment and Schwert 1995; Heron and Lie 2006, 2015; Kadyrzhanova and Rhodes-Kropf 2011) and bonding hypothesis (e.g., Knoeber 1986; Laffont and Tirole 1988; Shleifer and Summers 1988; Cen, Dasgupta, and Sen 2015; Johnson, Karpoff, and Yi 2015, 2018; Bhojraj, Senguota, and Zhang 2017;[7] Cremers, Litov, and Sepe 2017; Chemmanur and Tian 2018) of takeover defenses.

1. Data and Empirical Framework

1.1. Sample selection, definition of variables and summary 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

[5] In related work, Cain, McKeon, and Solomon (2017) study 16 different state antitakeover statutes and court rulings over the period 1965-2014 and find that PPLs do not impact hostile takeover activity, but do not consider the specific impact of these laws on overall firm value. Neither do other recent working papers considering PPLs, such as, for example, John and Kadyrzhanova (2017). They find that a classified board's power to deter a takeover during periods of heightened merger activity is strengthened by a PPL. Fich, Harford and Yore (2018) show that their main result linking BCLs to the marginal value of cash is robust to using PPLs.

[6] For example, see DeAngelo and Rice (1983); Linn and McConnell (1983); Demsetz and Lehn (1985); Malatesta and Walkling (1988); Morck, Shleifer, and Vishny (1988); Karpoff and Malatesta (1989); Brickley, Coles and Terry (1994); Lang and Stultz (1994); Yermack (1996); Cotter, Shivdasani, and Zenner (1997); Himmelberg, Hubbard, and Palia (1999); Daines (2001); Palia (2001); Bertrand and Mullainathan (2003); Gompers, Ishii, and Metrick (2003); Bebchuk, Cohen, and Ferrell (2009); Francis et al. (2010); Giroud and Mueller (2010, 2011); Cuñat, Gine, and Guadalupe (2012); Cremers and Ferrell (2014); Straska and Waller (2014).

[7] Bhojraj, Sengupta, and Zhang (BSZ) (2017) use two 1995 Delaware court decisions to examine how antitakeover provisions relate to *innovative* firms' value. Our study materially differs in the following three ways. First, we isolate the value relevancy of the shadow pill for the *average* firm. Second, we use the PPL setting, where the staggered adoption of state laws is generally accepted by the literature as providing identification (Catan and Kahan 2016; Karpoff and Wittry 2018). The setting in the BSZ study has been criticized by, e.g., Heron and Lie (2015), who show that these court rulings are unrelated to poison pills and takeover premiums and that their scope extends beyond Delaware, rendering difference-in-differences tests invalid. Third, we employ regression models with firm, industry-by-year, and headquarter-geography-by-year fixed effects (Gormley and Matsa 2016), while the BSZ study only includes industry fixed effects. When we consider the 1995 Delaware court decisions in our sample with appropriately specified fixed effects models, we do not find the rulings mattered for firm value.

databases. We supplement these observations with poison pill data from Comment and Schwert (1995), Catan 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.

The resulting sample contains firm-level poison pill information on 3,423 unique firms between 1983 and 2012, which we merge with the industrial firms (excluding utilities and financials) in the merged 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 model. This selection criterion results in a panel with 33,826 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 Barzuza (2009), Cain, McKeon and Solomon (2017) and Karpoff and Wittry (2018), and report each state's adoption month/year date in Internet Appendix Table A2. We use historical incorporation 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.[8] 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. PPL always equals zero for firms in states that never passed a PPL, including firms incorporated in Delaware. Given Delaware's prominence, its unique history of poison pill case law and the empirical uncertainty it creates for the validity of pill adoption and redemption, we verify that our main findings are robust to the exclusion of Delaware firms entirely (Internet Appendix Table A12).

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

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

existing pills ($Ln(PPill\ Duration)$) for firms incorporated in enacting states. $New\ PPill$ 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.

We primarily employ Tobin's Q (Q) as our main proxy for firm value, consistent with prior work examining the value relevancy of corporate governance arrangements (e.g., Demsetz and Lehn 1985; Morck, Shleifer, and Vishny 1988; Lang and Stultz 1994; Yermack 1996; Himmelberg, Hubbard, and Palia 1999; Daines 2001; Palia 2001; Bhagat and Jefferis 2002; Gompers, Ishii, and Metrick 2003; Bebchuk, Cohen, and Ferrell 2009; Cremers, Litov and Sepe 2017). We follow Fama and French (1992) and measure Q as the ratio of market to book value of assets using financial data from Compustat.

We recognize that Q is an imperfect measure of value, for example, because it can also proxy for a firm's growth opportunities (Smith and Watts 1992; Jung et al. 1996; Parise 2018) and is subject to potential measurement error (Erickson and Whited 2000, 2012; Bartlett and Partnoy 2018). Therefore, in robustness tests, we also analyze the implications of PPLs for the following alternative proxies of (changes in) firm value: 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) or three-factor (Fama and French 1993) 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); and 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).

Additionally, we include several control variables shown by the prior corporate governance literature to correlate with $PPill$ and Q: the natural logarithm of a firm's total assets ($Ln(Assets)$) and one plus its number of years in the Compustat database ($Ln(Age)$), the Herfindahl-Hirschman index (HHI) based on a firm's three-digit standard industrial classification (SIC) code, sales growth (SG), an indicator for whether a firm has negative net income for the fiscal year ($Loss$), debt-to-equity ratio (DEQ), firm liquidity ($FLIQ$), capital expenditures scaled by total assets ($CAPX/Assets$), research and development expenditures divided by net sales ($R\&D/Sales$), and a firm's level of institutional ownership (IO). Data for most of the controls come from Compustat with the exception of IO, which is obtained from Thomson Reuters. Further, in our default specifications, we follow Karpoff and Wittry (2018)

and include dummies for the other most common forms of state antitakeover statutes: business combination law (BCL), control share law (CSL), directors' duties law (DDL), and fair price law (FPL).

Our final set of sample construction procedures includes: excluding firms with observed lobbying activity for specific antitakeover statutes (information comes from Karpoff and Wittry 2018, Table III, p. 662); winsorizing all of the continuous variables at the 5% level in both tails to mitigate the influence of extreme outliers;[9] and adjusting dollar values for inflation using 2015 dollars. Internet Appendix A provides variable definitions.

Internet Appendix Table A3 presents summary statistics. In particular, Panel A (Panel B) of this table reports the mean, standard deviation, 25th, 50th and 75th percentiles, and the total number of observations for the dependent and independent variables in our dataset over the full sample 1983–2012 (for the period 1992–2012, which begins and ends three years before and after the first and last second SW-PPL states adopted their laws). Our main sample – accounting for the use of lagged controls and dropped singleton groups (Correia 2015)[10] – is comprised of 33,371 (26,254) firm-year observations.

The average percentage of firm-years in our sample in which a company has a $PPill$ in-place is 39.3% (40.7% for 1992-2012) and has a standard deviation of 0.49. Moreover, the average Q for our main sample is 1.9 with a standard deviation of 0.91, while 32.3% of the observations in our dataset are affected by a PPL. Overall, the descriptive statistics for our variables are similar to those in prior, empirical corporate governance studies.

1.2. Identification strategy

We investigate the relevancy of the shadow pill for firm-level pill adoptions and firm value by exploiting the quasi-natural experiment created by the staggered enactment of PPLs by firms' incorporating states. 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 states adopting a PPL is 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 our identification strategy.

[9] Our findings are unchanged if, instead, we winsorize continuous variables at the 1% or 2.5% level in both tails.
[10] Singleton groups (i.e., groups with only one observation) are common in regressions with interacted fixed effects – which we use in this study. Correia (2015) shows that maintaining singleton groups in linear regressions with multiple levels of fixed effects can lead to underestimated standard errors and incorrect inference.

To examine this concern, we follow a similar approach as Acharya, Baghai, and Subramanian (2014) and analyze the predictability of PPLs. We estimate a linear probability model, where we define the dependent variable as the adoption of PPLs, and where potential predictor variables include state-level firm, macroeconomic, political economy, and corporate law factors that a priori could determine these laws' enactment, along with state of incorporation and year fixed effects. For example, to explore the possibility of a reverse causality problem, we specify state-year ('SY') median across all firms in the sample incorporated in the state ('Inc'), both in levels and changes ('Δ') in the frequency of poison pills ($Inc\ SY\ PPill$ and $Inc\ SY\ \Delta\ PPill$, respectively), and include three separate proxies of firm value ($Inc\ SY\ Q$, $Inc\ SY\ \Delta\ Q$, $Inc\ SY\ Return$, $Inc\ SY\ \Delta\ Return$, $Inc\ SY\ ROA$, and $Inc\ SY\ \Delta\ ROA$). 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 ($Inc\ SY\ M\&A\ Vol$), GDP per capita ($Ln(Inc\ State\ GDPPC)$) and growth rate ($Inc\ State\ GDP\ Growth$), a dummy for whether the majority of a state's U.S. House of Representatives belongs to the Republican Party ($\%\ Inc\ State\ Republican$), a state's level of population ($Ln(Inc\ State\ Pop)$), rates of unemployment ($Inc\ State\ Unemploy$) and state business entry and exit ($Inc\ State\ Entry$ and $Inc\ State\ Exit$). We include state of incorporation fixed effects to control for unobserved and time-invariant heterogeneity at the state level, and year fixed effects to account for transitory U.S.-wide factors (e.g., macroeconomic conditions).

In the main analysis, we focus on SW-PPLs – which are unexplored by prior literature and form the basis for our key tests – using the sample period 1992 to 2012; in the Internet Appendix we show the analogous results for FW-PPLs over the time frame 1983 to 1993. 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 strongly suggests that state-level factors are unable to predict the passage of SW-PPLs. The coefficients pertaining to a states' median levels and changes in poison pills, Tobin's Q, stock returns, and ROA are separately (Columns (1) to (3)) and collectively (Column (4)) insignificantly different from zero, so that reverse causality is unlikely to be a concern for our identification. Likewise, the adoption of any of the other antitakeover laws do not predict SW-PPLs (unlike in the first wave, see Internet

Appendix Table A4). The coefficients on $Ln(Inc\ State\ GDPPC)$ and all other state-level macroeconomic and political factors are always statistically insignificant, indicating that the passage of SW-PPLs is not driven by local economic conditions (again in contrast to FW-PPL adoptions). We conclude that we do not find any evidence inconsistent with the assumption that the adoption of SW-PPLs provides an exogenous shock to individual firms' takeover protection.

1.3. Empirical specification

Our baseline investigation of the implications of the shadow pill employs a difference-in-differences (DD) regression model, comparing changes in either poison pill status or Tobin's Q amongst firms incorporated in states with PPLs relative to those of firms incorporated elsewhere.

Specifically, we estimate

$$y_{ijlst} = \beta PPL_{st} + \alpha' \mathbf{X}_{ijst} + \gamma_i + \omega_{lt} + \lambda_{jt} + \varepsilon_{ijlst} \quad (1)$$

where y denotes either $PPill$ or Q 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, is an indicator for whether a firm's incorporation state s has adopted a PPL as of the current year t, while X represents a vector of controls – outlined in Section 1.1 – including indicators for the other most common antitakeover laws. As a robustness check, some of our tests exclude firm-level controls since most of these variables are endogenous and likely also affected by the laws and could, therefore, bias our point estimates (Angrist and Pischke 2009; Roberts and Whited 2013). We 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.

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 inferences are robust to many sources of unobserved, time-variant heterogeneity that could bias our estimates, including local macroeconomic factors which 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, since this is generally where its major plants and operations are located (Henderson and Ono 2008).

The two-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; Maksimovic and Phillips 2001; Rhodes-Kropf, Robinson, and Viswanathan 2005). Thus, if the staggered adoption of PPLs across states is correlated with M&A activity – though Table 2 suggests this is not the case – and potentially correlated with other unobservable characteristics that also impact firms' actual pill policy and Tobin's Q, our use of industry-by-year fixed effects should account for this source of confounding variation. The division-by-year fixed effects should also control for some of this variation since most industries cluster by geography (Ellison and Glaeser 1997, 1999; Ellison, Glaeser and Kerr 2010).

A common alternative strategy developed in the BCL literature to deal with local sources of unobserved, confounding variation is to use fixed effects at the level of headquarter states rather than at the level of U.S. Census divisions (Gormley and Matsa 2014, 2016). We confirm that our results are robust to using this approach (see Table 5). A limitation of this approach, however, is that it relies on the assumption 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 incorporated 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 comparisons to almost exclusively Delaware 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. First Wave (FW-) 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 and promoted the widespread adoption of the pill for firms incorporated in both

Delaware and outside of the state (Helman and Junewicz 1986; Fleischer, Hazard, and Klipper 1988) – see our Figure 1. Law and finance scholars, however, tend to describe the legal status of the pill for firms incorporated in states other than Delaware as uncertain until these states adopted PPLs that validated the use of the pill (Catan and Kahan 2016; Cain, McKeon, Solomon 2017; Karpoff and Wittry 2018). The argument usually given to defend the uncertain status of the poison pill for firms incorporated outside Delaware before the enactment of PPLs is that state courts' decisions invalidated the use of the poison pill in the states of New York, New Jersey, Georgia, Wisconsin, Colorado, Virginia, and Indiana between 1986 and 1989 (Catan and Kahan 2016; Cain, McKeon, Solomon 2017).[11]

Yet the pervasive authority of Delaware judicial decisions over non-Delaware corporations (e.g., Ryngaert 1988; Cremers and Ferrell 2014; Dammann 2019) points to an opposite conjecture; namely, that the validity of the poison pill was fairly certain in the immediate aftermath of *Moran* for both firms incorporated in Delaware and outside of the state. Indeed, as evidenced by Figure 1, the widespread adoption of visible poison pills, even for non-Delaware firms, in the years immediately following *Moran,* supports the view that this ruling was understood to apply to non-Delaware firms as well.[12]

In order to test this conjecture, the first four columns of Table 3 regress a poison pill indicator variable ($PPill$) on dummies for whether a firm's state of incorporation adopts an FW-PPL ($FW\ PPL$), or eventually adopts one of these laws during the SW ($Eventual\ SW\ PPL$), over either the "Entire FW-period" of 1983 to 1993 (Columns (1)–(2)) or the "Post-*Moran*" period of 1986 to 1993 (Columns (3)–(4)). Following prior studies (e.g., Dowen, Johnson, and Jensen 1994; Comment and Schwert 1995; Heron and Lie 2006, 2015; Cremers and Ferrell 2014), we additionally include a number of firm-level controls: $Ln(Assets)$, $Ln(Age)$, HHI, SG, $Loss$, DEQ, $FLIQ$, $CAPX/Assets$, $R\&D/Sales$, and IO, as well as dummies for other common antitakeover laws: BCL, CSL, DDL, and FPL. Further, we specify firm fixed effects and interacted fixed effects – U.S. Census division-by-year and two-digit SIC industry-by-year. Lastly, the continuous independent variables are standardized to have a mean of zero and variance of one, and the standard errors are adjusted for clustering at the state of incorporation level.

[11] The uncertainty created by these decisions, however, did not last long, as each of these states passed a PPL shortly after the related invalidating court decision. For example, while the New York Supreme court invalidated the use of the pill in June 1988 (in *Bank of New York Co.* v. *Irving Bank Corp.*), the state of New York passed a PPL in December of the same year.

[12] This interpretation also finds support in the evidence that court decisions in states other than Delaware frequently referenced *Moran* in their own poison pill rulings.

Consistent with our 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 3. 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. Consistent with prior work analyzing the determinants of pill adoptions over a similar timeframe (see, e.g., Comment and Schwert 1995, who use the sample period 1977–1991), we also find that firms larger in size ($Ln(Assets)$) and with higher liquidity ($FLIQ$) are more likely to adopt a poison pill, while firms that have higher sales growth (SG) are less inclined to employ the use of a pill. Further consistent with this work, we document that the coefficient on Q (DEQ) is positive (negative), but statistically insignificant.

Under the view that Delaware common law shapes corporate law in all other states, we further conjecture that Delaware decisions that followed *Moran* could have mattered more for the uncertainty of the pill in other states than earlier state courts' decisions in those same states. In particular, in the fall of 1988, the Delaware courts issued two decisions – *City Capital Associates* v. *Interco Inc.* and *Grand Metropolitan PLC* v. *Pillsbury Co.* – that unexpectedly increased uncertainty about the use of the poison pill, although mostly in regard to the redemption of the pill, rather than its validity per se. In both of these decisions, the Delaware court halted the continued use of a visible poison pill that prevented an unsolicited tender offer, which prompted considerable comment at the time and even induced some corporate lawyers to recommend firms to move out of Delaware (Fleischer and Sussman 2013).

In line with this interpretation, Figure 1 indicates that the average percentage of "All Firms" in our sample with a pill in-place began to decrease sharply after 1988. However, this figure also suggests that the cohort of firms least affected by the *Interco* and *Pillsbury* decisions were those incorporated in states with an explicit right to adopt poison pills via their states' previously enacted PPLs (i.e., "FW PPL Firms"). Further supporting this view, the last two columns of Table 3 show that firms incorporated in states with FW-PPLs were 4.6% to 7.6% more likely to have a pill in-place in the "Post-*Interco* & *Pillsbury*" period (1989–1993), relative to division rivals sharing similar industry trends, but incorporated elsewhere.[13] Meanwhile, the results in Column (6) indicate that firms incorporated in states that eventually

[13] We additionally show that FW-PPLs also lead to value increases in the post-*Interco* and *Pillsbury* period (Panel B of Internet Appendix Table A11). Yet, our estimates in these tests are much noisier as they are exposed to confounding variation from prior pill-related court rulings (e.g., *Moran*, *Interco*, *Pillsbury*, and *Paramount*).

adopt an SW-PPL, but at the time of the analysis are not covered by a law, are 26.2% less likely to employ the use of a poison pill after 1988. This could plausibly explain why most states (27-out-of-the-35) decided to adopt PPLs post-November of 1988, as the viability of the poison pill as a strong defense was no longer assured after *Interco* and *Pillsbury*.[14]

Considering this legal context, and consistent with Karpoff and Wittry'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. From an identification perspective, doing so 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 – and, thus, mitigates the likelihood of measurement error that could bias our estimates.

3. Main Results

Our main research questions are how a "strengthened" shadow pill – as proxied by the adoption of a PPL – impacts actual pill policy and firm value. To address these questions, we first analyze the relation between PPLs – focusing on SW-PPLs for the reasons explained above – and firm decisions to adopt and maintain ($PPill$) poison pills, as well as the laws' effects on new pill adoptions ($New\ PPill$) and existing pill redemptions ($Ln(PPill\ Duration)$). Second, we estimate the value implications of PPLs using Tobin's Q (Q) regressions. Third and last, we show that our findings related to Tobin's Q are robust to additional tests, such as a matched sample analysis, and to using alternative value measures.

3.1. Shadow pills and actual 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 one of these laws. On the one hand, since PPLs sanction a firm's *right to adopt* a visible poison pill, we might expect that firms incorporated in PPL-adopting states will have them in-place more frequently

[14] The *Interco* and *Pillsbury* decisions were later reversed by the 1989 Delaware court decision in *Paramount Communications, Inc.* v. *Time Inc.*, which some commentators read as granting the board an unconstrained power "to just say no" to unsolicited tender offers (Bebchuk, Coates, Subramanian 2002). Several other commentators, however, maintain that Delaware case law on pill redemption cases remains in an unsettled state and tends to depend on specific circumstances that have limited general applicability for firms incorporated outside of Delaware (Fleischer and Sussman 2013). We interpret the back-and-forth rulings in Delaware on the validity and redemption of poison pills as a clear indication that firms outside of the state are more likely to rely on their own incorporating states' statutory and case law in the ensuing period.

(i.e., a "validation effect"). On the other 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 the pill or even decrease their reliance on them (i.e., a "substitution effect"). We start by testing these predictions in Panel A of Table 4, by regressing $PPill$ on PPL plus other controls and firm, division-by-year, and industry-by-year fixed effects.

In Column (1), without specifying firm-level controls, we find marginally significant evidence in favor of the validation effect. The point estimate on PPL suggests that firms incorporated in a PPL-adopting state are 5.8% more likely to put in place and maintain a visible pill ($PPill$), relative to firms operating in the same division and sharing similar industry trends, but incorporated in a non-PPL passing state. This finding is consistent with the general, unconditional, trend for "SW PPL Firms" as Figure 1 shows an upward movement in this cohort's use of $PPill$ beginning in 1995 and continuing for the next decade. However, when we expand the model to control for other firm-level characteristics in Column (2), the coefficient on PPL becomes insignificantly different from zero.

Further, in both columns, we also report the estimates on pre-determined levels of Q and find strong evidence of a potential reverse causality problem as the coefficients indicate that lower-valued firms – and thus firms more at risk of takeover – are 3% to 5% more likely to have a pill in-place, consistent with Cremers and Ferrell (2014). We then supplement this finding by estimating regressions of 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 in Catan 2019). The relative year dummies include indicators 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 Internet Appendix Figure A1. 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).

Motivated by these results, we consider the heterogeneous value implications of PPLs for firms with different levels of Tobin's Q in the last two columns of Panel A, Table 4.

Specifically, we create indicators for whether a firm's level of Q falls in the lowest ($Q(Lowest)$), middle-to-lowest ($Q(Low)$), middle-to-highest ($Q(High)$), or highest ($Q(Highest)$) part of its quartile distribution in our sample, and interact these dummies with PPL. Moving to our fully specified model in Column (4), we document evidence that PPLs validate the adoption and maintenance of $PPill$ for firms with the lowest values of Tobin's Q – and thus, most likely at risk of takeover – in our sample as these firms are 5.3% more likely to have a poison pill relative to division and industry rivals in non-PPL enacting states.

In Panel B of Table 4, we then decompose $PPill$ into $New\ PPill$ and $Ln(PPill\ Duration)$. The advantage of this approach is that it allows us to better determine how PPLs affect adoptions of new pills relative to the maintenance of existing pills. The first two columns specify $New\ PPill$ as the dependent variable and include our full set of controls and fixed effects. We find that the average firm incorporated in a PPL-adopting state does not alter their frequency of new poison pill adoptions (Column (1)); but, rather it is only firms with Tobin's Q in the lowest quartile of its distribution that significantly increase their use of new pills (Column (2)) – at a rate of 8.4%. We then regress $Ln(PPill\ Duration)$ on the same set of independent variables and find much stronger evidence that PPL affected firms increase their maintenance of existing pills. For instance, in Column (4), we document evidence that only firms with levels of Q in the highest quartile of its distribution ($Q(Highest)$) are unaffected by the laws, while the other three groups ($Q(Lowest)$, $Q(Low)$, and $Q(High)$) significantly increase the duration of their pills in-place.[15]

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 (i.e., "shadow pill") by focusing on Tobin's Q as our primary measure of firm value. We then address the concern that a potential selection bias (e.g., reincorporation) might drive our Q results by constructing a matched sample that restricts our tests to firms that were incorporated in PPL-adopting states at least one year before its passage. We then check the robustness of our findings on Tobin's Q in both the full and matched samples by examining the effect of PPLs on alternative measures of firm value such as stock returns and profitability.

[15] With respect to controls, firms that are larger in size ($Ln(Assets)$), older ($Ln(Age)$), have higher debt-to-equity ratios (DEQ) and levels of institutional ownership (IO) are more likely to adopt a pill, while firms in more concentrated industries (HHI), with greater sales growth (SG), liquidity ($FLIQ$) and capital expenditures ($CAPX/Assets$) are less inclined to use poison pills. The correlations we find between these standard firm-level controls and $PPill$ are mostly consistent with prior literature (e.g., Dowen, Johnson, and Jensen 1994; Comment and Schwert 1995; Heron and Lie 2006, 2015; Cremers and Ferrell 2014).

Finally, we briefly outline additional robustness tests that we include in a supplemental Internet Appendix.

3.2.1. Full sample. Table 5 reports the DD estimates of the impact of the adoption of PPLs by state legislatures – as a proxy for the strengthening of the shadow pill – on the Tobin's Q of firms in enacting states over the period 1992 to 2012. Each of the five columns employ Q as the dependent variable and include controls for the other four antitakeover laws (BCL, CSL, DDL, and FPL). Columns (1)–(3) specify our default set of fixed effects – firm, division-by-year, and industry-by-year – 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 last four columns include the full set of firm-level control variables ($PPill$, $Ln(Assets)$, $Ln(Age)$, HHI, SG, $Loss$, DEQ, $FLIQ$, $CAPX/Assets$, $R\&D/Sales$, and IO), and the standard errors are adjusted for clustering at the state of incorporation level.

The adoption of PPLs has a positive and statistically significant impact on the Tobin's Q of firms in enacting states. The results in the first two columns provide strong support for the key implication of the view that a strengthened shadow pill is value-enhancing for shareholders. In Column (1), without including any potentially endogenous firm-level controls (besides $PPill$ which correlates with both PPL and Q), we find that firms incorporated in a state with a PPL experience an increase in Q of 10.5 percentage points relative to firms incorporated elsewhere but operating in the same U.S. Census Division and sharing a similar industry trend. This represents an economically significant increase of 5.7% (=0.105/1.855) relative to the sample mean's Q. Further, the estimated coefficient on PPL in Column (2) confirms that the passage of these laws are valuable for shareholders, even after including controls for company characteristics, as firms incorporated in these states have Tobin's Qs that are 9.5 percentage points higher than those of division and industry rivals incorporated in non-PPL adopting states. This represents an economically significant 5.1% increase relative to the sample mean for Q of 1.855.

In addition, we show that our baseline point estimate in Column (2) is stable to the omission of any one SW-PPL-passing state. We present evidence for this stability in Internet Appendix Figure 2. In particular, on the *y*-axis we plot each of the coefficients we estimate from separate regressions that exclude SW-PPL states one-by-one – excluded state shown on the *x*-axis – along with their corresponding 95% confidence intervals. The results indicate that these coefficients compare favorably to our reference estimate, with magnitudes that fall

between 0.073 and 0.111 and *t*-statistics ranging from 2.58 to 3.78. Hence, we do not find evidence that an unobserved, state-specific factor (or outliers) drive our key result.

Meanwhile, each of the columns in Table 5 confirm the prior literature, finding a negative correlation between actual firm-level pills and Tobin's Q (e.g., Gompers, Ishii, and Metrick 2003; Bebchuk, Cohen and Ferrell 2009; Cremers and Ferrell 2014). However, in light of our results in Table 4 and Internet Appendix Figure A1, the negative association between visible poison pills and Tobin's Q seems plausibly endogenous and due to reverse causality. In Column (3), we then explore the possibility that the positive effect of PPLs is dependent on firms having poison pills in-place. Yet, when we regress Q on the interaction of PPL and $PPill$ we find that the point estimate (=0.011) on the interacted variables is both economically and statistically (*t*-stat=0.31) insignificant, while the standalone coefficient (=0.091) on PPL remains strongly significant at the 1% level (*t*-stat=2.76), which suggests that shadow pills create long-term value for shareholders independent of actual pills.[16]

The last two columns of Table 5 serve as robustness checks to our main specification, which includes division-by-year fixed effects by alternatively employing the use of U.S. Census Regions (i.e., Northeast, Midwest, South, and West) (as in Autor, Donohue, and Schwab 2006; Acharya, Baghai, and Subramanian 2014) or headquarter states (as in Gormley and Matsa 2014, 2016; Karpoff and Wittry 2018) to control for potential local confounding factors. Reassuringly, our key inference on the value relevance of the shadow pill remains unchanged using either of these alternative specifications. For example, the fully specified model with state-by-year fixed effects in Column (5) indicates that firms incorporated in PPL adopting states still experience increases in Q of 4.5% (=0.083/1.855) relative to its sample mean, when compared with firms located in the same headquarter state and sharing similar industry trends, but incorporated elsewhere. Still, we prefer the use of divisions as they provide a more granular geographical measurement than regions and are not susceptible to the econometric issues engendered by the use of headquarter states that we outlined in Section 1.3.

[16] Internet Appendix Table A5 decomposes $PPill$ into $New\ PPill$ and $Ln(PPill\ Duration)$ and tests whether PPLs differentially affect firms that adopt and maintain pills either before or after their law's enactment. Our results indicate the shadow pill always remains positive and significantly related to Q, and the only instance in which firm-level pill decisions might affect *Q* is when a company adopts a new poison pill after its incorporation state passes a PPL (point estimate=-0.214 and *t*-stat=-2.56). This finding may imply: (i) shadow pills provide sufficient protection and that actual adoption only serves to entrench managers (Manne 1965; Cary 1969; Easterbrook and Fischel 1991; Bebchuk, Coates, and Subramanian 2002; Bebchuk, Cohen, and Ferrell 2009), or (ii) as documented in Table 4 and Internet Appendix Figure A1, the decision to adopt a pill is endogenously correlated with Q and the negative association is due to reverse causality (Cremers and Ferrell 2014; Catan 2019).

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 DD estimate on PPL. As underscored in Angrist and Pischke (2009) and Roberts and Whited (2013), the fundamental assumption of this identification strategy is that of parallel trends in the outcome variable (i.e., 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 create Figure 2 by regressing our measure of Tobin's 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 then plot the point estimates on the relative year indicators on the *y*-axis, and indicators for each year in a plus or minus four-year window surrounding PPL adoption on the *x*-axis. We also plot a 95% confidence interval for the coefficient estimates, where the interval 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, but that the difference in Q starts to broaden in the year of adoption and becomes statistically different in the one- through four-years post-adoption.

3.2.2. Matched sample. We now turn our attention to addressing the concern that a selection effect (e.g., reincorporation) might bias our inference that PPLs positively impact the Tobin's Q of firms incorporated in enacting states. In particular, since using a DD design with a pooled panel entails considering firms that reincorporate or first-time incorporate into a PPL-adopting state, a strengthened shadow pill is non-random for these firms.

To account for this potential bias, we construct a propensity score-matched sample whereby we match firms in each of the SW-PPL adopting states in the year before passage (*t*-1) to a pool of firms incorporated in states without one of these laws in at least the three years following its matched counterparts' adoption year. We define firms incorporated in states that are one-year away from PPL enactment as "treated" firms ($Treated$), and their potential matches as "control" firms. The basic idea behind this research design is that by matching firms in the year prior to treatment, we ensure that our matched sample is restricted to firms that were incorporated in states before the PPLs were 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 the next closest division (one-digit SIC industry). Additionally, we construct propensity scores for matching from Q and $Total\ Assets$. We present the summary statistics for the two groups of firms in the year before treatment in Panel A of Table 6.

To begin, each of the matching variables is statistically indifferent between treated and control firms. For instance, the Q of treated firms in year *t*-1 is 1.744 with a standard deviation of 0.986, while the Q of the control firms is 1.814 with a standard deviation of 0.919. This negligible 0.071 absolute difference is statistically indifferent (*t*-stat=0.85). Furthermore, the firm-level poison pill dummy is matched exactly between the two groups, while the SIC2 and Divisions dummies are quite similar between the 264 treated (means: $SIC2$=43.6 and $Divisions$=5.5) and 264 control (means: $SIC2$=43.7 and $Divisions$=5.7) firms. Lastly, Internet Appendix Table A6 shows that the other (non-explicitly matched) firm-level controls are also comparable between the two groups, indicating that they are similar on observable characteristics in the year before treatment.

Shifting down to Panel B of Table 6, we present point estimates from regressions of Tobin's Q on a $Treated \times Post$ (we use this variable name instead of PPL in the matched sample) interaction term over a $t\pm3$ estimation window. The first two columns specify firm, division-by-year, and industry-by-year fixed effects to ensure our post-treatment comparisons remain between the *t*-1 matched pairs, while the last column only uses firm and year fixed effects for robustness. Further, each of the three columns include dummies for the other antitakeover laws and the last two append our firm-level controls. Focusing on Column (2), we find that our matched treated firms experience significant increases in their Q of 9.8 percentage points when compared to the control group, which translates to an economically significant 5.4% increase relative to the matched sample mean for Q of 1.831. Further, the coefficient on $PPill$ is now statistically insignificant since we exactly matched treated and controls firms on this variable.

Importantly, the coefficient on $Treated \times Post$ in the matched sample (point estimate=0.098 and *t*-stat=2.13) and on PPL in the full sample (point estimate=0.091 and *t*-stat=2.76) are quite similar in both economic magnitude and statistical significance. These

results are, thus, indicative that a selection effect (e.g., reincorporation) does not drive our main findings in the full sample.

3.2.3. Alternative value measures. Having demonstrated the robustness of the positive relationship between PPLs and Tobin's Q, we now investigate the reliability of Q as a proxy for firm value by considering alternative value measures. We start this analysis in Panel A of Table 7, by employing the same baseline specifications we use in the full (Table 5) and matched (Table 6) samples, but in these models, we replace Tobin's Q as the dependent variable with the following three measures. The first alternate proxy is $Excess\ Return$ (similar to 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). Second, we consider the profitability measure, return on assets (ROA), which we define as operating income before depreciation and amortization divided by the total book value of assets (Giroud and Mueller 2010). The third alternative value measure, Total Tobin's Q ($Total\ Q$), is proposed by Peters and Taylor (2017) and modifies Q by explicitly accounting for intangible capital in the firm's replacement cost of total capital.

The ultimate takeaways from the six columns in Panel A of Table 7 are that a strengthened shadow pill is valuable for shareholders and that Tobin's Q is a consistent proxy of this value. That is, irrespective of whether we employ $Excess\ Return$, ROA, or $Total\ Q$ as dependent variables, and whether we use our full sample or a matched sample, our inferences remain unchanged: PPLs are positively and significantly related to accepted empirical proxies of firm value. For example, Column (1) suggests that shareholders of firms incorporated in states with PPLs experience a significant 3.2 percentage point increase in the $Excess\ Return$ on their shares, relative to division and industry rivals incorporated in a non-PPL enacting state. Moving over to Columns (3) and (4), we examine the effect of SW-PPLs (i.e., post-1995) on ROA. We find that ROA improves for the subset of firms incorporated in states that strengthen the right to adopt a poison pill by 4.5% (=0.006/0.132) in the full sample (Column 3) and 8% (0.010/0.125) in the matched sample (Column 4) relative to its respective means.

As a final robustness check of our findings related to Tobin's Q, we consider an alternative research design and employ a monthly portfolio return approach, which can be viewed as a long-term stock event study, consistent with the prior corporate governance literature (Gompers, Ishii, and Metrick 2003; Bebchuk, Cohen, and Ferrell 2009; Cremers and Ferrell 2014; Cremers, Litov, and Sepe 2017). 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 underlying this framework 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 the realized returns for a treated firm would be systematically higher than those for a control firm.

Panel B of Table 7 (Internet Appendix Table A7) tests this conjecture and reports our findings that the long-short portfolios of treated and control firms result in positive and significant $Alpha$ over "6m36" and "12m36" holding periods, using a value-weighted (an equally-weighted) market factor, and estimating the risk-adjusted, excess returns with either the four-factor (Carhart 1997), three-factor (Fama and French 1993), or market-factor (Treynor 1962; Sharpe 1964; Lintner 1965; Mossin 1966) models. For instance, when we buy stocks of treated firms and short stocks of control firms 6-months or 12-months before the adoption date of their (matched firms') respective PPL and continue such strategy until 36-months after ("6m36" or "12m36"), we find an annualized abnormal return of 11.9% using the four-factor model. These magnitudes are economically significant and comparable to the 13% increase in $Alpha$ following the staggering up and (de)staggering of a board documented by Cremers, Litov and Sepe (2017).

3.2.4. Additional robustness. We conduct several additional robustness tests of our main finding that shadow pills are value-enhancing for shareholders. However, to conserve space, we include these supplemental analyses in the Internet Appendix (Tables A8 to A14). As a roadmap for interested readers, we include a brief synopsis of these tests below:

(i) State-by-year fixed effects.

a. Full sample. In Column (5) of Table 5, we verify the strength of our main finding to the inclusion of state-by-year fixed effects. However, following Gormley and Matsa (2016), we take this analysis a step further and decompose the effect of PPLs into cohorts of firms incorporated and headquartered in the same state ($Same\ Inc\text{-}HQ\ State$) and incorporated and 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, while the coefficient on $PPL \times Diff.Inc\text{-}HQ\ State$ is positive but insignificant (we argue) due to a lack of variation (i.e., statistical power), in both the PPL firm-years and non-Delaware, non-PPL firm-years.

b. Matched sample. As a further robustness check on the concern that unobserved, time-varying headquarter state factors are driving our results, we re-perform our matching procedure, but interchange the criterion of exact matching on U.S. Census divisions to matching on states of headquarters. We then regress Q on $Treated \times Post$ in this alternative sample and find our results continue to hold.

(ii) Placebo tests.

a. Full sample. We follow Cornaggia et al. (2015) and randomly assign states (without replacement) a PPL, but require that our assignment procedure follows the laws' actual empirical distribution – 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. Reassuringly, however, we find an economically and statistically insignificant point estimate on $Randomized\ PPL$ for our four proxies of value.

b. Matched sample. We provide evidence for the parallel trends assumption in our matched sample by moving back actual adoption dates of PPLs by four-years and reperforming our matching procedure in year t-1 of the pseudo adoption date. We then estimate our baseline model using either Q, $Excess\ Return$, ROA, or $Total\ Q$ with an ($t \pm 3$) estimation window and find the coefficients on $Pseudo\ Treated \times Post$ are always insignificant.

(iii) 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.

(iv) Sample adjusted for Delaware case law. We argue the 1985 Delaware court decision in *Moran* effectively validated the use of the pill for all firms, including those incorporated outside of Delaware, while successive rulings in *Interco* and *Pillsbury* in 1988 reestablish uncertainty regarding pill redemption, thus, limiting the general applicability of Delaware case law. Accordingly, to have a pre-treatment period unconfounded by these court rulings, we define the SW of adoptions to begin with Minnesota in 1995, rather than with PPLs adopted immediately after *Interco* and *Pillsbury* (1989 or 1990). For robustness, we adjust the SW to begin in 1989 and find that our full and matched sample results persist. Additionally, we show that our findings continue to hold if we focus exclusively on the value implications of FW-PPLs over the sample period 1989 to 1993.

(v) Excluding Delaware or multi-law adopting states. We show that both our full and matched sample Tobin's Q results are robust to excluding firms incorporated in Delaware and, separately, excluding states that enact other antitakeover laws in the same year they pass PPLs.
(vi) Poison pill validity index (*PPV-Index*). Our final robustness test considers an alternative proxy for shadow pills. Using PPLs and state-level court decisions on pills from Cain, McKeon, and Solomon (2017), we construct the $PPV\text{-}Index$ to capture changes across states and time on the validity of the pill. Substituting this measure for PPL in our full sample Q regressions, we continue to find that shadow pills are valuable.[17]

4. Economic Channels

What economic channels can explain our finding that a strengthened shadow pill, as sanctioned by the enactment of a PPL, adds to firm value? To address this question, we draw on the existing theoretical literature and examine two potential economic mechanisms. The first is the "bargaining power hypothesis," under which having the right to adopt a poison pill strengthens the bargaining power of the board vis-à-vis any potential bidder, thereby allowing directors to obtain a higher offer price for the target's shareholders (DeAngelo and Rice 1983; Linn and McConnell 1983; Stulz 1988; Berkovitch and Khanna 1990; Harris 1990; Kadyrzhanova and Rhodes-Kropf 2011). The second is the "bonding hypothesis" which posits that a board's power to deter a hostile takeover benefits shareholders by allowing a firm to more credibly "bond" itself to longer-term operational strategies – strategies that would otherwise be at risk of reversal if the firm underwent a change of control (Knoeber 1986; Laffont and Tirole 1988; Shleifer and Summers 1988).

4.1. Bargaining power hypothesis

We test the bargaining power hypothesis, following prior empirical studies (DeAngelo and Rice 1983; Linn and McConnell 1983; Comment and Schwert 1995; Field and Karpoff 2002; Heron and Lie 2006, 2015; Bates, Becher, and Lemmon 2008; Kadyrzhanova and Rhodes-Kropf 2011), by analyzing both target acquisition propensities and premiums. The data for these tests are pulled from the SDC M&A database and is comprised of 257 unsolicited acquisition attempts announced over the period 1992–2012. We define a takeover attempt as unsolicited if the SDC database classifies the attempt as hostile or otherwise unsolicited (as in Heron and Lie 2006, 2015).

[17] In unreported tests, we also show that our key full sample Tobin's Q results continue to hold after including controls for heterogenous PPL provisions (e.g., dead-hand pill and weak-pill provisions) and staggered boards (and their interaction with PPLs).

Panel A of Table 8 examines the impact of PPLs on the likelihood that firms incorporated in law adopting states receive a takeover bid (Columns (1)–(2)), as well as its effect on the probability that a deal is successfully completed (Columns (3)–(6)). We define the dependent variable, Bid ($Acquired$), using 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 our full set of baseline controls, plus an indicator for whether a firm receives multiple bids ($Multibid$), as well as its preceding year's stock return ($Return$). The first four (last two) columns specify firm, division-by-year, and industry-by-year (division) fixed effects.

As shown by Panel A's row of coefficients on PPL, firms with strengthened shadow pills are equally likely to receive a takeover bid or be successfully acquired as firms without the same legally sanctioned right to adopt poison pills.[18] Additionally, our analysis of the heterogeneous effect of PPLs for firms with pills in-place ($PPL \times PPill$), also 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. Lastly, consistent with prior studies (Ambrose and Megginson 1992; Bhagat and Jefferis 1993; Comment and Schwert 1995; Heron and Lie 2006, 2015; Bates, Becher, and Lemmon 2008; Karpoff, Schonlau, and Wehrly 2019), we do not find evidence that actual poison pills ($PPill$) materially deter takeovers either.

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: $Total\ Premium$, measured as the total percentage premium offered relative to the target's price four-weeks prior to the initial offer, and $Premium\ Increase$, defined as the percentage increase in the bid price relative to the target's stock price four-weeks prior to the initial offer. We specify the full set of controls in each of the five columns, in addition to $Multibid$ and $Return$, and we also include a dummy, $Morning\text{-}After\ PPill$, for whether a firm adopts a poison pill in response to a takeover bid (as in Heron and Lie 2006, 2015). Lastly, our specifications use division fixed effects, and not firm or interacted fixed effects, since we are focusing exclusively on the cross-section of

[18] There are, however, empirical challenges with this analysis. Specifically, we are unable to test how many target bids and would-be-successful acquisition attempts never occurred because of the laws, and how many ex-ante target firms became too expensive to acquire following the enactment of a PPL since, as we document, these laws significantly increased firms' market values. John and Kadyrzhanova (2017) find that the interaction of PPLs and merger waves is an economically (but not statistically) significant deterrent of takeovers, while the effect of the laws during off-wave periods is insignificant in both respects.

successful unsolicited bids and, since, correspondingly, our sample size is limited to 257 observations.

In Columns (1) and (3) of Panel B, we find that shadow pills do not increase the total premium or initial premium offered to successfully acquired firms as the point estimates on PPL are statistically insignificant. On the other hand, the coefficients on $PPill$ ($Morning\text{-}After\ PPill$) in each of the five columns (the last column) are (is) positive and statistically significant, suggesting that visible poison pills can benefit a target's shareholders via increased bargaining power (consistent with, e.g., Brickley, Coles and Terry 1994; Comment and Schwert 1995; Cotter, Shivdasani, and Zenner 1997; Heron and Lie 2006, 2015). However, when we interact PPL and $PPill$ in Columns (2) and (4)–(5), we find evidence that the enhanced validity of the pill as authorized by a PPL heterogeneously increases both $Total\ Premium$ and $Premium\ Increase$. For instance, in the second (last) column, shareholders of target firms incorporated in states with one of these laws and an actual pill in-place experience a 7.7% (14.6%) differential increase in the total premium received (premium relative to the initial bid).

Hence, the evidence from Table 8 indicates that PPLs can be value-enhancing for shareholders of acquisition targets as the strengthened right to adopt a poison pill enhances the negotiating position of firms with pills in-place – which can explain why the adoption of pills for firms with lower levels of Q and, thus, most likely at risk of takeover, can be valuable. Unfortunately, however, these findings on the bargaining power hypothesis have two major drawbacks in explaining our key result that shadow pills increase the long-term value of firms. First, our main findings indicate the PPLs are value relevant for the *average* firm (Tables 5–7), while the increase in takeover premiums is only for a subset of firms with visible poison pills. When we interacted PPL with $PPill$ in Column (3) of Table 5, 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 Q and $Total\ Q$ and altogether unable to explain our evidence on ROA.

4.2. Bonding hypothesis

We now move to examine the bonding hypothesis as an alternative economic channel. Underlying this framework, is the idea that empowering a board to commit the firm to a business strategy that cannot easily be reversed – 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 and promotes the undertaking of long-term projects and stronger stakeholder relationships (Knoeber 1986; Laffont and Tirole 1988; Shleifer and Summers 1988; Stein 1988, 1989; Johnson, Karpoff, and Yi 2015). In order to test this channel, we follow the prior literature (Johnson, Karpoff, and Yi 2015; Cen, Dasgupta, and Sen 2015; Cremers, Litov, and Sepe 2017; Chemmanur and Tian 2018) and analyze the differential value effects of PPLs for firms more reliant on operational strategies that center around long-term investments in innovation and stakeholder relationships.

4.2.1. Innovative firms. The strengthened right to adopt a poison pill and, thus, resist a takeover bid, could play an essential bonding device toward a firm's stakeholders. Further, 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 shadow pill could prove useful in preventing the ex-post expropriation of these investments and more credibly commit innovative firms toward its non-shareholders.

We test these critical predictions of the bonding hypothesis by considering the heterogeneous effect of PPLs for firms that are more dependent on investments in innovation using the following four proxies: (1) $R\&D/Sales$, is a measure for the importance of corporate expenditures on research and development activities (Chan, Lakonishok, and Sougiannis 2001; Eberhart, Maxwell, and Siddique 2004) (data comes from Compustat); (2) $Intangible\ Capital$, is a "catch-all" measure for the importance of intangible capital (Core, Holthausen, and Larcker 1999; Duru, Wang, and Zhao 2013) and is defined as a firm's intangible capital estimated replacement cost (as proposed by Peters and Taylor 2017) (data comes 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 (Hall, Jaffe, and Trajtenberg 2005; Atanassov 2013; Kogan et al. 2017; Chemmanur and Tian 2018) (data comes from the KPSS Google patents dataset);[19] and (4) RQ, or research quotient, which measures the output elasticity of R&D (as proposed in Knott 2008) (data comes from WRDS in the Research Quotient database).

Panel A of Table 9 presents our results. In each of the four columns, we specify Tobin's Q as the dependent variable and include our full set of default control variables and fixed effects. We find, consistent with the theoretical predictions of the bonding hypotheses, that when boards are better equipped to contest the potential disruption caused by a takeover – via a

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

shadow pill – firms that 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 heterogeneous gains in value. For example, in Column (1), we document that a one-standard-deviation increase in the proxy, $R\&D/Sales$, yields an economically significant increase in Q of 7.4% (=2.139×0.064/1.855) for firms incorporated in PPL adopting states, relative to its sample mean. Likewise, we find in Column (2) that firms with strengthened shadow pills that have a one-standard-deviation higher level of $Intangible\ Capital$ have a 3.8% (=0.198×0.353/1.855) higher Q compared to division and industry rivals with less intangible-based capital.

4.2.2. Stakeholder relationships. We continue our evaluation of the bonding hypothesis as a source of value of shadow pills by considering their various effect for firms that are more reliant on stakeholder relationships. To do so, we employ four different proxies which aim to capture the importance of these relationships directly. They include: (1) $Organizational\ Capital$, 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) (data comes from WRDS in the Peters and Taylor Total Q database); (2) $Large\ Customer$, which captures the significance of customers who are likely to have a longer-term association with the firm (Cen, Dasgupt, and Sen 2015; Johnson, Karpoff, and Yi 2015) and is measured using an indicator for whether a firm's percentage of customer sales, based on the Compustat segment level database, is above the sample average; (3) $Strategic\ Alliance$, an indicator of whether the business has a long-term partnership with another company (Bodnaruk, Massa, and Simonov 2013; Johnson, Karpoff, and Yi 2015); and (4) $Labor\ Intensity$, 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.

Panel B of Table 9 shows our results. Columns (1)–(4) maintain Tobin's Q as the dependent variable and include the full set baseline controls and fixed effects. Consistent with our conjectures under the bonding hypotheses, we find in Column (1) that firms incorporated in PPL passing states and with a one-standard-deviation higher level of $Organizational\ Capital$ have a 4.6% (=0.365×0.232/1.855) higher Q compared to firms operating in the same division and sharing similar industry trends but with a smaller share of organizational capital. Similarly, Column (3) shows that firms with a strengthened shadow pill and in a relationship with a $Strategic\ Alliance$ partner experience a 10.1 percentage point

increase in Tobin's Q, which represents an economically significant 5.4% increase relative to the sample mean for Q of 1.855.

In sum, the evidence in Table 9 indicates that the strengthened right to adopt a poison pill, as enabled by PPLs, is especially likely to benefit 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, by enabling these firms to more closely bond its stakeholders to its operational strategy and, in so doing, decrease its costs of contracting.

5. 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, studying the *right to adopt* the pill (which right constitutes the shadow pill) rather than endogenous, adopted pills. We do so by exploiting the quasi-natural experiment provided by the staggered passage 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.

While other studies have exploited these laws, our paper is the first to focus on the second wave (SW-) PPLs passed during the period 1995 to 2009 and to explore the implications of these laws for actual pill policy and long-term value. Given substantial changes in the underlying legal context since the enactment of first wave PPLs employed by prior studies, we conjecture that results for this later set of laws might well differ from earlier findings. Further, from an identification perspective, focusing on SW-PPLs ensures that we have a pre-treatment period unconfounded by Delaware court decisions that could have impacted the relevance of PPLs for affected firms, thus avoiding the likelihood that measurement error biases our estimates.

Our analysis delivers two main results. First, we show that firms incorporated in PPL adopting states are more likely to adopt new poison pills and maintain their use of existing pills. The result on new pill adoptions, however, is contingent on the ex-ante value of the firm being amongst the lowest (and thus, likely most at risk of takeover) in our sample's distribution of Tobin's Q, suggesting that the shadow pill promotes a "validation effect" for these firms' poison pill policy. Second, we document that the availability of a stronger shadow pill results in significant improvements in value for firms incorporated in the enacting states, and especially for those with existing pills that are targeted in acquisition attempts or for firms more engaged in innovation or with stronger stakeholder relationships. Moreover, using a comprehensive dataset of firm-level visible pills, we confirm the findings of the previous

literature on the determinants of actual pill adoption and its negative association with Tobin's Q. This suggests that a stronger shadow pill is beneficial to shareholders, even if the (endogenous) adoption of an actual pill is not.

Overall, our results support the view that the shadow pill serves a positive corporate governance function for some firms consistent with economic mechanisms that relate to the "bargaining power hypothesis" and the "bonding hypothesis" of takeover defenses. The first channel is rooted in the rationale that strengthening the board's ability to resist a hostile takeover attempt increases the board's power to negotiate with a potential bidder and, ultimately, to extract a higher purchasing price for the benefit of the target's shareholders. The second maintains that the right to adopt a pill increases firm value by re-empowering the board against short-term shareholder interference that can be disruptive of a firm's commitment toward more stable stakeholder relationships or longer-term investments projects, which reduces the costs of contracting.

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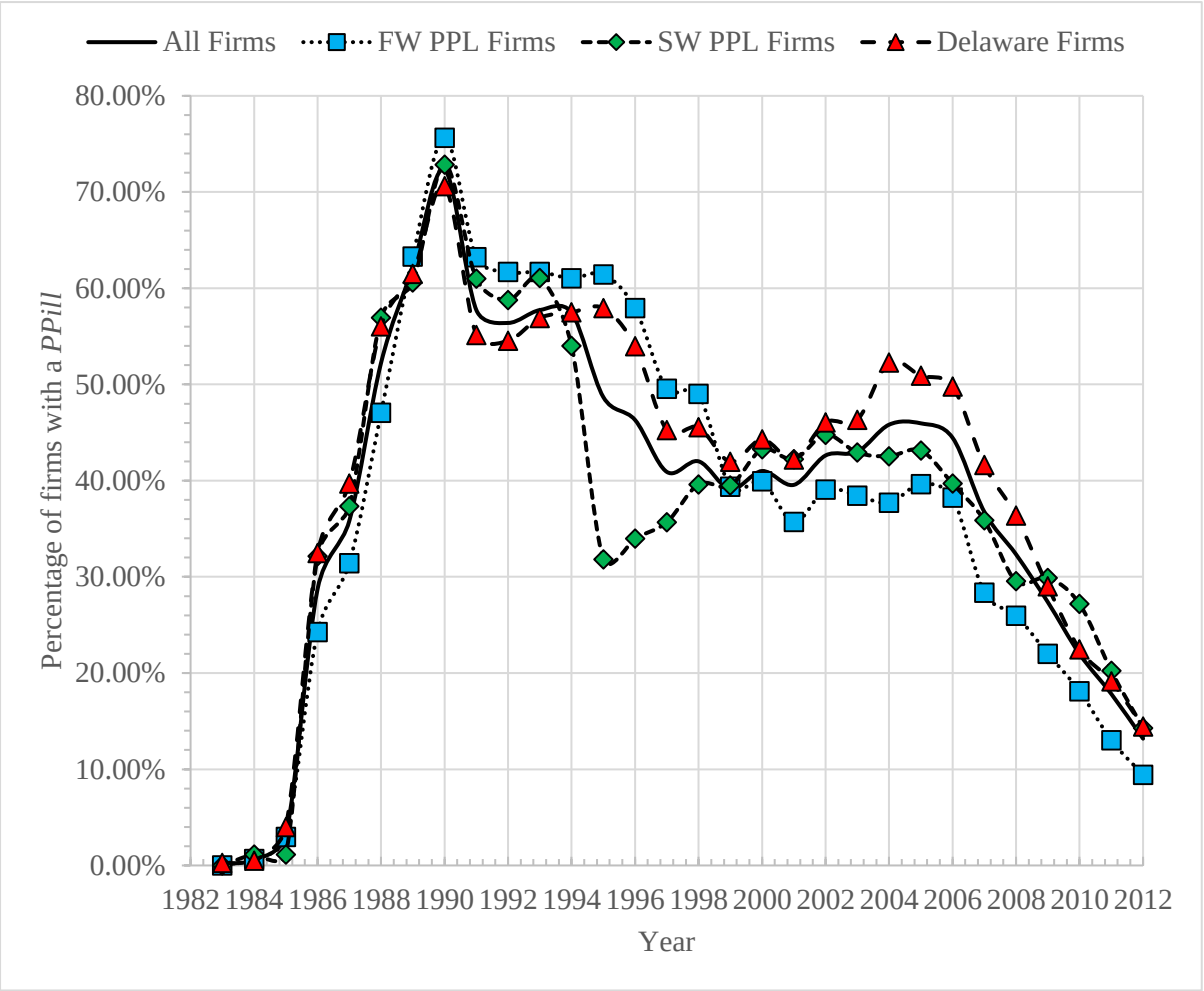

Figure 1
Percentage of firms with a poison pill
This figure plots a solid black line representing the percentage of firms with a poison pill in-place in our sample, each year from 1983 to 2012. We additionally partition the sample into the percentage of firms with a poison pill incorporated in a state that eventually adopts a poison pill law (PPL) between 1986 and 1990 (first wave – FW) with a dashed line and blue squares, 1995 and 2009 (second wave – SW) with a dashed line and green diamonds, and Delaware, which never adopts a PPL with a dashed line and red triangles.

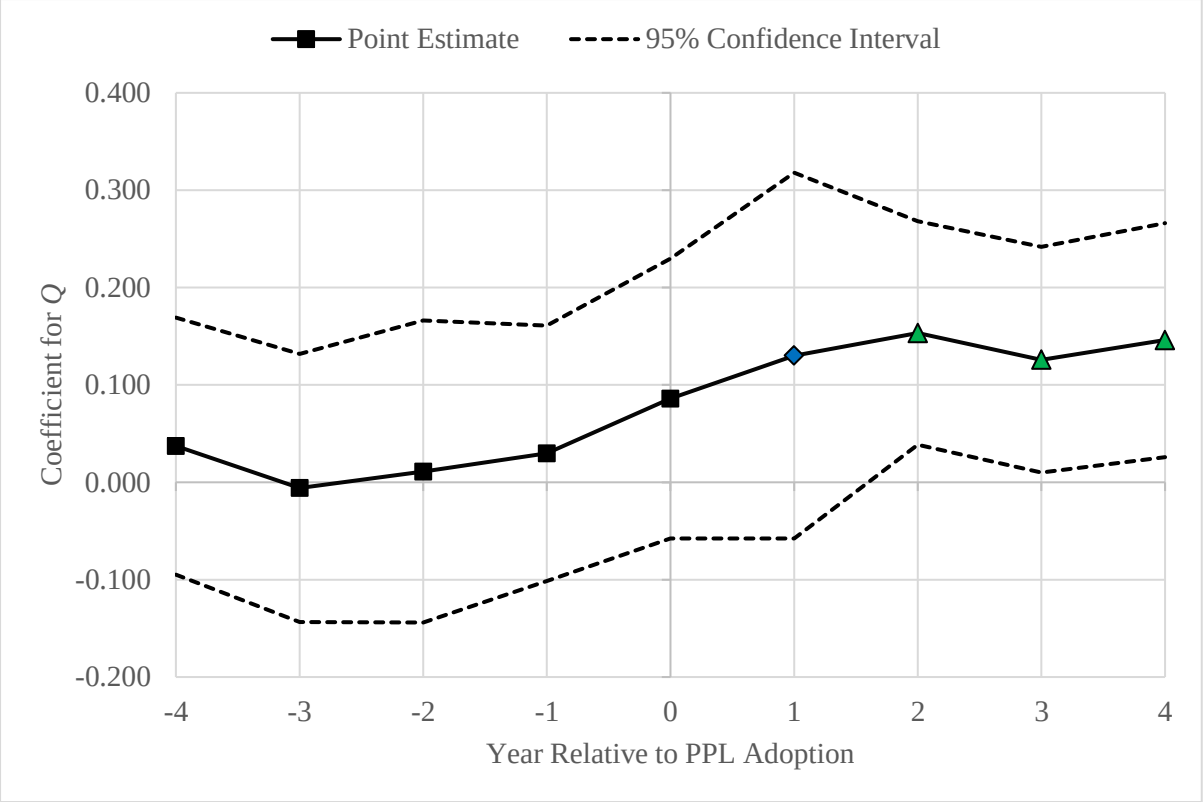

Figure 2
The timing of the PPLs effect on Tobin's Q
This figure plots the coefficient estimates (y-axis) from regressing Q on firm, division-by-year, and industry-by-year fixed effects, four other antitakeover law indicators, and dummies denoting the year relative to the adoption date of a firm's incorporating state's PPL (x-axis) over the period 1992 to 2012. The specification is the same as that reported in Column 1 of Table 5 except that we allow the effect of the law to vary annually in event time. Dashed lines correspond to the 95% confidence intervals of the coefficient estimates, calculated from robust standard errors clustered by the state of incorporation. Green triangles (blue diamonds) denote significance at the 5% (10%) level.

Table 1
First wave PPLs and firm value

	$Q_{[t]}$		$Excess\ Return_{[t]}$		$ROA_{[t]}$		$Total\ Q_{[t]}$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$FW\ PPL_{[t]}$	-0.013	-0.017	0.013	0.011	-0.000	0.000	-0.015	-0.021
	(-0.52)	(-0.62)	(0.57)	(0.46)	(-0.16)	(0.01)	(-0.44)	(-0.67)
$PPill_{[t-1]}$	-0.024	-0.042**	-0.017	-0.005	-0.003	-0.005**	-0.024	-0.033
	(-1.22)	(-2.27)	(-1.31)	(-0.33)	(-1.17)	(-2.36)	(-0.99)	(-1.58)
$Ln(Assets)_{[t-1]}$		-0.283***		-0.217***		-0.029***		-0.139***
		(-7.10)		(-15.08)		(-11.55)		(-2.96)
$Ln(Age)_{[t-1]}$		-0.358*		-0.173*		-0.000		-1.011***
		(-1.80)		(-1.99)		(-0.01)		(-3.63)
$SG_{[t-1]}$		0.311***		-0.089**		0.069***		0.399***
		(14.19)		(-2.33)		(10.70)		(13.81)
$Loss_{[t-1]}$		-0.036***		0.053***		-0.010***		-0.076***
		(-5.37)		(3.36)		(-6.19)		(-7.04)
$DEQ_{[t-1]}$		-0.019		0.030**		0.000		0.008
		(-1.67)		(2.40)		(0.25)		(0.78)
$FLIQ_{[t-1]}$		-0.141*		-0.341***		-0.028***		-0.017
		(-1.90)		(-5.13)		(-3.42)		(-0.27)
$CAPX/Assets_{[t-1]}$		0.264*		-0.922***		-0.048**		0.708***
		(2.01)		(-7.23)		(-2.19)		(4.37)
$R\&D/Sales_{[t-1]}$		0.573		0.544		-0.287**		-0.133
		(0.33)		(1.09)		(-2.25)		(-0.07)
$IO_{[t-1]}$		0.029		-0.417***		0.024***		0.120
		(0.61)		(-9.44)		(3.20)		(1.32)
Other antitakeover laws	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
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	9,037	9,037	9,037	9,037	9,037	9,037	9,031	9,031
Adjusted R^2	0.754	0.769	0.058	0.100	0.673	0.700	0.766	0.778

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

Table 2
Second wave PPL adoptions

	(1)	(2)	(3)	(4)
$Inc\ SY\ PPill_{[t-1]}$	0.019	0.024	0.019	0.025
	(1.51)	(1.62)	(1.52)	(1.61)
$Inc\ SY\ \Delta\ PPill_{[t-1]}$	0.003	0.003	0.003	0.003
	(0.39)	(0.40)	(0.45)	(0.40)
$Inc\ SY\ Q_{[t-1]}$	0.003			-0.000
	(0.48)			(-0.01)
$Inc\ SY\ \Delta\ Q_{[t-1]}$	-0.001			0.002
	(-0.41)			(0.51)
$Inc\ SY\ Return_{[t-1]}$		0.005		0.003
		(0.67)		(0.35)
$Inc\ SY\ \Delta\ Return_{[t-1]}$		-0.011		-0.011
		(-1.15)		(-1.27)
$Inc\ SY\ ROA_{[t-1]}$			0.002	0.002
			(0.37)	(0.22)
$Inc\ SY\ \Delta\ ROA_{[t-1]}$			-0.001	-0.001
			(-0.52)	(-0.53)
$BCL_{[t-1]}$	0.098	0.070	0.098	0.071
	(1.57)	(0.95)	(1.62)	(0.92)
$CSL_{[t-1]}$	0.023	0.066	0.082	0.074
	(0.04)	(0.11)	(0.15)	(0.11)
$DDL_{[t-1]}$	-0.023	-0.017	-0.018	-0.017
	(-0.73)	(-0.50)	(-0.61)	(-0.69)
$FPL_{[t-1]}$	-0.021	-0.029	0.029	-0.030
	(-0.30)	(-0.44)	(-0.49)	(-0.42)
$Inc\ SY\ M\&A\ Vol_{[t-1]}$	0.002	-0.001	0.002	-0.000
	(0.09)	(-0.06)	(0.14)	(-0.00)
$Ln(Inc\ State\ GDPPC)_{[t-1]}$	0.139	0.136	0.136	0.136
	(1.39)	(1.21)	(1.39)	(1.25)
$Inc\ State\ GDP\ Growth_{[t-1]}$	-0.006	-0.005	-0.006	-0.005
	(-0.42)	(-0.34)	(-0.42)	(-0.31)
$\%\ Inc\ State\ Republican_{[t-1]}$	-0.015	-0.011	-0.017	-0.013
	(-0.86)	(-0.57)	(-1.02)	(-0.69)
$Ln(Inc\ State\ Pop)_{[t-1]}$	-0.099	-0.096	-0.057	-0.101
	(-0.18)	(-0.18)	(-0.12)	(0.17)
$Inc\ State\ Unemploy_{[t-1]}$	0.000	-0.007	0.000	-0.006
	(0.03)	(-0.37)	(0.01)	(-0.26)
$Inc\ State\ Est\ Entry_{[t-1]}$	-0.019	-0.018	-0.017	-0.017
	(-0.50)	(-0.47)	(-0.47)	(-0.43)
$Inc\ State\ Est\ Exit_{[t-1]}$	0.007	0.005	0.007	0.005
	(0.39)	(0.27)	(0.35)	(0.29)
Incorporation state FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
N	305	305	305	305
Adjusted R^2	0.228	0.264	0.228	0.251

This table presents results from OLS regressions analyzing determinants of second (SW-) PPL adoptions over the period 1992-2012. We define the dependent variable as the passage of an *SW PPL* in a given state at any point in time during the SW. Once a state adopts an *SW PPL*, it is dropped from the sample. The independent variables are lagged one-year, and continuous independent variables are standardized to have zero mean and unit variance. We include the incorporation state and year fixed effects. Continuous variables are winsorized at the 5% level in both tails. t-statistics (clustered by the state of incorporation) are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Table 3
First wave PPLs and firm-level poison pills

	Entire FW-period		Post-*Moran*		Post-*Interco* & *Pillsbury*	
	(1)	(2)	(3)	(4)	(5)	(6)
$FW\ PPL_{[t]}$	0.069	0.072	0.038	0.044	0.076*	0.046*
	(1.32)	(1.38)	(0.59)	(0.68)	(1.85)	(1.76)
$Eventual\ SW\ PPL_{[t]}$		0.027		0.062		-0.262***
		(0.41)		(1.00)		(-4.91)
$Q_{[t-1]}$	0.000	0.000	-0.016	-0.016	0.006	0.008
	(0.00)	(0.01)	(-1.46)	(-1.47)	(0.85)	(1.05)
$Ln(Assets)_{[t-1]}$	0.080***	0.079***	0.102***	0.102***	0.027	0.027
	(4.18)	(4.09)	(3.79)	(3.76)	(0.91)	(0.91)
$Ln(Age)_{[t-1]}$	-0.034	-0.034	0.268***	0.268***	0.147**	0.149**
	(-0.43)	(-0.42)	(2.68)	(2.68)	(2.51)	(2.52)
$HHI_{[t-1]}$	-0.014	-0.014	-0.030*	-0.030*	-0.003	-0.002
	(-0.97)	(-0.97)	(-1.98)	(-1.99)	(-0.25)	(-0.14)
$SG_{[t-1]}$	-0.018***	-0.018***	-0.014***	-0.014***	-0.008**	-0.009**
	(-6.42)	(-6.36)	(-4.40)	(-4.39)	(-2.37)	(-2.47)
$Loss_{[t-1]}$	0.019**	0.019**	0.021**	0.021**	0.005	0.004
	(2.28)	(2.29)	(2.52)	(2.56)	(0.63)	(0.54)
$DEQ_{[t-1]}$	-0.000	-0.000	-0.016***	-0.016***	-0.008**	-0.008**
	(-0.05)	(-0.04)	(-3.03)	(-3.04)	(-2.66)	(-2.56)
$FLIQ_{[t-1]}$	0.025**	0.025**	0.015	0.015	0.012***	0.012**
	(2.56)	(2.55)	(1.48)	(1.46)	(2.77)	(2.55)
$IO_{[t-1]}$	0.085***	0.085***	0.066***	0.066***	0.024**	0.024**
	(6.20)	(6.23)	(4.76)	(4.79)	(2.23)	(2.17)
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	9,037	9,037	6,562	6,562	4,258	4,258
Adjusted R^2	0.709	0.709	0.765	0.765	0.918	0.918

This table presents results from OLS regressions exploring the implications of FW-PPLs for firm-level poison pills over the entire FW-period (1983 to 1993), as well as post-*Moran* (1986 to 1993) and post-*Interco* & *Pillsbury* (1989 to 1993). 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 and 2009. The "Other antitakeover laws" include BCL, CSL, DDL, and FPL. Unreported insignificant predictors include $CAPX/Assets$ and $R\&D/Sales$. Division fixed effects are measured using U.S. Census

divisions, and industry fixed effects are defined by two-digit SIC codes. Continuous variables are winsorized at the 5% level in both tails. Internet Appendix A provides variable definitions. t-statistics (clustered by the state of incorporation) are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Table 4
Firm-level poison pill adoptions

Panel A: Explaining the adoption and maintenance of $PPill$

	(1)	(2)	(3)	(4)
$PPL_{[t]}$	0.058*	0.030		
	(1.97)	(1.20)		
$Q_{[t-1]}$	-0.050***	-0.030***		
	(-9.13)	(-7.29)		
$PPL_{[t]} \times Q(Lowest)_{[t-1]}$			0.087**	0.053*
			(2.54)	(1.79)
$PPL_{[t]} \times Q(Low)_{[t-1]}$			0.072**	0.033
			(2.26)	(1.27)
$PPL_{[t]} \times Q(High)_{[t-1]}$			0.060*	0.031
			(1.88)	(1.17)
$PPL_{[t]} \times Q(Highest)_{[t-1]}$			0.017	0.002
			(0.49)	(0.09)
$Ln(Assets)_{[t-1]}$		0.088***		0.099***
		(5.25)		(6.03)
$Ln(Age)_{[t-1]}$		0.343***		0.347***
		(14.11)		(14.34)
$HHI_{[t-1]}$		-0.020**		-0.020**
		(-2.33)		(-2.31)
$SG_{[t-1]}$		-0.012***		-0.016***
		(-3.21)		(-3.83)
$Loss_{[t-1]}$		-0.005		-0.002
		(-0.75)		(-0.38)
$DEQ_{[t-1]}$		0.009**		0.011***
		(2.38)		(2.86)
$FLIQ_{[t-1]}$		-0.022***		-0.025***
		(-3.50)		(-4.02)
$CAPX/Assets_{[t-1]}$		-0.010**		-0.013***
		(-2.41)		(-2.98)
$R\&D/Sales_{[t-1]}$		-0.014*		-0.012
		(-1.77)		(-1.55)
$IO_{[t-1]}$		0.038***		0.035***
		(5.32)		(4.95)
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	26,248	26,248	26,248	26,248
Adjusted R^2	0.577	0.601	0.573	0.600

Panel B: The implications of PPLs for:

	$New\ PPill_{[t]}$		$Ln(PPill\ Duration)_{[t]}$	
	(1)	(2)	(3)	(4)
$PPL_{[t]}$	0.053		0.179*	
	(1.19)		(1.90)	
$Q_{[t-1]}$	-0.019***		-0.094***	
	(-4.24)		(-7.83)	
$PPL_{[t]} \times Q(Lowest)_{[t-1]}$		0.084*		0.272**
		(1.75)		(2.66)
$PPL_{[t]} \times Q(Low)_{[t-1]}$		0.076		0.196**
		(1.62)		(2.12)
$PPL_{[t]} \times Q(High)_{[t-1]}$		0.030		0.191*
		(0.64)		(1.96)
$PPL_{[t]} \times Q(Highest)_{[t-1]}$		0.020		0.082
		(0.46)		(0.76)
Other antitakeover laws	Yes	Yes	Yes	Yes
Control variables	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	6,372	6,372	17,482	17,482
Adjusted R^2	0.224	0.224	0.523	0.523

This table presents results from OLS regressions analyzing the implications of PPLs for firm-level poison pill dynamics over the sample period 1992 to 2012. The dependent variable in Panel A is an indicator for whether a firm has a poison pill in-place as of the current year ($PPill$). We define the dependent variables in Panel B as an indicator for the first time a firm adopts a poison pill ($New\ PPill$), and a count variable for the number of years a firm has a pill in-place ($Ln(PPill\ Duration)$). Specific to the first two columns, once a firm adopts a new pill, it is excluded from the sample. We only include firms that eventually adopt a pill in these regressions. In both panels, the independent variables are lagged one-year, and continuous independent variables are standardized to have zero mean and unit variance. 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 two-digit SIC codes. Continuous variables are winsorized at the 5% level in both tails. Internet Appendix A provides variable definitions. t-statistics (clustered by the state of incorporation) are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Table 5
PPLs and Tobin's Q

	(1)	(2)	(3)	(4)	(5)
$PPL_{[t]}$	0.105***	0.095***	0.091***	0.089***	0.083**
	(3.66)	(3.30)	(2.76)	(2.83)	(2.01)
$PPill_{[t-1]}$	-0.139***	-0.069***	-0.072***	-0.067***	-0.067***
	(-6.47)	(-3.09)	(-2.92)	(-3.02)	(-3.20)
$PPL_{[t]} \times PPill_{[t-1]}$			0.011		
			(0.31)		
$Ln(Assets)_{[t-1]}$		-0.327***	-0.327***	-0.325***	-0.334***
		(-19.64)	(-19.67)	(-22.41)	(-22.10)
$Ln(Age)_{[t-1]}$		-0.071	-0.070	-0.067	-0.062
		(-1.38)	(-1.37)	(-1.16)	(-0.95)
$HHI_{[t-1]}$		0.037	0.038	0.037	0.061
		(0.59)	(0.60)	(0.59)	(0.96)
$SG_{[t-1]}$		0.416***	0.416***	0.416***	0.420***
		(18.41)	(18.42)	(17.92)	(20.47)
$Loss_{[t-1]}$		-0.042***	-0.042***	-0.043***	-0.039***
		(-5.22)	(-5.22)	(-5.34)	(-4.83)
$DEQ_{[t-1]}$		-0.060***	-0.060***	-0.061***	-0.060***
		(-7.01)	(-7.00)	(-8.10)	(-5.91)
$FLIQ_{[t-1]}$		0.187***	0.187***	0.183***	0.200***
		(4.51)	(4.50)	(4.28)	(4.56)
$CAPX/Assets_{[t-1]}$		0.755***	0.755***	0.731***	0.678***
		(4.55)	(4.55)	(4.36)	(3.44)
$R\&D/Sales_{[t-1]}$		-0.394	-0.394	-0.407	-0.282
		(-0.83)	(-0.83)	(-0.88)	(-0.70)
$IO_{[t-1]}$		0.165***	0.165***	0.160***	0.168***
		(4.66)	(4.63)	(4.37)	(4.47)
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	26,254	26,254	26,254	26,254	26,254
Adjusted R^2	0.649	0.674	0.674	0.674	0.674

This table presents results from OLS regressions analyzing the value implications of PPLs over the period 1992 to 2012. The dependent variable is Tobin's Q (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 two-digit SIC codes. Continuous variables are winsorized at the 5% level in both tails. t-statistics (clustered by the state of incorporation) are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Table 6
PPLs and Tobin's Q 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)
$Divisions_{[t]}$	5.511	5.652	-0.140
	(2.388)	(2.571)	(-0.65)
N (by group)	264	264	

Panel B: The effect of PPLs on Q

	(1)	(2)	(3)
$Treated_{[t]} \times Post_{[t]}$	0.087**	0.098**	0.107**
	(2.10)	(2.13)	(2.40)
$PPill_{[t-1]}$	-0.047	-0.035	-0.046
	(-0.68)	(-0.46)	(-0.87)
Other antitakeover laws	Yes	Yes	Yes
Control variables	No	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	2,620	2,620	2,620
Adjusted R^2	0.729	0.752	0.724

This 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-stats in parentheses). Panel B shows the matched sample Q regression results over $t \pm 3$ estimation windows. The "Other antitakeover laws" include BCL, CSL, DDL, and FPL. "Control variables" include $Ln(Assets)$, $Ln(Age)$, HHI, SG, $Loss$, DEQ, $FLIQ$, $CAPX/Assets$, $R\&D/Sales$, and IO. Division fixed effects are measured using U.S. Census divisions, and industry fixed effects are defined by two-digit SIC codes. $Treated$ and $Post$ are omitted due to collinearity with fixed effects. Continuous variables are winsorized at the 5% level in both tails. t-statistics (clustered by the state of incorporation) are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Table 7
Alternative value measures

Panel A: The implications of PPLs for:

	$Excess\ Return_{[t]}$		$ROA_{[t]}$		$Total\ Q_{[t]}$	
	(1)	(2)	(3)	(4)	(5)	(6)
$PPL_{[t]}$	0.032**		0.006**		0.104***	
	(2.21)		(2.14)		(2.79)	
$Treated_{[t]} \times Post_{[t]}$		0.043**		0.010**		0.105**
		(2.40)		(2.09)		(2.50)
$PPill_{[t-1]}$	-0.012	-0.018	-0.003***	-0.013	-0.101***	-0.093
	(-1.01)	(-0.32)	(-2.85)	(-1.62)	(-5.21)	(-0.90)
Other antitakeover laws	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	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	26,253	2,621	26,239	2,578	26,225	2,570
Adjusted R^2	0.114	0.183	0.671	0.772	0.666	0.757

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)	1.048**	0.179	0.989**	1.007**	0.092	1.005**	0.926**	0.148	0.856*
	(2.28)	(0.46)	(2.15)	(2.21)	(0.24)	(2.23)	(2.09)	(0.41)	(1.92)
Average # firms	45.11	45.58	-	45.11	45.58	-	45.11	45.58	-
N	252	254	-	252	254	-	252	254	-
Adjusted R^2	0.278	0.372	0.033	0.280	0.367	0.037	0.271	0.361	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)	0.681	-0.136	0.991**	0.623	-0.225	1.014**	0.576	-0.165	0.882**
	(1.49)	(-0.37)	(2.23)	(1.37)	(-0.63)	(2.35)	(1.30)	(-0.48)	(2.04)
Average # firms	49.81	50.07	-	49.81	50.07	-	49.81	50.07	-
N	253	254	-	253	254	-	253	254	-
Adjusted R^2	0.297	0.402	0.029	0.297	0.396	0.033	0.295	0.392	0.004

This table examines the effect of PPLs on firm value using alternative measures. Panel A reports results from OLS regressions on both the full sample over the period 1992 to 2012 and the matched sample with $t \pm 3$ estimation windows. The dependent variables include $Excess\ Return$, ROA, and $Total\ Q$. The "Other antitakeover laws" include BCL, CSL, DDL, and FPL. "Control variables" include $Ln(Assets)$, $Ln(Age)$, HHI, SG, $Loss$, DEQ, $FLIQ$, $CAPX/Assets$, $R\&D/Sales$, and IO. Division fixed effects are measured using U.S. Census divisions, and industry fixed effects are defined by two-digit SIC codes. t-statistics (clustered by the state of incorporation) are reported in parentheses. 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. 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 a value-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. In both panels, continuous variables are winsorized at the 5% level in both tails. Internet Appendix A provides variable definitions. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Table 8
Testing the bargaining power hypothesis

Panel A: PPLs and takeover propensities

	$Bid_{[t]}$		$Acquired_{[t]}$		$Acquired_{[t]}$ (only takeover targets)	
	(1)	(2)	(3)	(4)	(5)	(6)
$PPL_{[t]} \times PPill_{[t]}$		0.010		0.005		0.135
		(0.22)		(0.35)		(1.05)
$PPL_{[t]}$	-0.026	-0.028	-0.002	-0.004	0.009	-0.063
	(-1.26)	(-1.31)	(-0.14)	(-0.28)	(0.07)	(-0.40)
$PPill_{[t-1]}$	0.022	0.020	-0.001	-0.002	-0.027	-0.070
	(1.25)	(1.07)	(-0.18)	(-0.52)	(-0.59)	(-1.07)
Other antitakeover laws	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Division × Year FE	Yes	Yes	Yes	Yes	No	No
Industry × Year FE	Yes	Yes	Yes	Yes	No	No
Division FE	No	No	No	No	Yes	Yes
N	24,653	24,653	24,653	24,653	257	257
Adjusted R^2	0.043	0.043	0.095	0.095	0.144	0.144

Panel B: PPLs and takeover premiums

	$Total\ Premium_{[t]}$		$Premium\ Increase_{[t]}$		
	(1)	(2)	(3)	(4)	(5)
$PPL_{[t]} \times PPill_{[t]}$		0.077**		0.139**	0.146**
		(2.18)		(2.28)	(2.42)
$PPL_{[t]}$	0.043	0.004	0.107	0.030	0.024
	(0.68)	(0.06)	(1.48)	(0.38)	(0.30)
$PPill_{[t]}$	0.049***	0.024*	0.093***	0.048**	0.049**
	(3.12)	(1.86)	(3.11)	(2.41)	(2.37)
$Morning\text{-}After\ PPill_{[t]}$					0.096**
					(2.19)
Other antitakeover laws	Yes	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes	Yes
Division FE	Yes	Yes	Yes	Yes	Yes
N	257	257	257	257	257
Adjusted R^2	0.039	0.053	0.104	0.127	0.133

This 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 and $PPill$ on takeover propensities. The dependent variables include Bid and $Acquired$, where the last two columns only consider firms that receive takeover bids. 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 two-digit SIC codes. Panel B explores the effect of PPLs and $PPill$ and $Morning\text{-}After\ PPill$ on takeover premiums. The dependent variables include $Total\ Premium$ and $Premium\ Increase$. $Total\ Premium$ ($Premium\ Increase$) 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. "Control variables" include $Multibid$, $Return$, $Ln(Assets)$, $Ln(Age)$, HHI, SG, $Loss$, DEQ, $FLIQ$, $CAPX/Assets$, $R\&D/Sales$, and IO. Continuous variables are winsorized at the 5% level in both tails. Internet Appendix A provides variable definitions. t-statistics (clustered by the state of incorporation) are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Table 9
Testing the bonding hypothesis

Panel A: Heterogeneous effects of PPLs for innovative firms

	(1)	(2)	(3)[a]	(4)
$PPL_{[t]} \times R\&D/Sales_{[t-1]}$	2.139**			
	(2.59)			
$PPL_{[t]} \times Intangible\ Capital_{[t-1]}$		0.198***		
		(3.04)		
$PPL_{[t]} \times CW\ Patents_{[t-1]}$			0.027**	
			(2.26)	
$PPL_{[t]} \times RQ_{[t-1]}$				0.780*
				(1.78)
$PPL_{[t]}$	0.036	-0.014	0.073**	-0.030
	(1.28)	(-0.30)	(2.30)	(-0.29)
$R\&D/Sales_{[t-1]}$	-0.640*			
	(-1.69)			
$Intangible\ Capital_{[t-1]}$		-0.509***		
		(-15.41)		
$CW\ Patents_{[t-1]}$			-0.000	
			(-0.02)	
$RQ_{[t-1]}$				0.303
				(1.41)
$PPill_{[t-1]}$	-0.070***	-0.061***	-0.062**	-0.100**
	(-3.12)	(-2.70)	(-2.52)	(-2.24)
Other antitakeover laws	Yes	Yes	Yes	Yes
Control variables	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	26,254	26,254	24,231	11,620
Adjusted R^2	0.675	0.679	0.680	0.669

[a] Our patent data ends in 2010.

Panel B: Heterogeneous effects of PPLs for stakeholder relationships

	(1)	(2)	(3)	(4)
$PPL_{[t]} \times Organizational\ Capital_{[t-1]}$	0.365***			
	(2.84)			
$PPL_{[t]} \times Large\ Customer_{[t-1]}$		0.056*		
		(1.79)		
$PPL_{[t]} \times Strategic\ Alliance_{[t-1]}$			0.101*	
			(1.83)	
$PPL_{[t]} \times Labor\ Intensity_{[t-1]}$				0.266***
				(3.73)
$PPL_{[t]}$	-0.009	0.101***	0.039	0.085***
	(-0.19)	(3.35)	(0.83)	(2.84)
$Organizational\ Capital_{[t-1]}$	-0.283***			
	(-3.03)			
$Large\ Customer_{[t-1]}$		0.037***		
		(2.91)		
$Strategic\ Alliance_{[t-1]}$			-0.063*	
			(-1.84)	
$Labor\ Intensity_{[t-1]}$				-0.154
				(-0.63)
$PPill_{[t-1]}$	-0.073***	-0.069***	-0.069***	-0.068***
	(-3.03)	(-3.10)	(-3.09)	(-3.15)
Other antitakeover laws	Yes	Yes	Yes	Yes
Control variables	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	26,254	26,254	26,254	25,965
Adjusted R^2	0.669	0.674	0.675	0.676

This table presents results from OLS regressions analyzing the heterogeneous value implications of PPLs for firms that are more innovative or reliant on stakeholder relationships over the period 1992 to 2012. Panel A (B) interacts PPL with the following proxies for innovation (stakeholder relationships): $R\&D/Sales$, $Intangible\ Capital$, $CW\ Patents$, and RQ ($Organizational\ Capital$, $Large\ Customer$, $Strategic\ ALliance$, and $Labor\ Intensity$). In both panels, the dependent variable is Tobin's Q (Q). The "Other antitakeover laws" include BCL, CSL, DDL, and FPL. "Control variables" include $Ln(Assets)$, $Ln(Age)$, HHI, SG, $Loss$, DEQ, $FLIQ$, $CAPX/Assets$, $R\&D/Sales$, and IO. Division fixed effects are measured using U.S. Census divisions, and industry fixed effects are defined by two-digit SIC codes. Continuous variables are winsorized at the 5% level in both tails. Internet Appendix A provides variable definitions. t-statistics (clustered by the state of incorporation) are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.