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Shadow Pills, Visible Pill Policy, and Firm Value

Finance Working Paper N° 595/2019
January 2022

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Abstract

We analyze the impact of the right to adopt a poison pill – a "shadow pill" – on visible pill policy and firm value by exploiting the quasi-natural experiment provided by U.S. states' staggered adoption of poison pill laws (PPLs) validating the pill. We document that by reducing uncertainty about pill validity, PPLs make visible pill policy more closely aligned with economic incentives, increasing pill adoption among low-valuation firms but decreasing it among high-valuation firms. Further, PPLs positively impact firm value for innovative firms with more intangible assets, which are more likely to benefit from the commitment to long-term investment strategies.

Keywords: Poison pill, antitakeover statutes, shadow pill, firm value

JEL Classifications: G32, G34, K22, O32

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Shadow Pills, Visible 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 visible pill policy and firm value by exploiting the quasi-natural experiment provided by U.S. states' staggered adoption of poison pill laws (PPLs) validating the pill. We document that by reducing uncertainty about pill validity, PPLs make visible pill policy more closely aligned with economic incentives, increasing pill adoption among low-valuation firms but decreasing it among high-valuation firms. Further, PPLs positively impact firm value for innovative firms with more intangible assets, which are more likely to benefit from the commitment to long-term investment strategies.

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Introduction

Law and finance scholars generally agree that the poison pill (formally known as a "shareholder rights plan") is among the most powerful antitakeover defenses (e.g., Malatesta and Walkling 1988; Ryngaert 1988; Comment and Schwert 1995; Coates 2000; Cremers and Ferrell 2014). While details vary across different implementations, the basic defensive mechanism of the pill provides existing shareholders with stock purchase rights that entitle them to acquire newly issued shares at a substantial discount in the "trigger" event that a hostile bidder obtains more than a pre-specified percentage of the company's outstanding shares, while withholding such rights from the hostile bidder. As a result, poison pills grant the board of directors the ability to dilute the ownership stake of a hostile bidder substantially, giving the board de facto veto power over any hostile acquisition.

After the Delaware Supreme Court validated the use of the pill in 1985, a significant literature investigated whether the adoption of a poison pill is beneficial or detrimental to shareholder interests. While earlier findings were mixed,[†] over the past decade, empirical studies have found that the adoption of a pill is negatively associated with firm value (e.g., Bebchuk, Cohen and Ferrell 2009; Cuñat, Gine, and Guadalupe 2012; Cremers and Ferrell 2014). However, this result is difficult to interpret, as the decision to employ a pill is endogenous and poison pills can be unilaterally adopted by the board of directors, so that even firms that do not currently have a pill in place still have a "shadow pill" (Coates 2000). The availability of the shadow pill exacerbates endogeneity concerns, as reverse causality or other omitted variables might explain both the board's decision to adopt a poison pill and the reported negative association between the adoption of a pill and firm value (Comment and Schwert 1995; Catan 2019).

In this paper, we contribute to the debate on the association between poison pills and firm value by shifting the focus from "visible" pills to "shadow pills" – i.e., studying the *right to adopt* a poison pill

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

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

Our main findings are twofold. First, after the passage of PPLs, firms with lower valuations (before the laws' adoption) increase their use of visible poison pills, while pill usage becomes relatively less common for firms with high valuation. Second, we show that the Tobin's Q of the companies incorporated in states that adopt a PPL increases significantly relative to similar firms incorporated elsewhere, while confirming that the association between visible pills and Tobin's Q is negative and due to reverse causality (Catan, 2019). These combined results support the interpretation that PPLs reduce legal uncertainty about pill validity, which in turn increases the correlation between economic incentives to adopt a pill and actual pill adoption. Our results underline the relevance of disentangling the value implications of the shadow pill from the ex-post endogenous decision to put an actual pill in-place.

Ex-ante, the relationship between PPLs and visible pill adoption is unclear. On the one hand, firms with a strong shadow pill might be less likely to use actual pills, under the assumption that the *threat* to be able to swiftly adopt a pill is enough of a deterrent to thwart a hostile takeover bid (what we call a "substitution effect"). On the other hand, the existence of frictions (like the threat of judicial challenge) for quick adoption of a pill may still lead some firms to adopt the visible pill. In this case, the passage of PPLs could make corporate boards less hesitant to do so, as they would be less worried about subsequent legal challenges and/or about the reputational harm of being viewed as a "pro-pill" board (Johnson, Karpoff, and Wittry 2019) (what we call a "validation effect"). Accordingly, we argue that decision to adopt a pill depends on two factors: 1) the economic benefits of the pill, which increase with the risk of takeover; and 2) the beliefs about the legal validity of the pill. Passage of PPLs reduces the importance of the second consideration (i.e., making the pill's validity more certain), and hence increases the correlation between actual pill adoption and the pill's economic benefits. Consistent with

that, our evidence suggests that passage of PPLs increase pill adoption by firms with low Tobin's Q (which are more likely to be worried about takeover risk, see, e.g., Edmans, Goldstein, and Jiang 2012)), and reduces adoption by firms with high Q, relative to a median firm.

As to the finding that firm value tends to increase after PPL adoption, we explore two economic mechanisms as explanations. First, the "commitment hypothesis" holds that the shadow pill promotes value enhancement for some firms by decreasing the likelihood of a sizable shift in firm's operations, which allows for a safer pursue of long-term plans and more profitable use of some assets, especially intangible ones. Firms that rely more heavily on intangible assets, such as R&D, organizational capital, or non-contractual relationships with various stakeholders, are more likely to be subject to asymmetric information and require more long-term horizon for their operation, and hence should benefit from PPLs more (Core, Holthausen and Larcker 1999; Duru, Wang and Zhao 2013; Cremers, Litov, Sepe 2017). Second, the "bargaining power hypothesis" holds that the strengthened legal validity granted to visible poison pills by PPLs increases firm value by helping boards of directors to bargain for a higher purchasing price after being targeted in a hostile takeover contest (Tirole, 2006).

We find that firm value increases more for firms that are R&D-intensive and have higher levels of intangible capital, consistent with the commitment hypothesis. At the same time, we find suggestive evidence that the likelihood of a takeover bid decreases, while takeover premia increase, also consistent with bargaining power hypothesis.

Because our study relies on PPLs to identify how a strengthened shadow pill affects visible pill policy and firm value, it is useful to provide some background on the legal environment relevant to these laws (which also informs our empirical strategy, following recent work such as, e.g., Catan and Kahan 2016; Cain, McKeon, and Solomon 2017; Karpoff and Wittry 2018). To start, we assume that the legal validity of the pill was arguably fairly certain after the Delaware Supreme Court's validation of the pill in their 1985 *Moran* v. *Household International, Inc.* decision, due to the pervasive influence of Delaware case law over other jurisdictions (e.g., Ryngaert 1988; Cremers and Ferrell 2014). In 1988, however, two subsequent Delaware decisions – *City Capital Associates* v. *Interco Inc.* and *Grand Metropolitan PLC* v. *Pillsbury Co.* – restricted the board's ability to maintain a pill indefinitely, creating novel uncertainty for the general application of pills (Catan and Kahan 2016). Indeed, in a memo sent

to the clients of the law firm Wachtell, Lipton, Rosen & Katz in 1988 after these decisions, Martin Lipton (the corporate lawyer who "invented" the poison pill) wrote: "The Pillsbury decision yesterday fulfills the threat to Delaware corporations presaged by the Interco decision…The effect of the Pillsbury decision will be disastrous for American business… [threatening] the effective use of the poison pill …" (Martin Lipton Memos, p. 146).

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

These changes pertaining to the legal validity of the right to adopt a poison pill motivate our focus on PPLs adopted during the period 1995 to 2009 – which we term the "second wave" (SW) of adoptions – rather than on PPLs passed between 1986 and 1990 (i.e., the "first wave" (FW) of adoptions) considered in prior studies. Starting in 1995 ensures that we have a relatively stable pre-treatment period (i.e., not confounded by the aforementioned Delaware court decisions or the hostile takeover wave of the 1980s), which helps address identification concerns. Additionally, the value implications of SW-PPLs have never been studied and, given the above changes in the legal environment, a priori it is reasonable to expect that results for this later set of PPLs might differ from results obtained by prior studies using FW-PPLs.[§]

[‡] While subsequent decisions in Delaware have ruled in favor of the pill (e.g., *Paramount Communications, Inc.* v. *Time, Inc.*; *Air Products and Chemicals, Inc.* v. *Airgas, Inc.*), we interpret the fact that similar cases are continually tried as indication that the pill's status in Delaware is *less* certain than in states with PPLs.

[§] Our results for FW-PPLs are in line with the prior literature. In particular, Karpoff and Wittry (2018) document that prior studies using business combination laws (BCLs) are potentially plagued by an omitted variable problem if they do not account for the legal context – including the passage of PPLs. They show that PPLs adopted during their sample period (i.e., 1976-1995) are negatively associated with return on assets (ROA), although this negative association becomes insignificant in their subsequent tests that control for firm-level defenses, such as visible

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

Our principal findings on the effect of SW-PPLs on visible pill policy and long-term value are estimated using difference-in-differences regressions that include firm, U.S. Census headquarters division-by-year, and industry-by-year fixed effects. First, we show that SW-PPLs significantly increased the propensity of affected firms with lower ex ante valuations to adopt visible poison pills, while reducing pill adoption among firms with higher ex ante valuations, relative to a median firm. Second, we document that a strengthened shadow pill, as enabled by the passage of PPLs, results in an economically and statistically significant increase of 4-5% on average in firms' Tobin's Q.

Our results are robust to various ways of constructing the sample. While in the main specification we focus on all firms for which we were able to collect data on the visible pill, the positive value effects are also estimated in both narrower and broader samples. We construct the narrower sample by performing a matching procedure, where each firm in a state that passes a SW-PPL is matched to a set of control of firms that are most similar in terms of Q, size, and visible pill but incorporated in a state without a SW-PPL. The broader sample is obtained by no longer requiring that visible pill data are available and including all public firms in both treated and control states. Both approaches confirm positive and significant impact of PPLs on firm value. Furthermore, we find analogous results of increased value after SW-PPL adoptions using other metrics for firm value than Tobin's Q: Total Q (Peters and Taylor 2017) and excess stock returns. We also conduct a long-term event study surrounding the adoption of SW-PPLs, employing long (short) portfolios that buy treated (control) stocks from our

poison pills (similar to prior work, e.g., Karpoff and Malatesta 1989). Consistent with their results, we show that firms incorporated in states adopting FW-PPLs did not experience significant changes in Tobin's Q, excess stock returns, or ROA (see Online Appendix Tables OA1), supporting our conjecture that FW-PPLs were not material due to arguably all firms having strong shadow pills in the period 1985-1988.

matched sample around the time their (matched sample counterpart's) state of incorporation adopts a law, and document a positive and significant alpha for the resulting long-short portfolio.

Next, we examine two possible economic explanations for our finding that the shadow pill seems to positively contribute to firm value; namely the "commitment hypothesis" and the "bargaining power hypothesis" (e.g., Stulz, 1988; Berkovitch and Khanna, 1990; Kadyrzhanova and Rhodes-Kropf, 2011). Under the commitment hypothesis, limiting the ability of shareholders to disrupt a firm's long-term strategy – including by strengthening a firm's shadow pill – serves as a commitment device that binds the shareholders to the firm's current long-term strategy and to a cooperative relationship with the board. Such commitment is especially valuable for firms with large share of intangible assets, that are more subject to asymmetric information and hence may be undervalued by outsiders (Core, Holthausen and Larcker, 1999; Duru, Wang and Zhao, 2013; Cremers, Litov, Sepe, 2017). This mechanism is also valuable for firms that rely on important relationships with external stakeholders, which is often referred to as the bonding hypothesis (e.g., Laffont and Tirole 1988; Shleifer and Summers 1988; Johnson, Karpoff, and Yi 2015). Under the bargaining power hypothesis, instead, having the right to adopt a poison pill strengthens the negotiating position of the board vis-à-vis any potential bidder, allowing directors to obtain a higher offer price for the target's shareholders.

In support of the commitment hypothesis, we find that firms incorporated in a state that adopts a PPL and for which intangible assets and thus asymmetric information concerns are likely more relevant – such as firms that are more engaged in research and development or have higher levels of intangible capital – experience a higher increase in Tobin's Q. Consistent with recent work showing that investments in intangibles have been steadily increasing over time (e.g., Corrado and Hulten 2010; Peters and Taylor 2017), this finding suggests that the commitment hypothesis would have become increasingly more important during the SW-PPL period. Conceptually, the same logic may also imply that firms with more relationships with external stakeholders, such as large suppliers or customers, are also more likely to benefit from PPL. Yet, we find only limited evidence of this being the case, suggesting that intangible asset within the firm and the relationship with firm's insiders (managers, employees) is the driving force of the mechanism.

We also find some evidence in support of the bargaining power hypothesis. Specifically, firms with a pill in place are less likely to receive a takeover bid and receive a higher premium increase after their state adopts a PPL. However, the evidence in favor of bargaining power hypothesis is only marginally statistically significant and, given relatively low levels of takeover activity, seems at best a partial explanation of the main results.

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

1. Data and Empirical Framework

1.1. Sample selection, definition of variables and descriptive statistics

We start the construction of our primary dataset by combining information on firm-level poison pills from two institutional providers and one prior academic study. The institutional data providers include the Institutional Shareholder Services (ISS) Governance and the Securities Data Companies

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

(SDC) Corporate Governance databases. We supplement these observations with poison pill data from Cremers, Litov and Sepe (2017). While the ISS and Cremers, Litov and Sepe (2017) data are panel datasets, SDC data contains only information about pill introduction and expected duration. Given that expected pill duration may differ from actual pill duration, we employ the following conservative procedure to include SDC data. We assume that: (i) there was no pill 2 years before the first adoption for a given firm (our results remain similar if instead we use 1 or 3 year windows); (ii) the pill was in place between two adoption events if the expected expiration for the first pill coincides with the adoption date for the second pill; and (iii) for the last adoption event the pill was in place in the year of adoption, but not necessarily later (and hence we code observations in later years as missing). We obtain similar results if we assume that the pill remained in place for 1 or 2 years longer. This procedure ensures the interpolation of the visible pill between adoption events but performs only limited extrapolation before the first and after the last adoption event. In robustness checks, we also utilize pill data from Catan (2019).

The resulting sample contains firm-level poison pill ($PPill$) information on 5,445 unique firms between 1983 and 2012, which we merge with the industrial firms (excluding utilities and financials) in the CRSP-Compustat database. To be included in the sample, we require that firms are incorporated and headquartered in the U.S. with non-missing or non-negative book value of assets or net sales and without missing observations for the dependent and independent variables used in our baseline regression models. This selection criterion results in a panel with about 40 thousand 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 (FW- PPL and SW-PPL, respectively).

Our study's key independent variable, PPL, is an indicator capturing whether a firm is incorporated in a state that has passed a PPL at any point between 1986 and 2009. We obtain information on whether states have passed one of these laws from Cain, McKeon, and Solomon (2017) and Karpoff and Wittry (2018) and report each state's adoption date in Online Appendix Table OA2. To obtain historical

incorporation data, we start with the database maintained by Holger Spamann (the "Spamann data").[††] While our hand checks confirmed that the accuracy of this dataset is superior to other data sources, the coverage of the data only starts in 1994, and remains limited until 1996. Backfilling states of incorporation results in errors that, as we checked, may in fact be consequential for the conclusions of the analysis. For that reason, we supplement the Spamann data with incorporation and location information from Compact Disclosure data 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.[‡‡] Combining law adoption dates and historical incorporation data, we construct the indicator variable, PPL, which is set equal to one in the adoption year and afterwards for all firms incorporated in the enacting states and set to zero in the years prior to adoption of the law. PPL always equals zero for firms in states that never passed a PPL, including firms incorporated in Delaware.[§§]

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

We employ Tobin's Q (Q) as our main measure of firm value, consistent with prior work examining the value relevancy of corporate governance arrangements (e.g., Morck, Shleifer, and Vishny 1988; Gompers, Ishii, and Metrick 2003; Cremers, Litov and Sepe 2017). We follow Fama and French (1992)

[††] The database is available at https://corpgov.law.harvard.edu/2020/01/02/a-new-dataset-of-historical-states-of-incorporation-of-u-s-stocks-1994-2019/.

[‡‡] We backfill states of incorporation (and location) for firm-years prior to 1986 using the oldest observation from either the Compact Disclosure or CRSP Historical database. Since backfilling may introduce some errors in the data, as we have learned analyzing the data from 1990s, the results based on pre 1986 data should be interpreted with caution. Those results, however, are not central to our paper.

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

and define Q as the ratio of market to book value of assets using financial data from Compustat. Using this definition of Tobin's Q, our dependent variable in most regressions is its natural logarithm ($Ln(Q)$) as in, for example, Bebchuk, Cohen, and Ferrell (2009) and Atanassov (2013).

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

Following Karpoff and Wittry (2018), we include controls 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). We further exclude firms with observed lobbying activity for specific antitakeover statutes (Karpoff and Wittry 2018, Table III, p. 662) and winsorize all of the continuous variables at the 5% level in both tails to mitigate the influence of extreme outliers.[***] As we generally use three-digit SIC group-by-year fixed effects, we drop firm-years with a unique three-digit SIC code (i.e., "singleton groups"). Appendix Table A1 provides variable definitions.

Table 1 reports the mean, standard deviation, 25th, 50th and 75th percentiles, and the total number of observations for the main variables in our dataset for the period, 1992–2012, which begins and ends three years before and after the first and last SW-PPL states adopted their laws. Our main sample is comprised of 29,197 firm-year observations (see Table 4, column 1). The average percentage of firm-years in our main sample in which a company has a $PPill$ in-place is 59% and has a standard deviation

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

of 0.49. The respective average Q in our focal SW-sample is 1.9 with a standard deviation of 1.4, while 31.2% of the observations during this period are affected by a PPL. Online Appendix Figure OA1 plots the percentage of firms in our sample that are incorporated in FW-PPL and SW-PPL states (and in Delaware) each year from 1992 to 2012.

1.2. Identification strategy

We investigate the relevance 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' state of incorporation. The key assumption underlying this strategy is that the enactment of these laws provides an exogenous "shock" to the takeover protection of firms incorporated in the adopting states through the strengthening of the shadow pill. An essential step in verifying the plausibility of this assumption is to assess the likelihood that state adoptions of PPLs might be related to certain local characteristics (e.g., state macroeconomic factors) that might also correlate with individual firms' decision to adopt a pill and/or firm value, as this would invalidate the exclusion restriction of the identification strategy.

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

Other predictors include the state's level of M&A activity ($Inc.SY\ M\&A\ Volume$), log GDP per capita ($Ln(Inc.SY\ GDPPC)$) and growth rate ($Inc.SY\ GDP\ Growth$), a dummy for whether the majority of a state's U.S. House of Representatives belongs to the Republican Party

($Political\ Balance$), a state's level of population ($Ln(Inc.SY\ Pop)$), rates of unemployment ($Inc.SY\ Unemploy$) and state business entry and exit ($Inc.SY\ Entry$ and $Inc.SY\ Exit$). We also include 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, using the sample period 1992 to 2012. The predictor variables are measured in the year prior to the law's passage and we drop states from the analysis once they adopt a PPL. We standardize the continuous variables to have a mean of zero and unit variance in order to ease comparisons across coefficients and estimate standard errors clustered at the state of incorporation level. Given the state-year level of analysis, which results in a relatively low number of observations, estimating a model with all covariates included simultaneously is impossible because of multicollinearity. Hence, we present 4 specifications including subsets of the above-mentioned covariates. Table 2 presents our findings.

The evidence from each of the four columns in Table 2 suggests that only the prior enactment of other antitakeover laws predicts the passage of SW-PPLs. In particular, states with pre-existing BCLs and FPLs are more likely to adopt PPLs during the SW-period than states without this legislation. The coefficients pertaining to a state's median level of poison pills, Tobin's Q, stock returns, and ROA are insignificant (Columns (2) to (3)), so that reverse causality is unlikely to be a concern for our identification. The coefficients on $Ln(Inc.SY\ GDPPC)$ and all other state-level macroeconomic and political factors are also always statistically insignificant, suggesting that the passage of SW-PPLs is not driven by local economic conditions. We conclude that the findings in Table 2 are consistent with the assumption that states' firm characteristics and economic and political factors do not significantly influence whether state legislators adopt SW-PPLs.

1.3. Empirical specification

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

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

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

In most of our specifications evaluating the effect of PPLs on visible pill policy (firm value), we also include $X_{[i\tau(s)-1]}$ to control for a PPL-firm's Tobin's Q (poison pill status) in the specific year before the adoption of its state's respective law – denoted with the subscript $\tau(s) - 1$, where $\tau(s)$ denotes the year that state s adopts a PPL. Therefore, $X_{[i\tau(s)-1]}$ is not time-varying. We then interact $X_{[i\tau(s)-1]}$ with the PPL dummy to control for PPL-affected-firms' pre-law X characteristic in the post-law adoption period. When data on poison pill at the moment of adoption is missing, the interaction is set to zero.

We undertake this approach to avoid the problem of specifying "bad controls" (Angrist and Pischke 2009). For example, if we included a time-varying control for firm value in the poison pill regression, this could bias the coefficient on PPL and render any causal inference invalid if the control itself may be affected by the PPL (which we provide evidence for in this case). Our models also include firm fixed effects, γ, to control for unobserved, time-invariant heterogeneity within firms, and U.S. Census division-by-year, ω, and industry-by-year interacted fixed effects, λ, to control for unobserved, time-varying heterogeneity within divisions of location and industries, respectively. Finally, we two-way cluster our standard errors by states of incorporation and year, which we believe to be most appropriate given the state-year level of analysis (our results, however, remain similar if we cluster only by state 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 inference is robust to many sources of unobserved, time-variant heterogeneity that could bias our estimates, including local macroeconomic factors that are likely shared by states within close

geographic proximity (Heider and Ljungqvist 2015). We assign a firm's division-of-location based on its (historical) state of headquarters because this is generally where a firm's major plants and operations are located (Henderson and Ono 2008).

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

A common alternative strategy developed in the literature to deal with local sources of unobserved confounding variation is to use fixed effects at the level of the state where the corporate headquarter is located (Gormley and Matsa 2014, 2016). Our results are robust to using this approach. A limitation of this strategy, however, is that it relies on the assumption 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, only around 20% of the non-Delaware-incorporated firms that are in states without these laws are headquartered outside of their incorporation state). In contrast, more than 99% of Delaware-incorporated firms are headquartered in a different state. Therefore, the use of headquarter-state-by-year fixed effects in our setting leaves only a relatively small amount of variation to estimate the coefficient on PPL. This limits our tests' statistical power and restricts our controls to almost exclusively Delaware-incorporated firms. This latter point is especially relevant, as it increases the likelihood that some other confounding events in Delaware (e.g., poison pill case law) might bias our point estimates. Therefore, even though our results remain robust when including state of headquarters-by-year fixed effects, our preferred specification uses U.S. Census division-by-year fixed effects.

2. Legal Background

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

2.1. First wave-PPLs in the shadow of Delaware

The landmark 1985 decision of the Delaware Supreme Court in *Moran v. Household International* affirmed the validity of the poison pill for firms incorporated in the state of Delaware. Whether or not this decision also affirmed the validity of the poison pill for firms incorporated outside of Delaware has been the subject of debate. Some law and finance scholars describe the legal status of the pill for non-Delaware firms as uncertain until these firms' states of incorporation adopted a PPL (Catan and Kahan 2016; Cain, McKeon, and Solomon 2017; Karpoff and Wittry 2018). The argument commonly given to defend this view is that, while court decisions in some of the other U.S. states upheld the validity of the pill in the years immediately following *Moran*, the states of New York, New Jersey, Georgia, Wisconsin, Colorado, Virginia, and Indiana all had court decisions that invalidated the use of the poison pill between 1986 and 1989 (Catan and Kahan 2016; Cain, McKeon, and Solomon 2017). Therefore, the uncertainty created by these decisions would only have been cleared when legislators in those states (and elsewhere) decided to pass a PPL.

Other scholars, instead, claim that the above argument discounts evidence on the pervasive authority of Delaware judicial decisions over non-Delaware corporations (Ryngaert 1988; Cremers and Ferrell 2014). The alternative argument made by these scholars – which is also our interpretation – is that the validity of the poison pill was, in fact, fairly certain in the immediate aftermath of *Moran* for firms incorporated both in Delaware and outside of Delaware. As shown by Figure 1, the widespread adoption of visible poison pills, even for non-Delaware firms (incorporated in states without PPLs) in the years immediately following *Moran,* is consistent with the view that this ruling was understood to apply to non-Delaware firms as well (Helman and Junewicz 1986; Fleischer, Hazard, and Klipper 1988). This interpretation is also supported by the evidence that many of the court decisions that did uphold the pill in non-Delaware states referenced *Moran* in their poison pill rulings. Further, even in

states where court decisions intervened to invalidate the pill, the uncertainty on the status of the pill did not last long. For example, while the New York Supreme court invalidated the use of the pill in June 1988 (in *Bank of New York Co.* v. *Irving Bank Corp.*), the state of New York passed a PPL in December of the same year.

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

Consistent with the above assumption about the uncertainty injected by *Interco* and *Pillsbury*, Figure 1 indicates that the share of firms in our sample with a pill in-place began to decrease after 1989 (after having steadily increased in the preceding takeover-intense years).[§§§] It is worth observing that the decline in pill's popularity is also likely to be related to a decline in mergers & acquisitions activity, as also depicted in Figure 1. Indeed, with less M&A, the negative signal sent by having the pill in place is stronger, which may explain why the share of firms with pill in place declines in the early 1990s and then increases again in the second half of the 1990s.

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

[‡‡‡] For example, Martin Lipton wrote to his clients that: "Unless Delaware acts quickly to correct the [Interco and] Pillsbury decision[s], the only avenues open to the half of major American companies incorporated in Delaware will be federal legislation…or leaving Delaware for a more hospitable state of incorporation" (Martin Lipton Memos, p. 146).

[§§§] Figure 2 in Cremers and Ferrell (2014) suggests, similarly to our Figure 1, that firms began adopting pills meaningfully in 1985 and continued to do so until roughly 1988.

Figure 1 also shows that firms incorporated in Delaware ("Delaware Firms") were adopting pills at a similar intensity as firms in states that had enacted PPLs early ("FW PPL Firms") until 1989, but afterwards pill adoption levels in Delaware Firms became visibly lower. This evidence is consistent with the view that Delaware firms benefitted from some certainty about the validity of the pill from the 1985 *Moran* decision and later decisions that reaffirmed the validity of the pill (such as the 1989 decision in *Paramount Communications, Inc.* v. *Time Inc.*),[****] but less so than firms incorporated in PPL-enacting states due to the residual uncertainty arising from the 1988 countervailing decisions in *Interco* and *Pillsbury*.

In contrast, firms incorporated in states that did not adopt a PPL ("No PPL Firms") -- which, in our interpretation, have the least amount of certainty regarding the validity of the poison pill as a takeover defense -- are the least likely cohort to have a pill post-1988 during the FW-period. As shown by Figure 1, the decline in pill usage amongst firms without PPL coverage appears to bottom out around 1995.

We provide further evidence to the importance of Delaware cases rulings on the pill in Online Appendix Table OA3 by regressing a poison pill indicator variable ($PPill$) on dummies for whether a firm's state of incorporation adopts a FW-PPL ($FW\ PPL$), interacted with the quartile of firm's Q at the moment of PPL adoption. Our sample periods encompass the "Entire FW-Period" of 1983 to 1993 (Columns (1)-(2)), the "Post-*Moran*" period of 1986 to 1993 (Columns (3)-(4)), and the "Post-*Interco* & *Pillsbury*" period of 1989 to 1993 (Columns (5)-(6)). Following prior studies (e.g., Comment and Schwert 1995), we include dummies for other common antitakeover laws (BCL, CSL, DDL, and FPL)

[****] The *Paramount*'s ruling reinstated the validity of the pill, so that some commentators read *Paramount* as granting the board an unconstrained power "to just say no" to unsolicited tender offers. Several other commentators, however, maintain that Delaware case law on pill redemptions remains in an unsettled state – which would explain why poison pill cases continue to be tried – 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 evidence of two distinctive facts. On the one hand, under these contradictory rulings, it seems plausible to assume that firms outside of Delaware are more likely to rely on their own incorporating states' statutory and case law in the ensuing period. On the other hand, these rulings point to a relatively less certain shadow pill within Delaware (than for PPL-adopting states). The fact that Delaware is among the few states that have maintained a separate court of equity, where the "stare decisis principle" does not apply (i.e., precedents have no binding authority), could explain this continued *relative* uncertainty.

and specify firm, division-by-year, and industry-by-year fixed effects. The standard errors are adjusted for clustering at the state of incorporation level given the short time frame of each regression.

Consistent with the argument that *Moran* validated the use of the pill for both Delaware and non-Delaware incorporated firms at least until November 1988, we document that the point estimates on $FW\ PPL$ are always statistically insignificant in Columns (1)–(4) of Table OA3. The last two columns of Online Appendix Table OA3 show that firms with Q lying in the first quartile and incorporated in states with FW-PPLs were significantly more likely to have a pill in-place in the "Post-*Interco* & *Pillsbury*" period (1989–1993). This could plausibly explain why most states (27-of-the-35) adopted PPLs post-November of 1988, as the viability of the pill as a strong defense was no longer assured after *Interco* and *Pillsbury*.

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

3. Main Results

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

3.1. Shadow pills and visible pill policy

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

On the other hand, if there are frictions to pill adoption – e.g., the cost of coordinating a board meeting on short notice and requiring directors to reach a quick consensus and/or an increased likelihood that a pill will be invalidated if it is adopted last minute[††††] – then we might expect firms to still need an actual pill in place. Moreover, if the threat of a shadow pill is not enough and corporate raiders or activist investors pursued a target in spite of a state's adoption of a PPL (e.g., because their beliefs rationalize a possibility of successfully acquisition of the target even in the presence of PPL), a visible pill would still be necessary. Under these circumstances, we would then expect firms incorporated in PPL-adopting states to have pills in-place more frequently (i.e., a "validation effect").

[††††] The last-minute adoption of a pill might more easily fail the so-called "*Unocal* test" adopted by Delaware courts in reviewing the validity of anti-takeover defenses in general. This is because under *Unocal*, among other requirements, a board needs to demonstrate that (i) the directors acted in good faith to achieve a legitimate corporate objective and conducted a reasonable investigation, and ii) the adopted defensive measures was reasonable in relation to the threat posed, if based on the investigation the board had grounds for concluding that a threat to the corporate enterprise existed. Unocal, 493 A.2d at 954. Adopting a pill last minute thus raises the risk that the board may, for example, be held not to have engaged in a sufficient number of meetings for the board's investigation to be deemed reasonable. For a most recent application of this doctrine, see *The Williams Companies Stockholder Litigation*, 2021 WL 754593 (Del. Ch. Feb. 26, 2021).

The strength of these two effects may vary by firm characteristics, resulting in potentially different signs of the response for different firms. Conceptually, the factors affecting the decision to adopt a visible pill can be summarized with a simple decomposition into benefits and costs:

$$U_i(Pill\ Adoption) = P_i(pill\ valid) \cdot P_i(host.takeover) \cdot E(Benefit\ of\ Avoiding\ Host.Takeover) - Cost\ of\ Adoption_i$$

In this framework, the benefit of pill adoption is the ability for the adopting firm to avoid a hostile takeover. Whether this benefit is large in expectation depends on 1) $P_i(pill\ valid)$, which represents decision maker i's (idiosyncratic) belief that the visible pill is legally valid and will perform its purpose, as opposed to being challenged and deemed invalid in court; 2) $P_i(host.takeover)$, which represents a belief about the likelihood that a firm will be threatened by a hostile takeover.‡‡‡‡ For simplicity, we consider the benefit of avoiding a hostile takeover as fixed, while the costs of pill adoption may represent e.g. reputational costs and, as such, may depend on beliefs about pill validity.

It follows that in the absence of variation in beliefs about pill validity (i.e., our baseline hypothesis), a pill adoption decision should be determined by the likelihood of a hostile takeover. This means that firms that are likely to be threatened by a hostile bidder would find the expected benefit of pill adoption to be higher than the cost, but firms with small risk of takeover would find the cost to be higher. The risk of takeover is strongly related to firm's valuation, or firm's Q: for firms with high Q, the risk of takeover is low, while for those with low Q, it is larger (Cremers, Nair, and John 2009; Edmans, Goldstein, and Jiang 2012; we also confirm this result in our data by regressing the likelihood of takeover on the level of Q and finding a significant negative relationship). Hence, absent variation in beliefs about pill validity, we would expect that a firm's decision to adopt the pill is inversely related to the firm's Q.

‡‡‡‡ This likelihood may also depend on the potential bidders' belief about pill validity, which in turn will be correlated with the decision maker's belief, $P_i(pill\ valid)$. This possibility, however, does not challenge the validity of our proposed decomposition, as one can easily restate it and argue that $P_i(pill\ valid)$ represents only the idiosyncratic components of the decision maker's beliefs, and hence is uncorrelated with the beliefs of potential bidders that may influence $P_i(takeover)$.)

However, in the presence of uncertainty about pill validity, the relationship between a firm's Q and pill adoption becomes less clear. In particular, one can conjecture that when a firm has reasons to consider pill adoption as likely valid – where these reasons may include, e.g., firm-specific factors that may influence a court's perception of pill validity in a given case, or directors' beliefs about interpretation of the law – that firm will be more likely to adopt a pill, independent of Q. Conversely, even low-Q firms may decide not to adopt the pill because they believe it is very likely to be held invalid, and therefore of no use.

Under this conceptual framework (and in light of our discussion in Section 2 on the relevant legal background), the passage of PPLs can then be thought of as reducing the variation of beliefs about pill validity, $P_i(pill\ valid)$. More specifically, we pose that before PPLs were passed, idiosyncratic beliefs significantly influenced a firm's decision to adopt the pill, but after the laws were passed, these beliefs became less relevant. As a result, the link between pill adoption decision and likelihood of takeover, $P_i(host.\ takeover)$, becomes stronger (i.e., it goes back to the baseline hypothesis). Under this argument, the passage of PPLs may affect not just the level of pill adoption, but also its correlation with the economic incentives for adoption, i.e., $P_i(takeover)$. That is, following the passage of PPLs, pill adoption may be more strongly aligned to proxies for $P_i(takeover)$, such as Tobin's Q.

Consistent with this argument, we speculate that the net effect of PPLs on pill adoption is likely to be positive for firms with lowest valuations (which are at the highest risk of takeover; Cremers, Nair, and John 2009; Edmans, Goldstein, and Jiang 2012), but negative for firms with highest valuations. Hence, in Table 3 we regress poison pill-based measures on PPL and its interactions with the indicators for the first and fourth quartile of Tobin's Q at the moment of PPL adoption, plus other controls and firm, division-by-year, and industry-by-year fixed effects.

Column (1) shows a lack of significant effect of PPLs on pill adoption on average, suggesting that the validation effect balances out the substitution effect on average. Yet, the average effect may hide significant response of firms with different levels of valuation. To address this concern, we create two binary indicators, $Q(Lowest)_{[t-1]}$, and $Q(Highest)_{[t-1]}$, by forming quartiles based on our sample's empirical distribution of Tobin's Q. The related dummies are set to one if the firm's level of Q lies in

the bottom or top quartile, respectively. Column (2) shows that relative to the control group (firms in the two middle quartiles), firms in the lowest quartile are significantly more likely to adopt the pill, while firms in the highest quartile are significantly less likely to do so. The positive coefficients on $Q(Lowest)$ and negative coefficient on $Q(Highest)$ suggest a potential reverse causality problem (consistent with, e.g., Cremers and Ferrell 2014; Catan 2019), casting doubt on the ability to clearly assess the effects of the poison pill just by looking at the consequences of endogenous adoption decisions.

Motivated by the relationship between firm's valuation and pill adoption, in columns (3)-(5) we interact the dummy variable for the presence of PPL with binary indicators of firm's Q being in the first or fourth quartile one year before the law adoption (we denote this period by $\tau(s) - 1$). The results confirm that the zero net effect in column (1) hides substantial heterogeneity: relative to the control group, firms with low valuation are more likely to adopt a poison pill following the passage of PPLs, while the effect for firms with high valuation is negative. While including both quartile indicators produces only marginally significant estimates of the differences relative to the control group, the difference between lowest and highest quartile is highly significant, as evidenced by the value of F-test for the equality of the effect for both of these groups. These findings demonstrate that PPLs can have both substitution and validation effects, and while the former dominates for firms with high valuation, the latter dominates for firms with low valuation.

We supplement these findings by estimating regressions of $Ln(Q)$ on "relative year" dummy variables that indicate the number of years before and after the year in which a firm adopts a poison pill, along with firm, division-by-year, and industry-by-year fixed effects (following a similar approach as in Catan 2019). We include relative year dummies for up to 5 years before and after a pill's adoption. The resulting point estimates and 95% confidence intervals of the relative year dummies are plotted in Online Appendix Figure OA2. The figure provides suggestive evidence that firm value significantly declines in the five years before a firm decides to deploy a poison pill, supporting the view that the negative association between the adoption of a visible poison pill and lower firm value reported in prior studies is likely attributable to reverse causality (Cremers and Ferrell 2014; Catan 2019).

In columns (6)-(7), we then separately consider the decision to adopt a new poison pill ($New\ PPill$) and how long pills are kept in place ($Ln(PPill\ Duration)$) to distinguish how PPLs affect adoptions of new pills relative to the maintenance of existing pills. Column (6), which uses $New\ PPill$ as the dependent variable and includes our full set of fixed effects, does not show a significant response of the frequency of new pill adoptions. In contrast, using $Ln(PPill\ Duration)$ in column (7) suggests that firms with lowest levels of Q in the year before PPL adoption significantly increase the duration of their pills in-place relative to the other PPL-firms, resembling the pattern documented in columns (3)-(5). These results suggest that responses in visible pill policy following the passage of a PPL are driven mostly by changing the duration of pills that are already in place. Yet, we consider this evidence to be only suggestive, because new pill adoption is a rare event, and the aggregate levels of pill adoption were generally declining during the analyzed period, which may make it difficult to detect significant responses of new pill adoption.

3.2. Shadow pills and long-term firm value

In this section, we investigate the value implications of a strengthened right to adopt a poison pill, focusing on the logarithm of Tobin's Q as our primary measure of firm value. We check the robustness of our findings by examining the effect of PPLs on alternative measures of value and using alternative methods of constructing our sample. Further supplementary robustness tests are included in the Online Appendix.

3.2.1. Main sample

Table 4 reports the difference-in-differences estimates of the impact of the adoption of PPLs by state legislatures on the Tobin's Q of firms in enacting states over the period 1992 to 2012. Each of the five columns employs $Ln(Q)$ as the dependent variable and includes controls for each of the other four antitakeover laws (BCL, CSL, DDL, and FPL). Columns (1)–(3) include our default set of fixed effects – firm, division-by-year, and industry-by-year – whereas, the last two columns check the robustness of our results to controlling for local "shocks" using regions or headquarter states instead of divisions. The standard errors are adjusted for two-way clustering at the state of incorporation and year level.

We find that the adoption of PPLs has a positive and statistically significant impact on the Tobin's Q of firms in enacting states. In Column (1), without including any firm-level controls, we find that firms incorporated in a state that adopts a PPL experience an increase in firm value of 4.7% relative to firms incorporated elsewhere, but operating in the same U.S. Census Division and sharing a similar industry trend.[§§§§] The estimated coefficient on PPL in Column (2) is 4.4%, showing robustness for controlling for visible poison pills ($PPill_{[t-1]}$). The estimated coefficient on $PPill_{[t-1]}$ confirms the results in the prior literature of a negative correlation between actual firm-level pills and Tobin's Q (e.g., Bebchuk, Cohen and Ferrell 2009; Cremers and Ferrell 2014). However, in light of our results in Table 3 and Online Appendix Figure OA2, the negative association between visible pills and Tobin's Q seems endogenous and due to reverse causality.

Further, the model in Column (2) suffers from an endogeneity problem because PPLs also affect visible pill policy, rendering $PPill_{[t-1]}$ a "bad control." Therefore, in the remaining columns we instead interact PPL with $PPill_{[\tau(s)-1]}$, i.e., indicator variable for whether the firm has a visible poison pill in place in the year before the adoption of the firm's respective state's PPL. We find that the point estimate on the interaction is negative but only marginally statistically significant while the standalone coefficient on PPL (point estimate = 0.068) remains significant at the 1% level. These results indicate that shadow pills create long-term value for shareholders, and that this effect might be stronger among firms that did not have a pill in place before PPL adoption. The effect for an average firm is between 4% and 5%.

The last two columns of Table 4 serve as robustness checks. Rather than using division-by-year fixed effects, we alternatively employ fixed effects based on U.S. Census Regions (i.e., Northeast, Midwest, South, and West) (Acharya, Baghai, and Subramanian 2014) or headquarter states (Gormley and Matsa 2014, 2016) to control for potential local confounding factors. The coefficient on PPL remains similar using either of these alternative specifications. We prefer the use of fixed effects based on U.S. Census Divisions, as these provide a more granular geographical measurement than regions

[§§§§] We show that our baseline point estimate in Column (1) is robust to the omission of any SW-PPL-passing state in Online Appendix Figure OA3.

and are not susceptible to the econometric issues (specific to our setting) engendered by the use of headquarter states that we outlined in Section 1.3.

3.2.2. Alternative samples

We next turn to considering alternative ways of constructing our sample. In our baseline approach, we include all firm-year observations for which we have information on visible pill status. One concern about this sample is that it is too broad and potentially affected by selection effects that might bias our inferences. In particular, firms may endogenously reincorporate into a PPL adopting state and hence unobserved characteristics of firms in states adopting PPLs may be different from characteristics of firms in other states.

We account for this by constructing a propensity score-matched sample, where we match each "treated" firm in the SW-PPL adopting states in the year before passage ($\tau(s) - 1$) to a "control" firm incorporated in a state without a PPL in the three years following its matched counterparts' adoption year. The basic idea behind this research design is that by matching firms in the year prior to treatment, we ensure that our matched sample is restricted to firms that were already incorporated in the state before the PPL was passed, disallowing the possibility that firms selected into treatment (i.e., a stronger shadow pill) via (re)incorporation, and improving comparability of analyzed treatment and control firms.

Our matching procedure requires that treated and control firms are identical on firm-level poison pill status and matches firms based on pre-treatment year levels of Q and $Total\ Assets$. To ensure sample size that allows a meaningful analysis, we match each treated firm with up to five control firms.

In columns (1)-(3) of Table 5, we present the results from regressions of $Ln(Q)$ on PPL and its interaction with visible pill at the time of PPL adoption over a $t\pm3$ estimation window. The first two columns use firm, division-by-year, and industry-by-year fixed effects, while the third column only uses firm and year fixed effects for robustness. Each of the three columns include dummies for the other antitakeover laws. We confirm that firms experience significant increases in their Tobin's Q after PPL adoption when compared to the control group. These results mitigate concerns that a selection effect

and other differences between PPL-adopting and non-adopting states drive our findings in the main sample.

A second concern about our sample may relate to it being too narrow, i.e., limited to firm-year observations for which we observe visible pill status. To address it, we extend the sample in the value analysis foregoing the possibility of interacting PPL with pill status at adoption.

The results with the extended sample are presented in columns (4)-(5) of Table 5. Column (4) includes all firm-year observations with historical incorporation data available in the Spamann's dataset.[*****] While, as confirmed by our hand checks, this data is highly reliable, its availability is greatly limited before 1995, and hence we extend it by using additional historical information from Compact Disclosure disks and CRSP Historical. Doing so generates a larger sample that is used to produce the results presented in column (5). Both columns confirm positive and significant impact of PPL adoption on firm value, and thus alleviate concerns that our main results are driven by a selection issue based on the availability of visible pill data.

Finally, column (6) presents the results with an alternative measures of poison pill. When measuring pills, we start with data used in Catan (2019), and supplement it with further datasets (ISS, Cremers, Litov and Sepe (2017) and SDC) sequentially when the Catan data is not available. The results we obtain are very similar to the main result from Table 4. Similar results are also obtained when only using Catan data, or when relying only on ISS data, even though the sample size becomes then visibly smaller (about 20,000 observations).

3.2.3. Alternative value measures

We investigate the robustness of our firm value results using alternative metrics of value. In Panel A of Table 6, we employ the same specification that we use in column (3) of Table 4, but replace $Ln(Q)$ as the dependent variable with the following four measures:

1. The level of Tobin's Q (Q);

[*****] The dataset is available at https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/KBPZ5V

2. Total Tobin's Q ($Total\ Q$), proposed by Peters and Taylor (2017), which modifies Q by explicitly accounting for intangible capital in the firm's replacement cost of total capital;
3. $Excess\ Return$ (Cohen and Wang 2013), estimated as the residual from regressions of annual stock returns on the Fama-French four (i.e., Market, SMB, HML, and MOM) factors (Fama and French 1993; Carhart 1997);
4. Return on assets (ROA), defined as operating income before depreciation and amortization divided by the book value of assets (Giroud and Mueller 2010).

Our main result that firm value increases after the firm's state of incorporation adopts a PPL is confirmed for three out of four alternative measures of firm value. That is, we confirm that PPLs are positively and significantly related to firm value measured with Q, Total Q and Excess Return. In contrast, we find no significant relationship for ROA. Lack of response of ROA may be due to the fact that the rise in firm value comes from future, and not necessarily current cash flows, or that it comes from lowering firm risk and a firm's cost of capital, which would be reflected in market-based measures of value, but not in profit-based accounting measures. This is particularly likely given the results in Table 7, which suggest that the increase in value is driven by firms with high intangible assets. While analyzing future ROA could in theory be a remedy for part of these problems, this measure is likely too noisy to allow drawing meaningful conclusions.[†††††]

We also consider a monthly portfolio return approach, which can be viewed as a long-term stock event study, consistent with prior corporate governance studies (Gompers, Ishii, and Metrick 2003; Bebchuk, Cohen, and Ferrell 2009; Giroud and Mueller 2011). In this approach, we focus on our matched sample of firms and construct long (short) portfolios of stocks from treated (control) firms around the time their (matched counterparts') state of incorporation adopts a PPL. The central premise is that if a strengthened shadow pill matters for a firm's long-term performance, but its impact is not immediately incorporated into stock prices because of, for example, inefficiencies in information across

[†††††] Johnson, Karpoff, and Yi (2015) similarly find strong results for firm value, but only marginal results for ROA (compare Table 8 and Table 10). Along similar lines, many studies in the literature present results either only for measures of operating performance (e.g. Danielson and Karpoff 2006; Giroud and Mueller 2010), or only for firm value (e.g. Cain, McKeon and Solomon 2017 and various others reviewed in Straska and Waller 2014).

states and time, then realized returns for a treated firm are systematically higher than those for a control firm. The long (short) portfolios are constructed as follows. For portfolios "6m36" and "12m36," we include all stocks of treated (control) firms starting either 6 or 12 months before the fiscal year-end of the year in which the matched treated incorporating state adopts a PPL, and hold these stocks for 36 months post-adoption. The long-short portfolios are then created by differencing the portfolio returns of the long and short portfolios for each respective month.

In Panel B of Table 6, we find that the long-short portfolios of treated and control firms have a positive and significant $Alpha$ over both "6m36" and "12m36" holding periods, using 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 models. For instance, when we buy stocks of treated firms and short stocks of control firms 12-months before the adoption date of their (matched firms') respective PPL and continue such strategy until 36-months after, we find an overall average annualized abnormal return of 12.7% using the four-factor model. These magnitudes are 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 having a stronger shadow pill (via the enactment of PPLs) is value-enhancing for shareholders. To conserve space, we include these supplemental analyses in the Online Appendix. As a roadmap for interested readers, we include a synopsis of these tests below:

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

[§§§§§] We unpack the dynamics of the buildup of shareholder returns around the date of PPL adoption by conducting a short-run event study surrounding important dates in the life of the legislation (e.g., introduction on the House or Senate floor, final passage by the House or Senate, and the Governor's approval). Online Appendix Table OA4 reports the respective cumulative abnormal returns (CARs) as showing a small positive reaction by the market for firms incorporated in SW-PPL states shortly after the bill is introduced, suggesting that the market responds favorably to the prospects of a PPL being passed. We further observe a significant positive market response during shortly before the final passage of the PPL, indicating that the market anticipated its successful ratification and perceived it as valuable for the relevant firms.

(i) Sample adjusted for Delaware case law

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

We check the robustness of our results to coding Delaware firms' PPL as a "0" in Tables OA5 and OA6. We use three separate approaches. First, we show that coding Delaware firms' PPL indicator as equal to "1" starting in 1985 and leaving it at this value throughout the sample does not change our main result. Second, we document that our Tobin's Q results are robust to excluding firms incorporated in Delaware entirely. In both cases, however, the precision of the estimates decreases and they become only marginally significant when Delaware firms, a large part of control group, are moved to the treatment group or excluded entirely. That is of course to be expected if our initial classification of Delaware as non-PPL state is appropriate. Third, instead of relying solely on the variation stemming from PPLs, we consider an alternative proxy for the strength of the shadow pill. Using PPLs and state-level court decisions (including *Moran* in Delaware) on pills from Cain, McKeon, and Solomon (2017), we construct a $PPV\text{-}Index$ that captures changes across states and time on the relative strength of the shadow pill. Substituting this measure for PPL in our full sample $Ln(Q)$ regressions, we continue to find that strengthened shadow pills are valuable.

(ii) Controlling for PPL-firm characteristics

As discussed above, in order to avoid misspecification by including bad controls in our regression models, we do not include time-varying firm-level controls used by prior corporate governance studies (e.g., $Ln(Assets)$, $CAPX/Assets$). We show that our findings on firm value are not dependent on the

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

exclusion of these controls in Table OA7, where we include time-invariant firm-level controls that are measured in the year prior to the adoption of a firm's state's respective PPL ($\tau(s) - 1$) and are interacted with PPL. We continue to find a positive relation between PPLs and firm value.

(iii) State-by-year fixed effects

Following Gormley and Matsa (2016), in Table OA8 we control for the state-by-year fixed effects and decompose the effect of PPLs into cohorts of firms incorporated and headquartered in the same state ($Same\ Inc\text{-}HQ\ State$) versus that of firms incorporated and 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 largely by the $Same\ Inc\text{-}HQ\ State$ firms. The coefficient on $PPL \times Diff.Inc\text{-}HQ\ State$ is positive, but insignificant, which we argue is due to a lack of variation (i.e., low statistical power).

(iv) Placebo tests

We construct a placebo test by randomly assigning states (without replacement) a PPL, where these assignments follow the laws' actual empirical distribution across time – thus, if our main results are driven by confounding factors that occur around the same time as PPL adoptions, they should remain present in the data and could continue to bias our findings. We repeat the simulation 1,000 times and then estimate the regression model in Column (3) of Table 4 on the simulated data. Figure OA4 plots the distribution of the coefficients and *t*-statistics. The vertical red lines represent the actual respective regression coefficient and *t*-statistic based on the actual data. We find that the actual regression coefficient and *t*-statistic lie at the tails of the distributions, suggesting that the effects we find on Tobin's Q are attributable to the actual PPLs.

(v) Sample period adjusted for Delaware case law

In our interpretation, the Delaware rulings in *Interco* and *Pillsbury* injected uncertainty in the legal validity of the pill both in Delaware and elsewhere after 1988. Accordingly, to have a pre-treatment period unconfounded by these court rulings, our main results focus on PPL adoptions beginning with Minnesota in 1995. For robustness, we move back the sample to include PPL adoptions beginning in 1989 in Table OA9. We find that our results persist.

(vi) Subperiods within the second wave

Table OA10 breaks down our SW-sample period into three subperiods, 1992-1998, 1999-2005, and 2006-2012, to test whether the adopted PPLs had comparable effects across these subperiods. We find that the first two subperiods in which most of the SW-PPLs were adopted have the largest and most significant effects. Meanwhile, we find no significant response to *PPL* in the last period, 2006-2012. This is likely explained by limited power, as only two states – Vermont and Wyoming (with very few firms) – adopted PPLs during this period.

(vii) Excluding multi-law adopting states

We show in Table OA11 that our Tobin's Q results are robust to excluding states that enacted other antitakeover laws in the same year they passed PPLs.

4. Economic Channels

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

4.1. Commitment Hypothesis

Stronger shadow pill may increase firm value because its existence allows the firm to commit to a value-enhancing, long-term business strategy. Without an ability to prevent the disruption that is caused by hostile takeovers, the firm may be unable or may find it suboptimal to pursue a strategy that may generate large long-term gains but comes with a risk of low performance in the short-term. Committing to long-term strategy may increase value because it lowers costs of contracting with external stakeholders (e.g., Laffont and Tirole 1988; Shleifer and Summers 1988), which in the existing literature is often referred to as the "bonding hypothesis" (Johnson, Karpoff, and Yi 2015).

Long-term commitment may also prove to be value-enhancing in relation to firm officers (Cremers, Litov, Sepe 2017; Cremers and Sepe 2016). Indeed, innovative firms are organized trough long-term incomplete contracts with several stakeholders, such as employees, which necessitates bonding firms' strategy in the long term. In addition, highly innovative firms are likely to be more affected by

asymmetric information because of their high assets' intangibility. As a result, the value of these firms is difficult to assess for outsiders, so that they might easily be undervalued when pursuing a long-term strategy that in the short-term may generate limited profits, which are the fundamental observable proxy (Jensen, 2000).

If the firm may be subject to a hostile takeover, the managers (and shareholders) of these firms may find it suboptimal to pursue long-term strategies, in line with the phenomenon of "rational myopia" of managers, discussed by Stein (1988, 1989) and which may reduce firm value. For that reason, enhancing the ability of managers (and shareholders) to commit to a long-term strategy may be beneficial even absent important relationships with external stakeholders. Further, firms with many intangible assets are also riskier and subject to highest uncertainty (Dixit, 1988), and hence have the largest scope for positive value effects stemming from lowering the risk of disruption of long-term strategies. For all these reasons, we use the broader term "commitment hypothesis," rather than the classic "bonding hypothesis."

Within this analytical framework, we pose that the importance of a commitment mechanism is likely highest in two circumstances: (1) for innovative firms that face high level of risk and uncertainty, and are subject to asymmetric information, as well as (2) for firms that have important relationships with external stakeholders.

To test the commitment hypothesis, we use the following measures of innovativeness:[††††††]

1. $Intangible\ Capital$, which is a "catch-all" measure for the importance of intangible capital (Eisfeldt and Papanikolaou 2014) and is defined as a firm's intangible capital estimated replacement cost (as proposed by Peters and Taylor 2017), based on data from WRDS in the Peters and Taylor Total Q database;
2. $Organizational\ Capital,$ which is a subset of intangible capital measured on data from WRDS in the Peters and Taylor Total Q database;

[††††††] We also test, but find no effect of RQ, or research quotient, which measures the output elasticity of R&D (as proposed in Knott 2008). This measure, however, is available only for a subset of firms.

3. $R\&D/Sales$, which is a measure for the importance of corporate expenditures on research and development activities (Chan, Lakonishok, and Sougiannis 2001), based on data from Compustat; we calculate the stock of R&D using perpetual inventory method with 10% discount rate.
4. $Patents$, which captures the novelty of a firm's innovative output based on the number of patents they hold (Atanassov 2013), based on data from the KPSS Google patents dataset;

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

Columns 1-4 in Panel A of Table 7 present our results. In each of the four columns, we specify the natural logarithm of Tobin's Q as the dependent variable and include our fixed $PPill_{[\tau(s)-1]}$ control interacted with PPL and the full set of fixed effects. Consistent with the theoretical predictions of the commitment hypothesis, we find that firm value is higher when boards are better equipped to contest the potential disruption caused by a takeover – via the adoption of a PPL that strengthens their shadow pill – especially for firms that (in the year before the PPL is adopted) have more intangible and organizational capital (Columns (1)-(2)), and are more engaged in research and development (Column (3)).

The interaction coefficient is positive but noisy (t-ratio of about 1.3) for the fourth measure, patents. While patents, R&D results, and organizational capital all share an intangible nature, patents are publicly observable and reduce asymmetric information. We interpret the difference between the effect for patents and the three other measure of innovation as suggestive that the asymmetric information channel is important in shaping the value benefits of PPLs. To confirm the importance of asymmetric information, column (5) shows that firms for which analysts earnings forecasts had higher mean forecast error (presumably because they are difficult to estimate, as the firm is subject to a lot of asymmetric information) experience a higher increase in firm value upon PPL passage.

The results in column (1)-(5) thus confirm the relevance of the broader commitment hypothesis relative to the bonding hypothesis. This inference is further supported in column (6), where we find no

evidence that the effect of PPLs is higher for firms with more important relationships with external stakeholders. More specifically, column (6) shows that PPLs' impact on firm value is no higher for firms with a large customer, and we also find that the effect is not larger for firms with strategic alliances. However, we do find some evidence for the importance of external stakeholders when we condition on high asymmetric information. That is, as illustrated in column 7, the effect on value is particularly strong for firms with large customer and high level of intangible capital. Hence, while the relationships with external stakeholders do not seem to be the main driver of the observed value effect, bonding the stakeholders seem to be relevant for firms that are exposed to more asymmetric information.

4.2. Bargaining power hypothesis

Under the bargaining power hypothesis, we pose that firm value increases because a strengthened shadow pill makes it more difficult to acquire a company for a potential bidder and allows existing shareholders to obtain better conditions in the process. In contrast to the commitment hypothesis, where the increase in value comes from the fundamentals, here it would come from the superior bargaining position of the firm in the takeover process.

If accurate, the bargaining power hypothesis should result in higher acquisition premiums and, following the law of demand, a lower likelihood of takeover bid and acquisition. We thus aim to shed light on whether takeover considerations play an important role in explaining the positive value effects of PPLs by analyzing both target acquisition propensities and premiums, following prior empirical studies (Comment and Schwert 1995; Heron and Lie 2006, 2015; Kadyrzhanova and Rhodes-Kropf 2011). The data on acquisitions are from the SDC M&A database and comprise 128 unsolicited acquisition attempts (for which we also have data on other related variables) announced over the period 1992–2012. We define a takeover as unsolicited if the SDC database classifies the bid as hostile or otherwise unsolicited (Heron and Lie 2006, 2015).

Panel B of Table 7 examines the impact of PPLs on the likelihood that firms receive a takeover bid (Column (1)), as well as on the probability that a deal is successfully completed (Column (2). The dependent variable, Bid ($Acquired$), is an indicator variable equal to one if a target firm announces

that it has received a bid (is acquired in a completed takeover, either through a merger or an acquisition) in the SDC M&A database, and zero otherwise. Both columns include division-by-year and industry-by-year fixed effects.

We find that firms with strengthened shadow pills that did not adopt a visible pill are less likely to receive a takeover bid and less likely to be acquired.‡‡‡‡‡‡ These effects, however, are marginally statistically significant and the total effect is smaller and insignificant for firms with a visible pill. Still, as shown in columns (5)-(6), the impact on takeover's likelihood is more likely to come from firms with high level of intangible capital, lending further support to the important role of asymmetric information. This suggests that PPLs may reduce opportunistic takeovers on firms with high intangible assets, and hence enable them to continue operating under the same ownership and pursue longer-term investment strategies.

Next, in columns (3)-(4) of Panel B, Table 7, we investigate whether takeover premiums are positively related to the adoption of PPLs, as the bargaining power hypothesis would suggest. In these tests, we employ the following two dependent variables: $Premium\ Increase$, defined as the percentage increase in the bid price scaled by the target's stock price 20 days prior to the initial offer, and $Total\ Premium$, measured as the sum of the initial premium and the premium increase, where the summed components are relative to the target's stock price 20 days prior to the initial offer. Our specifications use division and industry fixed effects, but not firm or interacted fixed effects, since we are focusing exclusively on the cross-section of successful hostile bids, such that our sample size is limited to 128 observations. We find that the adoption of a PPL is not associated with an increase in the Total Premium but we find a marginally significant positive impact on Premium Increase.

Overall, Panel B of Table 7 provides suggestive evidence that PPLs can be value-enhancing for shareholders of acquisition targets. It should be noted, however, that the distinction between the bargaining power hypothesis and commitment hypothesis is not clear cut. Under the commitment hypothesis, the value of the firm may also increase, and its undervaluation may be reduced, which may

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

also result in a lower likelihood of takeover. In addition, in light of the infrequent occurrence of hostile takeovers during the SW-period (only about 0.5% of the sample's firm-years), the higher takeover premia attributable to PPLs can only explain a small portion of the associated increase in firm value.

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

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

First, and consistent with the prominence and effectiveness of the poison pill as a takeover defense, we interpret these results as suggesting that, among antitakeover laws, PPLs play a more important role than the previous literature has considered.[§§§§§§] In particular, the finding in Table 2, that the prior enactment of BCLs and FPLs are strong and consistent predictors of SW-PPLs, combined with the evidence that PPLs increase firm value, add to our understanding of the relationship between, and relative importance of, different kinds of antitakeover state laws. Indeed, one possible interpretation of this finding is that, contrary to the common view in the literature that BCLs (and FPLs) provide substitute takeover protection to PPLs (e.g., Karpoff and Malatesta 1989; Garvey and Hanka 1999), these protections might, in fact, be more complementary.[*******] This would explain, for example, why almost all the states that eventually adopted a SW-PPL had already introduced a BCL several years earlier. A second possible interpretation is that, consistent with the view of PPLs as the strongest

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

[*******] Analyzing the function of the shadow pill vis-à-vis other governance mechanisms is outside the scope of this study. In practice, the adoption of a poison pill is frequently accompanied by the adoption of a staggered board (Cohen and Wang 2013). This is because the combination of these defenses substantially reduces the chances that a potential bidder might be able to have the pill removed (i.e., by replacing a majority of directors) through the ballot box, therefore strengthening the anti-takeover force of a visible poison pill. We investigate here the combined impact of the shadow pill and staggered boards on firm value. Table OA12 indicates that the effect of interaction of PPL and staggered boards is insignificant. We hypothesize that, unlike visible poison pills, shadow pills might act more as a substitute than complementary antitakeover measures, meaning that a firm's ability to adopt a visible pill when the firm is incorporated in state with a PPL would have a similar deterrent effect as the combination of a visible pill and a staggered board on prospective raiders. Thus, the shadow pill and the staggered board would both provide effective, yet independent, long-term bonding devices (Cremers, Litov, and Sepe 2017).

antitakeover law (Karpoff and Wittry 2018; Catan and Kahan 2016), legislators in SW-PPL states might have found BCLs less effective than desired in providing takeover protection, and opted to subsequently introduce PPLs to further enhance this protection. In support of the latter interpretation, Karpoff and Wittry (2018) find that BCLs do not matter as much as commonly reported by previous studies once other institutional details and the legal context are controlled for.

Second, our study sheds light on the relative strength of the shadow pill across different states and period of times, providing evidence that, until the Delaware decisions in *Interco* and *Pillsbury* in 1988, the validity granted to the pill in its 1985 ruling in *Moran* likely validated the pill in other states, whether those states had a PPL or not (see our discussion in Section 2). Our results also indicate that in the post-*Interco* and *Pillsbury* period, the shadow pill's validity is the most certain for firms incorporated in states that have adopted a PPL. The next highest level of certainty would stem from court decisions in a firm's state of incorporation that always upholds the use of the pill as a takeover defense. This would be followed by situations where court decisions in a firm's state of incorporation sometimes uphold the poison pill (e.g., Delaware – *Moran*, *Paramount*, and *Airgas* in favor and *Interco* and *Pillsbury* against). The least certain situation for a firm's right to adopt a visible pill would be *not* having a PPL or any court decision upholding a pill (e.g., Louisiana) or having a court decision or a statutory provision against the pill in the firm's respective state of incorporation (e.g., California).

6. Conclusion

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

Our paper is the first in the literature to focus on the second wave (SW-) PPLs passed during the period 1995 to 2009, in order to explore the implications of these laws for visible pill policy and long-term firm value. Given substantial changes in the underlying legal environment since the enactment of first wave PPLs (i.e., adopted from 1986 to 1990), we conjecture that results obtained by the prior

literature for these first wave PPLs might well differ from what we can learn from the SW-PPL adoptions. Further, from an identification perspective, focusing on SW-PPLs ensures that we have a pre-treatment period that is unconfounded by the (unprecedented) hostile takeover wave of the 1980s or major Delaware court decisions that could have impacted the importance of PPLs.

We document two main results. First, we show that having a stronger shadow pill via the enactment of a PPL has a validation effect for lower-valued firms, which are more likely to be exposed to future hostile takeover risk and activist investors, and face significant frictions in adopting a visible pill (e.g., the costs of assembling a board meeting on short notice, forcing the directors to achieve a quick consensus, and/or future legal challenges). For these firms, the passage of PPLs leads to an increase in visible pill adoption. The opposite is observed for high-valuation firms, for which the existence of a shadow pill has a substitution effect and reduces the adoption of visible pills. This is consistent with the fact that a stronger shadow pill ensures the validity of "off the shelf" pills (i.e. pills that can be adopted at the last moment, when the risk of takeover becomes significant), which for high-valuation firms that are not at the risk of immediate takeover provides a sufficient takeover defense on its own.

Second, we find that the availability of a stronger shadow pill is associated with significant improvements in firm value. Further, using a comprehensive dataset of firm-level visible pills, we also confirm and expand the findings of the previous literature on the visible pill's negative association with Tobin's Q. Overall, a stronger shadow pill seems beneficial to shareholders, even if the (endogenous) adoption of an actual pill might not be.

We conclude that our results support the view that the shadow pill serves a positive corporate governance function for some firms through the channels considered by the "commitment hypothesis" and, in part, the "bargaining power hypothesis" of takeover defenses. Consistent with the "commitment hypothesis", the right to adopt a pill increases firm value by allowing the board to commit to the firm's long-term strategy, promoting longer-term investments projects and protecting firm-specific investments. This effect is most important for firms that are subject to asymmetric information, and hence are likely to be misvalued by the market and subject to an undesired takeover bid.

Appendix

Table A1
Variable definitions

Bid (Acquired)	An indicator variable equal to one if a firm receives a takeover bid (is successfully acquired) per the SDC M&A database, and zero otherwise.
Est. Entry (Exit)	The establishment entry (exit) rate in a firm's state of incorporation. We use data from the U.S. Census Bureau.
Excess Return	Fama-French 4-factor adjusted excess returns are defined as the residual from annual regressions of raw returns on a value-weighted market factor, small-minus-big factor, high-minus-low factor and momentum factor (Carhart, 1997). Data comes from CRSP and Ken French's website.
Forecast Error	Absolute value of the mean of analysts' prediction minus the actual earnings per share, divided by the actual earnings per share for a given firm-year. Based on IBES data.
GDP Growth	The incorporated state-level GDP growth rate over the fiscal year. Data comes from the U.S. Bureau of Economic Analysis.
GDPPC	An incorporating state's GDP divided by its total population. Data comes from the U.S. Bureau of Economic Analysis. We take the natural logarithm of this variable: $Ln(GDPPC)$.
Inc. SY	Denotes that we use the median of the corresponding $[Variable]$ of all firms incorporated within a state, in a given year.
Intangible Capital	Firm's intangible capital estimated replacement cost scaled by the book value of assets. This measure is available on WRDS and follows Peters and Taylor (2017).
Large Customer	An indicator variable equal to one if a firm has at least one customer that accounts for more than 5% of their sales, based on the Compustat Customer Segments database.
M&A Volume	The ratio of M&A dollar volume in SDC to the total market capitalization from Compustat per state of incorporation, in a given year. We only include ordinary stocks (i.e., we exclude American depositary receipts (ADRs) and real estate investment trusts (REITs)). We also only consider transactions that are completed and where the acquirer achieves control of the target.
New PPill	An indicator variable equal to one if a firm adopts a new poison pill ($PPill$).
Organizational Capital	Organizational capital value based on data from WRDS, which follows Peters and Taylor (2017).
Other antitakeover laws: BCL, CSL, DDL, FPL	Four separate indicator variables set equal to one if a firm is incorporated in a state that has adopted a business combination (BC) or control share (CS) or directors' duties (DD) or fair price (FP) law, respectively, and zero otherwise. We use adoption dates from Karpoff and Wittry (2018).
Patents	The natural logarithm of one plus stock of the number of patents. The stock is calculated using the number of patents in all previous years and a current year, and 10% discount rate with perpetual inventory method. We use the KPSS patent data.
Political Balance	The proportion of incorporated state-level representatives in the U.S. House of Representatives who are affiliated with the Republican party, in a given year. We use data from the House of Representatives.
Pop.	The population in a firm's state of incorporation in a given year. We use data from the U.S. Census Bureau.
PPill	An indicator variable equal to one if a firm has adopted a poison pill. We use data from ISS (formerly Riskmetrics), SDC's Corporate Governance and M&A databases, Comment and Schwert (1995), Caton and Goh (2008), Cremers and Ferrell (2014), Cremers, Litov and Sepe (2017), and hand-collected information from Factiva.
PPill Duration	The number of years a firm has had a poison pill ($PPill$) in-place. We take the natural logarithm of one plus this variable: $Ln(PPill\ Duration)$.
PPL	An indicator variable equal to one if a firm is incorporated in a state that passes a PPL during the period 1986 to 2009, and zero otherwise. We use adoption dates

	provided by Cain, McKeon, and Solomon (2017) and Karpoff and Wittry (2018). We also partition this variable into first wave (FW) (1986-1990) and second wave (SW) (1995-2009) adoptions.
$Premium\ Increase$	The premium increase in percentage. Data comes from the SDC M&A database.
Q	Market value of assets (total assets – book equity + market equity) divided by the book value of assets. Book equity and this measure, in general, follows Fama and French (1992). We take the natural logarithm: $Ln(Q)$.
$Q(Lowest),\ Q(Highest)$	Four separate indicator variables set to one if a firm's level of $Ln(Q)$ lies in the bottom or top quartile, respectively, of its empirical distribution.
$R\&D/Sales$	R&D stock computed with perpetual inventory method based on R&D expenditures in all previous and current year, and a discount rate of 10%, divided by the value of sales. Data comes from Compustat.
$Return$	A firm's annual stock return. Measured as the current fiscal end-year price minus last fiscal end-year price all divided by last fiscal end-year price. Data comes from CRSP.
ROA	Return on assets, defined as operating income before depreciation and amortization divided by total assets. Data comes from Compustat.
$Total\ Q$	Market value of outstanding equity plus the book value of debt minus the firm's current assets divided by the sum of the book value of property, plant, and equipment, and the replacement cost of intangible capital (the sum of the firm's externally purchased and internally created intangible capital). Calculation follows Peters and Taylor (2017). Measure and source data is available on WRDS.
$Total\ Premium$	The total percentage premium (initial premium plus any increase in the premium) offered scaled by the target firm's stock price 4-weeks prior to the initial offer. Data comes from the SDC M&A database.
$Unemploy$	The unemployment rate in a firm's state of incorporation in a given year. Data comes from the U.S. Bureau of Labor Statistics.

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

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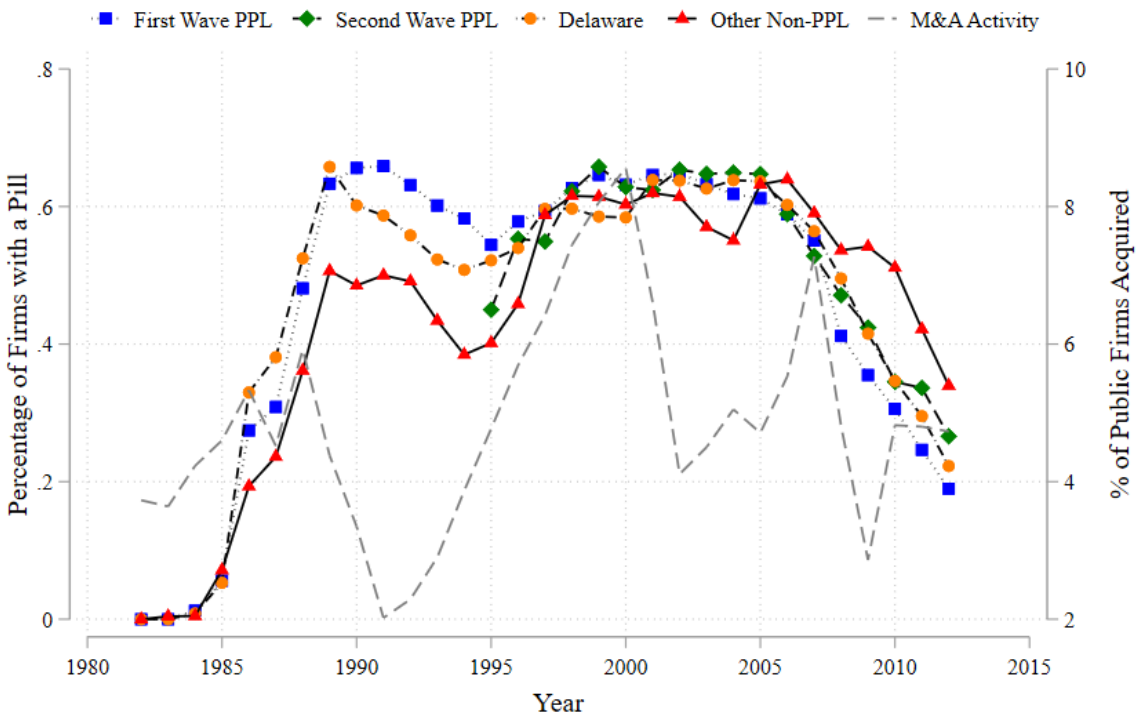


Figure 1
Percentage of firms with a poison pill
The figure plots the percentage of firms with a poison pill in-place ($PPill$) each year from 1982 to 2012, for various partitions of our sample: (i) firms incorporated in a state that has adopted a first wave-poison pill law (FW-PPL), enacted between 1986 and 1990 (dotted line with blue squares), (ii) firms incorporated in a state that has adopted a second wave-PPL (SW-PPL), adopted between 1995 and 2009 (dashed line with green diamonds), (ii) firms incorporated in Delaware (dashed line with orange circles), and (iv) firms incorporated in states that have not (or had not yet) adopted a PPL (No-PPL) (dashed line with red triangles). In addition, the gray dashed line shows the measure of M&A activity: (v) the share of public firms that were acquired in a given year (right axis).

Table 1
Descriptive statistics for the main variables

Panel A: Dependent Variables

	Mean	St. Dev	P25	Median	P75	Obs
Poison Pill	0.590	0.492	0	1	1	30461
Ln(Pill Duration)	1.974	0.668	1.61	2.08	2.48	17970
Ln(Q)	0.494	0.535	.109	.4	.783	30964
Q	1.938	1.409	1.12	1.49	2.19	30964
Ln(Total Q)	0.038	0.524	-.311	-.011	.342	30964
Excess Return	0.000	0.002	-7.0e-04	1.8e-04	1.1e-03	31501
Takeover Bid	0.008	0.087	0	0	0	23120
Acquired	0.028	0.525	0	0	0	23120
Total Premium	0.220	0.181	.087	.156	.307	178
Premium Increase	0.002	0.027	-8.6e-03	6.3e-04	.012	178

Panel B: Independent Variables

	Mean	St. Dev	P25	Median	P75	Obs
PPL	0.312	0.463	0	0	1	32011
BCL	0.919	0.272	1	1	1	32011
CSL	0.243	0.429	0	0	0	32011
DDL	0.299	0.458	0	0	1	32011
FPL	0.286	0.452	0	0	1	32011
R&D/Sales	0.126	0.226	6.6e-03	.035	.137	20327
Intangible Capital	0.588	0.402	.27	.539	.818	31997
Ln(1+Patents)	1.491	1.970	0	0	2.77	32011
Research Quotient	0.127	0.060	.094	.126	.163	13942

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

Table 2
Second wave-PPL adoptions

Dependent variable: $PPL_{[t]}$	(1)	(2)	(3)	(4)
$BCL_{[t-1]}$	1.756**	34.35***	37.32***	2.93***
	(2.43)	(39.46)	(26.56)	(3.13)
$CSL_{[t-1]}$	-0.450	-0.209	-0.387	-0.878
	(-0.82)	(-0.40)	(-0.62)	(-0.84)
$DDL_{[t-1]}$	-0.0352	-0.930	-0.872	-0.257
	(-0.05)	(-1.07)	(-1.03)	(-0.40)
$FPL_{[t-1]}$	1.876***	2.783***	2.787***	2.471
	(2.69)	(4.54)	(4.54)	(1.46)
$Inc.SY\ PPill_{[t-1]}$		-0.006	0.048	
		(-0.01)	(0.10)	
$Inc.SY\ Ln(Q)_{[t-1]}$		0.574	0.507	
		(0.82)	(0.74)	
$Inc.SY\ Return_{[t-1]}$		-0.106	-0.156	
		(-0.13)	(-0.20)	
$Inc.SY\ ROA_{[t-1]}$		-0.353	-0.461	
		(-0.91)	(-1.18)	
$Inc.SY\ Takeover\ Prob._{[t-1]}$			-1.129	
			(-1.23)	
$Inc.SY\ M\&A\ Volume_{[t-1]}$			-0.001	
			(-0.01)	
$Ln(Inc.SY\ GDPPC)_{[t-1]}$				-0.774
				(-0.95)
$Inc.SY\ GDP\ Growth_{[t-1]}$				0.550
				(0.66)
$Political\ Balance_{[t-1]}$				-0.462
				(-0.95)
$Ln(Inc.SY\ Pop)_{[t-1]}$				-0.406
				(-0.42)
$Inc.SY\ Unemploy_{[t-1]}$				-0.137
				(-0.18)
$Inc.SY\ Est\ Entry_{[t-1]}$				-0.0908
				(-0.09)
$Inc.SY\ Est\ Exit_{[t-1]}$				-0.513
				(-0.37)
Year FE	Yes	Yes	Yes	Yes
N	485	349	324	443

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

Table 3
PPLs and visible pills

Dependent Variables			$Ppill_{[t]}$			New $Ppilll_{[t]}$	Ln(PPill Duration)$_{[t]}$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$PPL_{[t]}$	-0.0274	-0.0243	-0.0501	0.0188	-0.00308	0.0113	-0.0274
	(-0.55)	(-0.49)	(-1.06)	(0.43)	(-0.08)	(0.18)	(-0.47)
$Q(Lowest)_{[t-1]}$		0.048***					
		(7.16)					
$Q(Highest)_{[t-1]}$		-0.050***					
		(-5.66)					
PPL[t] X Q(Lowest)$_{[}\tau(s)-1_{]}$			0.136***		0.0957*	-0.0504	0.250**
			(2.97)		(1.97)	(-0.54)	(2.76)
PPL[t] X Q(Highest)$_{[}\tau(s)-1_{]}$				-0.154**	-0.134*	-0.0740	-0.0858
				(-2.44)	(-2.00)	(-1.01)	(-1.36)
Other Antitakeover Laws	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm, Div X Year, Ind-Yr FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	31391	31391	31391	31391	31391	11779	16042
Adjusted R2	0.579	0.581	0.580	0.580	0.580	0.173	0.902
F-test for Q1=Q4					9.520	0.0718	17.67

The table presents results from OLS regressions analyzing the implications of PPLs for firm-level poison pill decisions over the sample period 1992 to 2012. Dependent variables include: $PPill_{[t]}$– an indicator for whether a firm has a poison pill in-place in year t; $New\ PPill_{[t]}$– an indicator for the first time a firm adopts a poison pill; $Ln(PPill\ Duration)_{[t]}$–a count variable for the number of years a firm has a pill in-place. $PPL_{[t]}$ is an indicator variable equal to one if the firm is incorporated in a state with a PPL. $Q(Lowest)_{[t-1]}$ and $Q(Highest)_{[t-1]}$ ($Q(Lowest)_{[\tau(s)-1]}$ and $Q(Highest)_{[\tau(s)-1]}$) are indicator variables that equal one if a firm's level of Tobin's Q (in the year before the adoption of its respective PPL ($\tau(s)-1$)) lies in the bottom and top quartile, respectively, of its empirical distribution. Columns 6-7 only include firms that eventually adopt a pill, while column 6 excludes firms after they adopt a pill. Controls for other antitakeover laws include: BCL, CSL, DDL, and FPL. Division fixed effects are measured using U.S. Census divisions and industry fixed effects are defined by three-digit SIC codes. Continuous variables are winsorized at the 5% level in both tails. The bottom row includes F-statistics for the test of the total effect for Q(Lowest) and Q(Highest) firms being equal. Appendix Table A1 provides variable definitions. t-statistics (two-way clustered by state of incorporation and year) are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Table 4
PPLs and firm value

Dependent variable: $Ln(Q)_{[t]}$	(1)	(2)	(3)	(4)	(5)
$PPL_{[t]}$	0.0472**	0.0442**	0.0680***	0.0645***	0.0715***
	(2.74)	(2.35)	(5.80)	(5.04)	(3.34)
$PPL_{[t]} \times PPill_{[\tau(s)-1]}$			-0.0357	-0.0292	-0.0362+
			(-1.47)	(-1.16)	(-1.72)
$PPill_{[t-1]}$		-0.0574***			
		(-5.07)			
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	29213	29213	29213	29213	29223
Adjusted R^2	0.659	0.660	0.659	0.658	0.659

The table presents results from OLS regressions analyzing the value implications of PPLs over the period 1992 to 2012. The dependent variable is the natural logarithm of Tobin's Q ($Ln(Q)$). The "Other antitakeover laws" include: BCL, CSL, DDL, and FPL. Division (region) fixed effects are measured using U.S. Census divisions (regions), state fixed effects are based on a firm's state of location, and industry fixed effects are defined by three-digit SIC codes. Continuous variables are winsorized at the 5% level in both tails. Appendix Table A1 provides variable definitions. t-statistics (two-way clustered by state of incorporation) and year are reported in parentheses. +, *, **, and *** denote significance at the 15%, 10%, 5%, and 1% level, respectively.

Table 5
PPLs and firm value in alternative samples

Dependent Variable: $Ln(Q)_{[t]}$	(1)	(2)	(3)	(4)	(5)	(6)
$PPL_{[t]}$	0.155*	0.218***	0.154***	0.0766***	0.0340**	0.0862***
	(2.04)	(3.34)	(3.81)	(4.32)	(2.25)	(6.94)
$PPL_{[t]} \times PPill_{[\tau(s)-1]}$		-0.0861	-0.0493			-0.0796***
		(-0.88)	(-1.20)			(-3.12)
Other antitakeover laws	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Division × Year FE	Yes	Yes	No	Yes	Yes	Yes
Region × Year FE	Yes	Yes	No	Yes	Yes	Yes
N	1735	1735	2278	66819	83489	33921
Adjusted R2	0.683	0.683	0.696	0.649	0.629	0.635
Sample		Matched		Full (Spamann)	Full	With Catan Pills

The table presents results from OLS regressions for a matched sample (columns 1-3) and extended samples (columns 4-5). Treated (control) firms in matching procedure 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 $PPill$. Columns 1-3 show the matched sample $Ln(Q)$ regression results over a $t \pm 3$ estimation window. Columns 4 and 5 drop the visible pill data requirement and extend the sample to all firm-years available in the data. Column 4 uses firm-years for which historical incorporations data is available in Holger Spamann's dataset. Column 5 extends this set by supplementing historical incorporation data with observations from Compact Disclosure and CRSP Historical. Column 6 uses the main sample but relies on an alternative measures of poison pills. The "Other antitakeover laws" include: BCL, CSL, DDL, and FPL. Division fixed effects are measured using U.S. Census divisions, and industry fixed effects are defined by three-digit SIC codes. Continuous variables are winsorized at the 5% level in both tails. t-statistics (two-way clustered by state of incorporation and year) are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Table 6
PPLs and alternative measures of firm value

Panel A: The implications of PPLs for:	$Q_{[t]}$	$Ln(Tot\ Q_{[t]})$	$ROA_{[t]}$	$Excess\ Return_{[t]}$
	(1)	(2)	(3)	(4)
$PPL_{[t]}$	0.160***	0.0462***	-0.00123	0.000181**
	(3.28)	(2.90)	(-0.27)	(2.09)
$PPL_{[t]} \times PPill_{[\tau(s)-1]}$	-0.0935	-0.0387	0.00197	0.000106
	(-1.32)	(-1.02)	(0.22)	(0.69)
Firm FE	Yes	Yes	Yes	Yes
Division × Year FE	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes
N	29213	29213	29503	29024
Adjusted R^2	0.574	0.625	0.725	0.108

Panel B: Portfolio analysis

Portfolio "6m36"	Four-factor model			Three-factor model			Market-factor model		
	Long	Short	Long - Short	Long	Short	Long - Short	Long	Short	Long - Short
$Alpha$ (monthly)	0.011**	-0.003	0.013***	0.012**	-0.002	0.014***	0.009**	0.001	0.007
	(2.31)	(-0.69)	(2.62)	(2.45)	(-0.64)	(2.78)	(1.96)	(0.33)	(1.50)
N	140	140	-	140	140	-	140	140	-
Adjusted R^2	0.262	0.351	0.022	0.261	0.353	0.026	0.232	0.340	0.005

Portfolio "12m36"	Four-factor model			Three-factor model			Market-factor model		
	Long	Short	Long - Short	Long	Short	Long - Short	Long	Short	Long - Short
$Alpha$ (monthly)	0.010***	-0.000	0.010**	0.009**	-0.002	0.011**	0.008	0.001	0.007*
	(2.63)	(-0.18)	(2.29)	(2.30)	(-0.90)	(2.52)	(2.27)	(0.29)	(1.65)
N	146	146	-	146	146	-	146	146	-
Adjusted R^2	0.280	0.375	0.014	0.281	0.378	0.018	0.257	0.359	0.004

The table examines the effect of PPLs on alternative measures of firm value. Panel A reports results from OLS regressions with dependent variables being: Q, $Ln(Total\ Q)$, $Excess\ Return$, and ROA. The "Other antitakeover laws" include: BCL, CSL, DDL, and FPL. Division fixed effects are measured using U.S. Census divisions, and industry fixed effects are defined by three-digit SIC codes *t*-statistics (two-way clustered by state of incorporation and year) are reported in parentheses. Continuous variables are winsorized at the 5% level in both tails. Panel B shows results from a portfolio analysis using the matched sample $Treated$ and control firms. The long (short) portfolios are constructed as follows: For portfolios "6m36" and "12m36," we include all stocks of matched $Treated$ (control) firms starting either 6 or 12 months before the fiscal year-end of the year in which the matched treated incorporating state adopts a PPL and hold these stocks for 36 months, post-adoption. The long-short portfolios are then created by differencing the portfolio returns of the long and short portfolios for each respective month. We use the four-factor, three-factor, and market factor models to estimate $Alpha$ (monthly), where each of the models uses an equally-weighted market factor, and we calculate the portfolio return with each stock weighted by its market capitalization immediately preceding its inclusion in the portfolio. t-statistics (based on robust standard errors) are presented in parentheses. The "N" row shows the total number of firms with useable returns. We include only years up to 2006, because PPLs in later years were passed only by a two small states, and as a result after 2006 the portfolio only consists of one company. Portfolio returns are winsorized at the 5% level in both tails, and the Long-Short difference is calculated by differencing the winsorized returns. Appendix Table A1 provides variable definitions. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Table 7
Testing the mechanisms

Panel A: Bonding Hypothesis
Dependent Variable: Ln(Q)[t]

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$PPL_{[t]}$	0.0747***	0.0705***	0.0684***	0.0526***	0.0659***	0.0679**	0.0582*
	(6.02)	(6.33)	(5.69)	(3.26)	(5.02)	(2.23)	(2.05)
$PPL_{[t]} \times PPill_{[\tau(s)-1]}$	-0.0364	-0.0404*	-0.0425	-0.0387	-0.0351	-0.0357	-0.054+
	(-1.49)	(-1.98)	(-1.68)	(-1.64)	(-1.41)	(-1.49)	(-1.72)
$PPL_{[t]} \times Intangible\ Capital_{[\tau(s)-1]}$	0.174***						
	(3.00)						
$PPL_{[t]} \times Organiz.Capital_{[\tau(s)-1]}$		0.067***					
		(3.77)					
$PPL_{[t]} \times R\&D\ Sales_{[\tau(s)-1]}$			0.115**				
			(2.51)				
$PPL_{[t]} \times Patents_{[\tau(s)-1]}$				0.0296			
				(1.33)			
$PPL_{[t]} \times Forecast\ Error_{[\tau(s)-1]}$					0.0421***		
					(5.26)		
$PPL_{[t]} \times Large\ Customer_{[\tau(s)-1]}$						0.0054	-0.044
						(0.07)	(-0.68)
$PPL_{[t]} \times Large\ Cust_{[\tau(s)-1]} \times High\ Intangibles$							0.130**
							(2.58)
N	29213	29213	29213	29213	29213	29213	29213
Adjusted R^2	0.659	0.659	0.659	0.659	0.659	0.659	0.659

Panel B: Bargaining Power Hypothesis

	$Bid_{[t]}$	$Acquired_{[t]}$	Total $Premium_{[t]}$	Premium $Increase_{[t]}$	$Bid_{[t]}$	$Acquired_{[t]}$
	(1)	(2)	(3)	(4)	(5)	(6)
$PPL_{[t]}$	-0.00630*	-0.0263^{+}	0.00487	0.0211*	-0.00626*	-0.0261+
	(-1.87)	(-1.58)	(0.04)	(2.07)	(-1.92)	(-1.58)
$PPL_{[t]} \times PPill_{[\tau(s)-1]}$	0.00513*	0.0377*	-0.0463	0.00115	0.00491^{+}	0.0370*
	(1.75)	(1.75)	(-0.68)	(0.20)	(1.65)	(1.72)
$PPL_{[t]} \times PPill_{[\tau(s)-1]} \times High\ Intng\ Cap_{[\tau(s)-1]}$					-0.0107***	-0.0372*
					(-3.41)	(-2.03)
N	22007	22007	128	128	22007	22007
Adjusted R^2	0.0016	0.0173	0.181	0.0467	0.0018	0.0174
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

The table presents results from OLS regressions analyzing the heterogeneous value implications of PPLs (Panel A) and takeover implications of PPLs (Panel B) over the period 1992 to 2012. The dependent variable in Panel A is the natural logarithm of Tobin's Q ($Ln(Q_{[t]})$). The main independent variable, $PPL_{[t]}$, is interacted with the following measures of innovative activity and stakeholder relationships – $R\&D/Sales_{[\tau(s)-1]}$, $Intangible\ Capital_{[\tau(s)-1]}$, $Patents_{[\tau(s)-1]}$, and $RQ_{[\tau(s)-1]}$– measured in the year before the adoption of a PPL-firm's respective PPL. The interacted variables are standardized to have a mean of zero and a standard deviation of one. In Panel B, the dependent variables include: $Bid_{[t]}$, $Acquired_{[t]}$, $Total\ Premium_{[t]}$ and $Premium\ Increase_{[t]}$. Bid ($Acquired$) is an indicator equal to one if a firm receives a takeover bid (acquired) as cataloged by the SDC M&A database. $Total\ Premium$ ($Premium\ Increase$) 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. Division fixed effects are measured using U.S. Census divisions, and industry fixed effects are defined by three-digit SIC codes. Continuous variables are winsorized at the 5% level in both tails. Appendix Table A1 provides variable definitions. *t*-statistics (two-way clustered by state of incorporation and year) are reported in parentheses. $^{+}$, *, **, and *** denote significance at the 15%, 10%, 5%, and 1% level, respectively

ONLINE APPENDIX OF

SHADOW PILLS, VISIBLE PILL POLICY, AND FIRM VALUE

Additional References

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

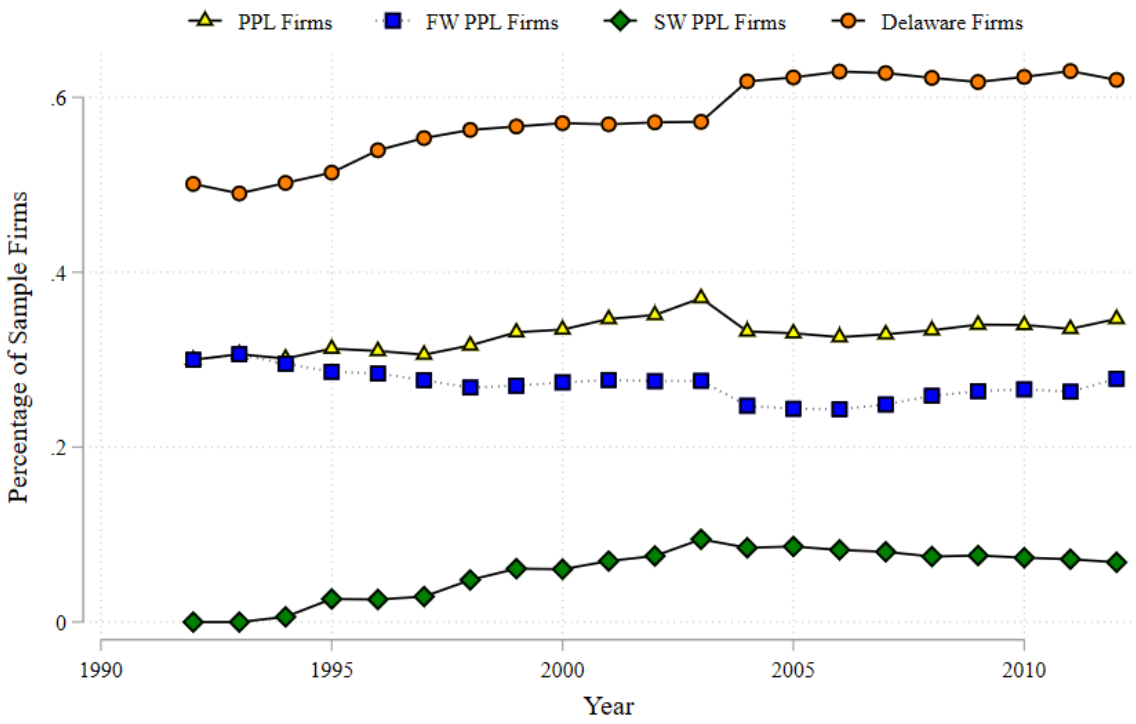


Figure OA1

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

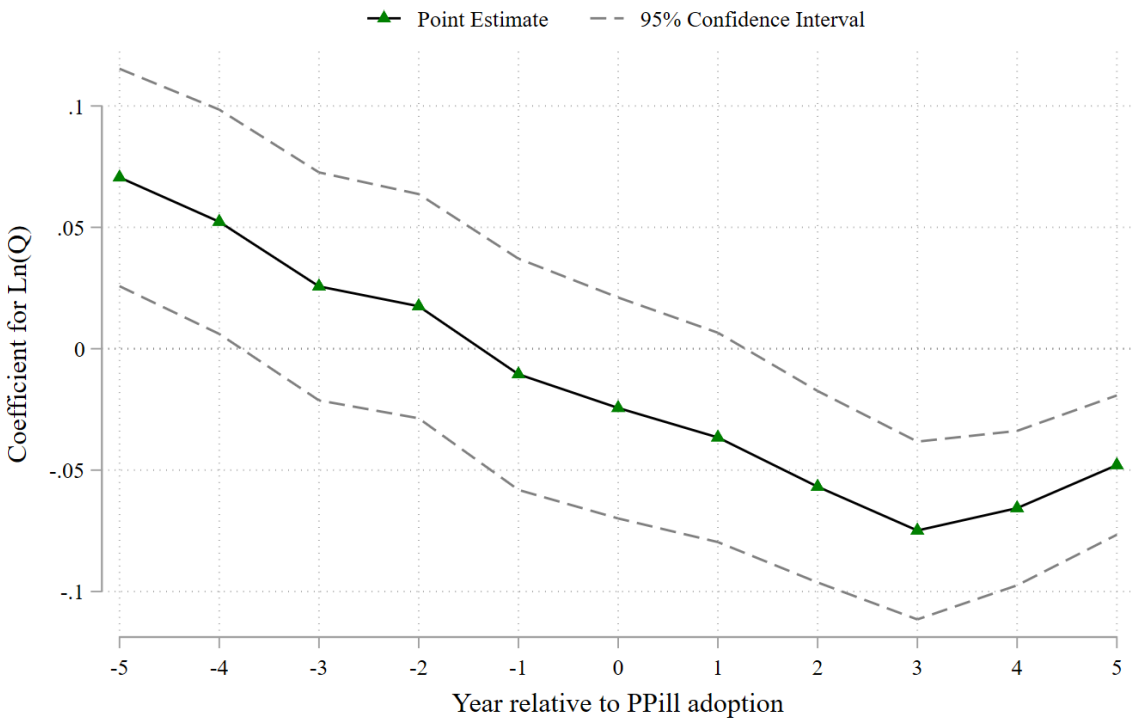


Figure OA2

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

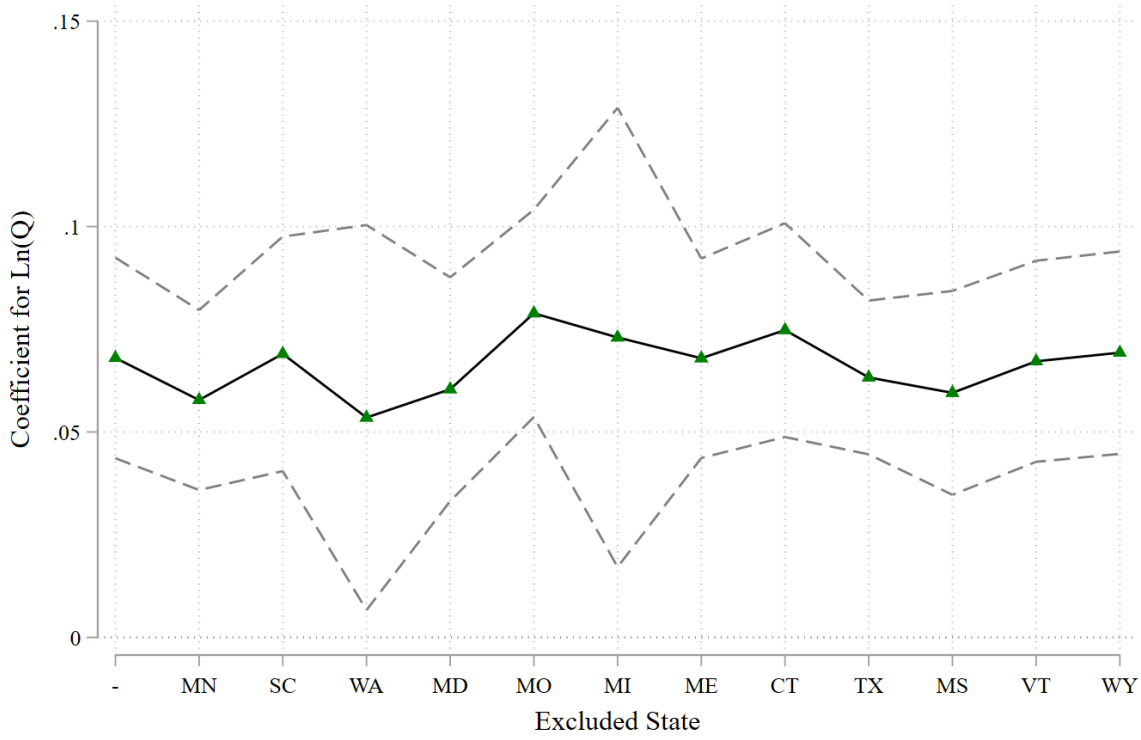
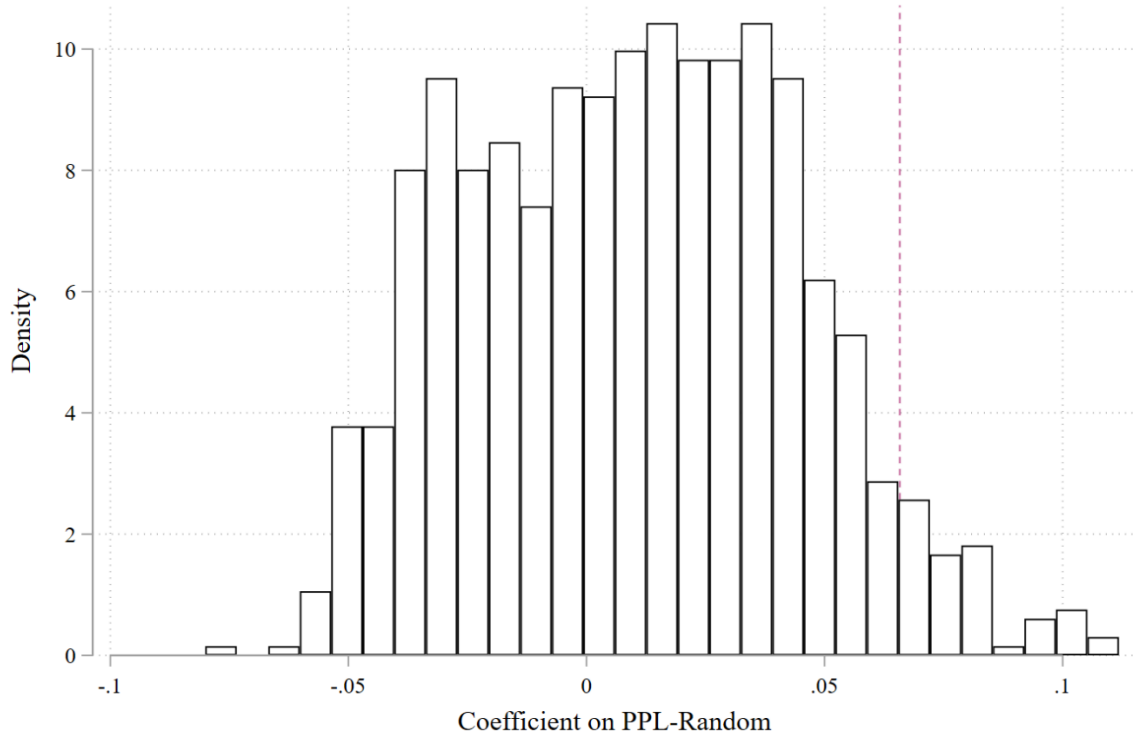


Figure OA3

The figure plots the point estimates (y-axis) for our baseline regressions of $Ln(Q)$ on PPL (i.e., Table 4, Column 3), but where we exclude each law adopting state (x-axis) one-by-one over the period 1992 to 2012. The dashed lines correspond to 95% confidence intervals and green triangles (blue squares) indicate significance at the 1% (5%) level.



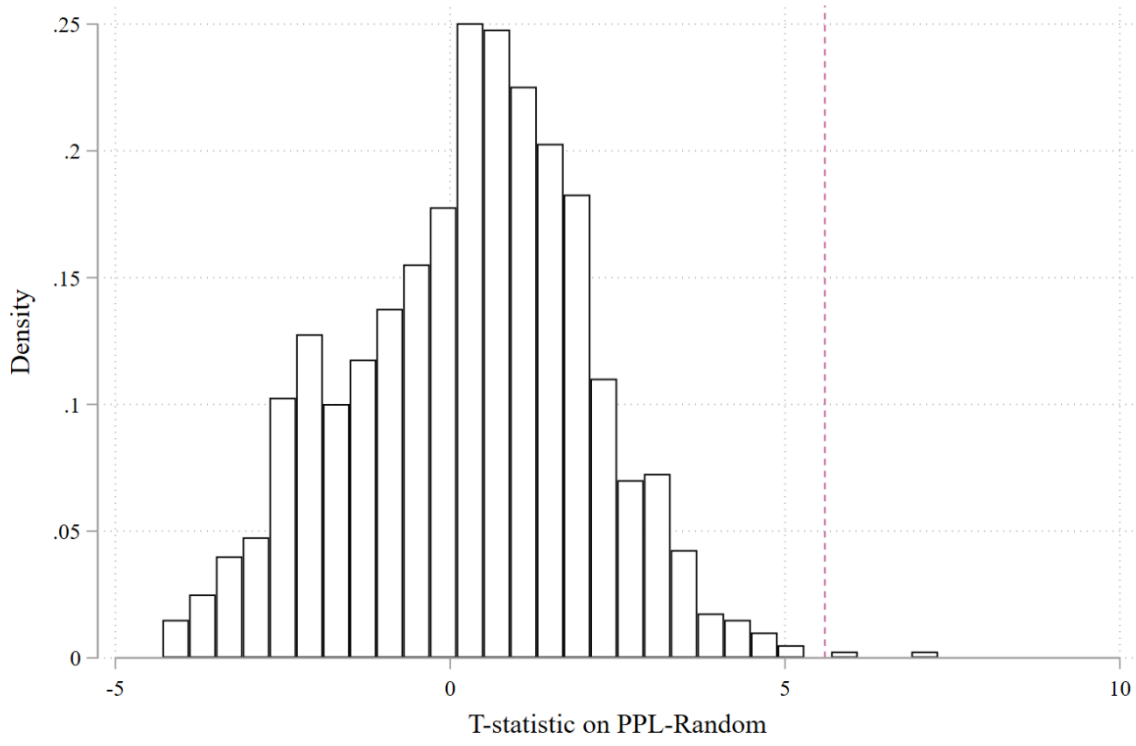


Figure OA4
Full sample placebo test
The figures plot the distribution of coefficient (top) and *t*-statistic (bottom) estimates from randomized PPL adoption dates across different states. We simulate fictitious adoptions by randomly assigning states PPLs but maintain the structure of the empirical distribution of actual adoptions. We repeat the estimation 1,000 times. In each of the pseudo samples, we then run the regression as in Table 4, Column 3 and plot the corresponding coefficients and *t*-statistics. The dashed red vertical lines represent the actual regression coefficient and *t*-statistic based on the actual data.

Table OA1
First wave-PPLs and firm value

Dependent variables:	$Ln(Q)_{[t]}$	$Q_{[t]}$	Ln($Tot\ Q_{[t]}$)	$Excess\ Return_{[t]}$	$ROA_{[t]}$
	(1)	(2)	(3)	(4)	(5)
$FW\ PPL_{[t]}$	-0.0238	-0.0438	-0.0277	0.000038	0.00032
	(-1.38)	(-1.18)	(-1.33)	(1.12)	(0.11)
$FW\ PPL_{[t]} \times PPill_{[\tau(s)-1]}$	0.0328	0.0235	0.0203	0.000093	0.00465
	(1.25)	(0.38)	(0.73)	(1.16)	(0.66)
Other antitakeover laws	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Division × Year FE	Yes	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes	Yes
N	8428	8428	8428	8523	8625
Adjusted R^2	0.786	0.763	0.785	0.122	0.732

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

Table OA2
PPL adoption dates

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

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

[a] The *Moran v. Household* court decision in Delaware in 1985 provides some legitimacy to poison pills. However, Delaware never issued a PPL, thus we treat Delaware as a control state.

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

Table OA3
First wave-PPLs and visible poison pills

Dependent variable: $PPill_{[t]}$	Entire FW-period (1983-1993)		Post-*Moran* (1986-1993)		Post-*Interco* & *Pillsbury* (1989-1993)	
	(1)	(2)	(3)	(4)	(5)	(6)
$FW\ PPL_{[t]}$	0.00245	0.00224	0.0539	0.0594	-0.0233	0.0204
	(0.07)	(0.06)	(1.44)	(1.48)	(-0.44)	(0.26)
$FW\ PPL_{[t]} \cdot Q(lowest)_{[\tau-1]}$	0.0366	0.0367	0.00848	0.00705	0.0744**	0.0844**
	(0.67)	(0.67)	(0.11)	(0.09)	(2.05)	(2.56)
$FW\ PPL_{[t]} \cdot Q(highest)_{[\tau-1]}$	-0.135	-0.135	-0.122	-0.128	0.262	0.290
	(-0.89)	(-0.88)	(-0.66)	(-0.69)	(1.14)	(1.25)
Eventual SW PPL		0.00350		-0.0523		-0.0971
		(0.07)		(-1.13)		(-1.60)
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	8302	8302	6572	6572	4436	4436
Adjusted R^2	0.674	0.674	0.685	0.685	0.833	0.833

The table presents results from OLS regressions exploring the implications of FW-PPLs for firm-level poison pill decisions over the entire FW-period (1983 to 1993), as well as post-*Moran* (1986 to 1993), and post-*Interco* & *Pillsbury* (1989 to 1993) sample periods. The dependent variable $PPill$ is an indicator for whether a firm has a poison pill in-place as of the current year. $FW\ PPL$ is an indicator for whether a state has adopted a PPL at any point in time between 1986 and 1990. $Q(lowest)_{[\tau-1]}$ ($Q(highest)_{[\tau-1]}$) is an indicator for firm's Tobin Q being in the lowest (highest) quartile at the moment of the passage of PPL law. $Eventual\ SW\ PPL$ is a dummy variable equal to one if a firm is incorporated in a state that adopts a PPL during the period 1995 to 2009. The "Other antitakeover laws" include: BCL, CSL, DDL, and FPL. Division fixed effects are measured using U.S. Census divisions, and industry fixed effects are defined by three-digit SIC codes. Continuous variables are winsorized at the 5% level in both tails variables and are standardized to have zero mean and unit variance. Appendix Table A1 and Online Appendix Table OA13 provide variable definitions. *t*-statistics (clustered by state of incorporation, but not by year given the short time frame of each regression) are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Table OA4
Short-run event study

Panel A: Event-study results

	Introduction	Final passage	Signed by Governor
	(1)	(2)	(3)
[-30,-2]	-0.68%	1.34%**	1.79%***
	(-1.01)	(1.97)	(2.81)
[-25,-2]	0.02%	1.17%*	1.92%***
	(0.03)	(1.82)	(3.11)
[-20,-2]	0.02%	0.93%*	1.76%***
	(0.04)	(1.65)	(3.26)
[-15,-2]	0.39%	0.81%	1.61%***
	(0.91)	(1.63)	(3.51)
[0,0]	0.24%	-0.07%	0.19%
	(1.01)	(-0.53)	(1.14)
[0,1]	0.64%**	-0.12%	0.26%**
	(2.07)	(-0.66)	(2.02)
[0,4]	1.30%***	0.31%	0.48%*
	(3.60)	(1.03)	(1.79)
[1,10]	1.00%**	1.09%***	0.54%
	(2.12)	(2.72)	(1.49)

Panel B: Event-study dates

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

The table reports on a short-run event study. Panel A presents the precision weighted cumulative abnormal returns (CARs) surrounding important events in the eventual ratification of a SW-PPL for firms incorporated in these states; these events include its introduction on the House or Senate floor, final passage by the House or Senate, and Governor's approval.[1] CARs are estimated over the event windows [0,0], [0,+1], [0,+4], and [+1,+10] and pre-event windows [-30,-2], [-25,-2], [-20,-2], [-15,-2]. CARs are estimated using the Fama-French four-factor model where the market factor is based on CRSP value-weighted returns, and the remaining three factors include: small-minus-big (SMB), high-minus-low (HML, and momentum (MOM). The parameters of the four-factor model are estimated over the window [-300,-101] relative to the respective event's date. The estimated *t*-statistics have been corrected for cross-sectional correlation (following Kolari and Pynnönen, 2010) and are shown in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively. Panel B provides the exact date of the three events used in this study for each of the SW-PPL adopting states. The sample includes 1510 firms incorporated in the treated states.

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

Table OA5
Adjusting the sample for Delaware case law

Dependent variable: $Ln(Q)_{[t]}$	Full Sample (1992 to 2012) Delaware firms' $PPL = 1$		Full Sample (1992 to 2012) Delaware firms excluded	
	(1)	(2)	(3)	(4)
$PPL_{[t]}$	0.0251+	0.0274*	0.0285	0.0685**
	(1.65)	(1.98)	(1.16)	(2.23)
$PPL_{[t]} \times PPill_{[\tau(s)-1]}$		-0.00452		-0.0635
		(-0.16)		(-1.42)
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	29213	29213	10131	10131
Adjusted R^2	0.659	0.659	0.665	0.665

The table presents results from OLS regressions analyzing the value implications of PPLs adjusted for Delaware case law. The first two columns adjust the sample by re-coding PPL equal to one for firms incorporated in Delaware after the *Moran* court decision in 1985, while the last two columns exclude firms incorporated in Delaware entirely. The dependent variable is the natural logarithm of Tobin's Q ($Ln(Q)$). The "Other antitakeover laws" include: BCL, CSL, DDL, and FPL. Division fixed effects are measured using U.S. Census divisions and industry fixed effects are defined by three-digit SIC codes. $Treated$ is absorbed by the firm fixed effects. Continuous variables are winsorized at the 5% level in both tails. Table A1 provides variable definitions. t-statistics (two-way clustered by state of incorporation and year) are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Table OA6
PPV-Index

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

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

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

	(1)	(2)	(3)	(4)
$PPV\text{-}Index_{[t]}$	0.0472**	0.0680***	0.0645***	0.0715***
	(2.74)	(5.28)	(5.04)	(3.34)
$PPL_{[t]} \times PPill_{[\tau(s)-1]}$		-0.0357	-0.0292	-0.0362
		(-1.48)	(-1.18)	(-1.72)
Other antitakeover laws	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Division × Year FE	Yes	Yes	No	No
Region × Year FE	No	No	Yes	No
State × Year FE	No	No	No	Yes
Industry × Year FE	Yes	Yes	Yes	Yes
N	29213	29213	29213	29223
Adjusted R^2	0.659	0.659	0.658	0.659

The table describes the poison pill validity index ($PPV\text{-}Index$) and reports results from OLS regressions analyzing its implications for firm value over the sample period 1992 to 2012. Panel A details the construction of the $PPV\text{-}Index$. We create this variable using poison pill statute and poison pill case information provided by Cain, McKeon, and Solomon (2017). Panel B explores the effect of $PPV\text{-}Index$ on $Ln(Q)$. The "Other antitakeover laws" include: BCL, CSL, DDL, and FPL. Division (region) fixed effects are measured using U.S. Census divisions (regions), state fixed effects are defined by a firm's state of location, and industry fixed effects are defined by three-digit SIC codes. Continuous variables are winsorized at the 5% level in both tails. Table A1 and Online Appendix Table OA13 provide variable definitions. t-statistics (two-way clustered by state of incorporation and year) are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Table OA7
Controlling for PPL-firm characteristics

Dependent variables:		$Ln(Q)_{[t]}$		$Q_{[t]}$	$Ln(Tot\ Q_{[t]})$	$Excess\ Return_{[t]}$	$ROA_{[t]}$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$PPL_{[t]}$	0.0742***	0.0764**	0.0760**	0.268***	0.0355	0.000215*	0.00194
	(3.12)	(2.72)	(2.58)	(3.00)	(1.12)	(1.89)	(0.25)
$PPL_{[t]} \times PPill_{[\tau(s)-1]}$		-0.0384	-0.0430	-0.0639	-0.0425	-0.000115	0.00652
		(-1.47)	(-1.13)	(-0.97)	(-0.80)	(-0.81)	(0.60)
$PPL_{[t]} \times Ln(Assets)_{[\tau(s)-1]}$	0.0242	0.0367	0.0624	0.0780	0.0503	0.000330	0.00796
	(0.66)	(0.92)	(1.42)	(0.49)	(0.95)	(1.45)	(0.73)
$PPL_{[t]} \times Ln(Assets)^2_{[\tau(s)-1]}$	-0.0324	-0.0392	-0.0794**	-0.180*	-0.0666*	-0.000300*	-0.00552
	(-0.91)	(-1.07)	(-2.65)	(-1.90)	(-2.02)	(-1.92)	(-0.77)
$PPL_{[t]} \times SG_{[\tau(s)-1]}$			-0.0180	-0.0123	-0.0162	-0.000222***	0.00423
			(-0.96)	(-0.19)	(-0.90)	(-3.78)	(1.06)
$PPL_{[t]} \times Loss_{[\tau(s)-1]}$			0.0162**	0.0640***	-0.00181	0.0000493	-0.000600
			(2.35)	(3.52)	(-0.19)	(0.81)	(-0.31)
$PPL_{[t]} \times DEQ_{[\tau(s)-1]}$			0.0341**	0.0861*	0.0325**	0.000138**	0.00568+
			(2.56)	(1.89)	(2.67)	(2.34)	(1.58)
$PPL_{[t]} \times FLIQ_{[\tau(s)-1]}$			-0.0113	-0.0353	-0.00675	0.000116	-0.00586
			(-0.79)	(-0.63)	(-0.28)	(1.21)	(-1.40)
$PPL_{[t]} \times CAPX/Assets_{[\tau(s)-1]}$			-0.0153	-0.0150	-0.0186	-0.0000437	-0.00603*
			(-0.69)	(-0.27)	(-0.75)	(-0.77)	(-1.79)
$PPL_{[t]} \times IO_{[\tau(s)-1]}$			0.0137*	0.0357*	0.0210**	-0.0000615	-0.000841
			(1.73)	(2.04)	(2.83)	(-1.45)	(-0.41)
Firm Age	0.0108	0.0112	0.0107	0.0427	0.0249***	0.00000688	-0.00217
	(1.49)	(1.47)	(1.39)	(1.27)	(3.42)	(0.20)	(-0.90)
Other antitakeover laws	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Division × Year FE	Yes	Yes	No	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	29213	29213	29213	29213	29213	29024	29503
Adjusted R^2	0.659	0.659	0.659	0.575	0.625	0.109	0.725

The table presents results from OLS regressions analyzing the value implications of PPLs over the period 1992 to 2012, controlling for firm characteristics. The dependent variables include: the natural logarithm of Tobin's Q ($Ln(Q)$); Tobin's Q (Q); Total Tobin's Q ($Total\ Q$); risk-adjusted, excess stock returns ($Excess\ Return$); return on assets (ROA). We control for PPL-firm characteristics by interacting the level of the respective PPL-firm characteristic in the year before the adoption of the respective PPL ($\tau(s)-1$) with PPL (except for firm age, which is deterministic and its contemporaneous value is included). The firm characteristics include: $Ln(Assets)$, sales growth (SG), a dummy indicating if a firm has negative net income ($Loss$), debt-to-equity ratios (DEQ), firm liquidity ($FLIQ$), $CAPX/Assets$, and institutional ownership (IO). The "Other antitakeover laws" include: BCL, CSL, DDL, and FPL. Division fixed effects are measured using U.S. Census divisions, and industry fixed effects are defined by three-digit SIC codes. Continuous variables are winsorized at the 5% level in both tails and the continuous controls are standardized to have a mean of zero and a standard deviation of one. Appendix Table A1 and Online Appendix Table OA13 provide variable definitions. t-statistics (two-way clustered by state of incorporation and year) are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Table OA8
State-by-year fixed effects

Dependent variable: $Ln(Q)_{[t]}$	(1)	(2)	(3)	(4)
$PPL_{[t]}$	0.0506*	0.0715***		
	(1.90)	(3.34)		
$PPL_{[t]} \times Same\ Inc\text{-}HQ\ State_{[t]}$			0.0595**	0.0831***
			(2.32)	(4.08)
$PPL_{[t]} \times Diff.Inc\text{-}HQ\ State_{[t]}$			0.0283	0.0485+
			(0.83)	(1.60)
$PPL_{[t]} \times PPill_{[\tau(s)-1]}$		-0.0362+		-0.0392*
		(-1.72)		(-1.99)
Other antitakeover laws	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes
N	29223	29223	29223	29223
Adjusted R^2	0.659	0.659	0.659	0.659

The table reports the results for OLS regressions with state-by-year fixed effects of $Ln(Q)$ on PPL indicator variables and their interactions with $Same\ (Diff.)\ Inc\text{-}HQ\ State$ indicator variables over the period 1992-2012. $Same\ (Diff.)\ Inc\text{-}HQ\ State$ equals one if a firm's state of incorporation is the same (different) as (than) its state of location, and zero otherwise. The "Other antitakeover laws" include: BCL, CSL, DDL, and FPL. State fixed effects are defined using a firm's state of location and industry fixed effects are measured using three-digit SIC codes. Continuous variables are winsorized at the 5% level in both tails. Appendix Table A1 and Online Appendix Table OA13 provide variable definitions. t-statistics (two-way clustered by state of incorporation and year) are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Table OA9
Adjusting the sample period for Delaware case law

Dependent variables:	$Ln(Q)_{[t]}$		$Q_{[t]}$		Ln($Tot\ Q_{[t]}$)		$Excess\ Return_{[t]}$		$ROA_{[t]}$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$PPL_{[t]}$	0.0504***	0.0453**	0.105**	0.101**	0.0282+	0.0126	0.00022***	0.00020***	-0.00114	-0.00225
	(3.45)	(2.44)	(2.49)	(2.66)	(1.59)	(0.54)	(3.15)	(2.87)	(-0.24)	(-0.57)
$PPL_{[t]} \times PPill_{[\tau(s)-1]}$		0.00904		0.00754		0.0279		0.0000391		0.00196
		(0.31)		(0.17)		(0.67)		(0.30)		(0.24)
Other antitakeover laws	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Division × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	31882	31882	31882	31882	31882	31882	31714	31714	32221	32221
Adjusted R^2	0.658	0.658	0.576	0.576	0.621	0.621	0.110	0.110	0.724	0.724

The table reports results from OLS regressions analyzing the effect of PPLs on firm value using alternative sample periods that adjust for important Delaware case law events. The dependent variables include: $Ln(Q)$, Q, $Ln(Total\ Q)$, $Excess\ Return$, and ROA. We adjust the SW to begin in 1989 such that the sample spans the period 1989 to 2012. The "Other antitakeover laws" include: BCL, CSL, DDL, and FPL. Division fixed effects are measured using U.S. Census divisions and industry fixed effects are defined by three-digit SIC codes. Continuous variables are winsorized at the 5% level in both tails. Appendix Table A1 provides variable definitions. t-statistics (two-way clustered by state of incorporation and year) are reported in parentheses. +, *, **, and *** denote significance at the 15%, 10%, 5%, and 1% level, respectively.

Table OA10
Subperiods within the second wave

Dependent variable: $Ln(Q)_{[t]}$	1992-1998		1999-2005		2006-2012	
	(1)	(2)	(3)	(4)	(5)	(6)
$PPL_{[t]}$	0.118*	0.186***	0.0721*	0.0208	-0.0763	-0.0763
	(1.92)	(2.99)	(1.83)	(0.71)	(-0.55)	(-0.55)
$PPL_{[t]} \times PPill_{[\tau(s)-1]}$		-0.119		0.110**		N/A
		(-1.41)		(2.08)		
Oth antitakeov 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	9108	9108	10744	10744	8435	8435
Adjusted R^2	0.757	0.757	0.720	0.720	0.784	0.784

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

Table OA11
Excluding multi-law adopting states

Dependent variable: $Ln(Q)_{[t]}$		
	(1)	(2)
$PPL_{[t]}$	0.0343*	0.0547***
	(1.90)	(6.80)
$PPL_{[t]} \times PPill_{[\tau(s)-1]}$		-0.0368
		(-1.60)
Other antitakeover laws	Yes	Yes
Firm FE	Yes	Yes
Division × Year FE	Yes	Yes
Industry × Year FE	Yes	Yes
N	28374	28374
Adjusted R^2	0.658	0.658

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

Table OA12
Staggered boards

Dependent variable: $Ln(Q)_{[t]}$	(1)	(2)	(3)	(4)
$PPL_{[t]}$	0.0480*	0.0553*	0.0776*	0.0723*
	(1.87)	(1.74)	(1.84)	(1.92)
$Staggered\ Board_{[t-1]}$	0.00741	0.0112		
	(0.42)	(0.53)		
$PPL_{[t]} \times Staggered\ Board_{[t-1]}$		-0.0130		
		(-0.37)		
$PPL_{[t]} \times Staggered\ Board_{[\tau(s)-1]}$			-0.0561	-0.0639
			(-0.84)	(-0.83)
$PPL_{[t]} \times PPill_{[\tau(s)-1]}$				0.0172
				(0.43)
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	18408	18408	18408	18408
Adjusted R^2	0.717	0.717	0.717	0.717

The table presents results from OLS regressions analyzing the value implications of PPLs over the period 1992 to 2012. The dependent variable is the natural logarithm of Tobin's Q ($Ln(Q)$). $Staggered\ Board_{[t-1]}$ is an indicator variable set to one if a firm has a staggered board, and zero otherwise. $PPL_{[t]} \times Staggered\ Board_{[\tau(s)-1]}$ interacts an indicator variable set to one if a PPL-firm has a staggered board in the year before the adoption of its respective PPL ($\tau(s)-1$) with PPL. The "Other antitakeover laws" include: BCL, CSL, DDL, and FPL. Division fixed effects are measured using U.S. Census divisions, and industry fixed effects are defined by three-digit SIC codes. Continuous variables are winsorized at the 5% level in both tails. Appendix Table A1 and Online Appendix Table OA13 provide variable definitions. t-statistics (two-way clustered by state of incorporation and year) are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Table OA13
Variable definitions

Age	The number of firm-year observations since the firm's first appearance in Compustat. We take the natural logarithm of one plus Age: $Ln(Age)$.
$Assets$	The value of total book assets in millions, where assets are adjusted using 2015 dollars. We take the natural logarithm of this variable: $Ln(Assets)$. Data comes from Compustat.
$CAPX/Assets$	Capital expenditures divided by total assets. Data comes from Compustat.
DEQ	Debt-to-equity, defined as long-term debt divided by book equity. Data comes from Compustat.
$Diff.(Same)\ Inc\text{-}HQ\ State$	An indicator variable set equal to one if a firm's state of incorporation is different than (the same as) its state of location, and zero otherwise.
$Eventual\ SW\ PPL$	An indicator variable equal to one if a firm is incorporated in a state that will eventually pass a PPL during the second wave – SW period 1995 to 2009, and zero otherwise. We use adoption dates provided by Cain, McKeon, and Solomon (2017) and Karpoff and Wittry (2018).
$FLIQ$	Current assets minus current liabilities divided by total assets. Data comes from Compustat.
$FW\ PPL$	An indicator variable equal to one if a firm is incorporated in a state that passes a PPL during the first wave (FW) period 1986 to 1990, and zero otherwise. We use adoption dates provided by Cain, McKeon, and Solomon (2017) and Karpoff and Wittry (2018).
IO	The percent ownership of a firm by its institutional owners, measured by their equity ownership in their 13F holdings reports from Thomson Reuters, weighted by the firm's market capitalization.
$Loss$	An indicator variable set to one if a firm has negative net income during a fiscal year, and zero otherwise. Data comes from Compustat.
$PPV\text{-}Index$	We create a poison pill validity index ($PPV\text{-}Index$) using poison pill statute and poison pill case information provided by Cain, McKeon, and Solomon (2017). The $PPV\text{-}Index$ captures the relative change or strength of poison pill validity over time and by state of incorporation. For a detailed description of the $PPV\text{-}Index$, see Online Appendix Table OA10.
SG	Sales growth, defined as the natural logarithm of the value of sales in in year *t* divided by the value of sales in year *t*-1. Data comes from Compustat.
$Staggered\ Board$	An indicator variable equal to one if a firm has a staggered board. We use data from Cremers, Litov and Sepe (2017).

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

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