POISON PILLS IN THE SHADOW OF THE LAW

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Abstract

Poison pills are among the most powerful antitakeover provisions, but studying their economic impact is challenging because of the obvious endogeneity concerns. We address the problem by studying U.S. states' staggered adoption of poison pill laws (PPLs), which strengthen the right to adopt a pill, i.e., the *shadow pill*, and increase the validity of visible pills. We document that PPLs make visible pill policy aligned with economic incentives, increasing pill adoption among firms with a high likelihood of takeover but decreasing it among firms with low takeover likelihood. We also document that PPLs positively impact firm value, especially for innovative firms with more intangible assets.

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I. Introduction

Numerous studies have investigated the impact of poison pill adoption on firm value, motivated by the view that the pill is among the most powerful antitakeover defenses.[1] While earlier studies produced mixed results,[2] more recently, several empirical studies have consistently documented that adopting a pill is negatively associated with firm value (e.g., Bebchuk, Cohen, and Ferrell (2009); Cuñat, Gine, and Guadalupe (2012); Cremers and Ferrell (2014)).

These results, however, are difficult to interpret because the decision to adopt a pill is endogenous. The board of directors can unilaterally decide to adopt a pill without shareholder approval so that even firms that do not currently have a "visible" pill still have a "shadow" pill,

[1] Poison pills give the board of directors the ability to dilute the ownership stake of a hostile bidder, giving the board de facto veto power over any hostile acquisition. While details vary across different implementations, the basic defensive mechanism provides that when a hostile bidder obtains more than a pre-specified percentage of the company's shares, the pill is "triggered," and existing shareholders receive rights to acquire newly issued shares at a substantial discount. At the same time, such rights are withheld from the hostile bidder, leading to a substantial dilution of their ownership stake.

[2] Some prior studies find a *negative* association between the adoption of a poison pill and 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 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)). For a review of earlier studies, see also MacIntosh (1989).

i.e., the right to adopt a poison pill (Coates (2000)). Therefore, the observed negative association between visible pills and firm value could be explained by selection effects, reverse causality or omitted variables (Comment and Schwert (1995); Catan (2019)). In addition, given the existence of the shadow pill, focusing on the effects of visible pills alone might be insufficient to capture the economic implications of poison pills in full (Klausner (2013)).

In response to these difficulties, this paper analyzes quasi-exogenous changes in the validity of both visible and shadow pills. To this end, we consider the staggered enactment of poison pill laws (PPLs) by U.S. states and their impact on firms' visible pill policy and financial value. As PPLs make the right to use a pill more certain and less likely to be challenged in court (Karpoff and Wittry (2018)), we interpret these laws as strengthening both the visible and shadow pill.

Our main findings are twofold. First, after the passage of PPLs, firms with a higher likelihood of takeover increase their use of visible poison pills, while pill usage becomes relatively less common for firms with a low likelihood of takeover, making visible pill adoption more closely aligned with economic incentives. Second, while we confirm that the association between visible pills and Tobin's Q is negative and can be explained by reverse causality (Catan (2019)), we find that the Tobin's Q of the companies incorporated in states that adopt a PPL *increases* relative to similar firms incorporated elsewhere. This increase is especially significant for innovative firms with more intangible assets.

To impose structure and clarity on the set of empirical tests we perform, we introduce a basic model in which both shadow and visible pills can have an impact on the value of the firm and on the utility of managers/directors who decide whether to adopt a visible pill. The model illustrates that the passage of PPL may have both the substitution and validation effect for visible

pill adoption, and hence, its impact on the average adoption rates is uncertain. At the same time, the changes in adoption patterns and firm value are predicted to be heterogeneous across firms and related to the likelihood of takeover and the presence of a visible pill before PPL passage. Motivated by our theoretical predictions, we conduct the empirical analysis in the sample of US public firms. We focus on the two decades between 1992 and 2012, which cover the period during which several states adopted "second wave" (SW) PPLs. During the 1986-1990 period, when "first wave" (FW) PPLs were adopted, several rulings of Delaware courts injected uncertainty about the status of the poison pill and, therefore, the role of PPLs. Thus, as we further discuss in Section 3, focusing on the post-1992 period provides a cleaner setting for estimating the effect of PPLs. [3]

We first explore the relationship of PPLs with visible pill policy, considering several hypotheses. On the one hand, PPLs may simply have no material effect on visible pill adoption. On the other hand, if they do, this effect may go in different directions. Firms might be less likely to adopt visible pills if the now-validated *threat* of swiftly adopting a pill is enough of a deterrent to thwart a hostile takeover bid. We call this the "substitution effect" of PPLs.

[3] Our results for FW PPLs are in line with the prior literature. In particular, Karpoff and Wittry (2018) document that PPLs adopted during their sample period (i.e., 1976-1995) are not significantly correlated with return on assets (ROA), once controls for firm-level defenses are included. 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). While some other studies also analyze the effect of FW PPLs (Karpoff and Malatesta (1989)), to the best of our knowledge, only one published study – Cain, McKeon, and Solomon (2017) – considers both FW- and SW PPLs. However, their 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.

Alternatively, 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 (Karpoff and Wittry (2018)) – passage of PPL may increase visible pill adoption levels by removing doubts on pill validity. We call this the "validation effect" of PPLs.

Our model predicts that the passage of PPLs, which reduces legal uncertainty about the status of the pill, increases the correlation between the visible pill adoption and the pill's benefits to a given firm. Consistent with this hypothesis, we find that SW PPLs, on the one hand, increase pill adoption by firms with low Tobin's Q or high predicted likelihood of takeover, which are more likely to be concerned about the takeover risk (see, e.g., Edmans, Goldstein, and Jiang, (2012)) and for which the validation effect is likely to be dominant. On the other hand, SW PPLs reduce pill adoption by firms with high Q or lower predicted takeover likelihood, for which the substitution effect is likely to be prevalent.

Next, we examine the effect of strengthening the shadow pill on firm value as measured by Tobin's Q. We find a positive effect of SW PPLs on Q on average. A strengthened shadow pill results in an economically and statistically significant increase of 4-5%, on average, in firms' Tobin's Q. The effect appears stronger among firms that did not have a visible pill in place before PPL passage, consistent with the uncertainty about pill validity being more relevant for these firms. Yet, that difference is not statistically significant. There is no significant difference in the evolution of Q before the passage of PPL, and the difference after the passage gradually increases over time and plateaus five years after the PPL passage. We also present several robustness checks for the value results.

Last, we explore several possible economic explanations for our finding that stronger pill validity appears to contribute positively to firm value. A stronger pill may increase firms' value by allowing firms to take a more long-term strategy, which might generate smaller short-term profits and thus may be negatively viewed by short-term-focused investors ("myopic market hypothesis"). The long-term strategy may only be feasible if the firm is able to secure long-term cooperation with external stakeholders, who themselves may be hesitant to enter the relationship if the firm is threatened by a hostile takeover and the stability of its strategy is in question ("bonding hypothesis"). The myopic market hypothesis is related to a manager-shareholder asymmetric information problem, which prevents shareholders from committing to a long-term managerial strategy. The bonding hypothesis, which generally involves a stakeholder-firm commitment issue, is indirectly related to asymmetric information and arises from incomplete contractibility issues (Hart and Moore, 1990). Both the myopic market and bonding hypotheses, which we jointly refer to as the "commitment hypothesis," involve a commitment to the *status quo* of policies and relationships that provides necessary stability. These hypotheses are particularly relevant for firms with large intangible assets, which are more prone to asymmetric information and therefore more likely to be undervalued by outsiders, as well as for firms that rely on significant relationships with external stakeholders.

Alternatively, the firm value may increase because a stronger 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 ("bargaining hypothesis"). In anticipation of these potential gains when an acquisition occurs, investors may be willing to pay more for the firm's shares earlier.

In support of both the commitment hypotheses, we find that the positive effect on Tobin's Q after PPL adoption is more pronounced for firms for which intangible assets and, thus, asymmetric information concerns are more relevant – such as firms that are more engaged in research and development or have higher levels of intangible capital. Conceptually, the same logic may also imply that firms with more important relationships with external stakeholders, such as large suppliers or customers, may also be more likely to benefit from PPL adoption. We find some evidence for this, which is also confirmed by the change in innovation output, which increases after the passage of a PPL. However, our analysis suggests that the driving force behind the mechanism through which a stronger shadow pill adds value to some firms may lie in other types of intangible assets, possibly including relationships with the firm's insider stakeholders (managers, employees).

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 tend to receive a higher premium after their state adopts a PPL. However, the evidence in favor of the bargaining power hypothesis is only marginally statistically significant and given the relatively low levels of takeover activity in the period we study, this seems, at best, to be a partial explanation of the main results.

II. Poison Pills – Conceptual Framework

In this section, we provide a simple model of the interaction between poison pill laws, visible pill adoption, and firm value. As our goal is not to provide an exhaustive model of firm value, the major fundamentals affecting firm value are captured by the error term. We focus on the impact of poison pills on firm value, which occurs either by transforming corporate

governance (e.g., by insulating managers), by giving the firm more bargaining power in a non-hostile takeover situation, by affecting the probability of becoming the target of a hostile takeover, or by influencing firm policy.

The value of firm *i* at time *t* depends on the poison pill strength, $PPS_{i,t}(PP_{i,t})$, which captures both the visible and shadow pill, the underlying takeover likelihood, and the cost associated with the adoption of a visible pill, $PP_{i,t}$:

$$V_{i,t}^{F} = a^{F} \cdot PPS_{i,t}(PP_{i,t}) + b^{F} \cdot \Pr(Takeover_{i,t}) \cdot PPS_{i,t}(PP_{i,t}) - \widetilde{c_{i}^{F}} \cdot PP_{i,t} + e_{i,t}^{F},$$

where a^F and b^F capture the effect of stronger poison pills as a linear function of the probability of takeover and can represent both the benefits (e.g., Stein (1989)) and (the agency) costs (e.g., Jensen and Meckling (1976)); and $\widetilde{c_{i}^{F}}$ may reflect the negative signaling effect of a visible pill adoption, such as the negative perception in the capital markets. A visible pill might lead the market to believe that the firm is not operating efficiently. Note that since the error term captures all other elements driving firm value, it also includes the baseline effect of takeover likelihood.

The strength of the pill, which captures the level of takeover deterrence afforded by both the visible and shadow pill:

$$PPS_{i,t}(PP_{i,t}) = PP_{i,t} \cdot valid_{s,t} + (1 - PP_{i,t}) \cdot valid_{s,t} \cdot conv_{s,t},$$

depends on the indicator for the firm having a visible pill, $PP_{i,t}$, the likelihood that a visible pill will be deemed valid, $valid_{s,t}$, and on the ability to quickly and effectively convert a shadow pill to a visible pill, $conv_{s,t}$. Both are determined by the legal environment in state *s* at time *t*. One can interpret 1- $conv_{s,t}$ as the measure of reduced protection from having a shadow pill rather than having a visible pill. Note that, since $PPS_{i,t}$ is a function of $PP_{i,t}$ and $conv_{s,t} < 0$, it follows that $PPS_{i,t}$increases with $PP_{i,t}$.

Managers/directors, who decide whether to adopt the pill, maximize:

$$V_{i,t}^{M} = \theta \cdot V_{i,m}^{F} + a^{M} \cdot PPS_{i,t}(PP_{i,t}) + b^{M} \cdot Pr(Takeover_{i,t}) \cdot PPS_{i,t}(PP_{i,t}) - \widetilde{c_i^M} \cdot PP_{i,t} + e_{i,t}^{M},$$

where θ measures the extent to which managers internalize the impact on firm value;[4] a^M , b^M capture the net private benefits (e.g., job security) increasing $PPS_{i,t}$; and $\widetilde{c_i^M}$ captures the managerial private cost, of a visible pill adoption (e.g., reputational concerns in the labor market).

Poison pill adoption is a decision of a manager, whose decision rule is:

$$PP_{i,t} = \mathbb{1}\{(b^M + \theta b^F) \cdot Pr(takeover_{i,t}) \cdot valid_{s,t} \cdot (1 - conv_{s,t}) + (a^M + \theta a^F)$$

$$\cdot valid_{s,t}(1 - conv_{s,t}) > \widetilde{c_i^M} + \theta \widetilde{c_i^F}\}$$

$$= \mathbb{1}\{[b \cdot Pr(takeover_{i,t}) + a] \cdot \left[\underbrace{valid_{s,t}}_{Validation} \cdot \underbrace{(1 - conv_{s,t})}_{Substitution}\right] > \tilde{c}_i\},$$

where $a = a^M + \theta a^F$; $b = b^M + \theta b^F$; and $\tilde{c}_i = \widetilde{c_i^M} + \theta \widetilde{c_i^F}\}$. If $\widetilde{c_i^M}$ and $\widetilde{c_i^F}$ are independent and normally distributed, their sum is also normally distributed: $\tilde{c}_i \sim N(c = c^F + c^M, \sigma = \sigma^F + \sigma^M)$. The value of $\tilde{c}_i$ varies across firms, and its variance depends on the level of uncertainty regarding the perceived validity (which in turn depends on the presence of a poison pill law), reputation effects, and signaling effect of the pill.

The term $valid_{s,t} \cdot (1 - conv_{s,t})$ captures the marginal impact of choosing a visible pill over a shadow pill on the poison pill's strength. The passage of PPL enhances the pill's validity ($valid_{s,t}$) and convertibility ($conv_{s,t}$), affecting the marginal impact in two ways. First, increased certainty in the pill's validity makes its adoption more appealing due to the *validation*

[4] We assume $0 < \theta < 1$. When $\theta = 1$, managers incentives are perfectly aligned with the firm incentives, but this occurs only when the manager is the sole owner of the firm. However, in this case, there would be no need of poison pills and other takeover defenses.

effect. Second, the improved ability to convert a shadow pill to a visible pill may make adopting a visible pill immediately less desirable due to the *substitution effect*.

Because $\tilde{c}_i$ is a firm-specific random variable, the probability that a manager adopts a visible pill is:

$$\Pr(PP = 1) = \Phi\left(\frac{\left[b \cdot \Pr\left(takeover_{i,t}\right) + a\right] \cdot \left[valid_{s,t} \cdot \left(1 - conv_{s,t}\right)\right] - c}{\sigma}\right).$$

We posit that the passage of PPL has potentially three effects on the probability of a visible pill adoption:

1. increase in $valid_{s,t}$;
2. increase in $conv_{s,t}$; and
3. decrease in σ.

The first two effects have opposite impacts on pill adoption, as they contribute to the marginal benefit of the visible pill with opposing signs, positive and negative, respectively, as long as $\left[b \cdot \Pr\left(takeover_{i,t}\right) + a\right] > 0$. However, their magnitude depends on the exact values of the parameters, and they may counterbalance each other.

Claim 1: *The impact of passing a PPL on the adoption of visible pills is uncertain, as PPL simultaneously increases adoption by increasing validity (validation effect) and decreases it by improving convertibility (substitution effect). The overall effect hinges on the magnitudes of these changes and the baseline values of both validity and convertibility.*

As PPL reduces the uncertainty about the legality of poison pills, the variance of visible pill adoption costs, σ , decreases. Denote $\left[b \cdot \Pr\left(takeover_{i,t}\right) + a\right] \cdot \left[valid_{s,t} \cdot \left(1 - conv_{s,t}\right)\right]$

as x. The sign of $\frac{\partial \Pr(\text{PP}=1)}{\partial \sigma} = -\frac{x-c}{\sigma^2}\phi(\frac{x-c}{\sigma})$ is the opposite of the sign of $x - c$. This implies that a decrease in σ increases the adoption of visible pills among firms for which the benefit of adoption, x, is high (i.e., above c), while reducing it among firms for which the benefit is low (i.e., below c). Since the benefit of adopting a visible pill increases with the likelihood of a takeover (assuming $a > 0$), we can make the following claim:

Claim 2: *The passage of PPL, which reduces σ, leads to an increase in the adoption of visible pills among firms with a high likelihood of takeover and a decrease among firms with a low likelihood of takeover.*

The intuition behind this relationship is that the benefit of adopting a visible pill increases in the likelihood of takeover, but this relationship is mudded by the idiosyncratic noise in the cost to adopt the pill. Decreasing the magnitude of that noise strengthens the link between fundamental reasons to adopt the pill and the actual adoption.

How does the passage of PPL affect firm value? Since PPL enhances the perceived validity of visible pills and the convertibility of shadow pills, it leads to an increase in poison pill strength $PPS_{i,t}$. The impact on firm value is positive as long as $\left[b^F \cdot \Pr\left(takeover_{i,t}\right) + a^F\right]$ is positive — which remains an empirical question — and assuming $\tilde{c}_i$ is held constant. Additionally, if PPL leads to a reduction in σ^F, it will result in a marginal decrease in value for firms with visible pills compared to those without. This occurs because firms with visible poison pills at the time of PPL's passage, *ceteris paribus*, are those with low $\tilde{c}_i$ and, therefore, will experience a marginal decrease in value relative to firms without visible poison pills due to the lower variance making below-mean values of $\tilde{c}_i$ higher. Finally, if $b^F > 0$, the passage of PPL

will create stronger value effects for firms with a higher likelihood of takeover. We can then make the following claim:

Claim 3:

(i) *The Impact of PPL on firm value depends on the sign of* $b^F \cdot \Pr(takeover_{i,t}) + a^F$*;*

(ii) *(ii) Firms with a higher likelihood of a hostile takeover will experience a greater increase in value when* $a^F > 0$*; and*

(iii) *(iii) Firms with visible pills at the time PPL is enacted will experience a decrease in value compared to those without visible pills.*

III. Legal Background

This section provides an overview of the legal environment relevant to the validity of the poison pill and the introduction of PPLs. The discussion is important for our analysis given the controversy on pill validity that has accompanied the pill's history since it was first introduced in 1982 (Catan (2019)).

The starting point in the landmark 1985 decision of the Delaware Supreme Court in *Moran v. Household International*, which affirmed the validity of the poison pill for firms incorporated in the state of Delaware. Whether this decision also affirmed the validity of the poison pill for firms incorporated outside of Delaware has been the subject of debate. Some scholars claim that the pervasive authority of Delaware judicial decisions over non-Delaware corporations (Ryngaert (1988); Cremers and Ferrell (2014)) made the validity of the poison pill fairly certain in the immediate aftermath of *Moran* for firms incorporated both in Delaware and outside of Delaware. This view is consistent with the widespread adoption of visible poison pills, even for non-Delaware firms in the years immediately following *Moran* (Figure 1). Other

scholars consider the status of the pill for non-Delaware firms to be uncertain until these firms' states of incorporation adopted a PPL (Catan and Kahan (2016); Cain, et al. (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.

[Figure 1]

Given the view that Delaware case law helps shape corporate law in all other states, it seems reasonable to further assume that subsequent Delaware decisions, which weakened *Moran*, also increased the uncertainty of Delaware's external validity regarding the pill for firms incorporated in states other than Delaware. 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.[5] 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).[6] And while only a year after those decisions, *Paramount Communications,*

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

[6] 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 even recommending firms to move out of Delaware (Fleischer and Sussman (2013)). 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).

Inc. v. *Time Inc.* reaffirmed the validity of the pill in Delaware, the precedent at an equity court that a poison pill can be invalidated remains, casting more uncertainty on the legal standing of the poison pill.

Overall, regardless of the specific view one holds about the specific impact of *Moran*, this legal background suggests that *Moran*'s external validation effect was stronger in the early years of PPL adoptions—what we refer to as the "first wave" (FW) of PPLs. These adoptions span from 1986, when the first PPLs were introduced in Indiana and Ohio, to 1990, when South Dakota and Virginia introduced the last FW PPLs. Over time, however, further Delaware courts' decisions increased uncertainty and weakened the validation effect, making PPLs increasingly important to validate the use of poison pills outside Delaware.

For these reasons, one can assume that during the "second wave" (SW) of the PPL era, firms in states with a PPL likely faced little to no doubt about the validity of the pill, while firms in states without a PPL continued to face persistent uncertainty due to prior conflicting judicial decisions in Delaware and elsewhere, in some cases, they even encountered a non-rebuttable presumption of invalidity due to statutory limits, such as those in California.[7] In contrast, during the FW PPLs period it is possible that even in states without PPL in place the pill might have been considered valid, which may obscure the effects of strengthening effect of the PPLs. Hence, our focus is on the 1992-2012 period.

While our analysis includes both FW and SW PPLs, the inclusion of state fixed effects means that the variation we exploit will primarily come from the introduction of SW PPLs. Our

[7] Although some interpretations of Delaware rulings might argue that poison pills would be valid in principle even without PPLs, the fact that some states decided to pass PPLs during the SW years suggests otherwise.

focus on 1992-2012 also ensures that we have a relatively stable pre-treatment period—unaffected by both the passage of Delaware court decisions related to the use of the pill and the hostile takeover wave of the 1980s—and mitigates the likelihood of measurement error that could bias our estimates.

Last, one remaining challenge is that PPLs in some states were passed in the same year as other anti-takeover laws. In particular, among states adopting SW PPLs, multiple-law adoptions occurred in Maryland and Texas. To address this concern, we include indicators for the presence of other anti-takeover statutes and, in robustness checks, exclude states that adopted multiple laws from the sample.

IV. Data and Empirical Specification

A. Data Sources

Our empirical analysis combines several different sources of data, including (1) data on visible pill adoption; (2) state-level poison pill laws data and data on historical states of firm incorporation; and (3) data on firm value and other characteristics.

Pill Data. We combine poison pill data from Institutional Shareholder Services (ISS) Governance and the Securities Data Companies (SDC) Corporate Governance databases and supplement these observations with poison pill data from Cremers, Litov, and Sepe (2017). While the ISS and Cremers, et al. (2017) data are panel datasets, SDC data contains only information about pill introduction and expected duration. Given that the expected pill duration may differ from the actual pill duration, we employ the following conservative procedure to include SDC data. We assume that: (i) there was no pill in place two years before the first adoption for a given firm (our results remain similar if instead we use one- or three-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 the pill remained in place for one or two 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.

PPLs and Incorporation Data. Our study's key independent variable, PPL, is an indicator capturing whether a firm is incorporated in a state that has passed a PPL. We obtain information on whether states have passed one of these laws from Cain et al. (2017) and Karpoff and Wittry (2018) and report each state's adoption date in Online Appendix Table OA1. To obtain historical incorporation data, we start with the database maintained by Holger Spamann (the "Spamann data").[8] Our checks confirm that the accuracy of this dataset is superior to other data sources. The coverage of the Spamann data starts in 1994 and remains limited until 1996, such that 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.[9] Combining law adoption dates and historical incorporation

[8] 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/.

[9] 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,

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 the adoption of the law. PPL always equals zero for firms in states that never passed a PPL, including firms incorporated in Delaware.[10]

Financial Data and Sample Construction. We merge our firm-level pill data with the data on 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 a 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 1983–2012. While this is the main period we focus on, we include some results that analyze an earlier period (1983 to 1993).

B. Key Variables

Our main measure of visible pill adoption is a binary indicator for the presence of a pill in a given year, $PPill$. We further 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

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.

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

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.

Our primary measure of firm value and dependent variable in most regressions is the natural logarithm of Tobin's Q ($Ln(Q)$) as in Bebchuk et al. (2009) and Atanassov (2013). Recognizing that Tobin's Q is an imperfect measure of value in robustness tests, we analyze the implications of PPLs for the following three alternative proxies 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 ($Excess\ Return$); and return on assets (ROA), measured as operating income before depreciation and amortization scaled by total assets (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 laws (BCL), control share laws (CSL), directors' duties laws (DDL), and fair price laws (FPL). We further winsorize all the continuous variables at the 5% level in both tails to mitigate the influence of 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. Our main sample is comprised of 29,213 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%. 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.

[Table 1]

C. Identification Strategy – Determinants of PPL Passage

Our empirical analysis relies on a quasi-natural experiment created by the staggered enactment of PPLs by firms' state of incorporation. The key assumption underlying this strategy is that enacting these laws provides a quasi-exogenous "shock" to the takeover protection of firms incorporated in the adopting states by reducing uncertainty around the validity of firms' poison pills and hence by strengthening both the shadow and visible pill.

To examine whether the adoption of PPLs by states might be related to certain local characteristics that could also correlate with individual firms' decisions to adopt a pill and/or firm value—potentially invalidating our identification strategy—we follow a similar approach to 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 the population ($Ln(Inc.SY\ Pop)$), rates of unemployment ($Inc.SY\ Unemploy$) and a state's business entry and exit rates ($Inc.SY\ Entry$ and $Inc.SY\ Exit$). We also include year fixed effects to account for transitory U.S.-wide factors (e.g., macroeconomic conditions). 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 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 four specifications, including subsets of the above-mentioned covariates. Table 2 presents our findings.

[Table 2]

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 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 always statistically insignificant, suggesting that local economic conditions do not drive the passage of SW PPLs. 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.

D. Empirical Specification

Our baseline specification estimates:

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

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. If a firm's fiscal year ends before June of a given year, we replace the values for year t with values for year $t+1$. Our main independent variable, $PPL_{[st]}$, is an indicator of 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 firm value (visible pill policy), 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 the 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 firm value itself is affected by the PPL (which is one of our main findings).

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). 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., 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.

In our baseline specification we include all firms, and hence the coefficient of PPL is identified by comparing firms that are treated with a change in PPL status (e.g., because their state of incorporation passes PPL) to control firms, which include those that are never treated (i.e., their state of incorporation never adopts PPL), are always treated (i.e., their state of incorporation adopted PPL before our sample begins), and not-yet-treated (i.e., their state of incorporation adopts PPL at a later date). While this is a natural and most comprehensive definition of the control group, the dynamic effects of PPLs may influence the estimates of the coefficient of interest in unexpected ways. To address these concerns, in robustness checks we

also employ specifications that limit the control group or account for dynamic effects in different ways (Section 5).

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 headquarters are located (Gormley and Matsa (2016)). While we will show that our results are robust when using this approach, a limitation of this strategy 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 elsewhere (similarly, only around 20% of the non-Delaware-incorporated firms 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 the state of headquarters-by-year fixed effects, our preferred specification uses U.S. Census division-by-year fixed effects.

V. PPLs and Visible Pill Adoption

We begin our empirical analysis of PPLs by examining their relationship with visible poison pill adoptions. As illustrated by the model in Section II, the passage of a PPL, if it materially affects firms' visible pill adoption at all, may have two potential effects: the *substitution effect* and the *validation effect*.

In Table 3, we regress the adoption of visible poison pills on PPL and its interactions with the indicators for the first and fourth quartile of Tobin's Q or a predicted likelihood of takeover at the moment of PPL adoption, other controls, and firm, division-by-year, and industry-by-year fixed effects.

[Table 3]

Our first result, as shown in column 1, is the lack of a significant effect of PPLs on pill adoption on average, suggesting that the *validation effect* balances out the *substitution effect*, consistent with Claim 1 in Section 2. Yet, column 2 shows that firms in the lowest quartile of Tobin's Q distribution are significantly more likely to adopt the pill, while firms in the highest quartile are significantly less likely to do so, relative to the control group (i.e., firms in the two middle quartiles of Q). The positive coefficients on $Q(Lowest)$ and negative coefficient on $Q(Highest)$ suggest that a firm's valuation, which proxies for the likelihood of takeover, is a key determinant of visible pill adoption. The results also illustrate a potential reverse causality problem, casting doubt on the ability to clearly assess the effects of the poison pill just by looking at the consequences of endogenous pill adoption decisions.

In columns 3-5, we interact the dummy variable for the presence of PPL with binary indicators of the 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 average effect in

column 1 hides substantial heterogeneity: relative to the control group, firms with low valuations are more likely to adopt a poison pill following the passage of PPLs, while the effect for firms with high valuations is negative. While including both quartile indicators leads to only marginally significant estimates of the differences relative to the control group, the difference between the lowest and highest valuation quartile is highly significant, as evidenced by the value of the F-test for the equality of the effect for both of these groups.

In columns 6-7, we consider an alternative proxy for a takeover risk, which is based on Cremers, Nair, and John (2009). We first run a Poisson regression of the realized takeover indicator on a series of firm characteristics: PPE to assets, debt to assets, current assets to assets, ROA, log market capitalization and an indicator for takeover occurring in a given SIC2 industry last year. We do not include the firm's Tobin Q to ensure that the alternative proxy is independent of the approach taken in columns 1 to 5. We use the estimated regression coefficients to create predicted likelihood for all firm-years in our data, standardize that likelihood, and include it in the regressions. In column 6, we include the likelihood as a linear term. In column 7, we include indicators for the first and fourth quartiles of the distribution. The results reveal that PPL increases the likelihood of visible pill adoption, particularly for firms with a high likelihood of takeover (column 6). The comparison of the effect sizes for firms with a low (Q1) and high (Q4) likelihood of takeover also reveals a sizable, statistically significant difference, as the F-test for the equality of the effects, presented in the bottom row of the table, rejects the hypothesis of equality.

The results in columns 3 to 7 are consistent with the predictions of our Claim 2. The passage of PPLs, by reducing legal uncertainty about the validity of the pill, decreases the dispersion in firm-specific beliefs about the costs of visible pills ($\sigma^{M} + \theta\sigma^{F}$), and aligns visible

pill adoption more closely with economic incentives for takeover protection, which are larger for firms at a higher risk of takeover.

In columns 8-9, we investigate our outcome variable in more detail. We 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 the adoption of new pills relative to the maintenance of existing pills. Column 8, 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 9 suggests that firms with the 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, consistent with 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.

Overall, the evidence from Table 3 is consistent with our hypothesis that PPLs have both validation and substitution effects. The validation effect dominates for firms with a high likelihood of takeover, while the substitution effect dominates for firms with a low likelihood of takeover. The combined effect makes visible pill adoption patterns more closely aligned with the likelihood of a takeover.

Appendix Table OA13 presets results analogous to those in Table 3, except that the dependent variable is a non-clear day poison pill, i.e., a pill that was likely adopted in response to an elevated risk of hostile takeover or activist pressure. To measure such pills, we combine

information on the exact date of pill adoption (that is available for around 70% of our pill observations) with 13D filings data that provide information about the data on which an investor crossed 5% ownership threshold in the firm. We define non-clear day pill as one adopted in the period 90 days before or after the date at which the 5% threshold was reached. Similar results are obtained when using 180 days instead, while using 30 days results in insignificant estimates due to the small number of non-clear day pills identified. Naturally, this approach is subject to measurement error: we do not know if pill adoption was indeed motivated by a direct threat of takeover or activist pressure, even if some investors did acquire a significant fraction of the firm's shares; at the same time, the threat of takeover does not need to manifest in the purchase of more than 5% of firm's shares.

Non-clear day pills are rare, as they are only passed by 3% of firms in our data that have a pill with an observable date of adoption (97 out of 3,149 firms). Nonetheless, Table OA13 suggests that the passage of PPL decreases the likelihood of adopting a non-clear day pill, indicating that the substitution effect dominates for this kind of pill. This result is consistent both with PPLs lowering the likelihood of a takeover or activism as well as with the strengthening of the shadow pill reducing the need for a non-clear day pill.

VI. PPLs and Firm Value

Our theoretical model predicts that the impact of PPL on firm value depends on the sign of $b^F \cdot \Pr(takeover_{i,t}) + a^F$**.** In this section, we attempt to uncover the sign of this expression and investigate the value implications of the enactment of a PPL, 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, using alternative methods of

constructing our sample and alternative estimation methods. Further supplementary robustness tests are included in the Online Appendix.

A. Visible Pill Adoption and Evolution of Q

To underscore the benefits of studying the strengthening of pill validity overall, including "shadow pills," as opposed to focusing on visible pills, we first illustrate the evolution of the firm's value around the visible pill adoption event. We do so 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. 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 Figure 2.

[Figure 2]

The figure provides suggestive evidence that firm value significantly declines in the five years before a firm decides to deploy a poison pill, and continues to decline after the adoption, 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)).

B. Main Sample

Table 4 reports the difference-in-differences estimates of the impact of the adoption of PPLs 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 to 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 headquarters

states instead of divisions. The standard errors are adjusted for two-way independent clustering at the state of incorporation and year level.

[Table 4]

We find that the adoption of PPLs has a positive and statistically significant impact on 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 et al. (2009); Cremers and Ferrell (2014)). However, in light of our results in Figure 2, 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." To address this concern, in the remaining columns, we interact PPL with $PPill_{[\tau(s)-1]}$, i.e., an 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 and non-negligible, but the p-value is 0.155, while the standalone coefficient on PPL (point estimate = 0.068) remains significant at the 1% level. [11] The effect for the average firm is between 4% and 5%. Columns 4 and 5 interact PPL with two proxies for the likelihood of takeover: the logarithm of Tobin's Q at

[11] We show that our baseline point estimate in column 3 is robust to the omission of any SW PPL-passing state in Online Appendix Figure OA1.

the moment of PPL adoption and the likelihood of takeover index based on Cremers et al. (2009). Neither interaction is significant (the p-value in column 5 is 0.135) but the economic magnitudes are non-negligible and suggestive of a higher value response for firms at a greater risk of takeover.

These results indicate that PPLs 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 and that were at a higher risk of hostile takeover. Consistent with Claim 3.i and 3.iii., the value effect is positive and possibly marginally lower for firms with visible pills in place, which suggests that $b^F \cdot \Pr(\text{Takeover}_{i,t}) + a^F > 0$ for an average firm, and that PPL reduces the variance of σ^F. Additionally, the negative coefficient on the interactions of PPL and proxies for the likelihood of a takeover supports Claim 3.ii. However, due to the limited precision of this estimate, we cannot conclude that the value effect is significantly larger for firms with a higher likelihood of takeover.

The probability of a takeover plays a significant role in the adoption of poison pills, as shown in Table 3. However, as detailed in Table 4, its impact on value is less clear. This suggests the existence of some size of agency problems associated with the adoption of visible pills, dependent on the likelihood of a takeover. It is conceivable that our proxies for the likelihood of a takeover primarily capture short-term takeover risk. This risk significantly influences managers' payoffs and hence plays a pivotal role in explaining pill adoption patterns. Concurrently, PPL also mitigates long-term takeover risk, which could be pivotal for firm value. If this long-term takeover risk is not correlated with our proxies for the likelihood of takeover, the value effect becomes apparent in the intercept term, a^F of our theoretical illustration. This

general term, *arguendo*, can also capture, among other things, the investors' belief in the long-term reduced probability of takeover, which is unobservable to us.

Additionally, while the average adoption of visible pills remains unchanged following the passage of a PPL, the value for the average firm increases. This suggests that the validation and substitution effects may neutralize each other, leaving visible pill adoption unaffected. Nevertheless, both effects increase the strength of poison pills, positively impacting firm value, in line with Claims 1 and 3.i.

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) (see Acharya, Baghai, and Subramanian (2014)) or headquarter states (see Gormley and Matsa (2016)) to control for potential local confounding factors. The coefficient on PPL remains similar using either of these alternative specifications. However, we prefer the use of fixed effects based on U.S. Census Divisions, as these provide a more granular geographical measurement than regions and are not susceptible to the econometric issues (specific to our setting) engendered by the use of headquarters states that we outlined in Section 3.

Figure 3 presents the estimates of the value effect of PPL in a graphical form, demonstrating how the effect varies over time. We regress the logarithm of Q on lags and leads of PPL passage variable, defined as the switch of PPL indicator from zero to one. The controls are analogous to those in Table 4, column 1. We include five leads and seven lags of PPL passage, which allows us to estimate the effects of PPL five years before and seven years after the passage (including n-th lag of PPL passage estimates the effect n years after passage, given that the outcome is contemporaneous Q). We also include a binary indicator for the (-5,7) time

window around the passage and exclude the first lag so that the coefficient on the first lag is normalized to zero and estimated coefficients on other leads and lags are relative to that year.

[Figure 3]

Figure 3 demonstrates that there are no significant differences between control and treated firms before the passage of PPL, alleviating concerns that pre-trends may be driving our results. At the same time, the figure shows significant effects that emerge after the passage of PPL. While coefficients right after the adoption are positive, they are insignificant, and it is only in further years that the effect gradually becomes statistically different from zero. We believe this significant medium- and long-term response is consistent with the PPL impact operating through improved ability to commit to long-term strategies – a hypothesis we further investigate in section 5.

C. Alternative Samples and Estimation Methods

We next turn to consider 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 state that has adopted a PPL, and hence, the unobserved characteristics of firms in states adopting PPLs may be different from the 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 to a "control" firm incorporated in a state without a PPL (in the three years following its matched counterparts' adoption year). Our matching procedure further 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 a sample size with sufficient statistical power, we match each

treated firm with up to five control firms. In columns 1 to 3 of Panel A of Table 5, we present the results from the matching strategy with different fixed effects and confirm that firms experience significant increases in their Tobin's Q after PPL adoption.

[Table 5]

A second concern about our sample may relate to it being too narrow, i.e., limited to firm-year observations for firms for which we have information on whether they have a visible pill. To address this, we extend the sample by not conditioning on having information on visible pills. The results with the extended sample are presented in columns 4-5 of Table 5, Panel A. Column 4 includes all firm-year observations with historical incorporation data available in the Spamann dataset, where coverage of firms is greatly limited before 1995. In column 5, we extend this sample by using additional historical information from Compact Disclosure disks and CRSP Historical. Both columns confirm the 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 of Panel A presents the results with an alternative measure of poison pill. When measuring pills, we start with data used in Catan (2019) and supplement it with further datasets (ISS, Cremers et al. (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 using only Catan data or when relying only on ISS data, even though the sample size becomes visibly smaller (about 20,000 observations).

In Panel B of Table 5 we consider alternative methods of estimation. Recent literature in econometrics has documented that difference-in-differences (DiD) methodology may produce biased estimates when the treatment is introduced in a staggered way (Baker, Larcker, and Wang

(2022); Callaway and Sant'Anna (2021)). The matched-sample results that we present in Panel A of Table 5 address the concerns about DiD methods, as they match a firm in a treated state to firms that are incorporated in an untreated state over the window of the analysis. Yet, to further alleviate these methodological concerns, we present additional results using alternative approaches. Columns 1 to 3 of Table 5, Panel B, present estimates of the Average Treatment Effect (ATT) estimated with a method developed by Callaway and Sant'Anna (2021). Following Baker, Larcker, and Wang (2022), we do not include additional controls in those specifications, and focus on estimating the effect of PPL over three event windows, [-3,+3], [-5,+5] and [-10,+10] years after the passage of the PPL. Our estimation includes never treated and not yet treated firms in the control group. The ATTs that we obtain are positive, statistically significant, and similar in magnitude to our main results.

In columns 4 to 6, we present the second approach to alleviate concerns about the bias in DiD estimates: stacked regressions, see for example Gormley and Matsa (2011) or Baker, Larcker, and Wang (2022), among others. Focusing on the same three windows around the PPL passage, we estimate the effects of the law for specification analogous to column 3 of Table 4. In all specifications, we confirm a positive and significant effect of the PPL passage, which suggests that concerns about the validity of DiD estimates are not driving our results.[12]

[12] Our approach is similar to Heath et al. (2022) as it does not exclude pre-treated observations within the stacked regression analysis reported in Table 5 Panel B. When we excluding these observations, or when we do not include any covariates except for PPL in the regression, similar to the approach in Gormley and Matsa (2011), we obtain significant and positive coefficients on PPL of 0.081 in all time windows with p-values of 0.032, 0.036 and 0.002 in columns 4-6, respectively.

D. 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);
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)).

[Table 6]

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 the firm value measured with Q, Total Q, and Excess Return. In contrast, we find no significant relationship for ROA. The lack of response of ROA may be because the rise in firm value comes from future rather than current cash flows. Alternatively, it may come 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 firms with high intangible assets drive the increase in value. While analyzing future ROA could, in theory, be a remedy for

part of these problems, this measure is likely too noisy to allow for drawing meaningful conclusions.[13]

E. 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 B together with their discussion The robustness checks include special treatments for firms incorporated in Delaware, inclusion of state-year fixed effects, placebo tests, and various approaches to account for the effect of other anti-takeover laws. These alternative approaches provide additional support on the positive value effects of PPLs.

VII. Economic Channels

What economic mechanisms can explain the positive value effects of PPL? The main hypotheses proposed by the existing theoretical literature are the "myopic market hypothesis" (Stein, 1988, 1989) and the "bonding hypothesis," which we unify under the "commitment hypothesis," as both involve a commitment to the *status quo* of policies and relationships, as well as the "bargaining power hypothesis." We analyze them in this section. While other potential channels, e.g., a reduction in misalignments of incentives between shareholders and managers,

[13] 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 et al. (2017); and various other studies as reviewed in Table A.1. in Straska and Waller (2014)).

could potentially play a role in shaping the magnitude of the PPL effect, we did not find evidence in favor of such a hypothesis. Appendix Table OA12 demonstrates that the effect of PPL does not significantly differ by firm's level of managerial ownership or by the magnitudes of executives' compensation sensitivity to firm value, which proxy for the misalignment of incentives.

Commitment of Shareholders (Myopic Market Hypothesis): A stronger shadow pill may increase firm value because its existence allows firms to commit to a value-enhancing, long-term business strategy by reducing short-term pressure on the stock price. If the stock market is myopic (Stein (1988, 1989)), without an ability to prevent the disruption that is caused by hostile takeovers, a firm may find it suboptimal to pursue a strategy that may generate large long-term gains but comes with a risk of lower performance in the short-term. This concern is particularly relevant for highly innovative firms that are likely to be more affected by asymmetric information because of their high assets' intangibility. As a result, the value of these firms is more difficult to assess for outsiders, so they might more easily be undervalued when pursuing a long-term strategy that may generate limited profits in the short term.

Commitment of Other Stakeholders (Bonding Hypothesis) Committing to a long-term strategy may also increase value by lowering costs of contracting with external stakeholders (e.g., Laffont and Tirole (1988); Shleifer and Summers (1988), Johnson et al. (2015)) and key internal stakeholders, including employees and directors (Cremers et al. (2017); Cremers and Sepe (2016)). Innovative firms are organized through long-term incomplete contracts with several stakeholders, which necessitates bonding firms' strategy in the long term. Remarkably, both the myopic market hypothesis and the bonding hypothesis, being focused on the firm's long-

term strategy, consider the future risk of takeover as central to the firm's strategic choices. This is because a future risk of takeover could compromise the long-term plan determined today.

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, allowing existing shareholders to obtain better conditions in the process (DeAngelo and Rice (1983)). In contrast to the commitment hypothesis, where the increase in value comes from improved fundamental value, 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 bids and acquisitions.

The implication of the "myopic market hypothesis" is that the importance of PPL is larger for innovative firms that face a high level of risk and uncertainty and are subject to a higher amount of asymmetric information. The "bonding hypothesis" suggests that the effect is larger for firms that have important relationships with external stakeholders. To test these predictions, we use the following measures of innovativeness:[14]

1. $Intangible\ Capital$, defined as a firm's intangible capital estimated replacement cost (as proposed by Peters and Taylor (2017)).
2. $Organizational\ Capital,$ which is a subset of intangible.
3. $R\&D/Sales$, calculating stock of R&D using perpetual inventory method with 10% discount rate.
4. The number of $Patents$ held by the firm.

[14] 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.

5. Mean $Forecast\ Error$ of analysts predicting firm's stock price.
6. $Large\ Customer$, an indicator for having a customer responsible for more than 5% of sales.

The data sources and additional details about construction of each variable are presented in Table A1. 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 ($\tau(s) - 1$). It should be noted that measuring both the importance of intangibles and asymmetric information, as well as the relationships with external stakeholders is difficult, and hence in practice the implications of two hypotheses are difficult to disentangle. Hence, when interpreting the results we consider them also as a joint test of both hypotheses, referring to them as "commitment hypothesis".

[Table 7]

Panel A of Table 7 presents our results. In each column, we use 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. We find that the impact of PPL on firm value is higher 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. A possible explanation for this result is that 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 measures 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.

Column 6 tests the interaction of PPL with an indicator for the firm having a "large customer", defined as presence of a customer responsible for more than 5% of firm's sales, which tests the bonding hypothesis. On average, the impact of PPL on firm value is no higher for firms with a large customer. 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 customers and a high level of intangible capital. Hence, while the pure bonding hypothesis does not seem to be the main driver of the observed value effect, it appears relevant for firms that are exposed to more asymmetric information.[15]

Panel B of Table 7 tests the bargaining hypothesis. We investigate 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

[15] An alternative way of testing this hypothesis is a direct analysis of how the stock of intangible assets evolves after PPL passage. While measuring year-to-year changes in the stock of intangible capital is generally difficult, for some asset classes, such as patents, it is feasible. In Table OA8 we analyze how PPL passage affects the likelihood of obtaining a patent, the number of patents at the intensive margin, and a forward-looking measure of patent citations, which may proxy for patent quality. While we do not find a contemporaneous effect on any of the three measures, we find positive and significant effects on patenting outcomes after two years. Thus, we conclude that PPL passage increases patenting activity on both extensive and intensive margin, as well as patents' quality. The fact that the impact is delayed is consistent with the nature of R&D and patenting process, which requires substantial amount of time to be completed.

(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)).

The dependent variables in columns 1 and 2 are equal to one if a target firm announces that it has received a bid (column 1) or is acquired in a completed takeover, either through a merger or an acquisition (column 2) in the SDC M&A database, and zero otherwise. 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.[16] 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 and 6, the impact on the takeover's likelihood is more likely to come from firms with high levels of intangible capital, which lends further support to the important role of asymmetric information. This suggests that PPLs reduce the likelihood of a takeover for firms with high intangible assets and enable them to continue operating under the same ownership and pursue longer-term investment strategies.

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

[16] 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.

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 *Total Premium,* but we find a marginally significant positive impact on *Premium Increase*. However, 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.

Overall, Table 7 provides suggestive evidence supporting both the commitment hypothesis and the bargaining power hypothesis, with the commitment hypothesis receiving the strongest support in our interpretation. However, when we disentangle the commitment hypothesis, it is the myopic market hypothesis that receives the strongest support. It should be noted, however, that there is no clear-cut distinction between the myopic market hypothesis and the bonding hypothesis, which together form the commitment hypothesis. Firms with the highest levels of intangible capital are more likely to be affected by myopic markets and rely on important relationships with external stakeholders.

Finally, these results need to be interpreted considering the suggestive evidence regarding the potential relevance of different mechanisms, as provided by the pattern of coefficients in Table 4. Although the value effect seems slightly more pronounced for firms with a higher likelihood of takeover, this relationship is only marginally significant and accounts for a relatively small portion of the positive value effect. Our proxy for the likelihood of a takeover is

likely to better capture short-term takeover risk, which may be a more prominent factor in the bargaining power hypothesis. The limited role of the interaction between PPL and the likelihood of takeover suggests that the value effect is driven by more long-term considerations. These considerations are challenging to measure and weakly correlated with today's proxies for takeover. Eventually, the belief in the future risk of takeover, which is unobservable to us, manifests in the baseline coefficient for PPL. The emphasis on long-term stability rather than immediate risk reduction is consistent with the timing of the value effect (Figure 3). This indicates that the commitment hypothesis is a more plausible explanation for the effects of PPL on firm value. This further suggests that the term capturing most of the value in our theoretical illustration is a^F rather than b^F.

VIII. Conclusion

This paper contributes to the debate on whether poison pills benefit or hurt shareholders by shifting the focus from visible pills alone to examining the validity of both visible and shadow pills. 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 its relevance as a takeover defense.

We document two main results. First, we show that 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 which increase visible pill adoption following the passage of PPLs. The opposite is observed for firms with a low likelihood of takeover, for which the strengthening of a shadow pill has a substitution effect and reduces the adoption of visible pills. This is consistent with a stronger shadow pill providing a sufficient takeover defense on its own for firms at a lower immediate risk of takeover.

Second, we find that the passage of PPL is associated with improvements in firm value. Increasing the validity of the pills seems beneficial to shareholders, even if the endogenous adoption of an actual pill might not be. Our results support the view that poison pills, whether shadow or visible, serve a positive corporate governance function for some firms through the channels of the "commitment hypothesis." Under this hypothesis, the pill increases firm value by enabling the board to commit to the firm's long-term strategy, promote longer-term investment projects, and protect firm-specific investments. This effect is particularly significant for firms subject to a higher amount of asymmetric information, which are more likely to be misvalued by the market and vulnerable to undesired takeover bids.

Appendix

Table A1

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

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 et al. (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 et al. (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 percentage increase in the premium (markup) from the initial bid (i.e., the price offered in the initial announcement) to the completion of the acquisition. 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 Initial Premium (runup) *plus* the Premium Increase, where the Initial Premium is the percentage increase in the target's stock price from 4 weeks (or 20 trading days) before the announcement to the price offered in the initial announcement date. 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.

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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).

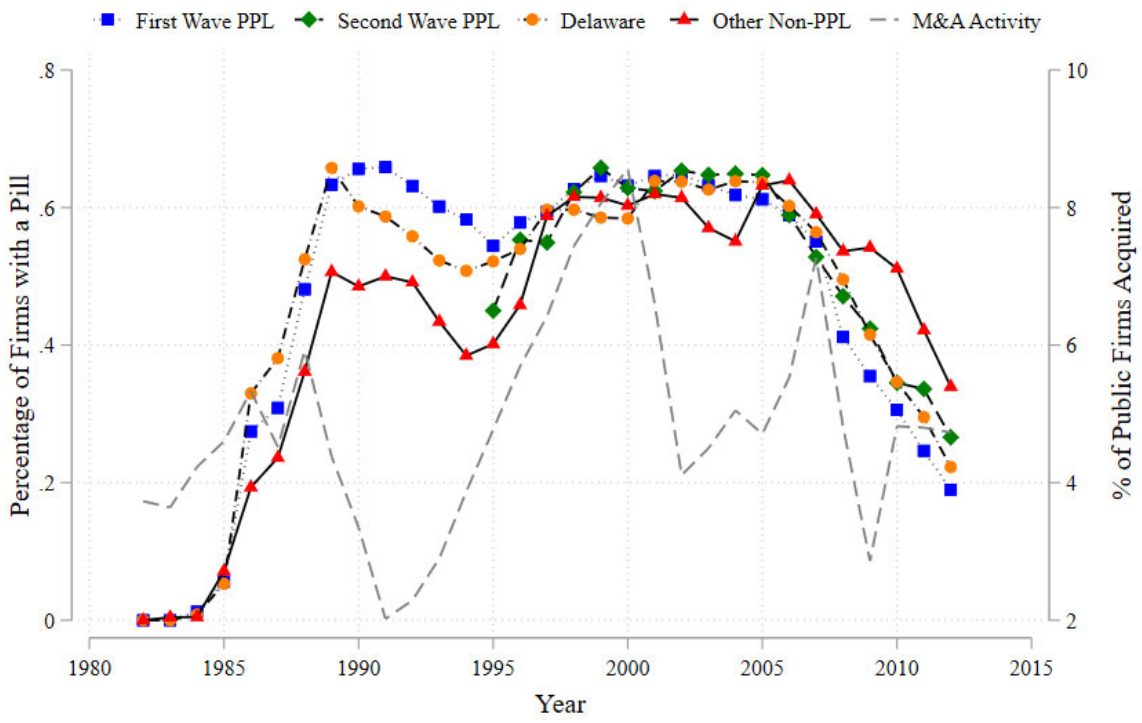

First Wave PPL
Second Wave PPL
Delaware
Other Non-PPL
M&A Activity
Percentage of Firms with a Pill
% of Public Firms Acquired
Year
0
.2
.4
.6
.8
2
4
6
8
10
1980
1985
1990
1995
2000
2005
2010
2015

Figure 2

Reverse causality: Firm Value and Visible Pill Adoption

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

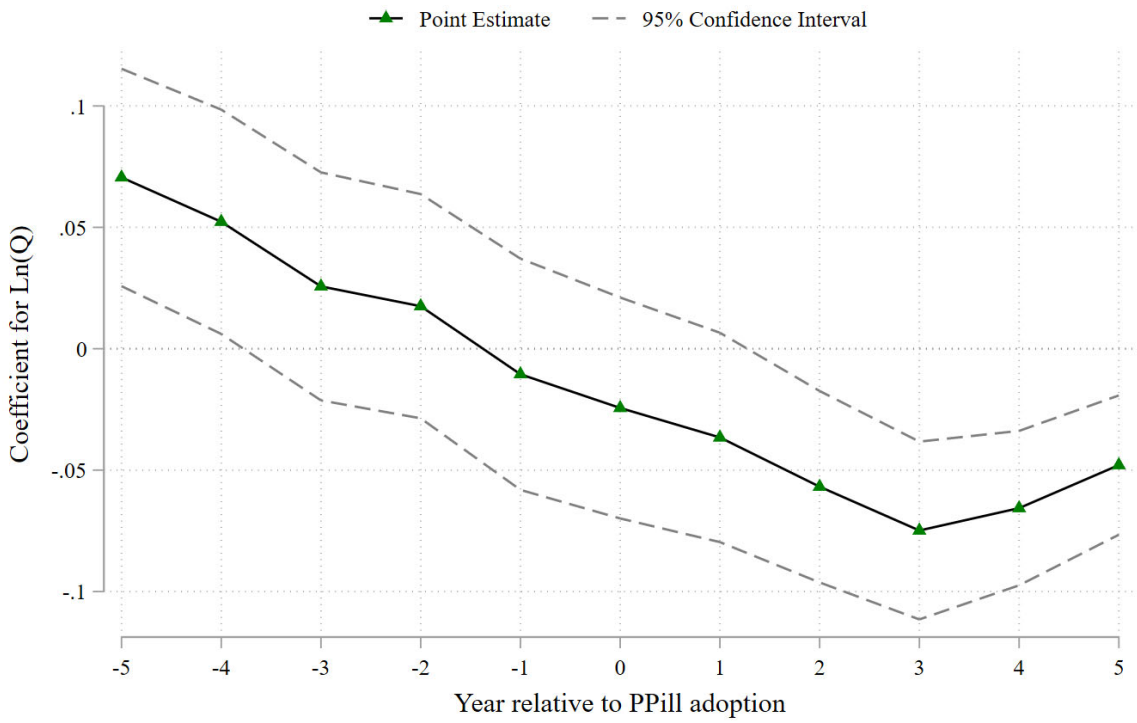


Figure 3

Adoption of Poison Pill Statuses and Firm Value over Time

The figure plots the coefficients and 95% confidence intervals from regressing natural logarithm of Q on lags and leads of PPL adoption (defined as change of value of the PPL variable from zero to one) by firm's state of incorporation. The coefficient at t=-1 is normalized to zero. The values for k>0 reflect coefficients of the k-th lag of PPL variable and represent the differential change in Q after PPL adoption. Values for k<0 reflect coefficients on the k-th lead of PPL variable and represent the difference in the levels of Q before treated and control states before the PPL adoption and reveals lack of pre-trends.

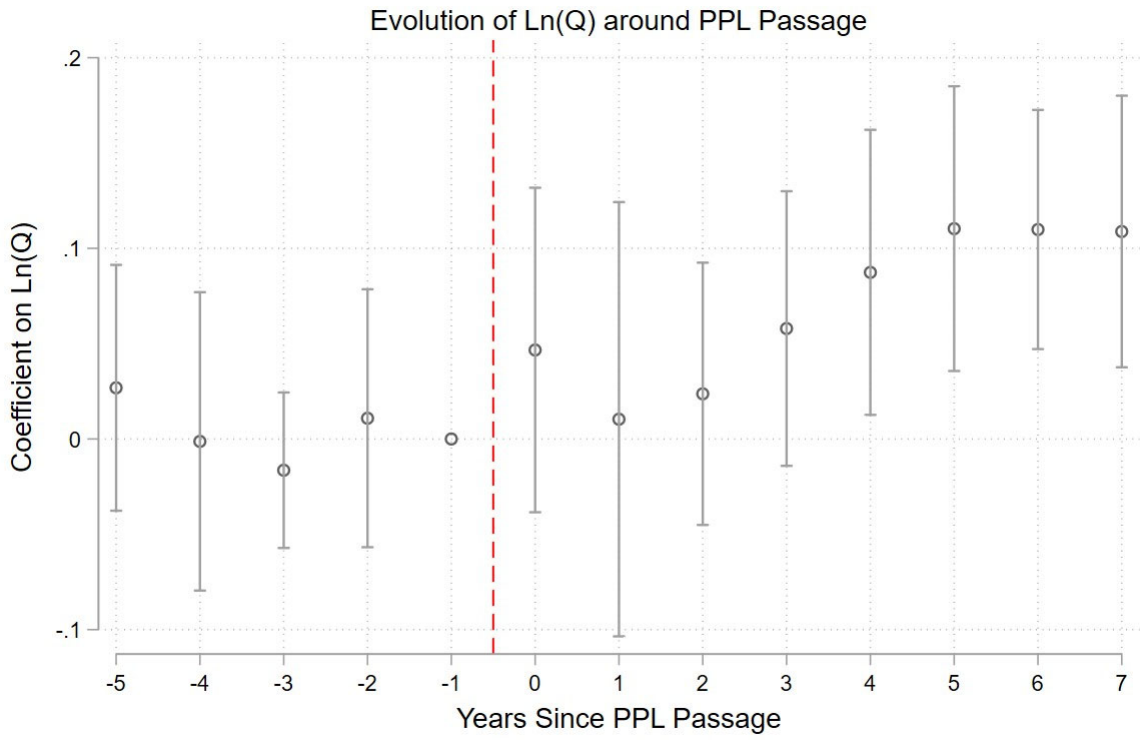


Table 1

Descriptive Statistics for the Main Variables

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.

Panel A: Dependent Variables						
	Mean	St. Dev	P25	Median	P75	Obs
Poison Pill	0.590	0.492	0	1	1	30,461
Ln(Pill Duration)	1.974	0.668	1.61	2.08	2.48	17,970
Ln(Q)	0.494	0.535	.109	.4	.783	30,964
Q	1.938	1.409	1.12	1.49	2.19	30,964
Ln(Total Q)	0.038	0.524	-.311	-.011	.342	30,964
Excess Return	0.000	0.002	-7.0e-04	1.8e-04	1.1e-03	31,501
Takeover Bid	0.008	0.087	0	0	0	23,120
Acquired	0.028	0.525	0	0	0	23,120
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	32,011
BCL	0.919	0.272	1	1	1	32,011
CSL	0.243	0.429	0	0	0	32,011
DDL	0.299	0.458	0	0	1	32,011
FPL	0.286	0.452	0	0	1	32,011
R&D/Sales	0.126	0.226	6.6e-03	.035	.137	20,327
Intangible Capital	0.588	0.402	.27	.539	.818	31,997
Ln(1+Patents)	1.491	1.970	0	0	2.77	32,011
Research Quotient	0.127	0.060	.094	.126	.163	13,942

Table 2

Second Wave-PPL Adoptions

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. The *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

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

Table 3

PPLs and Visible Pills

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 for firm's Tobin's Q (in the year before the adoption of its respective PPL ($\tau(s)-1$)) being in the bottom and top quartile of its empirical distribution. Column 6 interacts PPL with the continuous measure of the likelihood of takeover based on Cremers et al. (2009), except that we exclude Q from the list of factors used to create the likelihood of takeover. Column 7 includes interactions of this likelihood being in 1st and 4th quartile of distribution with PPL passage. Columns 8 and 9 only includes firms that eventually adopt a pill, while column 8 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 1st and 4th quartile firms being equal. t-statistics (two-way clustered by state of incorporation and year) are reported in parentheses. The *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Dependent Variables				Ppill$_{[t]}$				New Ppilll$_{[t]}$	Ln(PPill Duration)$_{[t]}$
	1	2	3	4	5	6	7	8	9
PPL$_{[t]}$	-0.0274	-0.0243	-0.0501	0.0188	-0.00308	-0.0002	0.0328	0.0113	-0.0274
	(-0.55)	(-0.49)	(-1.06)	(0.43)	(-0.08)	(0.00)	(0.57)	(0.18)	(-0.47)
Q(Lowest)$_{[t-1]}$		0.048***							
		(7.16)							
Q(Highest)$_{[t-1]}$		-0.050***							
		(-5.66)							
PPL[t] × Q(Lowest)$_{[}\tau(s)-1_{]}$			0.136***		0.0957*			-0.0504	0.250**
			(2.97)		(1.97)			(-0.54)	(2.76)
PPL[t] × Q(Highest)$_{[}\tau(s)-1_{]}$				-0.154**	-0.134*			-0.0740	-0.0858
				(-2.44)	(-2.00)			(-1.01)	(-1.36)
PPL[t] × Takeover Likelihood						0.0981***			
						(3.80)			
PPL[t] × Q4 of Takeover Lklhd							0.1013		
							(0.90)		
PPL[t] × Q1 of Takeover Lklhd							-0.1681***		
							(-3.70)		
Other Antitakeover Laws	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm, Div × Year, Ind-Yr FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	31,391	31,391	31,391	31,391	31,391	31,168	31,168	11,779	16,042
Adjusted R2	0.579	0.581	0.580	0.580	0.580	0.580	0.580	0.173	0.902
F-test for Q1=Q4					9.52		5.12	0.072	17.67

Table 4

PPLs and Firm Value

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. Interaction of PPL with the presence of poison pill, logarithm of Tobin's Q, and the likelihood of takeover index are of PPL passage. 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. The +, *, **, and *** denote significance at the 15%, 10%, 5%, and 1% level, respectively.

Dependent variable:	$Ln(Q)_{[t]}$						
	1	2	3	4	5	6	7
$PPL_{[t]}$	0.0472**	0.0442**	0.0680***	0.0572***	0.0536***	0.0645***	0.0715***
	(2.74)	(2.35)	(5.80)	(3.31)	(3.30)	(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)					
$PPL_{[t]} \times Q_{[\tau(s)-1]}$				-0.0062			
				(-0.58)			
$PPL_{[t]} \times Takeov.Lkl_{[\tau(s)-1]}$					0.0319+		
					(1.56)		
Other antitakeover laws	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Division × Year FE	Yes	Yes	Yes	Yes	Yes	No	No
Region × Year FE	No	No	No	No	No	Yes	No
State × Year FE	No	No	No	No	No	No	Yes
Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	29,213	29,213	29,213	29,213	29,175	29,213	29,223
Adjusted R^2	0.659	0.660	0.659	0.659	0.659	0.658	0.659

Table 5

PPLs and Firm Value with Alternative Samples and Estimation Methods

In Panel A, columns 1-3 show the matched sample $Ln(Q)$ regression results over a $t \pm 3$ estimation window. 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 4 and 5 of Panel A 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 measure 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. In Panel B, columns 1-3 show the average treatment effect together with its t-ratio based on the methodology developed in Callaway and Sant'Anna (CS-DID). Given that the CS-DID method reuses observations to estimate coefficients for different time horizons, we report N*, which represents the total number of observations used in estimating all time windows with the main CS-DID estimator. Columns 4-6 present the results from stacked regressions approach. Standard errors are clustered by cohort, state of incorporation, and year. The *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Panel A. Matched and Extended Sample

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
Industry × Year FE	Yes	Yes	No	Yes	Yes	Yes
N	1,735	1,735	2,278	66,819	83,489	33,921
Adjusted R2	0.683	0.683	0.696	0.649	0.629	0.635
Sample		Matched		Full (Spamann)	Full	With Catan Pills

Panel B. Callaway and Sant'Anna and Stacked Regressions Methods

Dependent Variable: $Ln(Q)_{[t]}$						
	1	2	3	4	5	6
$PPL_{[t]}$	0.0375**	0.0375***	0.0356**	0.0406*	0.0594***	0.0668***
	(2.22)	(2.87)	(2.46)	(1.97)	(3.27)	(5.47)

Method	Callaway & Sant'Anna			Stacked Regressions		
Window	[-3,+3]	[-5,+5]	[-10,+10]	[-3,+3]	[-5,+5]	[-10,+10]
N*		25,606		122,073	185,835	302,257

Table 6
PPLs and Alternative Measures of Firm Value

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. The *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

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	29,213	29,213	29,503	29,024
Adjusted R^2	0.574	0.625	0.725	0.108

Table 7

Testing the Mechanisms

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 ($Ln\left(Q_{[t]}\right)$). 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. The $^{+}$, *, **, and *** denote significance at the 15%, 10%, 5%, and 1% level, respectively.

Panel A: Commitment 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***	0.0659**	0.0582*
	(6.02)	(6.33)	(5.69)	(3.26)	(5.02)	(2.16)	(2.05)
$PPL_{[t]} \times PPill_{[\tau(s)-1]}$	-0.0364	-0.0404*	-0.0425	-0.0387	-0.0351	-0.0358	-0.055+
	(-1.49)	(-1.98)	(-1.68)	(-1.64)	(-1.41)	(-1.72)	(-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 (0.07)	-0.044 (-0.68)
$PPL_{[t]} \times Large\ Cust_{[\tau(s)-1]} \times High\ Intangibles$							0.130** (2.58)
N	29,213	29,213	29,213	29,213	29,213	29,213	29,213
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	22,007	22,007	128	128	22,007	22,007
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

ONLINE APPENDIX OF

POISON PILLS IN THE SHADOW OF THE LAW

Appendix A. Additional Information about Poison Pills and Poison Pill Statutes

I. Poison Pills

Poison Pills provide their holders with special rights in the case of a triggering event such as a hostile takeover bid. If a deal is approved by the board of directors, the poison pill can be revoked, but if the deal is not approved and the bidder proceeds, the pill is triggered. Typical poison pills give the holders of the target's stock other than the bidder the right to purchase stock in the target or the bidder's company at a steep discount, making the target unattractive or diluting the acquirer's voting power. Thus, poison pills are a crucial component of the "delay" strategy at the core of modern defensive tactics (see Gompers, Ishii and Metrick (2003)). For a description of a poison pill mechanism consider the following illustrative example from Allen, Kraakman, and Khanna (2021).

T Corp. distributes Rights as a dividend to its shareholders. Each Right purports to be a right to buy 1/100 of a share of the company's common stock in the future for an extravagant, "out of the money" price: say $50 when the common stock is selling for $75 a share. Given its terms, no one really expects this Right ever to be exercised (although the company's lawyers might argue that the Right's high exercise price represents the hidden long-term value of the company's stock). The Rights do not trade separately at this point but are embedded in the common stock on which the dividend is paid. However, should a "triggering event" occur, the Rights detach and are tradable separately. A triggering event might be the acquisition of 10 percent of the company's stock by any single entity or an affiliated group of persons or the announcement of a tender offer for 10 percent or more of the company's stock.

If a person or group did acquire a 10 percent block, then under a "flip-in" pill, each outstanding Right would "flip-into" a right to acquire some number of shares of the target's common stock at one-half of the market price for that stock. In other words, the Right's holder would be able to buy stock from the company at half price. Now, if every Right holder bought stock at half price, the aggregate effect is to increase the proportionate holdings of all shareholders except the "triggering person," whose Right would be canceled upon the occurrence of the triggering event and who, as a result, would only own a much smaller interest in the company than that for which she initially paid.

Consider now "flip-over" plans. When triggered, these plans create a right to buy some number of shares in the corporation whose acquisition of target stock had triggered the right. In this plan, a triggering event (when followed by a merger or sale of more than 50 percent of the target's assets to the triggering shareholder or an affiliate) results in the rights being exercisable.

The "back-end" poison pill and flip-over poison pill both serve as defenses against hostile takeovers but operate differently. In a flip-over poison pill, the plan activates when a hostile acquirer gains a set percentage of the target company's shares or completes a merger. Once this happens, existing shareholders of the target can buy the acquirer's shares at a discount, diluting the acquirer's equity and making the takeover more expensive and less attractive. On the other hand, a back-end poison pill comes into play after the hostile takeover is complete. Here, existing shareholders of the target company, excluding the acquirer, can sell their shares back to the acquirer at a premium. This places a financial burden on the acquirer by requiring them to repurchase shares at a higher cost than they initially paid. While both strategies aim to make hostile takeovers less appealing, they do so in different ways. The flip-over plan focuses on

diluting the acquirer's equity, whereas the back-end plan compels the acquirer to repurchase shares at a premium.

II. PPLs and Other Antitakeover Provisions

Poison pill statutes explicitly authorize boards to differentiate between various classes of shareholders when implementing rights plans. In many instances, these statutes were enacted to overturn court rulings that had invalidated poison pills (Kahan and Rock (2002)). Specifically, in every state that was part of the first wave of poison pill legislation, where courts had previously prohibited the use of poison pills, state legislatures responded by passing laws to validate the pill (Robinson, Coates, and Presser (1989), quoted by Kahan and Rock (2002)). This lends additional weight to our argument that, following the 1985 Moran case, a (rebuttable) presumption arose in other jurisdictions asserting the general validity of poison pills. However, this presumption faced significant erosion in 1988 when Delaware restricted the legitimacy of poison pills through the Pillsbury and Interco decisions. Moreover, this presumption was effectively overturned in jurisdictions where courts explicitly invalidated existing poison pills, a situation later rectified by the enactment of statutes specifically authorizing poison pills. Therefore, in the second-wave jurisdictions, uncertainty surrounding the validity of poison pills remained high until such statutes were enacted for two key reasons. First, the influence of Delaware law weakened. Second, in many first-wave jurisdictions, courts questioned the validity of poison pills until these concerns were alleviated by the introduction of poison pill laws.

Finally, it's also worth mentioning that some jurisdictions have statutes that explicitly permit a board to restrict future boards from redeeming the pill. Such a provision is not permitted

in Delaware, thereby making these statutes an avenue to legitimize poison pills that would otherwise be invalid under Delaware law (see Barzuza (2009)).

The other antitakeover statutes, aside from poison pill statutes, are:

- Control-Share Acquisition Laws require a majority of disinterested shareholders to vote on whether a newly qualifying large shareholder has voting rights.
- Business Combination Laws impose a moratorium on certain kinds of transactions (e.g., asset sales, mergers) between a large shareholder and the firm, unless the transaction is approved by the Board of Directors. Depending on the State, this moratorium ranges between two and five years after the shareholder's stake passes a prespecified (minority) threshold. This provision is also the only state antitakeover law in Delaware, the state of incorporation for most listed companies (see, Gompers et al. (2003)).
- Fair-Price Laws limit the range of prices a bidder can pay in two-tier offers. They typically require a bidder to pay to all shareholders the highest price paid to any during a specified period of time before the commencement of a tender offer, and do not apply if the deal is approved by the board of directors or a supermajority of the target's shareholders. The goal of this provision is to prevent pressure on the target's shareholders to tender their shares in the front end of a two-tiered tender offer, and they have the result of making such an acquisition more expensive (see, Gompers et al. (2003)).
- Directors' Duties Laws allow directors to consider constituencies other than shareholders. These constituencies may include, for example, employees, host communities, or suppliers. This statute also provides boards of directors with a legal basis for rejecting a takeover that would have been beneficial to shareholders (see, Gompers et al. (2003)).

Regarding the simultaneous enactment of other statutes alongside poison pill statutes, it's worth noting the following: The majority of states have approved poison pill laws as standalone legislation. In some instances, poison pill statutes were enacted in conjunction with directors' duties laws. Only in a few cases did states approve multiple takeover laws at the same time (see, Karpoff and Wittry (2018)).

However, directors' duties laws (also known as corporate "constituency statutes") are not solely antitakeover measures. These laws enable directors to take into account the interests of different stakeholders, thereby fostering a more stakeholder-centric governance model in corporate decision-making, as opposed to a shareholder-centric model. By considering the interests of various stakeholders, these laws can also influence firm policies, such as those related to innovation (see Flammer and Kacperczyk (2015)). In the context of takeover situations, these laws also have significant implications. They empower directors to legitimately reject acquisition proposals if deemed not to align with the broader interests of the corporation, including those of non-shareholder stakeholders, or to resist hostile takeovers.

For example, in Delaware, the Revlon doctrine (1986) establishes that once a corporation is up for sale or its breakup is inevitable, directors should act as neutral auctioneers. Their sole mandate is to ensure shareholders receive the highest possible price from bidders. When directors' duties laws are in place, the Revlon standard is generally not applicable. Therefore, the conjunction of these laws with poison pill laws doesn't reinforce the legitimacy of poison pills. Rather, it provides directors with a legal defense against shareholders who might argue that the poison pill was implemented solely to secure the directors' positions. With directors' duties laws in place, directors can more convincingly justify the adoption of antitakeover measures like

poison pills, arguing they are in the broader interests of all stakeholders, not just shareholders. However, this stronger defense does not per se enhance the validity of poison pills as the pill's legitimacy is not unconditional.

Finally, it's important to highlight that although other antitakeover provisions do serve to bolster the authority of incumbent boards in the takeover market, the implementation of a poison pill stands out as the most potent antitakeover mechanism. Its adoption effectively makes the completion of an acquisition almost unattainable without subjecting the acquirer to exorbitant costs. Additionally, the poison pill offers distinct advantages to target company managers, as it can be instituted by any corporation at any time without requiring shareholder approval.

III. Validity of PPL Statutes

Throughout the paper we assume that PPLs increase the strength of the shadow pill and reduce the legal uncertainty about the use of visible pills. This position reflects the fact that PPLs are commonly perceived as valid as they have never been overturned by courts. However, the history of antitakeover laws and their validity in general is more nuanced.

The question of the legitimacy of antitakeover laws is not new. The landmark case, Edgar v. MITE Corporation (1982), scrutinized the Illinois Business Take-Over Act, which imposed a series of regulations and delays on companies intending to acquire Illinois-based corporations. The Supreme Court ruled that the Act was unconstitutional under the Supremacy Clause, the Commerce Clause of the U.S. Constitution, and preempted by the Williams Act. The Court reasoned that Illinois couldn't impose such burdens on interstate commerce for minimal local benefit.

Following this case, states have repealed over time the so called first generation antitakeover statutes that were incompatible with federal law (statutes that had been approved from 1968 to 1982) and worked to approve new antitakeover laws in compliance with federal standards. These efforts were initially validated in CTS v. Dynamics Corp. of America (1987), where the Supreme Court upheld an Indiana statute, establishing that states could create regulations as long as they reasonably protected investors without undermining federal law (see Bebchuk and Jackson (2014)).

Federal courts, however, remain divided on the issue. Various circuits and state district courts have rendered conflicting opinions on the validity of the new antitakeover statutes. Nonetheless, none have been invalidated so far.

Regarding poison pill statutes specifically, although many judicial opinions have addressed the constitutionality of various state antitakeover measures, federal courts have overlooked the question of whether these state-level poison pill rules might be preempted by the Williams Act. Neither of the landmark Supreme Court cases in this area, MITE and CTS, explicitly addressed the issue of poison pill laws.

One notable exception exists: a federal trial court in Southdown, Inc. v. Moore McCormack Res., Inc., 686 F. Supp. 595, 604–05 (S.D. Tex. 1988) suggested, in dictum, that such poison pill rules "may be preempted by the Williams Act." The court posited that the primary justification for the poison pill strategy, which is to buy time, might conflict with the Williams Act. However, Texas subsequently passed poison pill legislation (in 2003, effective in 2006), and there are no doubts about the validity of the statute.

Aside from this isolated instance, courts that have recognized the Williams Act as setting meaningful limitations on state antitakeover measures have not specifically examined whether the Act also preempts state-level poison pill rules (see Subramanian, Herscovici, and Barbetta (2010)).

Appendix B. Additional Robustness Checks

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., *Air Products* v. *Airgas* in 2010) continued to uphold the certainty of pills in Delaware.[1]

We check the robustness of our results to coding Delaware firms' *PPL* differently in Tables OA3 and OA4. 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 preserves 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 a non-PPL state is appropriate, because even difference-in-difference estimates are affected if firms reincorporate. Given that Delaware attracts a sizable number of reincorporations, recoding its PPL indicator significantly affects

[1] 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.

regression estimates. 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. The construction of the index and the results are described in Table OA4.

II. State-by-Year Fixed Effects

Following Gormley and Matsa (2016), in Table OA5 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 or only marginally significant, which we argue is due to a lack of variation (i.e., low statistical power).

III. 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 OA2 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.

IV. Excluding Multi-Law Adopting States

We show in Table OA6 that our Tobin's Q results are robust to excluding states that enacted other antitakeover laws in the same year they passed PPLs.

V. Other Antitakeover Provisions

We also examine if the effect of PPL depends on the presence of other antitakeover provisions in the firm in Table OA11. While ex-ante one may hypothesize that the effect of shadow pill may depend on the presence of, e.g., a staggered board, we do not find empirical support for this hypothesis. The coefficients on interactions of PPL with other firm-level provisions are insignificant. However, we are cautious in interpreting these findings, as it is difficult to tell if these estimates reflect a genuine lack of effect, a zero net effect of complementarity and substitution effects in different contexts, or the inability to precisely estimate the true effect due to empirical limitations of the data and empirical setting.

Figure OA1

PPLs and Firm Value with Each Law Adopting State Dropped

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.

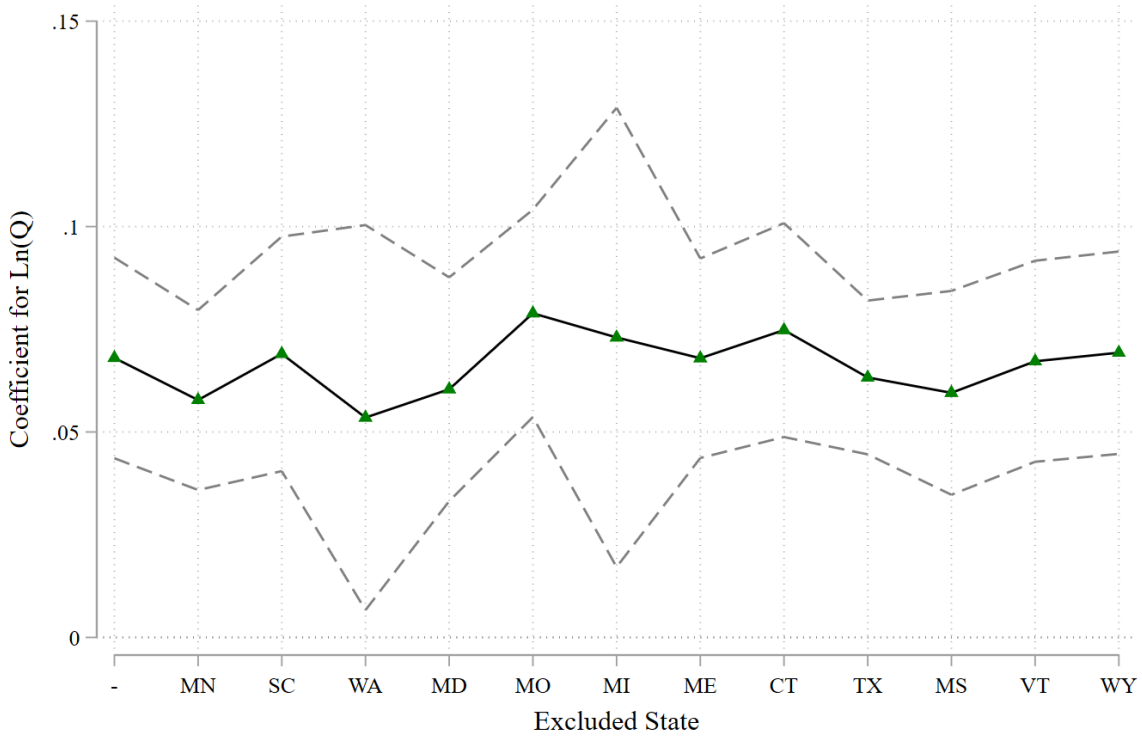
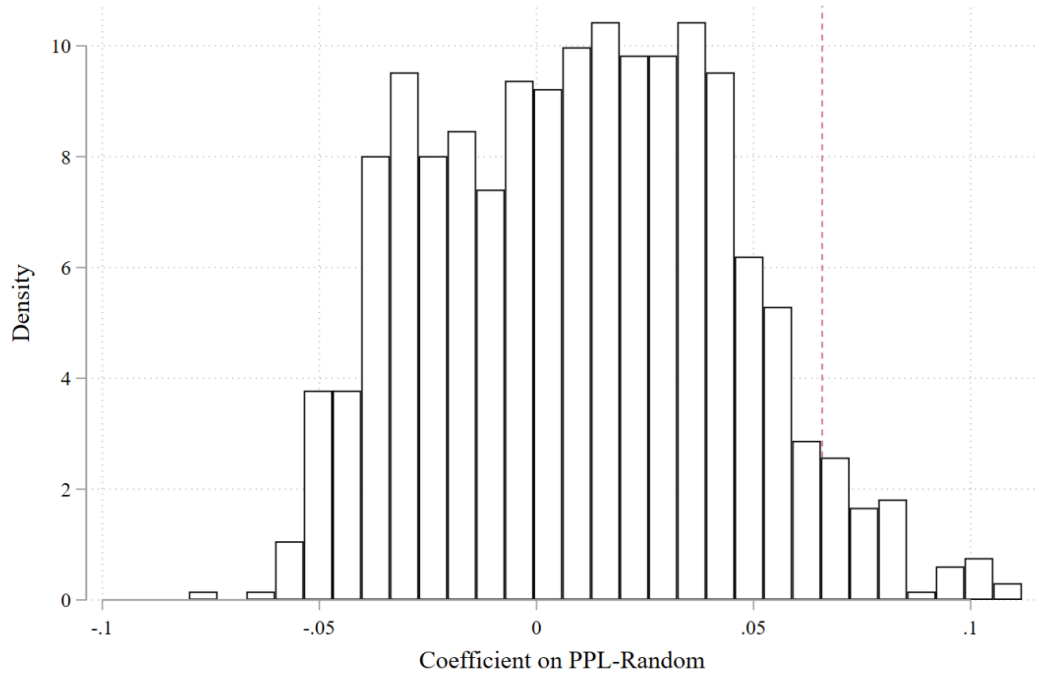


Figure OA2

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.


Density
0
2
4
6
8
10
-.1
-.05
0
.05
.1
Coefficient on PPL-Random

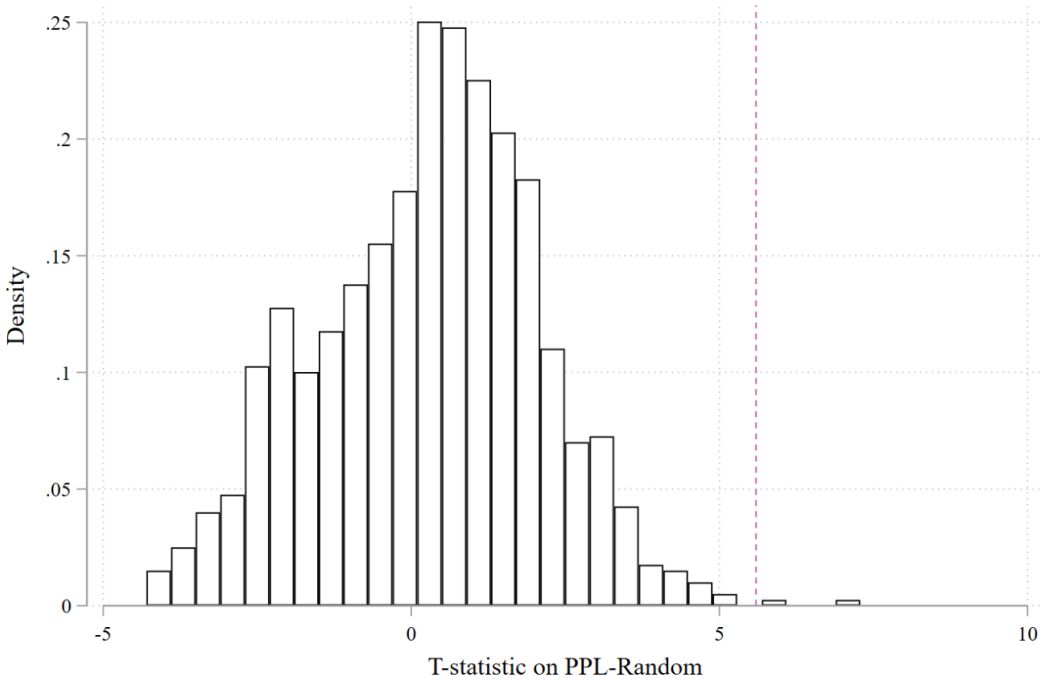

Density
0
.05
.1
.15
.2
.25
-5
0
5
10
T-statistic on PPL-Random

Table OA1

PPL Adoption Dates

The table reports the month and year in which a state adopts a poison pill law (PPL). The dates listed above come from Cain et al. (2017) and Karpoff and Wittry (2018). States followed by * have adopted other antitakeover laws in the same year as PPLs: , which include Florida (DD), Idaho (CS, BC, FP, DD), Illinois (BC), Kentucky (DD), Maryland (DD), Massachusetts (DD, BC), Oregon (DD), Pennsylvania (BC, FP), Rhode Island (BC, FP, DD), South Dakota (BC, FP, DD), Texas (DD), and Wisconsin (BC).

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

[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 OA2

First wave-PPLs and Visible Poison Pills

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. *T*-statistics (clustered by state of incorporation but not by year, given the short time frame of each regression) are reported in parentheses. The *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

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)
$FW\ PPL_{[t]} \times Q(lowest)_{[\tau-1]}$	0.0366	0.0367	0.00848	0.00705	0.0744**	0.0844**
	(0.67)	(0.67)	(0.11)	(0.09)	(2.05)	(2.56)
$FW\ PPL_{[t]} \times Q(highest)_{[\tau-1]}$	-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	8,302	8,302	6,572	6,572	4,436	4,436
Adjusted R^2	0.674	0.674	0.685	0.685	0.833	0.833

Table OA3

Adjusting the Sample for Delaware Case Law

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. t-statistics (two-way clustered by state of incorporation and year) are reported in parentheses. The +, *, **, and *** denote significance at the 15%, 10%, 5%, and 1% level, respectively.

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	29,213	29,213	10,131	10,131
Adjusted R^2	0.659	0.659	0.665	0.665

Table OA4

PPV-Index

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 et al. (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. t-statistics (two-way clustered by state of incorporation and year) are reported in parentheses. The *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

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	29,213	29,213	29,213	29,223
Adjusted R^2	0.659	0.659	0.658	0.659

Table OA5

State-by-Year Fixed Effects

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. t-statistics (two-way clustered by state of incorporation and year) are reported in parentheses. The +, *, **, and *** denote significance at the 15%, 10%, 5%, and 1% level, respectively.

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	29,223	29,223	29,223	29,223
Adjusted R^2	0.659	0.659	0.659	0.659

Table OA6

Excluding Multi-Law Adopting States

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. 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. The *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

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	28,374	28,374
Adjusted R^2	0.658	0.658

Table OA7

Variable Definitions

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

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 et al. (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 et al. (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 et al. (2017). The $PPV\text{-}Index$ captures the relative change or strength of poison pill validity over time and by the 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.

Table OA8

Innovation Response

The table reports the results for OLS regressions of changes in the indicator for having filed for any patents (column 1 and 4), log-changes in the number of patents (columns 2 and 5) and log-changes in the forward-looking measures of patent citations (columns 3 and 6) on PPL indicator variables and its interactions with having a poison pill at the moment of PPL passage. The "Other antitakeover laws" include BCL, CSL, DDL, and FPL. Division 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. t-statistics (two-way clustered by state of incorporation and year) are reported in parentheses. The +, *, **, and *** denote significance at the 15%, 10%, 5%, and 1% level, respectively.

	Δ Has Patents	Δ Ln (Patents)	Δ Ln (Citations)	Δ Has Patents	Δ Ln (Patents)	Δ Ln (Citations)
		t			t+2	
	1	2	3	4	5	6
$PPL_{[t]}$	-0.0148	0.0564	0.0847	0.0434***	0.140***	0.214***
	(-0.45)	(0.74)	(0.86)	(2.76)	(3.23)	(3.50)
$PPL_{[t]} \times PPill_{[\tau(s)-1]}$	0.0137	0.0984	0.174	-0.0356	0.0441	0.148
	(0.65)	(0.49)	(0.80)	(-0.97)	(0.31)	(0.81)
$BCL_{[t]}$	0.00418	-0.00896	-0.106	-0.0331**	-0.0307	-0.223*
	(0.31)	(-0.18)	(-0.70)	(-2.62)	(-0.46)	(-1.88)
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	29,557	10,371	10,117	26,093	9,212	8,882
R^2	0.169	0.266	0.248	0.179	0.269	0.250

Table OA9
First-Wave Poison Pill Laws and Firm Value

The table presents results from OLS regressions analyzing the value implications of First Wave PPLs. The sample in columns 1 and 2 covers years between 1982 and 1992, and variable Post1989 represents year 1989 and beyond. In columns 3 and 4, the specification is the same as in Table 4, column 3, except that the sample is extended back to 1982 (column 3) or 1989 (column 4). 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. 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. The *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Dependent variable: $Ln(Q)_{[t]}$				
	1	2	3	4
$FW\ PPL_{[t]}$	0.0043	-0.0001		
	(0.25)	(-0.01)		
$FW\ PPL_{[t]} \times Post1989$		0.0049		
		(0.28)		
$PPL_{[t]}$			0.0153	0.0453**
			(0.92)	(2.44)
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	33,481	33,481	36,410	31,882
Adjusted R^2	0.697	0.697	0.747	0.743
Time Period	1982-1992		1982-2012	1989-2012

Table OA10

Stock Returns Around the Passage of PPLs

The table shows cumulative announcement returns (based on a market model) regressed on poison pill statutes adoption indicator and industry fixed effects. The time of PPL's passage is defined as the day on which the governor approved the statute. Each column represents a different window around the adoption event. The * denotes significance at the 10% level.

	1 (t-3, t+3)	2 (t-2, t+2)	3 (t-1, t+1)	4 (t-1, t)	5 (t, t)	6 (t, t+1)
$PPL_{[t]}$	0.00208	0.00333	0.00262	-0.000798	0.00116	0.00438*
	(0.50)	(0.93)	(0.93)	(-0.34)	(0.66)	(1.84)
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
N	83,748	83,744	83,741	83,738	83,734	83,738
Adj. R-squared	0.00349	0.00274	0.00372	0.00264	0.00289	0.00446

Table OA11

Effects of PPL and Other Antitakeover Defenses

Columns 1-5 present results from OLS regressions analyzing the value implications of SW PPLs interacted with firm-level indicators for the presence of other antitakeover defenses. Each regression is analogous to the baseline specification presented in column 3, Table 4, but also includes the interaction of PPL with one of the five additional takeover defenses: golden parachutes, staggered board, and limits/supermajority on mergers approval, bylaws amendments, and charter amendments. The dependent variable is the natural logarithm of Tobin's Q ($Ln(Q)$). Columns 6-7 regress indicator of having staggered board on PPL. The "Other antitakeover laws" include BCL, CSL, DDL, and FPL. Division fixed effects are measured using U.S. 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. The *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Dependent variable:	$Ln(Q)_{[t]}$					$Classified\ Board_{[t]}$	
	1	2	3	4	5	6	7
$PPL_{[t]}$	0.0411	0.0410	0.0534*	0.0532+	0.0468+	-0.0456	-0.1032*
	(1.30)	(1.49)	(1.75)	(1.71)	(1.62)	(-1.18)	(-1.96)
$PPL_{[t]} \times PPill_{[\tau(s)-1]}$	-0.0104	-0.0061	-0.00431	-0.0050	-0.0061		0.1034**
	(-0.28)	(-0.68)	(-0.11)	(-0.13)	(-0.15)		(2.32)
$PPL_{[t]}$ × Golden Parachute	0.0156						
	(0.87)						
$PPL_{[t]}$ × Classified Board		-0.0061					
		(-0.15)					
$PPL_{[t]}$ × Supermajority (Mergers)			-0.0613				
			(-1.36)				
$PPL_{[t]}$ × Limits on Bylaws Amendments				-0.0559			
				(-0.96)			
$PPL_{[t]}$ × Limits on Charter Amendments					-0.0019		
					(-0.02)		
Other antitakeover laws	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Division × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	16,588	23,239	16,588	16,588	16,588	23,488	23,488
Adjusted R2	0.724	0.667	0.724	0.724	0.724	0.905	0.905

Table OA12

Effects of PPL Depending on Managerial Incentives

The table presents results from OLS regressions analyzing the value implications of SW PPLs interacted with firm-level measures of executive's compensation sensitivity with respect to firm value (Delta) as well as share of stocks of the firm owned by executives (managerial ownership). Each regression is analogous to the baseline specification presented in column 3, Table 4, but also includes the interaction of PPL with delta or managerial ownership. 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. 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. The *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	1	2	3	4
$PPL_{[t]}$	0.0468**	0.0698**	0.0435**	0.0641***
	(2.29)	(2.64)	(2.31)	(5.25)
$PPL_{[t]} \times PPill_{[\tau(s)-1]}$		-0.0445*		-0.0353
		(-1.77)		(-1.47)
Compensation Delta	0.00932***	0.00934***		
	(5.17)	(5.27)		
% Managerial Ownership			-0.00534	-0.00532
			(-0.89)	(-0.88)
$PPL_{[t]} \times Compensation\ Delta$	-0.0002	0.0002		
	(0.15)	(0.14)		
$PPL_{[t]}$			0.0113	0.0113
$\times$ % $Managerial\ Ownership$			(1.31)	(1.32)
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	18,679	18,679	29,213	29,213
Adjusted R2	0.691	0.691	0.659	0.659

Table OA13

PPLs and Pills Adopted Around the Threat of Takeover

The table is analogous to Table 3, except the dependent variable is an indicator for a non-clear day pill, i.e., a poison pill that was adopted in the period 90 days before to 90 days after the moment in which some investor obtained more than 5% of ownership in the firm, as measured by the 13D filings. Similar results are obtained for 180 days window, while 30 days window results in insignificant estimates. The 13D filings data is available for 1994-2011 period. Non-Clear Day pills are adopted by 96 firms in our data out of 3,149 firms that adopt any pill for which we observe the exact date of pill adoption (and thus can determine if it is a clear day pill or not). Equivalents of columns 8 and 9 from Table 3 do not show significant response and are not presented.

Dependent Variables	Non-Clear Day $Ppill_{[t]}$						
	1	2	3	4	5	6	7
$PPL_{[t]}$	-0.00838*	-0.00824*	-0.0106**	-0.0108+	-0.0145**	-0.00848*	-0.00959***
	(-2.03)	(-2.02)	(-2.64)	(-1.66)	(-2.50)	(-2.03)	(-2.88)
$Q(Lowest)_{[t-1]}$		0.00603***					
		(3.35)					
$Q(Highest)_{[t-1]}$		-0.00120					
		(-0.65)					
PPL[t] × $Q(Lowest)_{[}\tau(s)-1_{]}$			0.0107		0.0138		
			(0.74)		(1.02)		
PPL[t] × $Q(Highest)_{[}\tau(s)-1_{]}$				0.00967	0.0130		
				(0.93)	(1.47)		
PPL[t] × Takeover						-0.0010	
						(-0.30)	
PPL[t] × Q4 of Takeover Likelihood							-0.0022
							(-0.30)
PPL[t] × Q1 of Takeover Likelihood							0.0062
							(0.48)
Other Antitakeover Laws	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm, Div-Year, Ind-Yr FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	14301	14301	14301	14301	14301	14,255	14,255
Adjusted R2	0.416	0.416	0.416	0.416	0.416	0.417	0.417
F-test for Q1=Q4					0.00155		0.53

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