Institutional and political economy considerations in natural experiments: The case of state antitakeover laws [*]

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Abstract

We argue and demonstrate empirically that institutional and political economy considerations have first-order effects in tests that use state antitakeover laws for identification. A priori, the size and direction of a law's effect on a firm's takeover protection depends on (i) other state antitakeover laws, (ii) pre-existing firm-level takeover defenses, and (iii) the legal regime as reflected in important court decisions. In addition, (iv) state antitakeover laws are identifiably not exogenous for many firms. We show that the inferences from nine prior studies relating to nine different outcome variables change substantially when we include controls for these considerations.

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I. Introduction

Empirical corporate finance research is plagued by endogeneity. As Alchian (1950) points out, the economic environment tends to select the combination of firm characteristics – including organization, financing, payout policy, ownership structure, board structure, compensation, and governance – that minimize total production costs. This implies that each firm's characteristics and performance are the endogenous outcomes of the competitive process. Researchers nonetheless seek to explore questions of causality. For example, does better governance improve firm value, innovation, or transparency, or does it change the firm's financing and payout policy?

To address endogeneity issues, researchers frequently investigate firm characteristics around legislation, regulations, or court decisions that exogenously affect firms' costs or governance. For example, previous studies have used banking deregulations to examine innovation and economic growth (Bertrand, Schoar and Thesmar, 2007; Chava, Oettl, Subramanian, and Subramanian, 2013); tax changes to examine firm investment, financing, and payout decisions (Givoly, Hayn, Ofer and Sarig, 1992; Dharmapala, Foley, and Forbes, 2011; Faulkender and Petersen, 2012; Tsoutsoura, 2015); securities and financial reforms to examine firm value, governance, and financing (Zhang, 2007; Coates, 2007; Atanasov, Black, Ciccotello and Gyoshev, 2010; Iliev, 2010; Vig, 2013); legislated subsidies to examine financial constraints (Butler and Cornaggia, 2011); and court decisions to examine the impact of firm governance on firms' disclosures and exposure to securities lawsuits (Furchtgott and Partnoy, 2013; Licht, Poliquin, Siegel, and Li, 2013; Hopkins, 2013).[1]

This paper examines the use of regulatory and legal changes to identify exogenous variation in economic determinants in empirical tests. We focus on a specific application that has become particularly popular – state antitakeover laws and court decisions that affect firms' takeover vulnerability. Garvey and Hanka (1999) and Bertrand and Mullainathan (1999) pioneered the use of state antitakeover laws to identify empirical tests involving firm governance, and in an influential paper, Bertrand and Mullainathan (2003) argue that one type of antitakeover law – business combination laws – impose particularly meaningful changes in firms' governance. Bertrand and Mullainathan (2003) use business combination laws to examine the impact of firm governance on wages, investment, and productivity. Since then, more then 40 additional published and working papers have used business combination and other antitakeover laws to examine the effects of firm governance on operating performance, payout policy, cash holdings, innovation, bondholder returns,

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[1] This is only a partial listing of papers that use external shocks, typically in legal rules, to generate natural experiments. See Atanasov and Black (2015) for an analysis of shock-based studies in corporate governance.

stakeholder relations, financial statement informativeness, and other firm characteristics. The Appendix Table A1 lists and contains brief descriptions of these papers.

Our main argument is that institutional and political economy considerations have first-order effects on the appropriate specification and interpretation of tests that use regulatory and legal changes for identification. We focus on state antitakeover laws, particularly business combination laws, and show that these laws' institutional settings and historical backgrounds can change the interpretations of several popular tests.

Our argument is both theoretical and empirical. Sections II–V begin by showing that, a priori, the size and direction of an antitakeover law's effect on a firm's takeover protection depends on (i) coverage by a first-generation state antitakeover law or by any of the other four major types of second-generation state antitakeover laws, (ii) pre-existing firm-level takeover defenses, and (iii) the legal regime as determined by important court decisions. Coverage by first-generation laws is a particularly important concern for empirical tests that include data from before 1982 because it can reverse the inferences from these tests. An additional concern is that business combination and other state antitakeover laws are not exogenous for, or do not apply to, a substantial number of easily identifiable firms that are incorporated in the states that adopt these laws, either because these firms lobbied for their states' laws or they opted out of coverage by the laws.

In Section VI we argue that these institutional and political economy considerations can be viewed as omitted variable problems for which there exist simple data-oriented solutions. Table 1 presents data on the deployment of first-generation laws in 38 states. Table 2 presents state-by-state data on the adoption of all five major types of second-generation state antitakeover laws. Table 3 presents data on firms for which the enactment of state antitakeover laws is clearly endogenous, and which supplement the data on firms that opted out of coverage that are available in the RiskMetrics database. Cremers and Farrell (2014) have compiled data on firm-level defenses for a large number of firms from 1977 – 1989, which complement the post-1989 data on firm-level defenses that are available in the RiskMetrics database. Together, these data can be used to increase the accuracy and power of empirical tests that use business combination and other state antitakeover laws for identification.

In Section VII, we use these additional data to demonstrate that institutional considerations have large effects on economic inferences in a wide range of empirical tests. First, we use a generic empirical framework to replicate the basic results in eight prior studies that show that business combination laws are associated with significant changes in seven different outcome variables: return on assets, capital expenditures; growth in property, plant, and equipment; asset growth; cash holdings; selling, general, and administrative expense; and leverage. Adding controls for our additional institutional and political economy concerns substantially changes

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the inferences from these tests. For four of the seven outcome variables, the coefficient on business combination laws becomes statistically insignificant. For the other three outcome variables, the coefficient on poison pill laws also is statistically and economically significant, suggesting that poison pill laws impose changes in firms' takeover protections that are at least as important as business combination laws. For five of the seven outcome variables, the primary impact of a business combination law occurs among firms that lobbied for the laws and for which the laws are clearly not exogenous. Furthermore, the results for all seven outcome variables are insignificant among firms that have firm-level defenses, indicating that the impact of an antitakeover law on a firm's takeover protection is negligible when firms have their own defenses.

Next, we exactly replicate a specific study by Julian Atanassov (2013). Atanassov's main result is that coverage by a business combination law is associated with decreases in a firm's patents and patent citations. When we include controls for other antitakeover laws and relevant court decisions, however, these results disappear. Instead, we find that patents and patent citations are negatively related to coverage by poison pill laws. They also are negatively related to business combination laws, but only for the small subset of firms that lobbied for the laws and for which the laws are not exogenous.[2] By focusing on the poison pill law result, we can maintain Atanassov's (2013) conclusion that takeover protection is negatively related to a firm's innovative activity. To do so, however, we must drop the assumption that business combination laws identify important and exogenous changes in takeover protection.

Finally, we conduct simulations in which we assume that business combination laws have no impact on firms' governance or takeover vulnerability, and measure the frequencies with which misspecified tests erroneously find that business combination laws are significant determinants of an arbitrary outcome variable. We find that there exist many underlying structures for which a researcher would mistakenly infer that the passage of a business combination law has a meaningful impact on the outcome variable even though, in fact, the relation does not exist. In some simulations, tests that erroneously focus only on business combination laws yield significant coefficients with the opposite sign as the true impact of takeover protection on the outcome variable.

Our study is related to two contemporaneous working papers that seek to correct misperceptions in the finance literature regarding state antitakeover laws. Cain, McKeon, and Solomon (2014) construct an index that consists of fitted values from a takeover likelihood regression that, they argue, provides a better measure

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[2]In some tests, patents and patent citations are negatively related to coverage by a business combination law after a 1989 court decision that upheld a business combination law (*Amanda Acquisition Corp. v. Universal Foods Corp.*, 877 F.2d 496, 1989). These results, however, are not robust. In Section VII.C and the Appendix, we show that the *Amanda*-related results disappear when we include a control for a contemporaneous court decision that strengthened target firms' abilities to deploy poison pills and "just say no" to unsolicited takeover bids (*Paramount Communications, Inc. v. Time, Inc.*, 571 A.2d 1140, Del. Supr. 1989).

of a firm's governance environment than business combination laws. Catan and Kahan (2014) point out that, unlike finance researchers, legal scholars tend to view state antitakeover laws as having little effect on firms' takeover protections. They argue that most studies that use state antitakeover laws to identify changes in governance suffer from coding errors, the failure to account for the importance of poison pills and managerial ownership in driving many firms' takeover vulnerability, and the fact that coverage by state antitakeover laws is endogenous for many firms. Catan and Kahan (2014) also demonstrate that the results of three specific studies are sensitive to model specification.

Our paper is complementary to Catan and Kahan (2014). A key difference regards our empirical replications. Catan and Kahan's replications illustrate that, by modern standards that tend to require high-dimensional fixed effects (e.g., see Gormley and Matsa, 2014), the results from three prior studies are not robust. In our empirical replications and simulations in Section VII, in contrast, we include firm, industry-year, and state of location-year fixed effects and still find significant effects of business combination laws when we do not control for our institutional and political economy considerations. Our results isolate the relevance of our institutional considerations over and above time-insensitive firm characteristics such as managerial ownership, or time-varying industry and locational shocks. Unlike Catan and Kahan (2014), we do not advocate that finance researchers abandon the use of state antitakeover laws to identify tests, but rather, conclude with five suggestions that should increase the power and accuracy of such tests and their interpretations.

II. The empirical construct using business combination laws

Figure 2 illustrates the empirical construct pioneered by Bertrand and Mullainathan (1999, 2003). Using data from 1976 through 1995, Bertrand and Mullainathan (2003) track the levels of several key variables, including worker wages, investment, productivity, and profitability. They hypothesize that these firm characteristics are affected by the quality of the firm's governance, and that governance is negatively affected by the degree to which incumbent managers are protected from unsolicited takeover bids. Citing research by Romano (1987), Bertrand and Mullainathan (2003) also argue that business combination laws are exogenous for virtually all covered firms. The adoption of a business combination law therefore represents an exogenous increase in the level of takeover protection and a decrease in the quality of firm governance for firms incorporated in the state that adopts the law.

Figure 1 illustrates this research design for firms incorporated in Massachusetts, which adopted a business combination law on July 18, 1989. A researcher compares the characteristic of interest – e.g., firm investment or innovation – from before July 18, 1989 to after July 18, 1989, and attributes any change in the

characteristic to the (exogenous) adoption of the business combination law. Different states adopted business combination laws at different times, so the test is unlikely to be driven by any contemporaneous changes that affect all firms from a single state. This fact suggests that the test satisfies the parallel trends assumption that is essential for a valid difference-in-difference test (e.g., see Roberts and Whited, 2012). Using this approach, Bertrand and Mullainathan (2003) find that the passage of a business combination law is associated with an increase in workers' wages and decreases in the rates by which old plants are destroyed, new plants are created, productivity, and profitability. They infer that these changes are caused by the increase in takeover protection afforded by coverage under business combination laws.

III. Institutional considerations

III.A. First generation state antitakeover laws

The identification strategy illustrated in Figure 1 works as long as firms' takeover protections were low from 1976 until July 18, 1989, when Massachusetts' business combination law was passed. It turns out, however, that for nearly half this period, most firms' takeover protections were extremely high, not low. This is because, until 1982, firms in Massachusetts and 37 other states were covered by first-generation state antitakeover laws. Table 1 reports on the states that adopted first-generation laws and the dates each law became effective. Using these adoption dates, 90% of firms in the RiskMetrics Governance database that were covered by business combination laws previously were covered by first-generation laws, including 73% of all firms not incorporated in Delaware.

First-generation antitakeover laws were effectively invalidated by a U.S. Supreme Court decision in *Edgar v. MITE Corp.* on June 23, 1982.[3] Until the *MITE* decision, these laws provided managers with unusually aggressive defenses against unwanted takeover bids. As described by Jarrell and Bradley (1980), most first-generation laws contained four types of provisions. The first provision required a bidder to file disclosure statements with the state securities commissioner and to send copies of the disclosure statements to the target company. The required disclosures were more extensive than those required by section 13(D) of the federal Williams Act of 1968, and required the bidder to provide detailed information about itself and its plans for the target firm. The disclosures also had to be filed 10 to 30 days before the bidder announced its tender offer for the target. Such a long advanced warning could provide target firm managers time to implement defensive measures and alert investors to the prospect of a bid. To the extent that the bidder sought to gain from a toehold investment (e.g., see Walkling, 1985), the effect was to both increase the bidder's costs and decrease its expected gain from the acquisition.

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[3] See *Edgar v. MITE Corp.*, 457 U.S. 624 (1982), also discussed in Section IV.

The second provision common to most first-generation laws was a requirement to hold the tender offer open for longer periods than required by federal law under the 1968 Williams Act. Two states (Massachusetts and Michigan) required tender periods of 60 days, and most laws created administrative review procedures that effectively allowed for repeated extensions of the tender offer period. As Romano (1993, p. 852) notes, "…state review tended to be extremely protracted." Lengthy tender periods, especially periods that could be extended repeatedly, increase the bidder's expected cost and increase the probability of competitive bids from other bidders (e.g., see Jarrell and Bradley, 1980).

The third provision in most first-generation laws created administrative overview of the tender offer by the state's securities commissioner. Target companies could demand a hearing with the commissioner to seek an injunction to delay the tender offer. In most (22) states, the commissioner could seek an injunction in state court if the commissioner deemed the offer to be illegal, and in one state (Virginia), the commissioner could unilaterally declare the tender offer illegal. It is reasonable to believe that state elected or appointed officials would be more sympathetic to the complaints of the target firm's managers than those of the bidder, who typically would have fewer local political connections.

The fourth provision common to most first-generation laws was to make the bidding firm liable for damages arising from the takeover offer, and in some cases to establish criminal penalties – including imprisonment for bidding firm managers – for violations of the disclosure and administrative procedures established by the law. The prospect of such penalties, or of fighting them through the legal process, represented a significant cost that could deter many prospective bids.

Consistent with the view that first-generation laws provided very strong takeover protection, Jarrell and Bradley (1980) find that takeover premiums increased significantly after the imposition of first-generation laws, and Smiley (1981) finds that these laws are associated with a significant decrease in hostile takeover bids. The effect of first-generation laws' strong antitakeover protections is illustrated in Figure 2, which continues the example for firms incorporated in Massachusetts. Massachusetts adopted a first-generation state antitakeover law on May 22, 1976. For firms incorporated in Massachusetts, the level of takeover protection is very high from this date through June 23, 1982, the date of the *Edgar v. MITE Corp.* decision. For empirical tests that access data from before 1983, observations from the 1976-82 period should be coded as having the highest level of takeover protection, not the lowest. Note that the effect is to substantially muddy, and possibly even reverse, the interpretation of many tests. Suppose, for example, that we observe that capital expenditures decrease from the pre-1989 period to the post-1989 period. Roughly half of the pre-1989 period has the highest level of takeover protection. Should we infer that capital expenditures decreased because the firm's takeover protections were relatively high after 1989, or because they were relatively low?

III.B. Other second-generation state antitakeover laws

Almost immediately after the *MITE* decision in June 1982, states began adopting second-generation antitakeover laws and firms began adopting new firm-level takeover defenses. Both developments complicate tests based on state antitakeover laws, particularly business combination laws, even for tests that are limited to a post-1982 sample period. Since 1982, when Ohio passed the first control share acquisition law, a total of 43 states have adopted at least 153 second-generation antitakeover laws. Table 2 lists the five most prominent types of these laws and the dates each law became effective.[4] In addition to business combination laws, these include control share acquisition, fair price, poison pill, and constituency laws. The Appendix contains descriptions of each of these laws.

All antitakeover laws have the potential to increase a firm's protection from unsolicited takeover bids. However, many researchers assume that only business combination laws offer a meaningful increase in takeover protection. For example, Francis, Hasan, John, and Wasiman (2010, p. 128) observe that "… Business Combination laws … are considered in the extant literature as the most stringent state antitakeover laws." Amore and Bennedsen (2013, p. 6) claim that, "[B]usiness combination laws were the most stringent…" of all state antitakeover laws. Atanassov (2013, p.1101) concurs: "Academics and practitioners alike argue that, among different types of antitakeover laws, Business Combination laws increase managerial entrenchment the most."

These claims help to justify research designs that ignore the other types of antitakeover laws. The claims echo, and appear to be based on, Bertrand and Mullainathan's (2003, p. 1047) assertion that "The most stringent of the second- and third-generation laws were known as business combination laws, which will be the focus of our study … [the] other laws are thought to be, at best, marginally effective." To support their claim, Bertrand and Mullainathan (2003) cite an event study result in Karpoff and Malatesta (1989) in which news of a business combination law is associated with a larger stock price decline than control share acquisition or fair price laws. Bertrand and Mullainathan's argument has since become the foundation of the now-popular focus

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[4] State antitakeover laws passed since 1982 typically are referred to as second-generation laws, since they were designed to avoid the constitutional objections to the 38 first-generation laws that were effectively invalidated by the 1982 *MITE* decision. Some researchers refer to antitakeover laws passed after the 1987 U.S. Supreme Court decision in *CTS Corp. v. Dynamics Corp. of America* as third-generation laws. Most third generation laws have characteristics that are similar to one or more of the second-generation laws, so in this paper we do not distinguish between second- and third-generation laws. Also, we do not include laws that have been declared unconstitutional or have been adopted by only a few states. For example, Maine, Pennsylvania, Utah, and South Dakota have, or have had, "cash-out" laws that require a bidder in a partial acquisition to acquire all shares at a price set according to rules that are similar to the price established in fair price laws. See Cain, McKeon, and Solomon (2014) for a description of several of these less common laws.

on business combination laws and the practice of ignoring the takeover protection afforded by other types of antitakeover laws.

This argument, however, has three drawbacks. First, it is difficult to build an *a priori* case that business combination laws offer uniquely strong takeover protection while the other laws offer negligible protection. Second, the available empirical evidence does not support the argument that business combination laws are the most restrictive. Poison pill laws, for example, are associated with larger price drops among the affected firms. And third, a researcher who relies on event studies to justify the use of business combination laws faces an even more troublesome problem, as most of these studies indicate that firm-level defenses confound identification strategies that rely on antitakeover laws. The following sections describe each of these concerns in turn.

III.B.1. Are business combination laws theoretically the most stringent?

On a purely ex ante basis, it is not evident which type of takeover law offers the greatest protection from unsolicited takeovers.[5] Poison pill laws, for example, explicitly authorize firms to adopt poison pill takeover defenses. Coates (2000), Daines (2001), Daines and Klausner (2001), and Bebchuk, Coates, and Subramanian (2002) argue that poison pills are particularly effective takeover deterrents, especially when they are combined with classified boards. Cremers and Ferrell (2014) conclude that poison pills are the most effective of all takeover defenses in Gompers, Ishii, and Metrick's (2003) G-index. A 1985 Delaware Supreme Court decision validates Delaware firms' use of poison pills. But in many other states, poison pills have had ambiguous legal status until passage of a poison pill law. If we accept the argument that poison pills are particularly effective takeover deterrents, an *a priori* case can be made that poison pill laws are the most stringent type of antitakeover law.[6]

Alternately, directors' duties laws may provide the greatest takeover protection because they direct corporate directors to consider the interests of all firm stakeholders, including employees and local communities, in addition to shareholders' interests. The effect of this directive is to provide legal protection to board decisions that do not maximize shareholder value. For example, a board covered by a directors' duties law can reject a tender offer that is attractive to shareholders by claiming that the change in control would hurt

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[5] See Romano (1992) for an early comparison of the provisions of different types of state antitakeover laws.
[6] As discussed in Section IV, the Delaware Supreme Court upheld the use of a poison pill in 1985 in *Moran v. Household International, Inc.*, Delaware Supreme Court, 500 A.2d 1346. Pills in other jurisdictions, however, have been declared illegal (see Lipton and Steinberger, 2003; Catan and Kahan, 2014, p. 7). For example, a New Jersey corporation with its principal place of business in New York had its use of a poison pill declared illegal in *Amalgamated Sugar Co. v. NL Industries*, 644 F. Supp. 1229 (S.D.N.Y. 1986). New Jersey and New York both subsequently adopted poison pill laws.

employees or the local community. Such a broad directive to consider many constituents provides legal cover for boards who wish to "just say no" to unsolicited takeover bids (see Del Guercio, Seery, and Woidtke, 2008).

The provisions of control share acquisition laws provide another illustration of the weak theoretical foundation for the claim that business combination laws are the "most stringent." A control share acquisition law strips a bidder's voting rights unless a majority of other shareholders vote to restore such rights. Incumbent managers' voting rights, in contrast, are not affected. This requires a bidder to obtain supermajority support from disinterested shares to complete an acquisition. This increases the risk of failure and likely deters many unsolicited bids in the first place.

A business combination law, by comparison, imposes no hurdles on the bidder's acquisition of shares. Rather, it imposes a delay in the bidder's ability to sell or spin off the target firm's assets. This forced delay can increase the bidder's costs, especially if the proceeds of an asset sale are required to finance the acquisition. But, on an *a priori* basis, it is not evident that a forced delay provides more takeover protection than poison pill authorizations, the expansion of directors' duties to include non-investor stakeholders, or control share acquisition requirements.[7]

III.B.2. Event study evidence

The prior section argues that there is little theoretical justification for focusing only on business combination laws to the exclusion of other types of antitakeover laws. But might there be an empirical justification? Bertrand and Mullainathan (2003, footnote 8) argue that the empirical evidence supports such an exclusive focus: "Event study evidence has borne out this belief [that business combination laws are the most stringent], showing that business combination laws resulted in the biggest stock price drop (Karpoff and Malatesta 1989)." Consistent with this argument, Table 5 in the Karpoff and Malatesta (1989) paper shows that the mean two-day forecast error upon the revelation of a business combination law is -0.467%, with a z-

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[7] Anecdotal arguments swing both ways on whether one law is "more stringent" than another. For example, Alva (1990) argues that control share acquisition laws are weak deterrents because arbitrageurs can buy the target firm's shares and vote in favor of a previously proposed bid. (Such a scenario is explicitly prohibited by some control share acquisition laws, which strip voting rights from all shares acquired after the bidder acquires its control shares.) A counter-argument is that control share acquisition laws are particularly stringent because their provisions have never been triggered. The provisions of New York's business combination law, in contrast, were triggered in 1987 when the Bank of New York acquired Irving Bank Corporation. Commenting on the restrictions imposed by the law, a Bank of New York official stated that, "we could not achieve the same economies as in a true merger … It ties our hands to some extent." See Eric N. Berg, "Threat of Hostile Irving Bid," The New York Times, October 7, 1987, accessed on August 21, 2014 at http://www.nytimes.com/1987/10/07/business/threat-of-a-hostile-irving-bid.html. The fact that more firms have opted out of control share acquisition laws than business combination laws (reported in Section V.B. below) suggests that managers and investors consider control share acquisition laws to be more important.

statistic of -2.697, for firms incorporated in the state of adoption. The mean forecast errors for control share acquisition and fair price laws, while negative, are not statistically significant.

At first glance, these findings appear to support the claim that business combination laws are uniquely effective antitakeover laws. There are three problems, however, with this argument. First, the broader evidence is mixed on whether business combination laws are associated with negative stock price reactions. Mahla (1991) and Giroud and Mueller (2010) report statistically significant results, similar to those in Karpoff and Malatesta (1989). But Broner (1987), Pugh and Jahera (1990), and Jahera and Pugh (1991) report statistically insignificant stock price reactions to business combination laws. Second, the average stock price reactions to business combination laws, even in studies in which they are statistically significant, are not significantly different from the stock price reactions to control share acquisition or fair price laws (e.g., see Table 3 in Karpoff and Malatesta, 1989). So any claim that business combination laws are the "most stringent" must be based on point estimates without regard for standard errors of the estimates.

Third, point estimates of the average stock price reactions to different types of antitakeover laws indicate that business combination laws are not the "most stringent." Rather, that distinction goes to poison pill laws. In the same paper that examines the stock price reaction to business combination laws, Karpoff and Malatesta (1989) report that the average stock price reaction for firms that are newly affected by poison pill laws is 2.4 times larger in magnitude (−1.125%), and also statistically significant at the 5% level. Similarly, Ryngaert and Netter (1988) report a mean forecast error of −1.94% (t = −2.11) in the three-day window surrounding the first media report of the Ohio poison pill law. If we were to use event study results to identify the "most stringent" antitakeover law, researchers should use poison pill laws, and not business combination laws, to identify their empirical tests. Consistent with such an argument, in our empirical tests in Section VII we find that poison pill laws are negatively and significantly related to firms' operating performance and innovative activity (among other outcome variables), while the coefficient on business combination laws is small and insignificant.

III.B.3. Effects of other state antitakeover laws on test identification

In summary, there is no solid theoretical or empirical basis for the assertion that "business combination laws [are] the most stringent…" or for the notion that the effects of other types of takeover laws are, by comparison, negligible. It therefore is reasonable to infer that the effect of a business combination law on covered firms' takeover protection most likely depends on the presence of other types of antitakeover laws.

Figure 3 uses the laws adopted in Massachusetts to illustrate the effect of other antitakeover laws on tests that rely on business combination laws for identification. Before its business combination law was

adopted on July 18, 1989, Massachusetts adopted a control share acquisition law on July 22, 1987. In Figure 3, this is represented as an increase in the level of takeover protection for covered firms. It is plausible that the 1989 business combination law provided additional takeover protection over and above the pre-existing 1987 law. Reflecting this possibility, Figure 3 is drawn such that the passage of both the control share acquisition law and business combination law increased Massachusetts' firms' takeover protection. A correct specification would account for the increased takeover protection afforded by each of these laws. Note that it is easy to exploit the additional identification afforded by multiple laws' takeover protections by using the data on states' deployment of all major types of antitakeover laws that are summarized in Table 2.

Table 2 reveals that it is typical for states that adopt business combination laws to have preexisting antitakeover laws, or to adopt other types of antitakeover laws at the same time they adopt business combination laws. Only two of the 36 states with business combination laws – Delaware and Texas – passed their business combination laws on a stand-alone basis and without at least one other antitakeover law already in place. Of the 4,141 firms in the RiskMetrics Governance database, 20.7% are incorporated in states in which a business combination law was preceded by at least one other second-generation law. An additional 25.2% of firms are incorporated in states in which business combination laws were adopted in the same legislative bill as at least one other type of second-generation antitakeover law.[8]

III.C. Firm-level defenses

Relying on differences in stock price reactions to identify which type of takeover law is "most stringent" reveals an even more important concern about identification. In tables 3 and 4 of their paper, Karpoff and Malatesta (1989) report that state antitakeover laws are associated with statistically significant stock price effects only among firms that do not have preexisting firm-level takeover defenses. For firms with preexisting takeover defenses, the mean stock price reaction is near zero and statistically insignificant. This result, which has been supported in several subsequent studies, implies that firm-level defenses are substitutes for state antitakeover laws in their provision of antitakeover protection.[9]

Figure 4 illustrates the effect of firm-level defenses on takeover protection for one Massachusetts firm, Cullinane Database Systems, Inc. Cullinane adopted a supermajority vote requirement on July 26, 1982. If

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[8] Because approximately half of all exchange-listed firms are incorporated in Delaware, and because Delaware is one of only two states without other potentially confounding antitakeover laws, the potentially confounding effects of other antitakeover laws affects fewer than half of all firms in the RiskMetrics database. In most empirical studies that use business combination laws to identify tests, however, identification comes from time series and cross-sectional variation in firms' coverage by business combination laws. The Delaware business combination law is only one such time-varying event, even though it affects many firms.

[9] For corroborating evidence, see Szewczyk and Tsetsekos (1992), Karpoff and Malatesta (1995), Fields and Todd (1995), Pugh and Jahera (1995), and Alexander, Spivey, and Marr (1997).

firm-level defenses are substitutes for state antitakeover law protection, the 1989 adoption of Massachusetts's business combination law had little or no impact on the level of Cullinane's takeover protection. Rather, the increase in takeover protection occurred in 1982, when Cullinane adopted its supermajority vote requirement. Stated differently, if we want to rely on the event study evidence to support a focus on any single type of antitakeover law (e.g., business combination or poison pill laws), we must also accept the event study evidence that such laws increase takeover protection only for firms without prior firm-level defenses.

As reported by Gompers, Ishii, and Metrick (2003) and others, many firms deploy takeover defenses at the firm level. Karpoff and Malatesta (1989) report that 24.5% of firms in their sample through 1987 had at least one takeover defense when their states adopted antitakeover laws. To obtain a lower-bound estimate of the number of firms that had firm-level defenses when their states adopted business combination laws, we combine Cremers and Farrell's (2014) data on takeover defenses from 1977-1989 with the RiskMetrics Governance database for the 1990-2012 period. A total of 611 (14.75%) of the 4,141 firms in the RiskMetrics Governance database had at least one firm-level defense in place when the firm's state of incorporation adopted a business combination law. This fraction is a lower-bound estimate because the Cremers and Farrell (2014) data for the $1977 - 1989$ period cover only 1,294 firms, not all 4,141 firms in the RiskMetrics database.[10] This implies that Figure 4 illustrates the general pattern of takeover protection for a significant number of firms.

III.D. Low power tests

A defender of an empirical approach that relies exclusively on business combination laws might argue that the presence of other types of state antitakeover laws (Figure 3) or firm-level defenses (Figure 4) merely works to bias the experiment against finding significant results. If the test yields significant results, does that not indicate that the results are robust?

The existence of first-generation antitakeover laws constitutes the most obvious concern about such an argument, at least for tests that include data from before 1983. In such cases, the periods after business combination laws are passed are more accurately characterized as periods of moderate to low takeover protections, not high takeover protections.

Even for tests that exclude pre-1983 observations, however, low-power tests remain a concern, for three reasons. First, low-power tests may fail to uncover genuine patterns in the data. Thus, researchers may find insignificant results even when there is an underlying relation between governance and a firm

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[10] Cremers and Farrell (2014) report that they attempt to collect data on 1,483 firms for the 1977-1989 period. The data provided to us, however, contain G-index information for 1,294 firms.

characteristic. Second, as Burgstahler (1987) points out, low-power tests are relatively uninformative even when the test statistics are significant. And third, tests that ignore other state antitakeover laws and firm-level defenses cannot discern whether any changes in firm characteristics are due to the business combination law or to such other differences as different firm-level defenses. Danielson and Karpoff (1998) show that the use of firm-level defenses frequently clusters with coverage by state antitakeover laws. Many states that adopt business combination laws also adopt other types of antitakeover laws, indicating that coverage by omitted state laws also is correlated with business combination law coverage. So, whether the correlated omitted variable is coverage by another type of state antitakeover law or deployment of a firm-level defense, tests that rely solely on business combination law coverage suffer an omitted variable problem that can lead to biased estimators (e.g., see Roberts and Whited, 2012).

IV. Court decisions that affect firms' takeover protection

In addition to other state antitakeover laws and firm-level defenses, the incremental impact of an antitakeover law depends on the legal environment. We identify four court rulings that had large effects on the takeover protections offered by antitakeover laws. The Appendix and Internet Appendix also report on the effects of several additional court decisions in our empirical replication tests.

IV.A. Edgar v. MITE Corp. (1982)

Between 1978 and 1982, at least 32 lower court decisions issued decisions on the constitutionality of various states' first-generation laws. These decisions were mixed, with 17 ruling that some aspect of a first-generation law was unconstitutional and 15 upholding the laws.[11] The matter was settled on June 23, 1982, when the U.S. Supreme Court ruled in *Edgar v. MITE* that the Illinois first-generation antitakeover law was unconstitutional on the grounds it imposed an indirect burden on interstate commerce.[12] The ruling effectively invalidated all other 37 first-generation laws as well. As illustrated in Figure 2, the *MITE* decision had the effect of immediately decreasing the level of takeover protection for firms that previously were covered by a first-generation law. In our empirical tests, we incorporate the impact of the MITE decision in the coding of

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[11] For summaries of the lower court decisions, see Snipes (1983), Van Nuis (1983), Rainey (1983), and Brody (1984).

[12] For the Court ruling, see http://supreme.justia.com/cases/federal/us/457/624/case.html. Reflecting widespread legal disagreement and uncertainty when the case was brought, the Supreme Court Justices issued a total of six opinions in the MITE case. The case turned on whether the Illinois law violated the Supremacy and Commerce clauses of the U.S. Constitution. Only three of the nine justices agreed that the Illinois law was preempted by federal law, and only four justices found that the law directly violated the commerce clause. However, a fifth justice agreed that the law indirectly violated the commerce clause; thus the Court ruled the Illinois statute unconstitutional on the grounds that it imposed an indirect burden on interstate commerce.

our variable for coverage by a first generation law. This variable takes a value of 1 when a state adopts a first generation law and is flipped back to zero for all firms in all states in 1982.

IV.B. CTS Corp. v. Dynamics Corp. of America (1987)

After the *Edgar v. MITE* decision in 1982, state legislatures took effort to craft new antitakeover laws that met the constitutional objections to the first-generation laws. Litigation involving the legality of one of these second-generation laws reached the U.S. Supreme Court in a case involving Indiana's control share acquisition law, in *CTS Corp. v. Dynamics Corp. of America*.[13] Both the District Court and Court of Appeals ruled that the federal Williams Act preempted the Indiana law. But on April 21, 1987, the U.S. Supreme Court overturned these prior rulings and ruled that the Indiana law is constitutional. The Court rejected the claim that the Indiana law was preempted by federal law and concluded that the Indiana law actually furthered the goal of the Williams Act to protect shareholders because it allows shareholders to evaluate the fairness of a tender offer collectively. Further, the court rejected the argument that the law unduly burdened interstate commerce. Until the *CTS* decision, the level of takeover protection afforded by control share acquisition laws was uncertain and ambiguous.

IV.C.   Amanda Acquisition Corp. v. Universal Foods Corp. (1989)

While *CTS* affirmed the constitutionality of control share acquisition laws, the constitutionality of business combination laws was not established until a ruling by the U.S. Court of Appeals, Seventh Circuit in *Amanda Acquisition Corp. v. Universal Foods Corp.* on May 24, 1989.[14] In our empirical applications in Section VII, we find that business combination laws by themselves are not associated with meaningful changes in most outcome variables. In three cases, however, the interaction of coverage by a business combination law with the *Amanda* decision is significant, implying that the important identifying event is not the prior passage of a business combination law, but rather, the first appellate-level court decision that upheld a business combination law.

IV.D.   Moran v. Household International, Inc. (1985)

A fourth important court decision affirmed the legality of poison pills for Delaware firms. On November 19, 1985, the Delaware Supreme Court ruled in *Moran v. Household International, Inc.* that

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[13] *CTS Corp. v. Dynamics Corp. of America,* 481 U.S. 69 (1987). For a more detailed discussion of the *CTS* decision, as well as its history in the lower courts, see Choate (1987).

[14] *Amanda Acquisition Corp. v. Universal Foods Corp.,* 877 F.2d 496 (1989). For more information on the *Amanda* decision, see Cuomo (1989).

Household International's adoption of a poison pill was a legitimate exercise of its managers' business judgment.[15] Delaware Court decisions frequently apply *de facto* even to non-Delaware corporations. In Section VII.C we examine the effects on our empirical inferences if we assume that the *Moran* case validated the use of poison pills for non-Delaware firms. For our main tests, however, we assume that the *Moran* ruling upheld Delaware firms' rights to deploy poison pills and that the legal status of poison pills remained unclear for firms incorporated in other states. Consistent with this assumption, Lipton and Steinberger (2003, p. 6-76) and Catan and Kahan (2014, p. 7) cite court decisions in which poison pills were invalidated for companies from New York, New Jersey, Georgia, Wisconsin, Colorado, and Virginia. Each of these states passed a law to establish firms' rights to deploy poison pills soon after its related court decision. In total, 29 states have adopted poison pill laws. In our empirical tests in Section VII, we treat these poison pill laws as similar to the Delaware Court's decision in the *Moran* case. In total, firms in 30 states have explicit legal protection to adopt and use poison pills, either via statute or, in the case of Delaware firms, via the *Moran* decision.

V. Political economy and selection issues

Sections III and IV discuss how other state antitakeover laws, firm-level defenses, and court decisions can affect the use of business combination laws to identify empirical tests. Each of these is essentially a problem of omitted variables, and the data in Tables 1 and 2, and in the RiskMetrics database, can help to address these problems. In this section we discuss an additional concern, whether coverage by a state antitakeover law is exogenous for all firms.

V.A. Motivating firms

A key justification in the use of state antitakeover laws to identify empirical tests is the assumption that they are imposed exogenously on the covered firms. To justify this assumption, Bertrand and Mullainathan (2003) cite Romano's (1987) conclusion that most of these laws did not result from a broad-based coalition of firms incorporated in the state. Werner and Coleman (2014), however, argue that antitakeover laws are strongly affected by corporate lobbying, and Romano (1987) and Bertrand and Mullainathan (2003) both cite examples of laws that were passed at the behest of specific companies. For example, the Connecticut fair price law passed in 1984 after it was promoted by the Aetna Life and Casualty Company, and the 1987 Arizona antitakeover law that combines fair price, control share acquisition, directors' duties, and business combination provisions was promoted (and indeed, written) by Greyhound executives.

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[15] *Moran v. Household International, Inc.,* 500 A.2d 1346 (Del. 1985).

Karpoff and Malatesta (1989) identify 19 different antitakeover laws that were passed at the behest of specific companies through 1989. Using more current data, Gartman (2000) reports on a total of 49 state antitakeover laws in 23 different states were passed in part because they were promoted by at least 46 specific firms. These 49 laws and the associated motivating firms are listed in Table 3. In most cases, the motivating firm was the target of an actual or rumored takeover bid, and in most cases, the state legislature moved quickly to pass antitakeover legislation upon news of the actual or potential bid. For example, Mesa Petroleum announced plans to buy a $15 million stake in The Boeing Company on July 28, 1987. The very next day, new reports indicated that the Washington state legislature would consider antitakeover legislation to protect Boeing from the threat of outside takeover. The legislature met in a special session and adopted Washington's business combination law within two weeks of Mesa's announced toehold investment in Boeing. Similar legislative histories, although not always with the same alacrity, characterize business combination laws adopted in Kentucky (Ashland Oil), Minnesota (Dayton-Hudson), New Jersey (Schering-Plough), Wisconsin (G. Heilman Brewing), and the other antitakeover laws listed in Table 3.

Clearly, the assumption of exogeneity is inaccurate for these particular firms and laws. For example, Dayton-Hudson's coverage by the Minnesota business combination law is as endogenous as Dayton-Hudson's coverage by its own poison pill. The forces that give rise to both types of takeover protection reflect Dayton-Hudson's unique agency costs, governance, and performance characteristics. This implies that the assumption of exogeneity is violated for firms that helped to motivate specific antitakeover laws. In Section VII we use a dummy variable for motivating firms to isolate the effects of business combination laws on firms that lobbied for these laws compared with firms for which the laws are more plausibly exogenous. The results indicate that controlling for such endogeneity has material effects on empirical inferences.

V.B. Firms that opt out of coverage

The exogeneity assumption also is violated for many firms because most antitakeover laws include opt-out provisions. For example, Illinois corporations can opt out of coverage by the Illinois business combination law through a charter amendment or bylaw that is supported by a majority of the shares not held by an outside bidder. The option to decline coverage by a law's provision indicates that, to some extent, a firm's coverage is endogenous. An extreme view is that the existence of opt-out provisions completely undermines the exogeneity assumption that justifies the use of state antitakeover laws to identify empirical tests.

A middle ground, however, is that state law coverage is exogenous to some extent. This is because transaction costs impede many firms' management teams from adjusting quickly to changes in state law. For

such firms, the law's passage and coverage are exogenous. If the cost of adjustment is sufficiently high, these firms will not adjust their takeover defenses (e.g., by opting out of the law's coverage) to completely offset the incremental takeover protection afforded by the law.

In support of this middle view, most state laws impose explicitly high adjustment costs. To opt out of the Ohio business combination law, for example, requires a two-thirds majority of the outstanding shares and two-thirds of the outstanding shares not owned by a 10% stockholder. Even then, the opt-out would not take effect for 12 months and would never apply to a control transaction with a shareholder who owned 10% before the opt-out amendment passed. Of course, firms' adjustment costs are not infinite, and firms covered by state antitakeover laws eventually can opt out of coverage or adjust their firm-level defenses in response to the imposition of a state law. This implies that the exogeneity of any increase in takeover protection afforded by a state antitakeover law is likely to decay over time.

Empirical researchers can improve their tests by recognizing that many firms do, in fact, opt out of their state laws' coverage. The RiskMetrics Governance database indicates that 445 (10.75%) of the 4,141 firms represented in the database opted out of coverage by at least one state antitakeover law for at least one year during the 1990–2012 period. These include 320 firms that opted out of control share acquisition laws, 158 firms that opted out of coverage by business combination laws, 72 firms that opted out of fair price laws, 37 firms that opted out of directors' duties laws, and three firms that opted out of poison pill laws. In our tests below, we code these firm-years as not subject to the provisions of the applicable state antitakeover law.[16]

V.C. State of incorporation decisions

An additional reason coverage by a state takeover law may be endogenous is that firms choose their states of incorporation and can reincorporate to states that offer takeover defenses that best suit their needs. Catan and Kahan (2014) argue that the option to reincorporate implies that state takeover law coverage is not exogenous for many, if not most, firms. For three reasons, however, we do not focus on this source of endogeneity in our replication tests. First, the decision to reincorporate can be costly and take time, so any incremental takeover protection offered by a state takeover law will have at least a short term effect. Second, virtually all state takeover laws have opt out provisions, as discussed in Section V.B. Firms are likely to find it

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[16] Further complications arise because many laws contain idiosyncratic yet important features. For example, the Georgia business combination and fair price laws, as well as the Tennessee control share acquisition law, require firms to opt *into* coverage. A researcher using business combination laws in an empirical test therefore should not code firms incorporated in Georgia as experiencing an increase in takeover protection when the Georgia business combination law was adopted, as only the firms that opt into coverage are afforded this extra protection. The RiskMetrics Governance database tracks firms that opt into such laws, but the fact that firms must opt into coverage indicates that such coverage is not exogenous.

less costly to opt out of coverage than to reincorporate to a different state, and our tests control for firms that opt out of coverage by an antitakeover law. Third, the evidence does not indicate that a large number of firms reincorporate to avoid or to seek coverage by state antitakeover laws. Cain et al. (2014), for example, report that the large majority of reincorporations in their sample are to the firm's headquarter state, Delaware, or Nevada. Few reincorporations are to states such as Pennsylvania, which Gartman (2000), Szewcyzk and Tsetsekos (1992) and others argue has the most manager-friendly takeover environment. Nonetheless, the fact that firms have the option to reincorporate underscores one result from our empirical replications, which show that the relations between business combination laws and the outcome variables frequently are different among firms for which the laws are clearly not exogenous than for the overall sample of firms.

VI. Institutional and political economy concerns as an omitted variables problem

The estimation challenge raised by multiple types of state takeover laws and firm-level defenses can be viewed as an omitted variables problem. The political economy of the state laws, including the fact that some firms lobbied for the laws while others opt out of coverage, raise endogeneity and selection issues. As such, the statistical issues raised by each of these issues are well understood. Meyer (1995), for example, lists omitted variables, political economy considerations, and selection as three threats to the internal validity of empirical tests that seek to draw inferences about causation.

In Section VII below, we present results from several tests that illustrate the potential size and direction of the omitted variables bias (OVB) in empirical tests. In general, tests that rely on business combination laws are of the form:

$$y_{ijlst} = \alpha + \beta BC_{st} + \theta' X_{it} + \varepsilon_{ijlst} \tag{1}$$

where y is the outcome variable of interest for firm i in year t, where firm i is in industry j, located in state l, and incorporated in state s, and X is a vector of time-varying firm characteristics such as size or age and may include firm, year, or industry fixed effects. Following Angrist and Pischke (2009, p. 60), we call this the "short regression." Our argument is that other state laws, court cases, and firm-level defenses also affect a firm's takeover likelihood and governance, so the correctly specified empirical model is:

$$y_{ijlst} = \alpha + \beta BC_{st} + \theta' X_{it} + \delta' L_{ist} + \varepsilon_{ijlst} \tag{2}$$

where L is a vector of other state takeover laws, court cases, and firm-level defenses, to which we collectively refer as our institutional controls. Angrist and Pischke (2009) show that the omitted variable bias (OVB) from estimating equation (1) instead of equation (2) is:

$$OVB(L) = \delta' \gamma_{BC} \qquad (3)$$

where γ_{BC} is a vector of coefficients from regressions of the elements of L on BC. Thus, the omitted variable bias increases with the correlation between firms' coverage by business combination laws and the institutional controls (state laws, court cases, and firm-level defenses) and with the effect of these controls on the outcome variable y. Stated differently, the bias in the estimated coefficient from estimating equation (1) reflects the tendency for business combination laws to "steal" the explanatory power of the institutional control variables in explaining variation in the outcome variable y.

There are numerous empirical procedures to address the endogeneity of state law coverage for at least some firms, including deleting politically influential firms from the sample or modeling the selection process by which these firms become influential (see Roberts and Whited, 2012). The literature also identifies various measures of firms' political influence, including lobbying expenditures (e.g., Yu and Yu, 2011), campaign contributions (Hill et al., 2013), and federal contract revenues (Karpoff, Lee, and Vendrzyk, 1999). In our replication tests, we identify influential firms using data from Table 3 that identify firms that publicly lobbied for their takeover laws. These data have an advantage over lobbying expenditures or campaign contributions because they directly identify the firms that influenced the takeover legislation. We treat the fact that the laws are not exogenous for these motivating firms as an omitted variable by expanding the model in equation (2):

$$y_{ijlst} = \alpha + \beta BC_{st} + \theta X_{it} + \delta L_{ist} + \lambda BC_{st} \times MF_i + \varepsilon_{ijlst} \qquad (4)$$

In Eq. (4), $MF_i = 1$ for firms that motivated the law's adoption. (We do not include a separate control for MF_i because our replication tests include firm fixed effects.) The omitted variable bias from excluding a control for such firms is

$$OVB(MF) = \delta' \gamma_{BC \times MF} \qquad (5)$$

where $\gamma_{BC \times MF}$ is a vector of coefficients from regressions of the elements of $BC \times MF$ on BC. By construction, $\gamma_{BC \times MF} > 0$. This is admittedly a simplistic treatment for endogeneity, but it allows us to show in the empirical tests of Eq. (4) that, for many outcome variables, $\beta \neq \lambda$, i.e., the marginal effect of a business combination law differs between motivating and non-motivating firms. This implies that the omitted variable bias *OVB(MF)* is both material and can be positive or negative depending on the outcome variable.

VII. The importance of institutional and political economy considerations in empirical tests

In this section we report on three types of tests that demonstrate the empirical importance of our institutional and political economy considerations when using business combination laws to identify exogenous changes in firm governance. First, we contrast estimates from a generic specification of the short regression (Eq. 1) with estimates from the full model (Eq. 4) using seven different outcome variables that have been examined in previous studies. Second, we exactly replicate one specific study (Atanassov, 2013) and demonstrate the impact of our political economy variables on Atanassov's two main outcome variables. Third, we estimate the short regression (Eq. 1) using data simulated from a version of the full model (Eq. 4) to measure the frequency with the *BC* coefficient in the short regression appears to be statistically significant even when, by construction, business combination laws have no impact on firm governance.

VII.A. Empirical replications

The section examines the importance of our institutional controls in empirical tests using seven different outcome variables examined in eight prior studies: return on assets (ROA), capital expenditures (Capex); property, plant, and equipment (PPE) growth; asset growth; cash holdings; selling, general & administrative (SGA) expense, and leverage.[17] For each outcome variable, we compare the results from estimating the short regression (Eq. 1) with the results from the full model (Eq. 4). To match the sample period in Bertrand and Mullainathan (2003) and Giroud and Mueller (2010), we use annual Compustat data from 1976–1995 and exclude financial firms and utilities. All regressions include the control variables used by Giroud and Mueller (2010), which include the natural log of the book value of assets (size), size squared, firm age, and firm age squared. We winsorize all continuous control and outcome variables at the 0.5% and 99.5% levels.

Many firm-specific governance characteristics, including ownership concentration, board structure, and compensation policies, could affect the outcome variables we examine. To control for firm-specific and unobserved sources of heterogenous variation, we use firm, state (of location)-year, and industry-year fixed effects in all tests in Tables 4 and 5. Catan and Kahan (2014) show that some prior business combination law studies are sensitive to time- and firm-specific trends, and Gormley and Matsa (2015) show that firm, state-year, and industry-year fixed effects control for industry and state-related time trends that simple state,

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[17] Bertrand and Mullainathan (2003), Giroud and Mueller (2010), and Gormley and Matsa (2015) examine the impact of business combination laws on ROA (or return on capital); Bertrand and Mullainathan (2003), Giroud and Mueller (2010), and Francis, Hasan, and Song (2009) examine impacts on Capex; Giroud and Mueller (200) examine the impact on PPE and asset growth, as well as SGA expense; Yun (2009) and Gormley and Matsa (2015) examine the impact on cash holdings; and Garvey and Hanka (1999), Wald and Long (2007), Yun (2009), and John and Litov (2010) examine the impact on leverage.

industry, or year fixed effects do not pick up. Note that our state-year fixed effects are based on the location of the firm's headquarters, whereas coverage by a state antitakeover law is determined by the firm's state of incorporation.

Table 4 reports the results of tests in which the vector *L* in Eq. (4) includes first generation laws and the four other most common types of second generation laws (poison pill laws, control share acquisition laws, director duties laws, and fair price laws), plus two time-related dummy variables that reflect the resolution of uncertainty about the legal status of second generation laws. *CS x CTS* equals one for all firms covered by control share acquisition laws for observations beginning in 1987, when the U.S. Supreme Court upheld Indiana's control share acquisition law, and *BC x Amanda* equals one for all firms covered by business combination laws after the U.S. Seventh Circuit Court of Appeals upheld Wisconsin's business combination law in 1989. Finally, *BC x MF* is the interaction of business combination law coverage with a dummy variable set equal to one for firms that helped to motivate the business combination laws, as identified in Table 3. There are a total of 25 such motivating firms, affiliated with 356 firm-years in the sample. Again, we do not include a separate control for *MF* because the tests already include firm fixed effects.

Model (1a) reports the results from the short regression (Eq. 1) in which ROA is the dependent variable. The coefficient on the business combination law dummy variable is -0.017 and is significant at the 10% level. This result is consistent with the results in Bertrand and Mullainathan (2003) and Giroud and Mueller's (2010) baseline finding (without an industry concentration interaction), who infer that business combination law coverage is associated with a decrease in firm operating performance.

Model (1b) reports the ROA results using the full model (Eq. 4). Here, the coefficient on *BC* is smaller and statistically insignificant. In contrast, ROA is negatively and significantly related to coverage by a first generation state law, poison pill law, and control share acquisition law, as well as coverage by a business combination law after the 1989 *Amanda* court decision. These results do not necessarily refute the inference that ROA is negatively related to coverage by antitakeover laws, but they indicate that business combination laws provide poor identification for tests that seek exogenous changes in a firm's takeover vulnerability. In unreported tests, we confirm that the omitted variable bias in Model 1 is negative because coverage by a business combination law is positively related to three variables (poison pill law, control share acquisition law, and coverage by a business combination law after the 1989 *Amanda* court decision) that are negatively related to ROA.

In Model (1b), the coefficient for *BC x MF* is positive and significant at the 10% level. This result is consistent with the view that business combination laws are optimal for the firms that lobbied for the laws, or at least that the laws' effects are different for the motivating firms compared with other firms.

Models 2–7 in Table 4 report on similar tests for the six other outcome variables. In the short regression tests, business combination law coverage is significantly related to all of these variables. In three cases (Capex, PPE growth, and Cash holdings), however, the business combination coefficient is insignificant in the full model tests. Overall, the omitted variable bias from estimating the short equation instead of the full model leads to a fundamentally different inference about the effect of business combination laws for four of the seven outcome variables examined in Table 4.

The coefficients for poison pill laws and *BC x MF* each are significantly related to five of the seven outcome variables in Table 4. The poison pill law results are consistent with our previous observation that poison pill laws are associated with a larger average share price reaction than business combination laws. These results indicate that researchers seeking measures of exogenous variation in takeover vulnerability should consider poison pill laws. In four of the five tests in which *BC x MF* is significant, its coefficient has the opposite sign as *BC* in the corresponding short equation. For example, Giroud and Mueller (2010) argue that an increase in takeover protection will lead to entrenched managers, less efficient deployment of resources, and an increase in SG&A expense. This interpretation is consistent with the positive coefficients in Model 6b for business combination and Poison pill laws. The coefficient for *BC x MF*, in contrast, is negative and significant, indicating that firms that lobbied for their business combination laws experienced a decrease in SG&A expenses. These results indicate that the effects of an increase in state-level takeover protection frequently are different for motivating firms than for other firms.

To examine the impact of firm-level defenses, we combine data from the RiskMetrics database, which reports on firm-level defenses for S&P 1500 firms dating back to 1990, with data on firms' takeover defenses dating back to 1978 that were kindly provided to us by Martijn Cremers and Allen Ferrell (2014). These sources indicate that 2,045 out of the 10,213 firms in the Compustat database over the 1976-1995 period had one or more firm-level takeover defenses. Table 5 reports the results from the short regression (Eq. 1) for each of the seven outcome variables using only these 2,045 firms. This approach is similar to the empirical approach used by Karpoff and Malatesta (1989) and Szewczyk and Tsetsekos (1992) to examine the valuation effects of state takeover laws among firms with preexisting firm-level defenses.

In Table 5, the *BC* coefficient is statistically insignificant in all seven regressions. This differs from the results using the short regression (Eq. 1) in Table 4. All seven outcome variables, are significantly related to *BC* in the short regression when we include firms without firm-level defenses. Among firms with firm-level defenses, however, this result disappears even without considering our other institutional and political economy variables. These results are consistent with the situation characterized by Figure 4, in which the incremental impact of a state takeover law is negligible among firms that already have firm-level defenses.

VII.B. An application to Atanassov's (2013) investigation of corporate innovation

The results in Tables 4 and 5 illustrate the importance of our political economy concerns in a generic test setting that uses a sampling interval and control variables that are similar to those in Giroud and Mueller (2010). We do not claim, however, to have exactly replicated any prior tests. Our tests regarding ROA, for example, are similar in nature to those in Bertrand and Mullainathan (2003), but are not exact replications because we do not have the U.S. census and plant-level data that were available to Bertrand and Mullainathan. In this section, we demonstrate the importance of our institutional and political economy concerns in a setting in which we can exactly replicate a previous empirical result because Julian Atanassov graciously provided us with his data for his 2013 *Journal of Finance* paper on corporate innovation.

Atanassov (2013) finds that antitakeover laws, particularly business combination laws, are associated with a decrease in both the number of patents filed by the affected firms and the citations to these new patents. Atanassov infers that the extra takeover protection afforded by business combination laws is associated with a decrease in both the quantity and quality of these firms' innovative activity. He concludes that antitakeover laws exacerbate the managerial agency problem as managers respond to an increase in takeover protection by pursuing activities that are less challenging and less valuable than new innovation. Atanassov's results thus offer an important contribution to the debate over whether takeover protections facilitate or impede innovative activity.[18]

Panel A of Table 6 reproduces Atanassov's main result regarding new patent activity, as reported in Table 3 of his paper. The dependent variable, $Ln(1+Pat)_{t+3}$, is the natural log of the number of new patents filed by a firm in a given year divided by the mean number of patents for the same year. Atanassov (2013) uses the number of patents filed three years in the future to capture the time lag between investments in innovation and the resulting patents. As in Tables 4 and 5, the key variable of interest is *BC*, an indicator variable set equal to one if a business combination law is in place in the firm's state of incorporation in a given year. Control variables, which are not reported in our table, include firm sales, leverage, age, profitability, asset tangibility, and industry concentration. As reported in Model 1 and by Atanassov (2013), the coefficient on coverage by a business combination law is -0.014 and is statistically significant at the 1% level.

For our replication tests we use a subset of 86,521 firm-years in Atanassov's sample for which we can identify states of incorporation using Compustat. As reported in Model 2, the *BC* coefficient falls in magnitude

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[18] See also Stein (1988), Holmstrom (1989), Meulbroek, Mitchell, Mulherin, Netter, and Pousen (1990), Francis, Hasan, Song (2009), Manso (2011), O'Connor and Rafferty (2011), Chemmanur and Tian (2013), and Sapra, Subramanian, and Subramanian (2014).

to -0.012 for this sample, but still is significant at the 1% level. In Model 3, we make two changes to account for the fact that the laws do not apply to all firms. First, we account for the fact that some firms opted out of business combination law coverage by coding these firm-years as not covered by the business combination law. This coding change affects a total of 36 firms and 143 firm-years out of 86,521 firm-years in the sample. Second, we code Georgia firms as not covered by a business combination law because the Georgia law requires firms to opt into coverage. This affects an additional 115 firms and 1,039 firm-years.[19] Additionally, we correct for six errors in the passage dates for business combination laws.[20] The result of these corrections is that the *BC* coefficient falls in magnitude to -0.009, but remains significant at the 1% level.

Model 4 reports the full model results that include our institutional and political economy control variables. The *BC* coefficient is 0.000 and is statistically insignificant. In contrast, the coefficient for *BC x MF* is -0.297 and is significant at the 5% level. This result indicates that patent activity does decrease after passage of a business combination law when we control for the other effects, but only among the small subset of 25 firms that lobbied for their business combination laws in the first place. This result is similar to several of the results in Table 4, in which the motivating firms experience significant changes around the adoption of business combination laws even when the *BC* coefficient is insignificant.

In Model 4, firms' patent activity is negatively and significantly related to *Poison pill law* and *Amanda x BC*. These results lead to substantially different inferences from the tests in columns 1-3, which focus only on business combination laws. If we maintain the assumption that coverage by business combination laws is a primary driver of a firm's takeover protection, these results contradict Atanassov's (2013) inference that patent activity is related to a firm's takeover protection. It is possible to maintain Atanassov's (2013) inference, but only if we also conclude that business combination laws do not, by themselves, provide substantial incremental takeover protection. Rather, the important drivers of firms' takeover protection are poison pill laws and the *Amanda* decision. Furthermore, in robustness tests reported in Section VII.C, we find that the coefficient for *Amanda x BC* is not robust. When we include a control for a nearly contemporaneous court decision that strengthened a firm's use of poison pills (*Paramount Communications, Inc. v. Time, Inc.*), the coefficient on *Paramount x Poison pill law* is negative and significant

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[19] When adding other second-generation laws in subsequent models, we also account for firms that opted out of control share acquisition, fair price, poison pill and directors' duties laws. In sum, 105 firms and 585 firm-years are affected by firms' ability to opt-out of state antitakeover laws. Further, we account for the Georgia fair price law and the Tennessee control share acquisition law, which also have opt-in clauses. A total of 162 firms and 1539 firm-years are affected by opt-in laws.

[20] Three of Atanassov's passage dates, which are taken from Bertrand and Mullainathan (2003), differ from those in Table 2: the Connecticut law was adopted in 1988 (Atanassov has 1989); the Kentucky law was adopted in 1986 (not 1987), and the Pennsylvania law was adopted in 1988 (not 1989). In addition, Oregon (1991), Iowa (1997), and Texas (1997) adopted business combination laws that are not included in Atanassov's state law data.

while the coefficient on *Amanda x BC* is insignificant. We infer that business combination laws are not significantly related to firms' patent activity, although poison pill laws are.

Model 5 estimates the short regression for the subsample of firms in the Atanassov (2013) data that have prior firm-level defenses. Even in this truncated model, the *BC* coefficient is statistically insignificant. This result is consistent with our results in Table 5, which indicate that the incremental impact of a business combination law on the outcome variables is insignificant among firms with prior firm-level defenses.

Panel B of Table 6 replicates Atanassov's (2013) main result regarding the effects of business combination laws on citations to a firm's new patents. The dependent variable is $Ln(1+Cit/Pat)_{t+3}$, which is the natural log of one plus the average number of citations per patent filed by the firm. Again, Atanassov (2013) uses a three-year leading citation measure to account for the fact that it takes time for investments in research and development to yield patentable discoveries. Table 6, Model 6 replicates the central result in Atanassov's (2013) Table 3, Model 4. The coefficient on *BC* is -0.018 and is statistically significant. The coefficient on *BC* remains significant in Model 7, using our reduced sample in which we can identify states of incorporation for the firm-years in Atanassov's sample, and in Model 8, in which we adjust for opt-out firms and opt-in laws, as well as for six errors in the dates on which business combination laws were adopted.

Model 9 reports on the results from the full model, which includes controls for other state antitakeover laws, court decisions, motivating firms, and firm-level defenses. The results are largely the same as in Panel A: patent citations are not significantly related to coverage by business combination laws but are negatively related to coverage by poison pill laws. Although patent citations are not significantly related to coverage by a business combination law for most firms, they are significantly related to coverage by a business combination law for the small number of firms that lobbied for these laws. Model 9 in Panel B reports a significant coefficient on the interaction *Amanda x BC*. As reported below, however, this result is not robust. We also observe a statistically significant but positive coefficient for coverage by a control share acquisition law. This result is not consistent with the joint hypothesis that control share acquisition laws provide exogenous takeover protection and that firms respond to such protection by innovating less. We do not have a good explanation for this anomalous result.

In the Internet Appendix we report on a series of tests in which we examine the sensitivity of the Table 6 results to different assumptions about the lag between a firm's decision to invest in innovation and any resulting patents. Such sensitivity tests might be particularly important in our replication tests because our controls for other state laws and court decisions have the effect of parsing the sample observations into more and smaller time-related buckets. The robustness test results are consistent with the joint hypothesis that coverage by poison pill laws affects firms' innovative activity and that investments in innovation take three or

four years to become apparent in the firm's patents. Throughout these tests, however, coverage by a business combination law is not related to patents or patent citations.

VII.C. Robustness tests

In this section we report on tests that probe the importance of two implicit assumptions in the specification of the tests we report in Tables 4–6. The first assumption is about which court decisions had material impacts on firms' takeover protections. Our tests include controls for four important court decisions that affect firms' takeover protections. Cain, Davidoff, and McKeon (2014), in contrast, identify three other court decisions as having potentially large effects on firms' takeover defenses, and Catan and Kahan (2014) identify at least seven additional (typically lower) court decisions that potentially affect the legal status of state takeover laws and firms' uses of poison pills. In the Internet Appendix we report on tests that are analogous to those in Tables 4 and 6 that include controls for these additional court decisions. With one exception, the additional court decisions are not significantly related to the outcome variables, and their inclusion in the tests do not affect our other results. The exception involves the 1989 Delaware Supreme Court decision in *Paramount Communications, Inc. v. Time, Inc.* (571 A.2d 1140, Del. Supr. 1989). The *Paramount* decision affirmed Time, Inc. board's right to implement its poison pill and "just say no" to Paramount's acquisition bid, rather than allow Time's shareholders to vote on Paramount's offer. When we include a control for *Paramount x Poison pill law*, the coefficient is statistically significant for five of the nine outcome variables in Tables 4 and 6. Furthermore, for the four outcome variables for which *Amanda x BC* is significant in Tables 4 and 6, the inclusion of *Paramount x Poison pill law* makes these coefficients insignificant. Both the *Amanda* and *Paramount* decisions were issued in 1989. We infer that the *Paramount* case had a substantive effect on many firms' takeover protections, particularly firms that are covered by poison pill laws, and that the *Amanda x BC* coefficient picks up this effect in a few tests when the *Paramount* case is not included.

A second assumption in our test specification is about which firms were affected by decisions rendered by the Delaware courts. Cain et al. (2014) report that 55% of the firm-years in their sample are for firms incorporated in Delaware, so Delaware court decisions directly affect the majority of large publicly traded firms. Delaware corporate law frequently leads, or becomes a template for, corporate law in other states. So it is possible that Delaware court decisions have extra-territorial impact on non-Delaware firms. The possibility of such impact is particularly strong for the 1985 *Moran* decision. Cremers and Ferrell (2014), Cain et al. (2014), and others argue that *Moran* may have provided legal sanction, or the likelihood of legal sanction, for poison pills by non-Delaware firms, even if those firms were incorporated in states that did not have poison pill laws. Catan and Kahan (2104) point out that *Moran* plausibly might have provided

nationwide sanction for so-called flip-over poison pills, leaving the legal status of only more aggressive flip-in poison pills in doubt until subsequent court decisions and states' adoption of poison pill laws.

To explore this possibility, we replicated the tests in Tables 4 and 6 under alternate assumptions about the firms impacted by the *Moran* decision. In one test we assume that non-Delaware firms received 50% of the legal sanction to use poison pills as did Delaware firms.[21] We code the *Poison pill law* variable as increasing from 0 to 1.0 for all Delaware firms following the *Moran* decision, and from 0 to 0.5 for all non-Delaware firms that are not covered by poison pill laws. For firms that subsequently are covered by poison pill laws, *Poison pill law* increases from 0.5 to 1.0. The results of this alternate assumption about the reach and impact of the *Moran* decision are reported in the Appendix. The effect is to increase the size and significance of the *Poison pill law* coefficient in some tests, which becomes statistically significant in seven of the nine outcome variables examined in Tables 4 and 6. These results are consistent with the joint hypothesis that poison pill laws increased many firms' takeover protections and that such protections are related to the various firm outcome variables.

VII.D. Simulation tests for the size of the omitted variable bias

In this section we use simulated data to measure the frequency with which the omitted variable bias in the short regression (Eq. 1) is large enough to yield a significant *BC* coefficient even when the true effect of *BC* is zero. We simulate data by assuming that only our institutional and political economy variables affect some arbitrary outcome *y*, estimate the short regression (Eq. 1) using these simulated data, and examine the sign and statistical significance of the resulting *BC* coefficient.

In each iteration, we assume the true model is:

$$y_{ijlst} = a + d'L_{ist} + gBC_{st} \times MF_i + u_{ijlst} \tag{6}$$

where *y* is a constructed random variable for firm *i*, in industry *j*, located in state *l*, incorporated in state *s*, in year *t*. *L* is a vector of dummy variables indicating coverage in a given firm-year by first- and second-generation antitakeover laws and the *CTS* and *Amanda* court cases, and $BC_{st} \times MF_i$ is a dummy variable equal to one for firm-years in which a firm is covered by a business combination law for which it lobbied. E[*y*$_{ijlst}$] depends on our assumptions about the impacts of the institutional and political economy variables (*d'* and *g*) in a given simulation. In our first simulation, for example, we specify that coverage by a first-generation state takeover law increments E[*y*$_{ijlst}$] by 1 and that coverage by all other political economy variables used in Table 4

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[21] We thank Bernie Black for this suggestion. The results are similar using alternate weighting assumptions, such as 25% or 100%.

have zero impact on y. So in the first simulation, $E[y_{ijlst}] = 1$ for firm-years in which a first-generation state takeover law is in force, and $E[y_{ijlst}] = 0$ for all other firm-years.

In each iteration of a simulated model, we generate a firm-year panel of y_{ijlst} values for all firms in the Compustat database from 1976-1995 using the assumed structure for d' and g and adding a unit normal random error u_{ijlst}. The error term u_{ijlst} is an equal weighted sum of mutually independent and standard normal shocks for the firm (ε_i), industry (ε_j), state of location (ε_l), and year (ε_t), plus an idiosyncratic firm-year shock (ε). Specifically,

$$ u_{ijlst} = \frac{1}{\sqrt{5}} \left[\sum_{p \in \{i,j,l,t\}} \varepsilon_p + \varepsilon \right] \sim N(0,1) \tag{7} $$

We then estimate the short regression (Eq. 1) using the simulated data, intentionally applying this misspecified model that incorrectly assumes that business combination laws, and only business combination laws, affect the outcome variable y_{iljst}. As in Table 4, we include firm, industry-year, and state-year fixed effects in this regression. Generating outcome values y for actual firms in the Compustat database ensures that the simulation results mimic the actual distribution of firms and firm-years across states and isolates the impact of our political economy variables.[22] For a given model, we conduct 2500 iterations of this process and report on the distribution of the estimated coefficient for BC in the misspecified short regression.

The simulation results are summarized in Table 7. In Model 1, only first-generation state laws are assumed to affect y, and the effect is to increment y by one standard deviation. Nonetheless, the BC coefficient from the short regression is statistically significant at the 10% level in more than half (52.4%) of the iterations. Furthermore, the mean BC coefficient is negative, -0.046, even though the assumed impact of coverage by first-generation laws on the outcome variable is positive. This is the outcome illustrated by Figure 2: If first-generation laws have a meaningful effect on firm governance, an empirical model that focuses only on business combination laws frequently will get the wrong sign for the true impact of takeover protection on the outcome variable.

Table 7 reports results for 6 different combinations of assumptions about the true underlying impact of takeover protections on the outcome variable y. In Model 3, for example, we assume that y is affected by business combination law coverage only for the firms that lobbied for the business combination law. Here, the mean value for the BC coefficient in the short regression is small (0.002), but the BC coefficient is significant at the 10% level in 20.2% of the iterations. Thus, an unwary researcher would detect a significant impact of BC

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[22] Because of missing data for some outcome variables examined in Section VI.A, the simulation sample sizes are larger than in Tables 4 and 5: 88,653 for the full sample and 11,007 for those using firm-level takeover defenses.

on the outcome variable 20% of the time even though the underlying effect occurs only for firms for which the business combination law is clearly endogenous.

Models 4–6 present different ways to analyze the effects of firm-level defenses given limitations in our data. Model 4 includes the entire sample of Compustat-listed firms and assigns a one standard deviation increase to the outcome variable y for the firms that the Cremers and Ferrell (2014) data indicate have at least one takeover defense. Models 5 and 6 are run only on the firms in the Cremers and Ferrell (2014) data. Model 5 assigns a one standard deviation increase to the outcome variable y for firms with a higher G-index than the median value, and Model 6 increments the outcome variable y by 1/16 times the firm's G-index (the maximum G-index value in the sample is 16). (These G-index values are modified to exclude coverage by any state antitakeover laws.)

Overall, the simulation results in Table 7 yield four broad insights. First, there exist many underlying structures for which a researcher would mistakenly infer that the passage of a business combination law has a meaningful impact on the outcome variable even though, in fact, the relationship does not exist (see Models 1, 3, 4, 5, 6). Second, there exist some scenarios (Models 1, 2, 5, 6) in which the mean *BC* coefficient in the short regression is *negative*, even though the true relationship between takeover protection and the constructed variable *y* is positive. Third, when we assume that business combination laws affect the outcome variable, but only for motivating firms (Model 3), the short regression frequently yields a positive and statistically significant estimate for *BC*. This result indicates that the endogenous adoption of business combination laws, at least for a small number of motivating firms, can generate a spurious a finding of a significant *BC* coefficient for all firms that are covered by a business combination law. And fourth, the presence of firm-level defenses can drive significant results for the *BC* coefficient. In Model 4, the BC coefficient is significant at the 10% level in 25.2% of the iterations. The *BC* coefficient also is frequently significant in Models 5 and 6, but with an average sign that is opposite to the assumed effect of takeover protection on the outcome variable.

The models we simulate assume a specific impact of first generation laws, poison pill laws, motivating firms, and firm-level takeover defenses on the outcome variable. In the Internet Appendix we report tests that employ alternate assumptions about the size of the marginal impact of a each institutional feature (e.g., does it increase *y* by one standard deviation or two?), and whether the impacts are substitutes or complements. The lessons from using these alternate assumptions, however, are qualitatively similar to those in Table 8. These results illustrate the large potential for bias and misinterpretation when researchers focus primarily on business combination laws and ignore the potential effect of other antitakeover laws, court decisions, firm-level defenses, and endogeneity. We propose that such institutional and political economy considerations are likely to be prevalent in other natural and quasi-experimental settings as well.

VIII. Conclusion

This paper examines the use of regulatory and legal changes to identify exogenous variation in empirical tests and argues that the institutional setting of such changes can have a large effect on inferences. We focus on a particularly popular strategy that uses state antitakeover laws to identify exogenous changes in firms' takeover vulnerability and governance. Implementing this strategy requires sensitivity to four concerns that arise from the institutional background from which state antitakeover laws emerged.

The first concern is based on the fact that states have been adopting antitakeover laws since at least 1968, soon after passage of the federal Williams Act. To date, researchers have assumed that the pre-1982 period is characterized by unusually low takeover protection. An examination of first-generation state laws that were in effect in 38 states until June 1982, however, indicates that the pre-1982 period is more accurately characterized as a period of unusually high takeover protection for most firms. This fact alone can reverse the interpretation of tests that rely heavily on pre-1982 data.

Since 1982, 43 states have adopted a total of 153 versions among the five most common types of second-generation antitakeover laws. A focus on the adoption of just one type of law, e.g., business combination laws, can lead to misspecified tests because all state antitakeover laws potentially offer some level of takeover protection. Some researchers have argued that only business combination laws offer meaningful takeover protection. But such claims are not apparent from the laws' provisions, and event study results indicate that poison pill laws have a larger impact on firm values than business combination laws. In replications of prior studies that include controls for our institutional and political economy concerns, poison pill laws are more consistently related to such outcome variables as firm ROA or innovation than are business combination laws.

The second concern arises because firm-level takeover defenses substitute for the takeover protection afforded by state antitakeover laws. Event study evidence indicates that any significant share price reaction to the adoption of a state antitakeover law is limited to firms that do not already have firm-level defenses, implying that the marginal impact of an antitakeover law on a firm's takeover protection depends on the firm's firm-level defenses. Consistent with these prior findings, we find that business combination laws are not significantly related to any of the outcome variables we examine when we limit the sample to firms that already have firm-level defenses, even in tests that do not include controls for our other institutional concerns.

The third concern is that the incremental takeover protection afforded by a state antitakeover law depends on the legal regime. We identify four particularly important court decisions that resolved uncertainty over the legality and impact of antitakeover laws. Our fourth concern is that the imposition of a state

antitakeover law is identifiably not exogenous for a substantial number of firms that either lobbied for their laws or opted out of coverage.

Replications of previous empirical findings reveal that controls for these institutional considerations have large effects on empirical inferences. For six of the nine outcome variables we examine that are used in previous studies that use business combination laws for identification, the coefficient for business combination laws becomes insignificant when we include controls for other state antitakeover laws, court decisions, and motivating (i.e., endogenous) firms. The results for all nine outcome variables are insignificant among firms that have prior firm-level takeover defenses.

We also conduct simulations that demonstrate how tests that focus solely on business combination laws frequently will generate significant results even when – by construction – such laws have no impact on firms' governance or takeover vulnerability. Under some scenarios, tests that erroneously focus only on business combination laws will yield significant coefficients with the opposite sign as the true impact of takeover protection on the outcome variable.

Overall, our replication and simulation tests yield five suggestions for researchers who seek to use takeover laws to identify exogenous changes in firms' takeover protection. First, researchers should control for the takeover protection afforded by poison pill sanctions, including the 1985 *Moran* decision for Delaware firms and the poison pill laws adopted by 29 other states. Second, researchers should control for the impact of court decisions that clarified the legality of poison pills, control share acquisition laws, and business combination laws, especially the *Moran*, *CTS*, and *Amanda* decisions. Third, researchers should examine whether state laws afford incremental takeover protections for firms with firm-level defenses. Fourth, researchers should examine whether the empirical relations they measure are affected by the inclusion of firms that lobbied for their laws or opted out of the laws. And fifth, researchers who use natural experiments to identify empirical tests should explicitly recognize the joint nature of their hypotheses. In general, we test not only whether there is an impact on our outcome variable of interest, but also the hypothesis that the events we use for identification are meaningful. In the case of state antitakeover laws, the underlying joint hypothesis is that these laws do, in fact, increase firms' takeover protections.

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Table 1: First-generation state antitakeover laws, 1968 – 1982
This table lists in chronological order the first-generation state antitakeover laws adopted by 38 states from 1968 through 1981, which were effective until a split U.S. Supreme Court ruling in *Edgar vs. MITE Corp.* on June 23, 1982. First-generation laws regulated cash tender offers and generally imposed extremely strong takeover protections for the covered corporations. The protections included requirements that the bidding firm file extensive disclosure statements with the state securities commissioner, provisions that required long and variable open periods for tender offers, state administrative overview of the tender offer, and potential civil and criminal liability for bidding firms and their managers for violations of the antitakeover provisions.

State	Effective date	Repeal date	State	Effective date	Repeal date
Virginia	03/05/1968	07/01/1989	Kentucky	07/01/1976	07/15/1986
Nevada	03/04/1969	10/01/1991	Maryland	07/01/1976	07/01/1986
Ohio	10/09/1969		Michigan	07/01/1976	04/01/1988
Wisconsin	07/01/1972		New York	11/01/1976	
Minnesota	08/01/1973		Georgia	03/23/1977	03/28/1986
Hawaii	05/24/1974	04/23/1985	Arkansas	03/24/1977	03/24/2000
Kansas	07/01/1974	04/21/1988	New Hampshire	03/25/1977	
Indiana	05/01/1975		Nebraska	04/27/1977	04/08/1988
Colorado	07/01/1975	07/01/1984	New Jersey	04/27/1977	
Idaho	07/01/1975	07/01/1986	North Carolina	06/28/1977	10/01/2001
South Dakota	07/01/1975	07/01/1990	Mississippi	07/01/1977	
Utah	02/05/1976	07/01/1983	Illinois	09/08/1977	07/01/1984
Pennsylvania	03/03/1976		Texas	08/29/1977	
Tennessee	03/17/1976		Florida	10/01/1977	09/01/1979
Delaware	05/01/1976	07/01/1987	Maine	03/24/1978	07/16/1986
Massachusetts	05/22/1976		South Carolina	06/12/1978	01/01/1989
Connecticut	06/02/1976		Missouri	08/13/1978	
Alaska	06/12/1976		Iowa	01/01/1979	01/01/2005
Louisiana	06/28/1976	08/15/1987	Oklahoma	07/21/1981	07/22/1985

Table 2: Second-generation state antitakeover laws, 1982 – 2013
This table lists the dates of adoption for the five most common types of antitakeover laws adopted by states since 1982. The law types are: control share acquisition laws (CS), business combination laws (BC), fair price laws (FP), directors' duties laws (DD), and poison pill laws (PP). A total of 43 states have adopted a total of 153 of these laws, including 33 states that have business combination laws. The different types of laws are described in Appendix A. Dates in parenthesis represent the effective date of the law if it is different from the adoption date.

State	CS	BC	FP	DD	PP
Arizona	07/22/1987	07/22/1987	07/22/1987	07/22/1987	
Colorado					03/31/1989
Connecticut		06/07/1988	06/04/1984	06/07/1988	06/26/2003 (10/01/2003)
Delaware		02/02/1988 (12/23/1987)			
Florida	07/02/1987		07/02/1987	06/27/1989	06/27/1989 (07/01/1990)
Georgia		03/03/1988[1]	03/27/1985[1] (07/01/1985)	04/10/1989 (07/01/1989)	04/07/1988 (07/01/1989)
Hawaii	04/23/1985			06/07/1989	06/17/1988
Idaho	03/22/1988	03/22/1988	03/22/1988	03/22/1988	03/22/1988
Illinois		08/02/1989	08/23/1985	08/23/1985	08/02/1989
Indiana	03/05/1986 (04/01/1986)	03/05/1986 (01/08/1986)	03/05/1986 (04/01/1986)	03/05/1986 (04/01/1986)	03/05/1986 (04/01/1986)
Iowa		05/02/1997 (07/01/1997)		06/01/1989 (12/31/1989)	06/01/1989 (12/31/1989)
Kansas	04/14/1988 (04/21/1988)	04/10/1989 (07/01/1989)			
Kentucky		03/28/1986	04/09/1984 (07/13/1984)	07/15/1988	07/15/1988
Louisiana	06/11/1987		07/13/1984	07/10/1988	
Maine		04/06/1988		06/21/1985 (09/19/1985)	04/08/2002 (07/01/2003)
Maryland	04/11/1989	04/11/1989	06/21/1983	05/13/1999 (06/01/1999)	05/13/1999 (06/01/1999)
Massachusetts	07/21/1987	07/18/1989		07/18/1989	07/18/1989
Michigan	03/19/1988[2] (04/01/1988)	05/24/1989	05/24/1984 (05/29/1984)		07/23/2001
Minnesota	04/25/1984 (08/01/1984)	06/25/1987 (06/01/1987)	05/02/1991 (08/01/1991)	06/25/1987 (06/01/1987)	
Mississippi	03/15/1990 (01/01/1991)		03/29/1985 (07/01/1985)	04/04/1990 (07/01/1990)	
Missouri	06/13/1984	06/23/1986	06/23/1986	05/06/1986	

(continued on next page)

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[1] The Georgia business combination and fair price laws are opt-in.
[2] The Michigan control share statute was repealed effective 01/06/2009.

State	CS	BC	FP	DD	PP
Nebraska	04/08/1988	04/08/1988		04/08/1988[3]	
Nevada	06/06/1987 (07/01/1987)	06/25/1991 (10/01/1991)	06/25/1991 (10/01/1991)	06/25/1991 (10/01/1991)	06/21/1989 (10/01/1989)
New Jersey		08/05/1986 (01/23/1986)	08/05/1986 (01/23/1986)	02/04/1989	06/29/1989
New Mexico				04/09/1987	
New York		12/16/1985	12/16/1985	07/23/1987	12/21/1988 (07/24/1986)
North Carolina	05/13/1987		04/23/1987	07/24/1993 (10/01/1993)	06/08/1989 (07/01/1990)
North Dakota				04/12/1993 (08/01/1993)	
Ohio	11/18/1982	04/11/1990	04/11/1990	07/11/1984 (10/10/1984)	11/22/1986
Oklahoma	06/24/1987	04/09/1991 (09/01/1991)			
Oregon	07/18/1987	04/05/1991		03/05/1989	03/05/1989
Pennsylvania	04/27/1990	03/23/1988	03/23/1988	04/27/1990	03/23/1988
Rhode Island		07/03/1990	07/03/1990	07/03/1990	07/03/1990
South Carolina	04/22/1988	04/22/1988	04/22/1988		06/09/1998
South Dakota	02/20/1990 (07/01/1990)	02/20/1990 (07/01/1990)	02/20/1990 (07/01/1990)	02/20/1990 (07/01/1990)	02/20/1990 (07/01/1990)
Tennessee	03/11/1988[4]	03/11/1988	03/11/1988	03/11/1988	05/29/1989
Texas		05/28/1997 (09/01/1997)		05/29/2003 (01/01/2006)	
Utah	05/29/1987				03/13/1989 (04/24/1989)
Vermont				04/16/1998	
Virginia	02/22/1989 (07/01/1989)	03/31/1988	03/24/1985 (06/01/1985)	03/31/1988	04/02/1990
Washington		08/11/1987	05/13/1985 (07/28/1985)		03/23/1998 (06/11/1998)
Wisconsin	04/18/1984[5] (04/24/1984)	09/17/1987 (09/10/1987)	04/18/1984 (04/24/1984)	06/09/1987 (06/13/1987)	09/17/1987 (04/30/1972)
Wyoming	03/20/1990	03/11/1989		03/09/1990 (01/01/1990)	

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[3] The Nebraska directors' duties statute was repealed effective 04/25/1995, but was later reenacted effective 03/07/2007.
[4] Tennessee control share statute is opt-in.
[5] The Wisconsin control share statute was repealed effective 04/22/1986.

Table 3: Motivating or lobbying firms
This table lists the firms that Karpoff and Malatesta (1989) and Gartman (2000) identify as lobbying for specific state antitakeover laws. For these firms, the laws were not exogenously imposed. The abbreviations for law types are: control share acquisition laws (CS), business combination laws (BC), fair price laws (FP), directors' duties laws (DD), and poison pill laws (PP). The different types of laws are described in the Appendix, and their adoption dates are in Table 2.

State	Law types	Motivating firm(s)
Arizona	CS, BC, FP, DD	Greyhound
Colorado	PP	KN Energy
Connecticut	BC	Singer
	FP	Aetna
Delaware	BC	Texaco
Florida	CS, FP	Harcourt Brace Jovanovich
Georgia	BC	Ashland Oil
Hawaii	CS	Amfac
Illinois	BC, FP	Illinois Retail Merchants Association including: Abbott Labs, Sears, Roebuck, Walgreens
Indiana	CS, BC, FP, PP	Arvin Industries
	DD	Cummins Engine
Kansas	BC	United Telecommunications, Centel, Coleman
Kentucky	BC, FP	Ashland Oil
Louisiana	CS	Baton Rouge Water Works
Maryland	BC, CS	State Chamber of Commerce, United Food and Commercial Workers Union; Martin Marietta, McCormick, PHH Group
	FP	McCormick, PHH Group, Foremost-McKesson
Massachusetts	CS	Gillette
	DD, PP	Stop and Shop, Polaroid, Prime Computer, Gillette
Minnesota	BC, CS, DD	Dayton Hudson
Missouri	CS	TWA
New Jersey	BC, FP	Schering-Plough, Singer
New York	BC, FP	CBS, Champion International, GE
	PP	Ogilvy Group, Avon, Champion International, International Paper, and Xerox, among others.
North Carolina	CS	Burlington
	FP	PepsiCo
Ohio	PP	Goodyear Tires
Pennsylvania	BC, FP	Mellon Bank, PPG, Westinghouse
	CS, DD	Armstrong World Industries
Washington	BC, FP	Boeing
Wisconsin	BC, PP	G. Heileman Brewing

Table 4: The effects of business combination laws on operating performance and other firm characteristics

This table reports results of tests of the impact of business combination laws on seven different outcome variables and then tests for the effects of including controls for other types of state takeover laws, court decisions, and motivating firms. The outcome variables are ROA, capex, PPE growth, asset growth, cash, SGA expense and leverage. ROA equals EBITDA divided by total assets; capex, PPE, cash, SGA expense are scaled by total assets; PPE and asset growth are the percentage change in PPE and total assets, respectively; and leverage equals debt divided by total assets. All outcome variables and continuous control variables are winsorized at the 0.5% and 99.5% levels. Control variables include size, size squared, age, and age squared, as in Giroud and Mueller (2010). All regressions also include firm, state-year, and industry-year fixed effects. Robust standard errors, clustered at the state of incorporation level, are reported in parentheses, with *, **, and *** denoting two-tailed significance at the .10, .05, and .01 levels.

	Dependent variable:													
	(1) ROA		(2) Capex		(3) PPE growth		(4) Asset growth		(5) Cash		(6) SGA expense		(7) Leverage	
	(a) Short regression	(b) Full model	(a) Short regression	(b) Full model	(a) Short regression	(b) Full model	(a) Short regression	(b) Full model	(a) Short regression	(b) Full model	(a) Short regression	(b) Full model	(a) Short regression	(b) Full model
Business combination law (BC)	-0.017*	-0.007	0.003*	0.002	-0.016**	-0.005	-0.044*	-0.042**	-0.008***	-0.005	0.018**	0.012**	0.023**	0.021**
	(0.010)	(0.007)	(0.002)	(0.002)	(0.008)	(0.013)	(0.024)	(0.020)	(0.003)	(0.003)	(0.008)	(0.005)	(0.010)	(0.008)
First-generation law		-0.030*		0.004		-0.000		-0.028		0.004		0.018		-0.019
		(0.015)		(0.003)		(0.012)		(0.022)		(0.007)		(0.012)		(0.022)
Poison pill law (PP)		-0.010*		0.003*		-0.004		-0.032*		-0.003		0.012**		0.027***
		(0.005)		(0.001)		(0.012)		(0.017)		(0.003)		(0.005)		(0.009)
Control share acquisition law (CS)		-0.020**		0.002		-0.002		0.017		0.008*		0.011		0.020
		(0.010)		(0.003)		(0.017)		(0.030)		(0.004)		(0.009)		(0.014)
Directors' duties law (DD)		0.002		-0.001		0.013		0.024		0.005		-0.002		-0.005
		(0.009)		(0.001)		(0.011)		(0.024)		(0.004)		(0.008)		(0.013)
Fair price law (FP)		-0.011		0.003		-0.026**		0.013		-0.004		-0.004		-0.004
		(0.010)		(0.002)		(0.011)		(0.020)		(0.003)		(0.008)		(0.014)
CS x *CTS*		-0.001		-0.000		-0.012		-0.028		-0.001		0.022**		0.005
		(0.011)		(0.002)		(0.016)		(0.029)		(0.006)		(0.010)		(0.021)
BC x *Amanda*		-0.027*		0.002		-0.017		-0.024		-0.003		0.026**		0.015
		(0.015)		(0.002)		(0.031)		(0.028)		(0.007)		(0.011)		(0.015)
BC x MF (motivating firms)		0.201***		0.012		0.062*		0.141**		-0.041***		-0.113***		-0.056
		(0.048)		(0.010)		(0.036)		(0.056)		(0.013)		(0.040)		(0.039)
Constant	-0.230***	-0.212***	0.120***	0.117***	0.386***	0.394***	1.682***	1.710***	0.256***	0.258***	0.784***	0.764***	0.412***	0.392***
	(0.043)	(0.039)	(0.003)	(0.003)	(0.031)	(0.030)	(0.135)	(0.140)	(0.009)	(0.010)	(0.020)	(0.022)	(0.050)	(0.052)
Observations	88,310	88,310	87,251	87,251	79,606	79,606	80,649	80,649	88,496	88,496	81,431	81,431	88,283	88,283
R-squared	0.67	0.67	0.54	0.54	0.23	0.23	0.38	0.38	0.65	0.65	0.76	0.76	0.58	0.58

Table 5: The effects of business combination laws among firms with prior firm level takeover defenses

This table reports results of "short regression" tests of the impact of business combination laws on seven different outcome variables among firms with pre-existing firm-level takeover defenses. The outcome variables are ROA, capex, PPE growth, asset growth, cash, SGA expense and leverage. ROA equals EBITDA divided by total assets; capex, PPE, cash, SGA expense are scaled by total assets; PPE and asset growth are the percentage change in PPE and total assets, respectively; and leverage equals debt divided by total assets. All outcome variables and continuous control variables are winsorized at the 0.5% and 99.5%. Control variables in all regressions include size, size squared, age, and age squared, as in Giroud and Mueller (2010). All regressions also include firm, state-year, and industry-year fixed effects. Robust standard errors, clustered at the state of incorporation level, are reported in parentheses, with *, **, and *** denoting two-tailed significance at the .10, .05, and .01 levels.

	Dependent variable:						
	(1) ROA	(2) Capex	(3) PPE growth	(4) Asset growth	(5) Cash	(6) SGA expense	(7) Leverage
Business combination law (BC)	**-0.005**	**0.005**	**0.021**	**0.008**	**0.003**	**0.002**	**0.007**
	(0.003)	**(0.003)**	**(0.018)**	**(0.019)**	**(0.006)**	**(0.009)**	**(0.008)**
Constant	0.205***	0.032	0.080	-0.232*	0.278***	0.490***	0.236**
	(0.038)	(0.027)	(0.233)	(0.121)	(0.074)	(0.066)	(0.111)
Observations	10,963	10,835	10,937	10,968	10,987	10,517	10,963
R-squared	0.81	0.78	0.42	0.58	0.82	0.96	0.82

Table 6: The effects of antitakeover laws on the number of patents and patent citations

This table reports results of tests that replicate the central results in Atanassov (2013) and then include controls for other types of state takeover laws, court decisions, motivating firms, and firm-level defenses. The dependent variable in each regression of panel A is the natural log of 1 plus the number of patents filed by the firm three years in the future, and the dependent variable in each regression of panel B is the natural log of 1 plus the average citations per patent filed by the firm three years in the future. Each model includes control variables that include firm sales, leverage, profitability, asset tangibility, and industry concentration, and includes firm and year fixed effects (coefficients not reported). Models 1 and 6 are a replication of the results in Atanassov's (2013) Table 3, Models 1 and 4, respectively. Models 2 and 7 are identical tests, but limited to the subsample of firms for which we can identify states of incorporation using Compustat data (necessary for including controls for other state antitakeover laws). Models 3 and 8 adjust for firms that opt out, laws that require firms to opt in and also corrects for six errors in the dates and states that adopted business combination laws. Models 4 and 9 include controls for our institutional and political economy controls, including a firm's coverage in a given firm-year by first-generation antitakeover laws and four other important second-generation state antitakeover laws; changes in the legal regime affecting these laws, including the 1987 U.S. Supreme Court decision in *CTS v. Dynamics Corp. of America* and the 1989 7th Circuit Court of Appeals decision in *Amanda Acquisition Corp. v. Universal Foods Corp*; and an indicator variable that denotes coverage by a business combination law for firms that lobbied for the law. Models 5 and 10 are estimated on a reduced sample of firms that had takeover defenses when the business combination law was enacted, as identified by RiskMetrics or from the Cremers and Farrell (2014) data. Robust standard errors, clustered at the firm level, are reported in parentheses, with *, **, and *** denoting two-tailed significance at the .10, .05, and .01 levels.

	Dependent Variable									
	Panel A: $ln(1+Pat)_{t+3}$					Panel B: $ln(1+Cit/Pat)_{t+3}$				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Atanassov's (2013) original test	Sample with state id data	Opt-outs, opt-ins and corrected *BC* dates	Full model	Short regression, firms with takeover defenses	Atanassov's (2013) original test	Sample with state id data	Opt-outs, opt-ins and corrected *BC* dates	Full model	Short regression, firms with takeover defenses
Business combination law (*BC*)	-0.014***	-0.012***	-0.009***	0.000	-0.006	-0.018***	-0.012***	-0.009**	-0.001	0.019
	(0.004)	(0.004)	(0.003)	(0.005)	(0.015)	(0.004)	(0.005)	(0.005)	(0.006)	(0.019)
First-generation law				-0.001					-0.006	
				(0.007)					(0.010)	
Poison pill law (PP)				-0.012***					-0.008*	
				(0.004)					(0.005)	
Control share acquisition law (CS)				-0.005					0.025**	
				(0.007)					(0.010)	
Directors' duties law (DD)				-0.002					0.001	
				(0.006)					(0.006)	
Fair price law (FP)				-0.006					0.005	
				(0.007)					(0.007)	
CS x *CTS*				0.009					-0.012	
				(0.009)					(0.011)	
BC x *Amanda*				-0.014***					-0.017**	
				(0.005)					(0.007)	
BC x MF (motivating firms)				-0.297**					-0.105**	
				(0.137)					(0.048)	
Constant	0.049***	0.053***	0.053***	0.051***	-0.243*	0.094***	0.104***	0.104***	0.109***	0.335***
	(0.012)	(0.013)	(0.013)	(0.015)	(0.124)	(0.009)	(0.010)	(0.010)	(0.013)	(0.067)
Observations	101,700	86,521	86,521	86,521	15,559	101,700	86,521	86,521	86,521	15,559
R-squared	0.85	0.85	0.85	0.85	0.86	0.62	0.62	0.62	0.62	0.65

Table 7: Simulation results

This table reports the results of simulations that measure the size and frequency of bias in the estimated BC coefficient when the short regression $y_{ijlst} = a_0 + bBC_{st} + c'FE_{ijlt} + e_{ijlst}$ is estimated under different assumptions about the underlying true effects of takeover laws and firm-level defenses on an arbitrary outcome variable y. In each simulation, the true model is:

$$y_{ijlst} = a + d'L_{ist} + gBC_{st} \times MF_i + u_{ijlst},$$

where *d* reflects the vector of assumed true coefficients for six variables: first generation laws, poison pill laws, coverage by a business combination law times an indicator for motivating firms that lobbied for the law (*BC x MF*), and three measures of firm-level takeover defenses. *Any defense* is set equal to one for all firms in the Cremers-Ferrell (2014) data that have at least one firm-level defense, and zero for all other firms. Models 5 and 6 are estimated only for firms in the Cremers-Ferrell (2014) database. *High G-index* is set equal to one for firms with a higher G-index than the median, and *1/16 x G-index* equals the numerical value of .0625 times the firm's G-index. The dependent variable in each model, *y*, is an arbitrary random variable with mean equal to $d'1$ and a variance of 1. In each iteration of a simulation, the assumed true model is used to generate a panel dataset for *y* and then the short regression is estimated. In generating the panel dataset for *y*, a random error is generated as the sum of standard normal random firm, industry, state of location, year, and firm-year shocks and is scaled such that the mean is zero and the variance is one. *Mean BC coefficient* refers to the mean of the coefficient *b* from the short regression from 2500 iterations, and *% of p-values* reports the fraction of the 2500 iterations in which the coefficient *b* is significant at the 10%, 5%, and 1% levels, respectively.

| | Assumed underlying structure | | | | | | Empirical "short equation" results | | | |
| | First Generation Law | Poison pill law | BC x motivating firm | Firm-level defenses | | | Mean BC coefficient | % of p-values: | | |
Model				Any defense	High G-index	1/16 x G-index		< 0.10	< 0.05	< 0.01
1	1	0	0	0	0	0	-0.046	52.4%	19.6%	0.6%
2	0	1	0	0	0	0	-0.095	0.0%	0.0%	0.0%
3	0	0	1	0	0	0	0.002	20.2%	13.8%	4.6%
4	0	0	0	1	0	0	0.011	25.2%	15.5%	4.1%
5	0	0	0	0	1	0	-0.055	15.4%	6.7%	0.9%
6	0	0	0	0	0	1	-0.031	24.5%	16.4%	5.7%

Figure 1: The main empirical construct using business combination laws
This figure illustrates the use of business combination laws to identify empirical tests. Massachusetts adopted a business combination law on July 18, 1989. Researchers assume that Massachusetts firms' level of takeover protection was low before the law was adopted, and high afterward. Any changes in firm characteristics (e.g., investments, innovation) are attributed to the change in takeover protection afforded by the law.
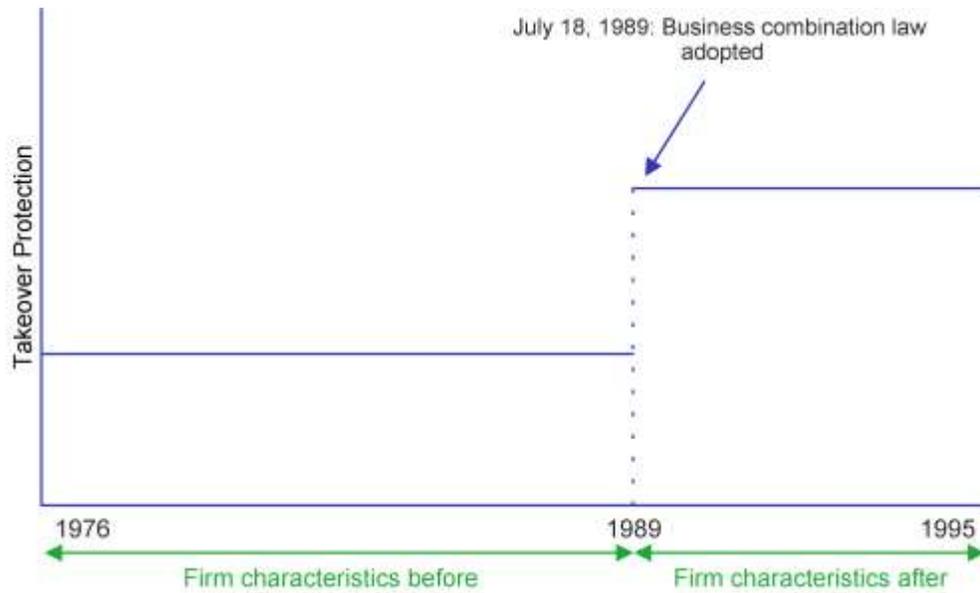
Figure 2: The effect of first-generation state antitakeover laws
This figure illustrates the effect of first-generation state antitakeover laws on the identification of tests that infer a change in the level of takeover protection when a business combination law is adopted. In the period before the U.S. Supreme Court decision in *Edgar v. MITE Corp.* on June 23, 1982, first-generation state antitakeover laws provided a very high level of takeover protection. By comparison, the period after the business combination law was passed on July 18, 1989 has a moderate level of takeover protection.
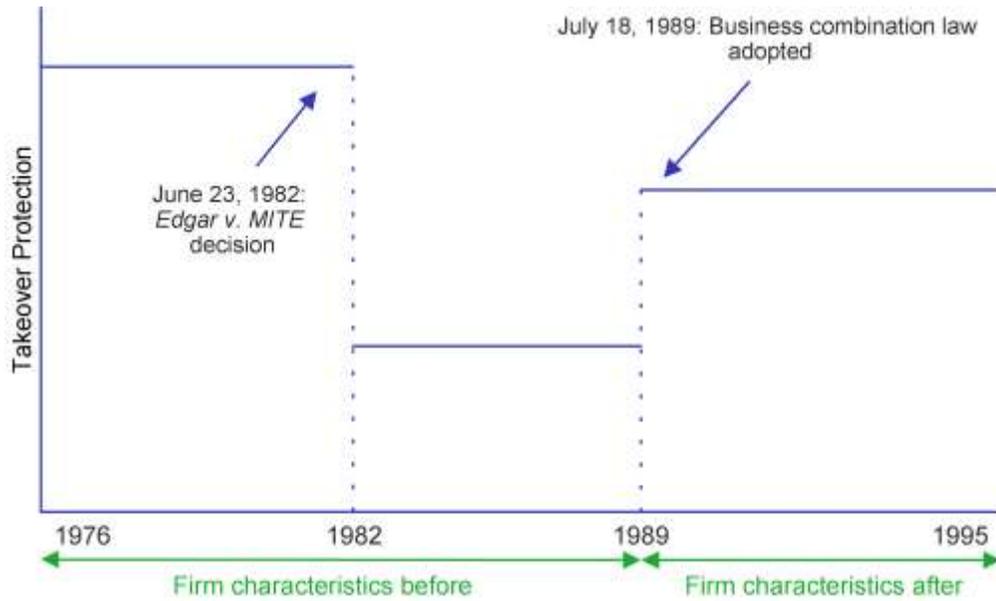
Figure 3: Effects of other state antitakeover laws
This figure illustrates the effect of the adoption of another type of antitakeover law before a business combination law is adopted. In this example, Massachusetts adopted a control share acquisition law on July 21, 1987, and a business combination law on July 18, 1989. As illustrated, the pre-existence of the control share acquisition law decreases the incremental effect of the business combination law on Massachusetts firms' takeover protection.
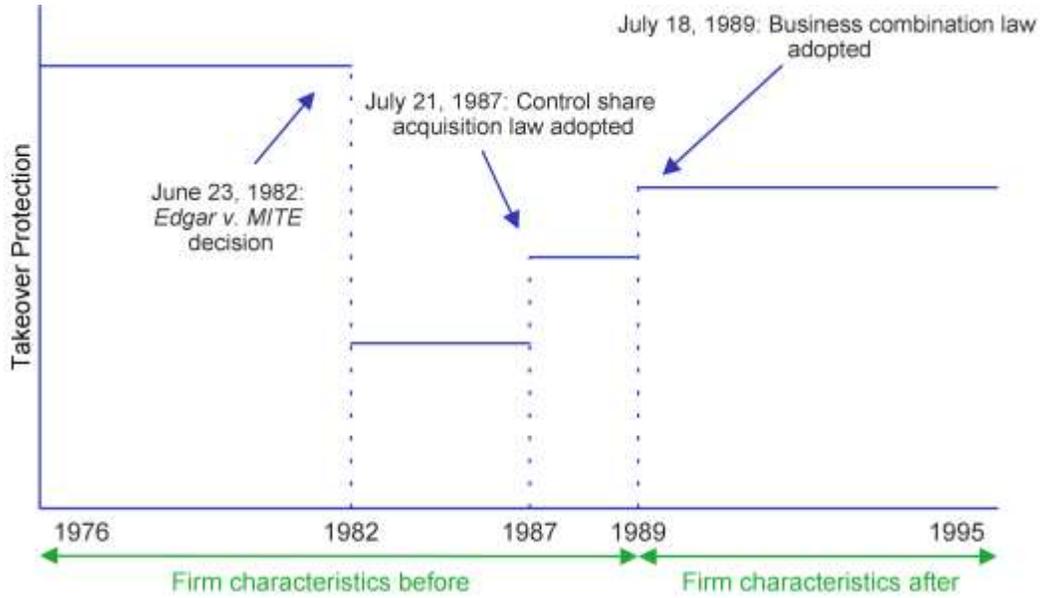
Figure 4: Effects of prior firm-level defenses

This figure illustrates the effect of firm-level defenses on the incremental takeover protection afforded by a business combination law. Soon after the *Edgar v. MITE Corp.* decision on June 23, 1982, many firms adopted firm-level protections. Cullinane Database Systems, Inc., for example, adopted a supermajority vote requirement on July 26, 1982. Massachusetts adopted a business combination law on July 18, 1989. Karpoff and Malatesta (1989, 1995), Szewczyk and Tsetsekos (1992), Fields and Todd (1995), Pugh and Jahera (1995), and Alexander, Spivey, and Marr (1997) report evidence that state antitakeover laws have negligible effects when the firm has a prior takeover defense.
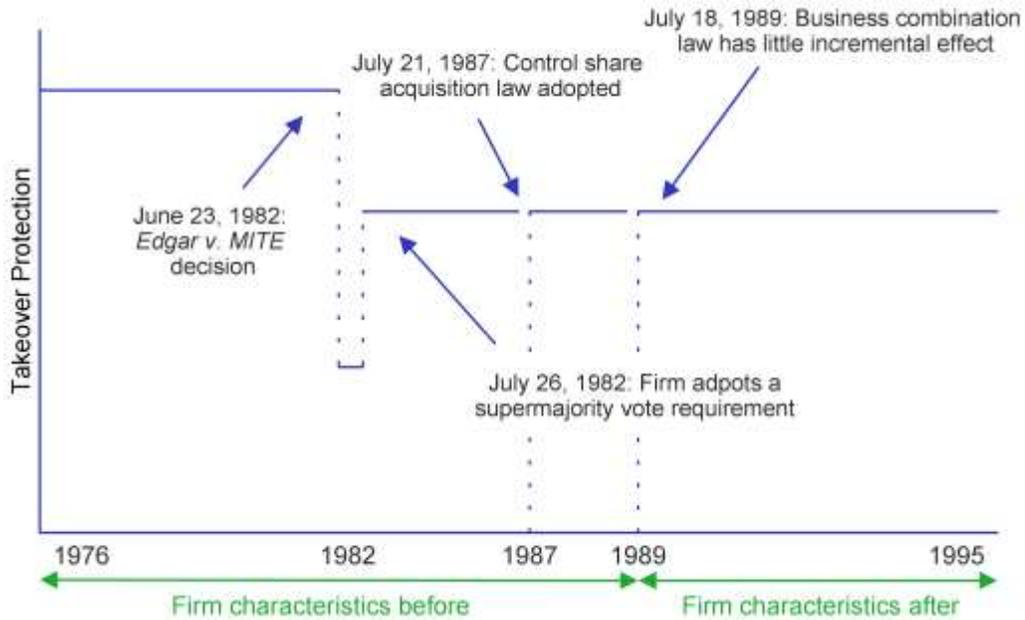
Appendix: Descriptions of the five major types of second-generation state antitakeover laws

Business combination (BC) laws – Business combination laws, also called freeze-out laws, impose a moratorium on significant asset sales or mergers between a large shareholder and the covered firm once the large shareholder's stake passes a threshold level. For example, the New York business combination law prohibits asset sales or a merger with a 20% shareholder for five years. Even after the moratorium, most business combination laws allow the business combination to proceed only if the transaction satisfies fair price provisions. Thus, the typical business combination law is like a fair price law with a forced delay. In most laws, the provisions of the law can be relaxed if the business combination is pre-approved by shareholders or the target firm's board of directors.

Control share acquisition (CS) laws – A Control share acquisition law requires shareholder approval before a large shareholder may vote shares obtained in a control share acquisition. For example, Indiana's control share acquisition law defines a control share acquisition as a series of acquisitions over time that, without the law, would increase a large shareholder's share of the total voting rights to 20%, 33 1/3%, or 50%. To obtain the voting rights associated with the control shares, the large shareholder must receive approval from the majority of all disinterested shares (i.e., shares not owned by the large shareholder or officers of the firm). Many control share acquisition laws are patterned after the Indiana law because in 1987 the U.S. Supreme Court upheld the Indiana law in *CTS v. Dynamics Corp. of America*.

Directors' duties (DD) laws – Also called constituency laws, directors' duties laws explicitly expand board members' duties to act in the best interests of the company to include the interests of non-investor stakeholders. The effect is to provide legal authorization to justify decisions that do not serve shareholders' interests. Pennsylvania's directors' duties law, for example, states that "… In determining the best interests of the corporation, a director may consider: (1) the interests of the corporation's shareholders, employees, customers, creditors, suppliers and communities in which it is located; (2) the long-term and short-term interests of the corporation, including the possibility that these interests may be best served by its continued independence… The board shall not be required to consider the interests of any particular group as dominant or controlling…or take other action solely because of the effect it might have on the consideration that might be paid to shareholders in an acquisition."

Fair price (FP) laws – Fair price laws impose restrictions that are similar to fair price provisions adopted by many firms. The law regulates the back-end price in a two-tiered takeover bid or other significant business combination involving a large shareholder. The typical fair price law prohibits business combinations between the firm and a large stockholder unless one of two conditions is met. Either (1) prior approval is granted by a supermajority (e.g., 80%) of all outstanding voting stock and by a supermajority (e.g., two-thirds) of the outstanding stock not held by the interested stockholder; or (2) stockholders receive a stipulated price for the stock acquired by the large stockholder as part of the business combination. The stipulated price is set by a formula that guarantees the price paid will be very high.

Poison pill (PP) laws – Poison pill laws, also known as poison pill endorsement laws, grant firms that are covered by the law the right to adopt poison pill takeover defenses. For example, Virginia's poison pill law authorizes a corporation to "… issue rights, options or warrants for the purchase of shares of the company upon such terms and conditions and for such consideration, if any, and such purposes as may be approved by the board of directors." Poison pill laws can be important because the right to use a poison pill defense is more secure when explicitly authorized by statute, and is less likely to be limited by court action.

Appendix Table A1: Papers that use business combination laws and other state antitakeover law for identification
This table lists in chronological order 46 papers that use state antitakeover laws as all or part of their identification strategies in empirical tests. Complete citations are in the references. For each paper, the table describes the types of law used and the main inference regarding the impact on firms covered by the antitakeover laws. The types of antitakeover laws examined include business combination laws (BC), control share acquisition laws (CS), fair price laws (FP), poison pill laws (PP), and directors' duties laws (DD). Several studies use the Bebchuk and Cohen (2003) index, which is a count variable that increases by one for coverage by BC, CS, FP, PP, and DD laws, and ranges in value from 0 to 5.

	Study	Types of state antitakeover laws used	Main inference regarding the impact of coverage by a state antitakeover law
1	Bertrand and Mullainathan (1999)	BC	Average firm-level wages increase by 1-2%.
2	Garvey and Hanka (1999)	Not precisely specified	Covered firms reduce their use of debt while uncovered firms increase their use of debt.
3	Barnhart, Spivey, and Alexander (2000)	BC, CS, FP, PP	Covered CEOs' performance relative to compensation declines.
4	Bertrand and Mullainathan (2003)	BC	Plant-level wages increase by 2%; plant births and deaths, profitability, and productivity all decline.
5	Cheng, Nagar, and Rajan (2005)	BC, CS, FP	Directors and officers reduce their ownership levels by around 12%.
6	Rauh (2006)	BC, CS, FP; PP case law	Employee ownership in defined contribution plans decline by 0.3-1.0% of the firm's market value.
7	Wald and Long (2007)	Bebchuk-Cohen index	Controlling for selection bias from incorporation decisions, leverage increases.
8	Cheng, Evans, and Nagarajan (2008)	BC, CS, FP	Board size increases.
9	Juks (2008)	BC	Compliance with workplace safety regulation drops, leading to 11% more workplace safety violations.
10	Qi and Wald (2008)	Bebchuk-Cohen index; antigreenmail laws	Firms increase their use of debt covenants that restrict takeovers.
11	Cheng and Indjejikian (2009)	BC, CS, FP	CEO compensation and its sensitivity to a "good luck" component of performance both increase.
12	Francis, Hasan, and Song (2009)	BC, CS, FP	Investment-cash flow sensitivity increases; access to external capital markets and shareholder payout decreases.
13	Huang and Zhao (2009)	BC	CEO turnover and the sensitivity of turnover to performance both increase.
14	Mansi, Maxwell, and Wald (2009)	Bebchuk-Cohen index	There is no effect on credit ratings or bond yield spreads above and beyond the effect of state total asset constraints.
15	Qiu and Yu (2009)	BC	Credit spreads rise by an average of 28 basis points.
16	Yun (2009)	BC, CS, FP; PP case law	Blockholder and institutional ownership and capital structure do not change; cash holdings and unused lines of credit change significantly.
17	Zhao and Chen (2009)	BC	Firms have smaller abnormal accruals and higher levels of earnings informativeness.
18	Francis, Hasan, John and Waisman (2010)	BC, Bebchuk-Cohen index	Bond yields and leverage decrease; stock price reactions and bond values increase.

19	Giroud and Mueller (2010)	BC	Operating performance (ROA) decreases, especially in non-competitive industries.
20	John and Litov (2010)	Bebchuk-Cohen index	Covered firms increase debt and receive higher credit ratings and better financing terms.
21	Afonso (2011)	DD, PP	Covered firms are 5% less likely to receive a takeover bid and the wealth effect of the announcement is 2-3% lower.
22	Becker-Blease (2011)	Bebchuk-Cohen index	The number of patents a firm files decreases.
23	Francis, Hasan, John, and Song (2011)	BC	The likelihood of dividend payments (by 9%) and the size of the dividend ratio (by 2%) decrease.
24	Armstrong, Balakrishnan, and Cohen (2012)	BC, CS, FP, DD, PP, others	Information asymmetry and private information gathering decrease; financial statement informativeness increase.
25	Bharath, Dahiya, and Hallak (2012)	BC, CS, FP	Loan syndicates become less concentrated for covered firms.
26	Huang and Peyer (2012)	BC	Covered firms increase the percentage of outside board members and long-term incentive pay for the CEO.
27	Jon, Li, and Pang (2012)	BC	Operating and stock market performance declines, but only for firms with high free cash flows.
28	Tang (2012)	BC	ROA decreases, but less so for firms in highly competitive industries and industries with high correlation in firm performance.
29	Atanassov (2013)	BC	Corporate innovation, as measured by patents (by 11%) and citations (by 16%) decrease.
30	Cen, Dasgupta, and Sen (2013)	BC	ROA and the ability to attract and retain customers increase for firms in which stakeholder relationships are important.
31	Chemmanur and Tian (2014)	BC, CS, FP, DD, PP	State laws are used as an IV for firm-level provisions; patents and patent citations increase.
32	Jayaraman and Shivakumar (2013)	BC	Demand for asymmetric timeliness of loss recognition increases for debtholders rather than equity holders.
33	John, Knyazeva, and Knyazeva (2013)	BC, CS, FP	Precommitment through dividend payouts and debt, and the ratio of dividends to cash payouts all increase following the passage of state antitakeover laws
34	Kim and Purnanandam (2013)	BC	Price reactions to a seasoned equity offering announcement are nearly 2% lower (more negative) for covered firms.
35	Loderer, Stulz, and Waelchli (2013)	BC	The decline in Tobin's q over time as a firms age is less pronounced in covered firms.
36	Santos (2013)	BC, CS, FP	FP laws increase R&D activities by increasing the expected takeover premium, while CS and BC laws decrease R&D activities by reducing external pressure on management.
37	Sauvagnat (2013)	BC	ROA increases for firms with a high proportion of intangible assets, while low-tangibility firms issue less equity and experience reduced sales and asset growth.
38	Waisman (2013)	BC, CS, FP, DD, PP, others	Covered firms have access to lower cost (by 18 basis points) bank debt, especially in competitive industries (40 basis points).
39	Zhao, Allen, and Hasan (2013)	Bebchuk-Cohen index	After controlling for the self-selection bias of incorporation decisions, voluntary disclosure and stock market liquidity increase.
40	Callen, Guan, and Qiu (2014)	BC	Conditional accounting conservatism increases, especially for firms in less competitive industries and with higher ROA and lower institutional ownership.

41	Grullon and Michaely (2014)	BC	Coverage increases the negative relation between industry concentration and dividend payout.
42	Paligorova and Yang (2014)	BC, CS, FP, DD, PP	The cost of debt decreases, especially in competitive industries.
43	Sapra, Subramanian, and Subramanian (2014)	Bebchuk-Cohen index	Document a U-shaped relationship between state antitakeover laws and innovation.
44	Amore and Bennedsen (2015)	BC	Covered firms produce fewer "green" patents relative to all patents, especially firms with small institutional ownership and greater financing constraints.
45	Amore and Zaldokas (2015)	BC	Following the Canada-US Free Trade Agreement in 1989, ROA dropped by 4% more for firms incorporated in states with BC laws.
46	Gormley and Matsa (2015)	BC	Firms become less risky, including lower stock price volatility, higher cash holdings, and a higher propensity to make diversifying acquisitions.

Appendix Table A2: The effect of *Paramount Communications, Inc. v. Time, Inc.*

This table replicates the results from the full model regressions in Tables 4 and 6 adding a control for the interaction of *Poison pill law* (*PP*) with the 1989 decision in *Paramount Communications, Inc. v. Time, Inc.* For columns 1-7, robust standard errors are clustered at the state of incorporation level (as in Table 4). For columns 8 and 9, robust standard errors are clustered at the firm level (as in Table 6, which replicates the Atanassov (2013) results). Standard errors are reported in parentheses, with *, **, and *** denoting two-tailed significance at the .10, .05, and .01 levels.

	Extensions of the full model results in Table 4:							Extenstion of full model results in Table 6:	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent variable	ROA	CapEx	PPE Growth	Asset Growth	Cash	SGA Expense	Leverage	ln(1+ Pat)$_{t+3}$	ln(1+ Cit/Pat)$_{t+3}$
Business combination law (BC)	**-0.007**	**0.002**	**-0.006**	**-0.042****	**-0.005**	**0.012****	**0.021****	**-0.000**	**-0.003**
	(0.007)	**(0.002)**	**(0.013)**	**(0.019)**	**(0.003)**	**(0.005)**	**(0.009)**	**(0.005)**	**(0.006)**
First-generation law	-0.030*	0.004	0.000	-0.028	0.004	0.017	-0.019	0.001	-0.005
	(0.015)	(0.003)	(0.012)	(0.022)	(0.007)	(0.011)	(0.022)	(0.007)	(0.010)
Poison pill law (PP)	-0.009	0.002	0.006	-0.035*	0.000	0.006	0.021***	-0.007*	-0.003
	(0.007)	(0.002)	(0.009)	(0.020)	(0.002)	(0.005)	(0.007)	(0.004)	(0.005)
Control share acquisition law (CS)	-0.020**	0.002	0.000	0.016	0.008*	0.009	0.019	-0.006	0.027**
	(0.010)	(0.003)	(0.016)	(0.030)	(0.004)	(0.008)	(0.014)	(0.007)	(0.011)
Directors' duties law (DD)	0.002	-0.001	0.011	0.025	0.004	-0.000	-0.004	-0.003	0.001
	(0.009)	(0.001)	(0.011)	(0.024)	(0.004)	(0.008)	(0.011)	(0.007)	(0.006)
Fair price law (FP)	-0.011	0.003	-0.023**	0.012	-0.003	-0.006	-0.006	-0.005	0.008
	(0.010)	(0.002)	(0.011)	(0.021)	(0.003)	(0.007)	(0.013)	(0.007)	(0.007)
CS x *CTS*	-0.001	0.000	-0.019	-0.027	-0.003	0.026***	0.009	0.007	-0.017
	(0.011)	(0.002)	(0.016)	(0.030)	(0.006)	(0.010)	(0.022)	(0.010)	(0.011)
BC x *Amanda*	-0.026	0.001	-0.003	-0.028	0.001	0.017	0.008	-0.009	-0.010
	(0.019)	(0.003)	(0.034)	(0.035)	(0.007)	(0.015)	(0.019)	(0.006)	(0.007)
PP x *Paramount*	-0.003	0.002	-0.038**	0.009	-0.012*	0.024**	0.020	-0.014*	-0.016*
	(0.013)	(0.003)	(0.015)	(0.031)	(0.006)	(0.012)	(0.021)	(0.008)	(0.008)
BC x Motivating firm	0.201***	0.012	0.060	0.142**	-0.041***	-0.112***	-0.055	-0.327**	-0.120**
	(0.048)	(0.010)	(0.036)	(0.056)	(0.013)	(0.040)	(0.039)	(0.144)	(0.050)
Observations	88,310	87,251	79,606	80,649	88,496	81,431	88,283	82,725	82,725
R-squared	0.67	0.54	0.23	0.38	0.65	0.76	0.58	0.84	0.62

Appendix Table A3: Robustness test considering a possible extra-territorial effect of the *Moran v. Household* decision

This table replicates the results from the full model regressions in Tables 4 and 6 using an alternate coding for the *Poison pill law* variable. In these tests, the 1985 *Moran* decision switches *Poison pill law* from 0 to 1 for Delaware firms and from 0 to 0.5 for non-Delaware firms. This differs from the tests in Tables 4 and 6, in which *Poison pill law* is coded as if the *Moran* decision affects only Delaware firms. For non-Delaware firms that subsequently are covered by a state poison pill law, *Poison pill law* switches from 0.5 to 1. For columns 1-7, robust standard errors are clustered at the state of incorporation level (as in Table 4). For columns 8 and 9, robust standard errors are clustered at the firm level (as in Table 6, which replicates the Atanassov (2013) results). Standard errors are reported in parentheses, with *, **, and *** denoting two-tailed significance at the .10, .05, and .01 levels.

		Extensions of the full model results in Table 4:						Extenstion of full model results in Table 6:	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent variable	ROA	CapEx	PPE Growth	Asset Growth	Cash	SGA Expense	Leverage	$ln(1+Pat)_{t+3}$	$ln(1+Cit/Pat)_{t+3}$
Business combination law (BC)	**-0.007**	**0.002**	**-0.005**	**-0.042****	**-0.005**	**0.012****	**0.021****	**-0.000**	**-0.003**
	(0.007)	**(0.002)**	**(0.013)**	**(0.019)**	**(0.003)**	**(0.005)**	**(0.008)**	**(0.005)**	**(0.006)**
First-generation law	-0.030*	0.004	-0.000	-0.028	0.004	0.018	-0.019	0.001	-0.005
	(0.015)	(0.003)	(0.012)	(0.022)	(0.007)	(0.012)	(0.022)	(0.007)	(0.010)
Poison pill law (PP)	-0.020*	0.005*	-0.008	-0.064*	-0.006	0.024**	0.054***	-0.024***	-0.019*
	(0.011)	(0.003)	(0.023)	(0.034)	(0.006)	(0.011)	(0.019)	(0.009)	(0.010)
Control share acquisition law (CS)	-0.020**	0.002	-0.002	0.017	0.008*	0.011	0.020	-0.006	0.027**
	(0.010)	(0.003)	(0.017)	(0.030)	(0.004)	(0.009)	(0.014)	(0.007)	(0.011)
Directors' duties law (DD)	0.002	-0.001	0.013	0.024	0.005	-0.002	-0.005	-0.002	0.002
	(0.009)	(0.001)	(0.011)	(0.024)	(0.004)	(0.008)	(0.013)	(0.007)	(0.006)
Fair price law (FP)	-0.011	0.003	-0.026**	0.013	-0.004	-0.004	-0.004	-0.007	0.006
	(0.010)	(0.002)	(0.011)	(0.020)	(0.003)	(0.008)	(0.014)	(0.007)	(0.007)
CS x *CTS*	-0.001	-0.000	-0.012	-0.028	-0.001	0.022**	0.005	0.010	-0.014
	(0.011)	(0.002)	(0.016)	(0.029)	(0.006)	(0.010)	(0.021)	(0.009)	(0.011)
BC x *Amanda*	-0.027*	0.002	-0.017	-0.024	-0.003	0.026**	0.015	-0.012**	-0.015**
	(0.015)	(0.002)	(0.031)	(0.028)	(0.007)	(0.011)	(0.015)	(0.005)	(0.007)
BC x MF	0.201***	0.012	0.062*	0.141**	-0.041***	-0.113***	-0.056	-0.326**	-0.119**
	(0.048)	(0.010)	(0.036)	(0.056)	(0.013)	(0.040)	(0.039)	(0.144)	(0.050)
Observations	88,310	87,251	79,606	80,649	88,496	81,431	88,283	82,725	82,725
R-squared	0.67	0.54	0.23	0.38	0.65	0.76	0.58	0.84	0.62