

# Do State Antitakeover Provisions Matter?

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## **Abstract**

A longstanding debate over the impact of state antitakeover provisions has emerged among scholars in law and finance. Corporate law practitioners and researchers argue that the second generation of antitakeover statutes were made redundant by the affirmation of rights plans, while empirical scholars have found wide-ranging impacts from their adoption when used as an exogenous shocks to managerial entrenchment. This paper subjects the standard approach used in the empirical literature to a series of straightforward sensitivity analyses, consistent with the present best practice in panel data analysis. Contrary to the majority of published research, there is scant evidence for a consistent and reliable impact of antitakeover statute adoption on common firm outcome measures. These findings are consistent with the legal argument that takeover statutes provide little additional takeover deterrence in the presence of a “shadow pill.”

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# 1 Introduction

Over the past two decades a robust literature in corporate law and finance took shape studying the impact of state antitakeover legislation—most notably business combination, fair price provisions, and control share acquisition statutes—on firm-level outcomes. These provisions, designed largely to encumber hostile takeover attempts, are thought to further entrench a management class that often prioritizes its own interest over those of its shareholders when facing a change of control decision (Macey, 1998). Because coverage by an antitakeover statute ostensibly shifts the balance of power within a firm towards executives and away from shareholders, the staggered adoption of such statutes in the 1980s and '90s has been used extensively to test how emboldened managers change their actions when faced with a decreased likelihood of a successful takeover attempt.

This largely empirical work is part of a broader literature across subfields in economics, finance, and law, devoted to the study of the relationship between managers, shareholders, and stakeholders. A nascent field in industrial organization and antitrust has emerged devoted to the topic of common ownership, and how the network of large institutional investors shape managerial incentives (Anton, Ederer, Gine, and Schmalz, 2020; Azar, Schmalz, and Tecu, 2018; Backus, Conlon, and Sinkinson, 2019). Management scholars have long been consumed with how the level of authority delegated to corporate executives impacts entrepreneurship and strategy (Davis, Greg Bell, Tyge Payne, and Kreiser, 2010; Medcof, 2001). Most importantly for this paper, the fundamental driving question in corporate legal studies since the 1980s has been the cause and consequence of separated management and control. After Jensen and Meckling (1976) developed their influential model of agency frictions within the firm, corporate law has devoted itself to studying the pitfalls of delegating power to managers whose preferences often diverge from those of their shareholder constituents.

Any research on corporate outcomes must grapple with the equilibrium nature of the

measurements used. Perhaps more than any other field in economics, corporate finance and corporate law research is “plagued by endogeneity” (Karpoff and Wittry, 2018). Managers of firms face a dynamic competitive environment, and select their combination of policies and structure subject to these market constraints. As a result, firm attributes and performance are endogenous to their specific market conditions, and inferring causal connections between firm actions and outcomes is difficult, if not often impossible. State antitakeover statutes are thought to provide a “plausibly exogenous shock” to the market environment, by wholesale shifting the structure of managerial power at a level higher than where corporate actions are taken. As a result, researchers have used the passage of antitakeover statutes to study a diverse set of firm outcome measures, from compliance with workplace safety regulations to information asymmetry and investment.

A number of legal scholars raised a conceptual institutional concern with this line of research. A rights plan, colloquially known as a “poison pill”, is the strongest deterrent to a hostile takeover attempt, arguably rendering other forms of takeover deterrence redundant. As a result, once Delaware confirmed the legality of the poison pill in the mid-1980s, the presence of other state-level antitakeover provisions became moot. Moreover, the legal critique notes that many of the published papers use data with clear measurement error and design flaws. One article in particular, Catan and Kahan (2016), replicated three influential papers in the field and showed how each was driven by flaws in the data structure or research design process. However, these concerns were arguably idiosyncratic to the papers being reviewed, and the critique in Catan and Kahan (2016) does not explain the substantial number of significant effects found in the literature, or whether it is reflective of a more pervasive problem with the approach taken across areas of empirical legal research.

In a recently published rejoinder, finance professors Jonathan Karpoff and Michael Wittry purport to partially confirm and partially rebut the legal critique (Karpoff and Wittry, 2018). Many results in the literature are indeed driven by the failure to consider firms’



institutional and legal context, and the effect of an antitakeover provision on takeover protection depends critically on the interaction between multiple firm level defenses. However, they strongly disagree that antitakeover laws provide no incremental takeover protection for firms, citing consequences for managers and directors that invoke a poison pill, and the continued significance of such statutes in many of their regression estimates.

In this paper I undertake a more systematic review of the prior results on antitakeover statutes to determine the magnitude of the disagreement between institutionally and empirically oriented scholarship. While [Catan and Kahan \(2016\)](#) document flaws in a limited number of studies, their paper does not explain how dozens of prior studies consistently found positive impacts from the law. Moreover, they take the general empirical approach used in most prior work as implicitly valid, focusing instead on more specific and contestable arguments about institutional structure and data quality. I instead test whether the basic underlying framework is generating biased results.

Tying the specific study design used in these papers to recent advances in econometrics regarding the pitfalls of using fixed effects regression models with time-varying policy changes, I establish a series of results that further calls into question this line of research. First, even before correcting for problems generated by the staggered nature of antitakeover statute adoption over time, a standard “event-study” model shows that the timing of differences between covered and uncovered firms is often inconsistent with the inferences drawn in prior work. This holds even using identical data and the same underlying model of expected outcomes as in prior work that finds consistent significant effects. When using modern empirical methods designed expressly to address the shortcomings of the standard approach, there is simply no evidence to support the proposition that antitakeover statutes had a material effect on corporate outcomes, consistent with the critique by corporate law scholars.

These findings carry significance outside of the debate over takeover protections and tender offers. In concurrent work, [Baker, Larcker, and Wang \(2020\)](#) document how this

methodological concern impacts a diverse range of findings in law and finance, from the effect of bank deregulation on the income distribution to whether securities enforcement fosters equity ownership. Moreover, such fixed effects panel regressions are used extensively in legal study outside of corporate law, from the law and economics of crime (Ludwig, 1998) to tort reform (Avraham, 2007) and affirmative action (DuBois, 2016). While nothing in this paper is meant to cast doubt on specific prior research, it is consistent with the argument that the rise in empirical legal studies needs to be matched with methodological rigour, especially when being used to influence policy debates (Donohue, 2015).

## 2 History of Antitakeover Provisions

Hostile takeovers are governed by a series of overlapping regulatory provisions at both the state and federal level that have changed substantially over time. Both the empirical and institutional approach to takeover research has emphasized the importance of the presence and interaction of federal and state regulation with firm-level defenses. The following section details the laws that govern takeover defense, including the specific antitakeover provisions that have been the subject of much empirical research.

### 2.1 Federal Oversight

Corporate takeovers are governed by federal law through the Williams Act and the Hart–Scott–Rodino Antitrust Improvements Act (HSR Act). The HSR Act does not address hostile takeovers specifically, but instead provides for general pre-merger notification to the Federal Trade Commission and the Department of Justice, and a mandatory waiting period before transactions can be completed (Baer, 1997). Prior to the Williams Act,

there in fact existed minimal substantive regulation of tender offers,<sup>1</sup> outside of the broadly applicable civil liability provisions in the Securities Acts. In the absence of such regulation, bidders frequently provided little or no information about the purpose of their stock purchases, and, as a result, shareholders deciding whether to tender shares lacked the requisite information to guide their decisions. In addition, bidders commonly gave shareholders a short period of time to make tender decisions, prohibited withdrawals of tendered shares, and used two-tiered transactions<sup>2</sup> which generated a prisoner’s dilemma for target shareholders.<sup>3</sup>

In response to complaints about the deficient information environment surrounding hostile takeovers, and a surge in tender offers in the 1960s, Congress passed the Williams Act in 1968, which is codified in Sections 13(d), 13(e), 14(d), 14(e), and 14(f) of the Securities Exchange Act. Sections 13(d) and 13(e) govern the provision of information on share acquisition by bidders and go-private transactions respectively, while Section 14(d) applies explicitly to tender offers.<sup>4</sup> Section 14(d) requires the disclosure of bidder identity and purpose, offer terms, and bidder’s financial statements (14(d)(1)) and whether the target is negotiating with another “white knight” bidder (14(d)(4)). Crucially it also requires that bidders accept tendered shares on a pro-rata basis, allows shareholders to withdraw their tendered shares while the offer is open, ensures all tendering is paid at an equivalent price, and forces the offer to remain open for twenty business days. The sole purpose of these provisions was to provide information to shareholders in a way that did not create an informational advantage

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<sup>1</sup>While the term “tender offer” is left undefined by the Williams Act, it generally refers to a situation where a bidder seeks to acquire control of a target company by offering to purchase some or all of the outstanding shares of the target firm directly from the target’s shareholders, often at a premium over the prevailing market price ([Easterbrook and Fischel, 1981](#)).

<sup>2</sup>A two-tiered transaction is one where the bidder initially presents a superior tender offer for a limited number of shares of the target to achieve majority control, followed by a inferior offer for the remaining shares once control is achieved. The secondary transaction to “squeeze out” the remaining shareholders usually comes with inferior terms—at either a lower value per share or substituting junk bonds for cash.

<sup>3</sup>Because the terms in the second tier are inferior to the first tier, each shareholder may find it beneficial to tender their shares quickly even if they would all be better off by collectively not tendering.

<sup>4</sup>Section 14(f) mandates disclosure concerning a change in the majority of a target’s board occurring outside of shareholder voting, and Section 14(e) prohibits fraud in the connection of tender offers.

for either bidder or target management.<sup>5</sup> The language of the Williams Act makes clear that it merely confers limited protections to shareholders of target companies, “while disclaiming any intent to strengthen or weaken the hands of bidders or opponents” (Johnson and Millon, 1989).

## 2.2 First-Generation Statutes

Meanwhile, the continued rise in hostile takeovers after passage of the Williams Act spurred state governments to pass legislation expressly designed to suppress takeover activity. Unlike Congress, which declined to speak to the normative implications of the market for corporate control, state legislatures acted on a “perception that hostile takeovers disrupt local economies and harm resident nonshareholders dependent on corporate activity—perceptions that increasingly are explicitly acknowledged in the language and legislative history of these recent statutes” (Johnson and Millon, 1989).

The state law provisions designed to slow takeover activity put in place shortly following the Williams Act are colloquially known as “first-generation statutes.” As noted in Romano (1987), they were enacted across states at a faster rate than any other modification to state corporate law codes at the time. While first-generation statutes contained certain provisions that mirrored those in the Williams Act, focused largely on increased disclosure provisions, they also mandated substantially more extensive disclosures, which were required to be filed with the target and the state securities commissions in advance of any takeover. In addition, some states also required tender offers to be held open for longer periods of time than required under the Williams Act, and others created administrative review of tender offers by state securities regulators. The practical effect of such provisions was to generate lengthy delays for any hostile takeover attempt, and provide target management with ample opportunity to use tactics to foreclose a takeover bid. Most available evidence suggests that first-generation

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<sup>5</sup>S.Rep. No. 550, 90th Cong., 1st Sess., 2 (1967)

statutes raised average cash bids and decreased the quantity of hostile takeovers (Jarrell and Bradley, 1980; Smiley, 1981).

Although first-generation statutes were popular in state legislatures, they quickly encountered judicial opposition. Threats to the constitutionality of first-generation statutes were typically founded on the argument that they violated both the Commerce and the Supremacy Clauses of the Constitution (Snipes, 1983). Court decisions held that the onerous requirements imposed in the statutes “destroyed the delicate balance reached by the Williams Act”, and as a result were preempted under federal law.<sup>6</sup> In addition, because they usually applied to offers made outside of the state, as well as to companies incorporated elsewhere, first-generation laws were often construed as inappropriately regulating interstate commerce. Given that the purpose of first-generation statutes was often explicitly to limit plant closures and related effects on the state’s economy, courts found them in contravention of the holding in *Hood & Sons v. Du Mond* that such statutes could not be enacted “solely for protection of local economic interests”.<sup>7</sup> Ultimately, of the 32 lower courts rulings on the constitutionality of first-generation laws, 17 ruled that at least one provision of a first-generation law was unconstitutional, while 15 upheld the law (Karpoff and Wittry, 2018).

The constitutional question was ultimately resolved in a 1982 Supreme Court decision, *Edgar v. MITE Corp.*<sup>8</sup>. At issue was the Illinois Business Take-Over Act, which required any takeover of a firm for which shareholders located in Illinois owned 10% of the class of equity securities subject to the tender<sup>9</sup> to register the offer with the Secretary of State. The Secretary of State could then call a hearing at any point during the 20-day waiting period

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<sup>6</sup> See *Great W. United Corp. v. Kidwell*, 439 F. Supp. 420 (N.D. Tex. 1977), aff’d, 577 F.2d 1256 (5th Cir. 1978), rev’d on other grounds sub nom. *Leroy v. Great W. United. Corp.* 443 U.S. 173 (1979).

<sup>7</sup> *Id.* at 438 (citing *Hood & Sons v. Du Mond*, 336 U.S. 525 (1949)).

<sup>8</sup> 457 U.S. 624 (1982).

<sup>9</sup> It also covered any target firm meeting at least two of the following three conditions: the corporation had its principal executive office in Illinois, was incorporated in Illinois, or had at least 10% of its stated capital and paid-in surplus represented within the State. *Edgar v. MITE Corp.*, 457 U.S. 624, 627 (1982).

to rule on the “substantive fairness of the offer.” In addition, during the waiting period bidders were restricted from communicating with shareholders, while target management was free to disseminate information concerning the offer. The Court found these provisions to unconstitutionally “frustrate the congressional purpose” of the Williams Act by imposing unnecessary delays in the tender process, and also that it was a “direct restraint on interstate commerce, [with] a sweeping extraterritorial effect.”<sup>10</sup>

## 2.3 Second-Generation Statutes

Although *MITE* struck down the Illinois statute, it plainly did not preempt all state regulation of tender offers due to the Williams Act. Given the Court’s acute unease with the broad jurisdictional reach taken by states and their securities regulators in first-generation statutes, states crafted new legislation that minimized the scope of regulated activity. The statutes passed after the decision, known in the literature as “second-generation statutes”, only covered tender offers of firms incorporated in the state of the law’s passage, and required confirmation only by a firm’s shareholders, and not state regulatory agencies. While second-generation statutes all avoided the specific provisions held unconstitutional in the *MITE* decision, they exhibited substantially more variation in their specific provisions, in what has been called “a striking example of extensive experimentation by states in legal innovation” (Romano, 1987).

Second-generation laws are the subject of the overwhelming majority of empirical papers on the effect of state antitakeover provisions, and thus will be explained more fully here. The first state to pass a second-generation statute was Ohio, which implemented what is now known as a “control share acquisition statute”. Control share acquisition statutes allow a target firm to avert changes in corporate control by restricting voting rights when a person or firm acquires the ownership of an amount of stock over a specified percentage of the

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<sup>10</sup>*Edgar v. MITE Corp.*, 457 U.S. 624, 642 (1982).

company's total voting power, referred to as "control shares". Once an entity crosses the percentage threshold stipulated in the statute, they are no longer able to vote their shares unless the firm's stockholders affirmatively approve the restoration of voting rights by a specific proportion (usually a two-thirds majority excluding interested parties) (SEC, 2020). Control share acquisition statutes align tender offers with the historical requirements for asset purchases and mergers, thus treating different forms of acquisitions similarly (Romano, 1987).

The second form of antitakeover provision to be implemented after *MITE* was Maryland's "fair price law". Fair price laws govern two-tiered acquisitions, and are the statutory equivalent to one of the more common antitakeover defenses implemented in company bylaws and charter amendments. The law prevents business combinations between a target firm and significant stockholder unless certain conditions are met. The offer must either i) receive prior approval from a supermajority (typically 80%) of all outstanding voting shares and a supermajority (e.g., two-thirds) of the stock held by disinterested parties, ii) be approved by a majority of the disinterested board of directors, or iii) stockholders must receive a "fair price", which is typically the higher of the price the bidder paid for its shares or the market price at the time of the combination.(Romano, 1987; Karpoff and Wittry, 2018) This essentially prevents a bidder from receiving any of the benefit of the post-announcement stock price increase.

Other second generation statutes include business combination laws, which impose a waiting period, typically three to five years, for significant asset sales or mergers between large shareholder and targeted firms once a shareholder crosses an ownership threshold. Business combination statutes frequently stipulate that the merger can only be consummated after the waiting period if it also satisfies the fair price provisions, and are often considered the most stringent of the second-generation laws. Directors' duties laws explicitly allow board members to consider the interests of non-shareholder (i.e. stakeholder) interests when

making corporate decisions, and poison pill endorsement laws codify in statute the right of a corporation to adopt a rights plan, also known as a poison pill. Poison pills allow a firm to effectively prevent takeovers by issuing “rights”, usually in the form of a stock dividend for non-bidding shareholders, diluting the position of the bidder and making the cost of a takeover prohibitive. In the time since Ohio passed its control share acquisition statute in 1982, 43 states have adopted at least 157 second generation antitakeover laws. The dates of adoption for each statute are presented in Table II of [Karpoff and Wittry \(2018\)](#), and reproduced in Table [A.1](#) of the Appendix.

Second-generation statutes avoid the specific provisions found by the *MITE* Court to violate the Supremacy and Commerce Clauses, but also faced robust constitutional challenges. For example, [Romano \(1987\)](#) notes that control share acquisition laws had universally been held constitutional upon judicial review. This was eventually resolved by the Supreme Court in a surprise 1987 ruling, *CTS Corp. v. Dynamics Corp. of America*.<sup>11</sup> The case overturned a series of district and circuit court holdings in declaring that Indiana’s control share acquisition did not burden interstate commerce, and in fact furthered the intent of the Williams Act by allowing for more collective decisionmaking in tender offers. The constitutionality of business combination statutes was similarly upheld by the Seventh Circuit in *Amanda Acquisition Corp. v. Universal Foods Corp.*<sup>12</sup>, and the legality of poison pills was established for Delaware corporations<sup>13</sup> in 1985 in *Moran v. Household International, Inc.*<sup>14</sup> and confirmed in 1990 in *Paramount Communications, Inc. v. Time, Inc.*<sup>15</sup>

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<sup>11</sup>481 U.S. 69 (1987)

<sup>12</sup>877 F.2d 496 (1989).

<sup>13</sup>Delaware is far and away the most important jurisdiction for corporate law purposes. The majority of large corporations are incorporated in the state, and there is a commonly held belief that their common law court sets the standard followed by other jurisdictions.

<sup>14</sup>500 A.2d 1346 (Del. 1985).

<sup>15</sup>571 A.2d 1140 (Del. 1990). While *Moran* upheld the right of a Delaware corporation to implement a poison pill in advance to discourage a takeover bid, *Paramount* confirmed the ability to “just say no” to hostile bids without a preconceived plan or alternative transaction.



### 3 Prior Research

The field of corporate governance and corporate law has historically revolved around a narrow set of questions concerning power relations within the corporation and is typically defined by most economists and legal scholars in terms of a defense of shareholder interest (Tirole, 2001). Building upon Jensen and Meckling (1976), a seminal article in finance laying out the theoretic argument for the prevalence of agency costs within the corporation, governance scholars have been occupied by the question of whether managerial self-interest harms shareholders. Given the separation of ownership and control within the firm, managers are believed to take actions that harm shareholders, including engaging in external activities that distract from internal duties, building corporate “empires” that sacrifice profitability for visibility, or by entrenching themselves within the firm. Most trends in governance practice over the past three decades—from the renewed market for corporate control with private equity buyouts and hedge fund activism, to the increased prevalence of equity based compensation—have been implemented with an eye towards minimizing the divergence between managerial and shareholder interests.

Yet, while the agency-theoretic arguments are often clear in their conclusions, evidence supporting their propositions has been decidedly mixed (Klausner, 2013; Kershaw and Schuster, 2019). From removal rights and staggered board provisions in corporate bylaws,<sup>16</sup> to hedge fund activism<sup>17</sup> and board independence,<sup>18</sup> advocates can almost always find empir-

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<sup>16</sup>Compare Cremers and Ferrell (2014) (finding that firms with staggered boards have lower firm values) with Johnson, Karpoff, and Yi (2015) (finding that management insulation measures are instead associated with higher value at the time of initial public offering (IPO)) and Cremers, Litov, and Sepe (2017) (finding “no evidence that staggered board changes are negatively related to firm value”).

<sup>17</sup>Compare Brav, Jiang, Partnoy, and Thomas (2008) (hedge fund activism is associated with increased firm value and operating performance) with deHaan, Larcker, and McClure (2019) (finding no relation between activism and firm value).

<sup>18</sup>Compare Pi and Timme (1993) (finding that cost efficiency and return on assets is lower for firms where the CEO is also the chairman of the board) with Baliga, Moyer, and Rao (2016) (finding only minor evidence that leadership structure and CEO duality matter) and Brickley, Coles, and Jarrell (1997) (arguing that CEO duality is efficient and generally consistent with shareholders’ interests for the typical large firm).

ical support suggesting that the measures are either value destructive or enhancing. While inconclusive findings are part and parcel to observational econometric work, the consistent inconsistency in corporate governance research stands apart from other areas of research. Almost 50 years after [Jensen and Meckling \(1976\)](#), we still have little empirical evidence for the optimal allocation of rights and control within the firm, even taking shareholder value maximization as the correct normative framework.

One reason why solid empirical evidence is so difficult to achieve in corporate governance research is because of the equilibrium nature of market outcomes. Scholars have long recognized that firms respond to their economic environment when making decisions of payout policy, board structure, and governance ([Alchian and Demsetz, 1972](#); [Karpoff and Wittry, 2018](#)). The endogenous relationship between firm characteristics and past and future performance makes attributing causality to specific corporate actions challenging, at least without putting substantially more structure on the research problem. As an example, in an influential paper in governance research, [Gompers, Ishii, and Metrick \(2003\)](#) developed a now commonly-used index (called the G-Index) to test the relationship between firm governance quality and equity prices. This index is meant to serve as a “proxy for the balance of power between shareholders and managers”, and is constructed by counting, at the firm level, the number of 24 specific governance-related provisions implemented by a firm. Even setting aside measurement issues ([Daines, Gow, and Larcker, 2010](#); [Larcker, Reiss, and Xiao, 2015](#)), because firms expressly adopt the provisions in the index, studies risk confusing correlation with causality if, as one would expect, the firms that adopt provisions most heavily are those with high takeover likelihoods ([Karpoff, Schonlau, and Wehrly, 2017](#)).

Recognizing this difficulty in assigning causality to firm-selected governance measures, researchers have actively sought out “exogeneous shocks” to the governance environment. Rather than testing the change in firm performance in response to a firm’s *choice* in governance provision, one should instead focus on changes instituted at a higher level (either

through statutory enactment or judicial decree). Unlike with firm-level defenses, laws passed by a state legislature, or, even better, changes initiated through district court decisions, are plausibly disconnected from firm-specific market conditions. While second-generation laws were often passed at the behest of lobbying by individual local firms (Karpoff and Malatesta, 1995), these firms have been at least partially identified, and there is little evidence of simultaneous lobbying by broader coalitions including organized labor and community groups (Romano, 1987).

### 3.1 Empirical Research on Antitakeover Statutes

In order to study the effect of managerial power, corporate finance research has turned to studying the connection between antitakeover statutes and a range of valuation and productivity measures. Because each second-generation statute ostensibly functions to insulate managers from takeover pressure (also called the market for corporate control), they are used as plausibly exogenous increases in the relative power of management in relation to shareholders and other outsiders. Under standard agency theory, managers who face lower prospects of a credible takeover threat are, all else equal, less likely to engage in efficient management to the benefit of shareholder wealth (Manne, 1965).

One of the first papers to explore this relationship empirically was Garvey and Hanka (1999), which tested the relationship between corporate debt (also called firm leverage) and antitakeover statutes. Based on a series of theoretical results in corporate finance,<sup>19</sup> the authors posit that corporate debt functions to constrain managers who would otherwise prefer to issue less than the value-maximizing amount. Comparing firms incorporated in states that passed antitakeover statutes between 1987 and 1990 to those incorporated in states that did not, the authors find that covered firms substantially reduced their use of debt, and that uncovered firms did the reverse. In a contemporaneous paper, Bertrand and

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<sup>19</sup>Grossman and Hart (1982), Stulz (1990), and Hart and Moore (1995).

Mullainathan (1999) focus exclusively on business combination statutes and, using firm-level data, find that managers covered by the statute raised annual wages for workers by 1-2%.

In an influential follow-up article, Bertrand and Mullainathan (2003) use a more granular plant-level dataset to control for both plant location and firm state of incorporation effects. Again using the adoption of a business combination statute as a shock to managerial entrenchment, the authors find that insulated managers increase worker wages, dismantle fewer old plants, and are associated with lower productivity and profitability. This is argued to be evidence consistent with a “quiet life hypothesis”, wherein insulated managers care more about worker outcomes than shareholders do, but are also more likely to avoid the difficult decisions that drive profitability growth.

Following Bertrand and Mullainathan (2003) a burgeoning interest for studies on the impact of state antitakeover statutes developed. Karpoff and Wittry (2018) list 81 papers that use the passage of second-generation laws as a feature in their identification strategy. These studies either follow Bertrand and Mullainathan (2003) and use the passage of a business combination statute as the shock to governance,<sup>20</sup> use a combination of different second-generation laws,<sup>21</sup> or create an index that counts the number of discrete provisions covering a firm.<sup>22</sup> They have in turn applied the strategy of testing changes across second-generation statute adoption to a wide range of firm-level outcomes, from director and officer ownership levels (Cheng, Nagar, and Rajan, 2005) to credit spreads (Qiu and Yu, 2009) and innovation (Becker-Blease, 2011). While the setting and time periods differ, the studies by and large find that second-generation antitakeover statutes have a significant impact on corporate outcomes.<sup>23</sup>

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<sup>20</sup>Amore and Bennedsen (2016), Atanassov (2013), and Kim and Ouimet (2014).

<sup>21</sup>Barnhart, Spivey, and Alexander (2000), Armstrong, Balakrishnan, and Cohen (2012), and John, Knyazeva, and Knyazeva (2015)

<sup>22</sup>Wald and Long (2007) and John and Litov (2010)

<sup>23</sup>However, two concurrent working papers also show empirical issues with this strand of literature. Spammann (2019) notes that cluster sizes in research using corporate law changes are extremely unbalanced, largely because more than half of all public corporations are incorporated in Delaware, creating issues for

## 3.2 The Academic Legal Critique

In contrast to the growing empirical literature on the impacts of antitakeover laws, legal academics have expressed skepticism about the practical importance of second-generation statutes. [Coates IV \(2000\)](#) argues that poison pills are the most effective takeover deterrent, making a hostile tender offer effectively impossible once implemented. Moreover, pill adoption can usually occur within a single business day, requiring only a board meeting and approval, and lawyers can keep necessary documents held in abeyance. Thus, even firms that have yet to adopt a rights plan are effectively covered by its takeover deterrence, given that a poison pill can almost surely be implemented in the delay induced by the Williams and HSR Act requirements. This latent ability to adopt a poison pill, now frequently called a “shadow pill”, has effectively covered every firm in Delaware since the 1985 *Moran* decision, making tests of the effect of antitakeover measures something of a fool’s errand. While most of his critique dealt with the impact of the shadow pill on firm-specific antitakeover measures, [Coates IV \(2000\)](#) also notes that pill validation “rendered vestigial” state antitakeover laws, because a pill already prevents bids not coupled with proxy fights.

The irrelevance of antitakeover provisions in the presence of a shadow pill was reiterated by [Klausner \(2013\)](#). In a general critique of the state of empirical research in corporate law and governance, Klausner similarly argues that “at the margin, alternative measures can have no causal impact on takeover exposure or firm value” once the Delaware Supreme Court upheld the use of the poison pill. Even without a staggered board, which makes a proxy contest to remove the board (and the pill) substantially more costly, the poison pill has rendered essentially all other defenses moot, including business combination, fair price, and control share acquisition provisions. It might be possible to study firms incorporated

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cluster-robust inference. [Heath, Ringgenberg, Samadi, and Werner \(2019\)](#) caution against the repeated use of a natural experiments in empirical research, using business combination statutes as an example where repeated studies without the use of multiple testing significantly increases the likelihood of false discoveries.

outside of Delaware after *Moran*, but most states are thought to follow Delaware case law, and it would regardless be an “exercise of short-lived utility” given that all states shortly thereafter accepted the legality of the pill.

The most thorough critique of the empirical literature on second-generation statutes was delivered by [Catan and Kahan \(2016\)](#). They argue that the existing empirical work fails to take into account how various provisions interact with each other in practice. The article again stresses how standard antitakeover provisions do not materially alter the legal ability of management to prevent a takeover in the presence of a shadow pill, and also directly explores why studies consistently find evidence for an empirical effect in light of the statutes’ redundancy. [Catan and Kahan \(2016\)](#) document a series of design flaws—from using static measure of firm state of incorporation to ignoring the interaction and overlap among takeover defenses—that seemingly plague the literature. In addition they identify specific design and measurement issues in three studies, which when corrected render the results insignificant. The authors attribute these flaws to the incentives of empirical economists to use readily available policy measures to churn out empirical studies even when conflicting with existing theory. Ultimately, the authors conclude that “decades of empirical studies have yielded little empirical knowledge.”

### **3.3 The Finance Response**

In a recent paper published in *The Journal of Finance*, [Karpoff and Wittry \(2018\)](#) respond to the substantive critiques of the empirical literature made in [Catan and Kahan \(2016\)](#), [Klausner \(2013\)](#) and [Coates IV \(2000\)](#). Building on the legal arguments, they confirm that statistical evidence for the law’s impact on a firm’s takeover protection and outcomes depends on “(i) other state antitakeover laws, (ii) preexisting firm-level takeover defenses, and (iii) the legal regime as reflected by important court decisions. In addition, (iv) state antitakeover

laws are not exogenous for many easily identifiable firms.” Using a common research design and multiple different outcome measures, they find that inferences from many studies change substantially once accounting for firm’s institutional and legal context.

However, while conceding that some prior results were biased by not adjusting for pre-existing defenses and concurrent changes to the legal regime, [Karpoff and Wittry \(2018\)](#) strongly reject the strong form critique that antitakeover statutes are de-facto redundant. They argue that poison pills are costly for managers to implement, and thus even if a shadow pill could effectively prevent all unwanted takeover attempts, in equilibrium they would be less than fully preemptive. Moreover, even correcting for the presence of other statutory enactments and defenses, [Karpoff and Wittry \(2018\)](#) continue to find that the business combination and poison pill statutes have a causal effect on a number of firm-level outcomes. The authors claim that economics is a naturally inductive science, and in light of “the current lack of evidence about which laws and court decisions best identify exogenous shocks to firms’ takeover protections, research in this area may best proceed, at least in the short run, by embracing an inductive approach to empirical inference.” They conclude by “rejec[ing] the notion that all empirical relations between state antitakeover laws and firm outcomes must be spurious.”

## 4 Karpoff & Wittry

We are left with a residual disagreement between scholars who address the issue of anti-takeover statutes from a doctrinal or institutional perspective, and those who instead use an inductive-empirical approach. [Karpoff and Wittry \(2018\)](#) concede that many of the results from the prior literature are likely confounded by unaddressed features of the legal environment, but continue to find a significant effect for antitakeover statutes across a number of measures. [Catan and Kahan \(2016\)](#) find it implausible that any of the results are truly

causal, but directly explain away the results from only a handful of studies. At the same time, a series of papers in econometrics have documented that the standard method used in empirical studies of second-generation statutes—called a “difference-in-differences” (DiD) design—suffers from limitations when the policy adoption is staggered across time, as is the case here. In the following sections I will explore whether more general flaws in the empirical approach taken in the literature can explain this disagreement, above and beyond the already documented conceptual concerns.

## 4.1 Difference-in-Differences

Most studies of the impact of second-generation statutes use a DiD design, which is one of the more commonly used methods for identifying causal effects in applied research.<sup>24</sup> The intuition behind DiD is best understood through the simplest example, which uses a single policy adoption or treatment, two discrete time periods—pre- and post-treatment—and two treatment groups, one receiving treatment (“treated”) and one not (“control”). In this simple 2x2 design, the effect of the treatment on the outcome of interest can be estimated by simply comparing the change in the average outcome in the treated units to the change in the average outcome in the control units.

As a clarifying example, imagine that a researcher seeks to test the impact of the resolution of a circuit split on the number of filings (assume for this hypothetical that plaintiffs have no discretion in *where* to file their case). In this stylized example, that there are only two circuits in the country—the 7th and 9th Circuits. Before the Supreme Court issues its ruling, the 7th Circuit allows for plaintiff-friendly expedited discovery for a category of case, while the 9th Circuit does not. The Supreme Court holds that expedited discovery is warranted, thus resolving the split in favor of plaintiffs. Under certain assumptions the

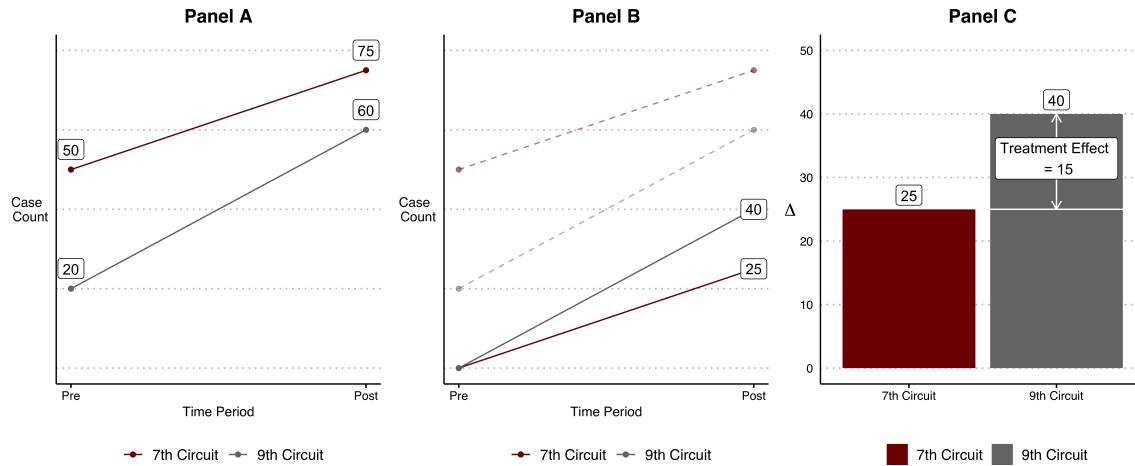
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<sup>24</sup>For a longer treatment on the DiD design, see [Angrist and Pischke \(2009\)](#); [Goodman-Bacon \(2019\)](#); [Callaway and Sant’Anna \(2020\)](#)



researcher can estimate the causal effect of the decision on filings in the 9th Circuit (where the standard changed) using difference-in-differences.

**Fig. 1**  
Basic DiD Example



Note: Figure 1 graphically represents the DiD analysis for this stylized example. Panel A shows the trends in case filings between circuits in both the pre- and post-decision periods. Panel B shows how the first difference works—for both the treated (9th Circuit) and untreated (7th Circuit) units, we subtract the pre-decision case count to get a measure of differences over time. In Panel C we calculate the differences-in-differences—under the assumption that the 7th and 9th Circuits would have had similar trends in case filings without the Supreme Court decision, this can be thought of as the causal impact of the holding.

Figure 1 presents the schematics of how a DiD analysis would calculate the effect of the decision in this case. Panel A shows a hypothetical set of trends in case filings by circuit for the periods before and after the Supreme Court issues its decision. Initially, there is a higher level of filings in the 7th Circuit (50) than the 9th Circuit (20), consistent with the prevailing plaintiff-friendly discovery rules. In the post-decision period both circuits see an increase in filings, but there is a larger change in the 9th Circuit, which increases from 20 to 60, while the 7th Circuit only increases from 60 to 75.

Under the assumption that both circuits would have experienced a similar change in

filings over time in the absence of the Supreme Court ruling,<sup>25</sup> a researcher can generate an estimate of the causal effect of the decision using a simple double-differencing technique. First, they would find the change in case filings between the post- and pre-decision period for each circuit, which is visualized in Panel B of Figure 1. The residual difference between the treated (9th) and untreated (7th) circuits, shown in Panel C, is thus an unbiased estimate of the effect of the law under general assumptions. In this example we see that the Supreme Court decision led to fifteen more case filings in the 9th Circuit than would have occurred without the holding.

This “treatment effect” can also be estimated through the use of a linear regression model. If we define a treatment indicator variable,<sup>26</sup>  $TREAT_i$ , to be set equal to one for the 9th Circuit and zero for the 7th Circuit, and another,  $POST_t$ , to be one in the post-decision period and zero before, then we can recover the same treatment effect from the coefficient on  $\delta$  in:

$$y_{it} = \alpha + \beta_1 TREAT_i + \beta_2 POST_t + \delta (TREAT_i \times POST_t) + \epsilon_{it} \quad (1)$$

Here  $y_{it}$ , our outcome variable, is the number of cases filed in period  $t$  for circuit  $i$ . The advantage of the regression-based DiD model is that it provides an estimate for the treatment effect  $\delta$ , as well as a measure of its statistical uncertainty (called the standard error) when there are multiple units per group and/or multiple time periods. In settings where there are more than two units and two time periods, the regression DiD model is most often modified as:

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<sup>25</sup>This is known in the literature as the “parallel trends assumption.” In addition to this assumption, to generate a causal interpretation from the DiD one must also assume that there is no anticipation of the ruling by plaintiffs, and that there are no spillovers from the 9th Circuit to the 7th Circuit as a result of the change in discovery rules.

<sup>26</sup>An indicator variable is a binary measure that equals one when a condition is satisfied and zero when it is not.

$$y_{it} = \alpha_i + \gamma_t + \delta^{DD} D_{it} + \epsilon_{it} \quad (2)$$

Here,  $\alpha_i$  and  $\gamma_t$  are firm and time “fixed effects”, which are a series of indicator variables that identify the unit or the time period,<sup>27</sup> and  $D_{it}$  is an indicator variable that is set to one only in the periods after the treatment (here the decision) and only for units that receive the treatment (here the 9th Circuit). The variables for  $TREAT_i$  and  $POST_t$  are subsumed by the unit and time fixed effects. This model is called the two-way fixed effects estimator and is pervasive in the applied economics literature, where it frequently includes the use of additional control variables (called covariates).

## 4.2 The Karpoff & Wittry Results

In the following sections I test the sensitivity of the empirical results in [Karpoff and Wittry \(2018\)](#) to recently proposed design considerations with the use of data that measures units over multiple periods (called panel data). I focus on the primary results of the paper, which are located in their Table IV. The authors examine the impact of second-generation laws (with a focus on business combination statutes) for seven different outcome variables that have been explored in prior published papers: return on assets (ROA), capital expenditures (Capex), growth in plant, property, and equipment (PPE growth), growth in firm assets (Asset Growth), selling, general and administrative expense (SG&A expense), and debt (Leverage). In order to match the sample period in [Bertrand and Mullainathan \(2003\)](#) and [Giroud and Mueller \(2010\)](#) they use annual firm data from 1976 to 1995, excluding financial firms and utilities that face separate takeover regulations.

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<sup>27</sup>For example if the analysis also included the 2nd Circuit, then there would be three indicators for whether the firm is in the 7th Circuit, 9th Circuit, or 2nd Circuit. Because of how linear regression functions, only two of these can be included in the regression model, while the third will be the reference unit. Including time and unit fixed effects controls for the fact that different units (circuits) and different time periods may have unobserved differences that are reflected in the level of the outcome variable.

The model used to test the impact of antitakeover provisions on firm outcomes is just a minor variant of Equation 2:<sup>28</sup>

$$y_{it} = \alpha_i + \gamma_{lt} + \eta_{jt} + \delta BC_{it} + \Omega X_{it} + \epsilon_{it} \quad (3)$$

Here  $y_{it}$  is one of the seven outcome measures listed above, each measured at the firm  $i$ , year  $t$  level. Instead of having only two levels of fixed effects (e.g. just firm and year), the authors use a higher dimensional model, which along with firm fixed effects ( $\alpha_i$ ), controls for headquarter state-year ( $\gamma_{lt}$ ) and industry-year ( $\eta_{jt}$ ) effects.<sup>29</sup> The main analysis in their paper follows the bulk of the literature and focuses on business combination statutes in particular—thus  $BC_{it}$  is an indicator variable set to one when a firm is incorporated in a state with a business combination statute, and zero otherwise. The regression coefficient on  $\delta$  is now assumed to be the average effect of the adoption of a business combination statute on the outcome variable.

The authors also control for a set of variables measured at the firm-year level,  $X_{it}$ , which vary with the regression specification. In what they label their “short regression model”, [Karpoff and Wittry \(2018\)](#) follow [Giroud and Mueller \(2010\)](#) in controlling for firm size and age.<sup>30</sup> Their “full model” corrects for some of flaws noted in [Catan and Kahan \(2016\)](#) by including, in addition to the short regression model controls, other variables that reflect the regulatory and legal environment of the firm. These include whether the firm is covered by the other common second-generation statutes (poison pill laws, control share acquisition

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<sup>28</sup>Note that [Karpoff and Wittry \(2018\)](#) do not refer to their model as a difference-in-differences, although they motivate their tests based on other papers which do (e.g. [Bertrand and Mullainathan \(2003\)](#) and [Giroud and Mueller \(2010\)](#)) who do.

<sup>29</sup>It is more common in the literature to use two levels of fixed effects. [Karpoff and Wittry \(2018\)](#) claim that their high dimensional model “decrease[s] the likelihood of finding a significant coefficient” on second-generation laws, which is actually only true under certain assumptions. Because I am looking modify their approach as little as possible I stick with their choice of fixed effects structure.

<sup>30</sup>This includes the natural logarithm (log) of firm age, the square of the log of firm age, the log of firm size, as measured in assets, and the square of the log of firm assets.

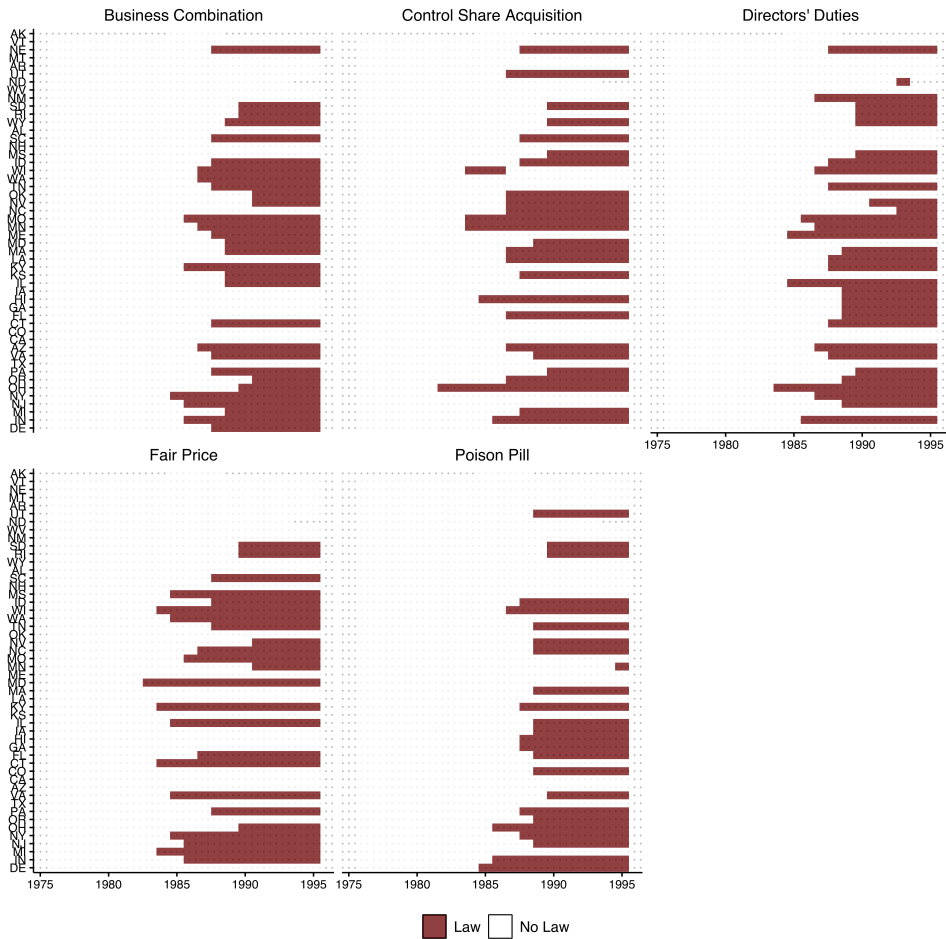
statutes, directors duties laws, and fair price statutes) and whether the firm is covered by a first-generation statute (only applicable to the pre-*MITE* period). The full model also controls for whether there is a business combination statute after the *Amanda* decision or a control share acquisition law after *CTS*, when the legality of the statutes were affirmed. Finally, it includes a control for corporations that were active in lobbying for a business combination statute’s passage, to adjust for the clearly endogenous impact for those firms.<sup>31</sup> The standard errors are clustered at the level of the state of incorporation to adjust for correlation in the errors.

Before reporting the results for the short and full regression models, it is useful to visualize the distribution of policy adoption across states. DiD models exploit the difference in outcomes across treated and untreated units, as well as the differences across treatment timing groups—i.e. the comparisons of firms that begin coverage by a business combination statute in 1990 or 1992. Figure 2 below reports the coverage of firms incorporated in different states by each of the five second-generation statutes. It is evident that most of the adoptions occur in the mid 1980s and that there is reasonable dispersion in the timing across states. In addition, because states often adopt multiple statutes simultaneously, there is some correlation across laws *within* a state.

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<sup>31</sup>Karpoff and Wittry (2018) note that most other papers drop these firms from the sample.

**Fig. 2**  
Statutory Adoption Timing



Note: Figure 2 presents the timing of adoption for each second-generation law across states in the data from [Karpoff and Wittry \(2018\)](#). Gray squares signify state of incorporation years with no law in place, maroon squares reflect when the law covers firms, and empty squares represent years with no observations for that state of incorporation.

The empirical results from [Karpoff and Wittry \(2018\)](#) Table IV are replicated in Table 1 below.<sup>32</sup> The table confirms findings from prior studies that business combination statutes are associated with statistically significant changes in a range of outcome variables when the legal and institutional environment is omitted from the model. For each outcome variable,

<sup>32</sup>I thank Professors Karpoff and Wittry for generously sharing their data and code used in their study.

the coefficient on the presence of a business combination is statistically significant at at least the 10% confidence level in the short regression model. However, once controls for multiple second-generation statutes and court decisions are included (the “full model”), the results only hold for leverage, SG&A expenses, and asset growth. There is some evidence that the passage of a business combination statute was important after the *Amanda* decision for firm ROA and SG&A expenses, and that poison pill laws are associated with statistically significant decreases in ROA, and increases in SG&A expenses and leverage. Overall, these results are presented as confirmation that, “a firm’s institutional and legal context has first-order effects in tests that use state antitakeover laws for identification”, but that “one would have to maintain unrealistically strong priors to therefore infer that all previous results are spurious” (Karpoff and Wittry, 2018).

**TABLE 1**  
Replication of Table IV from Karpoff-Wittry (2018)

	Dependent Variable													
	(1) ROA		(2) Capex		(3) PPE Growth		(4) Asset Growth		(5) Cash		(6) SGA Expense		(7) Leverage	
	Short Regres- sion	Full Model	Short Regres- sion	Full Model	Short Regres- sion	Full Model	Short Regres- sion	Full Model	Short Regres- sion	Full Model	Short Regres- sion	Full Model	Short Regres- sion	Full Model
Business combination law (BC)	-0.017*	-0.008	0.003**	0.002	-0.016**	-0.008	-0.044*	-0.041**	-0.008***	-0.005	0.018**	0.011**	0.023**	0.020**
First-generation law		-0.030**		0.004		-0.001		-0.026		0.004		0.017		-0.019
Poison Pill law (PP)		(0.015)		(0.003)		(0.012)		(0.022)		(0.007)		(0.012)		(0.021)
Control share acquisition law (CS)		-0.011**		0.002		-0.005		-0.027		-0.002		0.012**		0.027***
Directors' duties law (DD)		(0.005)		(0.002)		(0.012)		(0.019)		(0.003)		(0.005)		(0.009)
Fair price law (FP)		-0.021**		0.002		-0.003		0.018		0.008*		0.011		0.020
CS x CTS		(0.010)		(0.003)		(0.017)		(0.029)		(0.004)		(0.008)		(0.014)
BC x Amanda		0.002		-0.001		0.010		0.022		0.005		-0.004		-0.007
BC x MF (motivating firms)		(0.009)		(0.001)		(0.011)		(0.023)		(0.004)		(0.009)		(0.012)
Observations		-0.009		0.003		-0.017		0.014		-0.003		-0.001		0.000
R2		(0.010)		(0.002)		(0.013)		(0.025)		(0.003)		(0.008)		(0.013)
		0.000		0.000		-0.012		-0.029		0.000		0.021**		0.004
		(0.011)		(0.002)		(0.016)		(0.029)		(0.006)		(0.010)		(0.020)
		-0.026*		0.002		-0.016		-0.024		-0.003		0.026**		0.015
		(0.015)		(0.002)		(0.031)		(0.027)		(0.007)		(0.011)		(0.015)
		0.201***		0.012		0.060*		0.142**		-0.041***		-0.114***		-0.057
		(0.048)		(0.010)		(0.036)		(0.056)		(0.013)		(0.040)		(0.039)
Observations	88,307	88,307	87,248	87,248	79,603	79,603	80,646	80,646	88,493	88,493	81,428	81,428	88,280	88,280
R2	0.603	0.604	0.438	0.438	0.053	0.053	0.247	0.247	0.575	0.575	0.706	0.706	0.485	0.485

Note: Table 1 replicates the estimates from Table IV of Karpoff and Wittry (2018). The table reports results of the impact of business combination laws on seven different outcome variables, and tests for the effects of including controls for other types of states, court decisions, and motivating firms. The outcome variables are ROA, Capex, PPE growth, asset growth, cash, SGA expenses, and leverage. ROA is EBITDA divided by total assets, Capex, PPE, cash, and SGA expenses are scaled by total assets; PPE and asset growth are the percentage change in PPE and total assets, respectively; and leverage is 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. All regressions include firm, state-year, and industry-year fixed effects. Robust standard errors are clustered at the state of incorporation level. \*, \*\*, and \*\*\* denote two-tailed significance tests at the 10%, 5%, and 1% levels, respectively.



## 5 Testing Model Robustness

In light of the continued disagreement between legal<sup>33</sup> and finance scholars regarding the materiality of state antitakeover provisions, this section explores whether the disconnect can be explained by data quality concerns and methodological challenges induced by using standard DiD designs with staggered policy adoption. In other words, it may be more than just a philosophical disagreement about feature-based attributes of the pre-existing studies; rather the now-ubiquitous use of standard fixed-effects DiD designs in corporate governance and other areas of legal studies simply may not provide credible estimates of the impacts of the laws.

Section 5.1 tests whether data concerns with the commonly-used financial database Compustat affect the DiD estimates. While the analysis in [Karpoff and Wittry \(2018\)](#) fix many of the design flaws from the literature described in [Catan and Kahan \(2016\)](#), they do not address certain important critiques about the use of stale identifying information. Section 5.2 extends the static DiD estimates from Table 1 to an “event-study” design, which tests whether the timing of the differences between covered and un-covered firms is consistent with a causal story for the results being driven by the adoption of antitakeover statutes. Finally, Sections 5.3 and 5.4 incorporate recent work in the econometric literature about the use of fixed effects models to test for causality with staggered treatment adoption. I use stacked regression and semi-parametric methods, designed explicitly to avoid some of the more problematic attributes of fixed effects DiD methods, to re-test the analyses.

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<sup>33</sup>To be clear, I do not allege that corporate legal scholars universally agree that state antitakeover provisions are clearly redundant. For example, Professor Steven Davidoff Solomon uses a slightly different research design to find that fair price provisions *have* reduced hostile takeovers ([Cain, McKeon, and Solomon, 2017](#)). Nothing in this paper is meant to call into question those results, which rely on a different method and sample.

## 5.1 Data Quality

In this section I explore whether data quality issues drive the results found in the prior literature. [Karpoff and Wittry \(2018\)](#) address a number of concerns raised in the legal critique—most importantly by including additional variables to control for the legal environment, and by using unit and time-level fixed effects, the exclusion of which [Catan and Kahan \(2016\)](#) found to bias other prior studies. However, other criticisms of the methods used in the empirical literature are left unaddressed.

First, and most importantly, [Catan and Kahan \(2016\)](#) raise an issue with the financial database Compustat that compiles and reports annual and quarterly firm financial information, and is used in the majority of papers in empirical finance that test the impact of antitakeover statutes. For certain identifying attributes—including the state of headquarters and incorporation—Compustat reports what they call “header information”, which contains the *most recently available* value for a given firm identifier. Compustat typically then back-fills this information to all prior observations for a given firm, meaning that the information will be inaccurate for some firms in prior years. This is particularly problematic for the state of incorporation, which can change for a given firm and would impact the variables that identify coverage by a second-generation statute.<sup>34</sup>

To test the importance of data quality on this area of study, I re-estimate the models from Table 1 on four different but related datasets. The first dataset—Data (1)—is that used in [Karpoff and Wittry \(2018\)](#) and which produces the published results. I construct Data (2) in an identical manner, but use the most recent publicly available download from Compustat. The only difference between Data (1) and Data (2) should be changes induced by firms changing their headquarter location, state of incorporation, or industry designation, between the time that [Karpoff and Wittry \(2018\)](#) downloaded the data for their study and

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<sup>34</sup>This is a known issue that impacts numerous areas of research using commonly available financial datasets ([Spamann and Wilkinson, 2020](#); [Jennings, Kim, Lee, and Taylor, 2020](#)).

today.<sup>35</sup>

Data (3) partly corrects for the measurement error in Compustat. Since the advent of the SEC’s EDGAR system in 1994, this identifying information has been stored electronically on the internet. Using publicly available “scrapes” of the filings, I allow for changes in the state of incorporation, headquarter location, security identifier, and industry classification, in addition to coding law coverage using fiscal year filing date rather than calendar year.<sup>36</sup> The data construction process, including notes of all differences from the data construction process used in [Karpoff and Wittry \(2018\)](#), are explained in detail in Appendix C.

Finally, in Data (4) I make two additional research design changes that I believe are warranted. First, rather than including industry-year fixed effects based on a firm’s 3-digit SIC industry classification as done in [Karpoff and Wittry \(2018\)](#), I reduce the dimensionality by instead assigning each firm to one of 48 industries identified by Eugene Fama and Ken French ([Fama and French, 1997](#)). There is substantial measurement error in the industry classification—indeed identifying the SIC code from EDGAR filings, Compustat, and CRSP (a repository of firm stock prices) consistently leads to different values. Assigning each firm to a broader industry definition allows us to still control for industry-level trends without imposing as much structure on the data and fitting thousands of additional nuisance parameters. Finally, I also follow the standard in the literature and do not recode the law indicator variables for firms that opt out of coverage. This is an endogenous choice made by a narrow set of companies, and isn’t called for under the standard assumptions of a DiD design.

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<sup>35</sup>Note that Compustat also occasionally fixes other data quality issues or changes in firm reporting that may occur. The data used in this paper was downloaded from Compustat on January 18, 2021.

<sup>36</sup>Compustat reports financial information based on firm fiscal year, and different firms have different fiscal year end dates. It is more appropriate to match second-generation law changes to the time period in which the data is measured—which calls for using fiscal rather than calendar year dates in the merging process.

**TABLE 2**  
Comparison of DiD Estimates on Leverage Across Data and Models

	Leverage							
	Short Regression Model				Full Model			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Business combination law (BC)	0.023** (0.010)	0.044*** (0.016)	0.015*** (0.006)	0.018*** (0.007)	0.020** (0.008)	0.030** (0.013)	0.015** (0.006)	0.015** (0.006)
First-generation law					-0.019 (0.021)	-0.023 (0.030)	0.003 (0.009)	-0.001 (0.008)
Poison Pill law (PP)					0.027*** (0.009)	0.035*** (0.012)	0.027*** (0.007)	0.026*** (0.007)
Control share acquisition law (CS)					0.020 (0.014)	0.036** (0.018)	0.023 (0.015)	0.006 (0.014)
Directors' duties law (DD)					-0.007 (0.012)	-0.003 (0.018)	-0.008 (0.008)	-0.011 (0.007)
Fair price law (FP)					0.000 (0.013)	-0.012 (0.016)	0.006 (0.008)	0.009 (0.009)
CS x CTS					0.004 (0.020)	0.007 (0.026)	-0.017 (0.017)	0.000 (0.016)
BC x Amanda					0.015 (0.015)	0.050* (0.027)	0.000 (0.010)	0.006 (0.010)
BC x MF (motivating firms)					-0.057 (0.039)	-0.185** (0.076)	-0.075** (0.037)	-0.081** (0.033)
Observations	88,280	88,050	90,014	90,014	88,280	88,050	90,014	90,014
R2	0.485	0.441	0.521	0.518	0.485	0.441	0.522	0.518

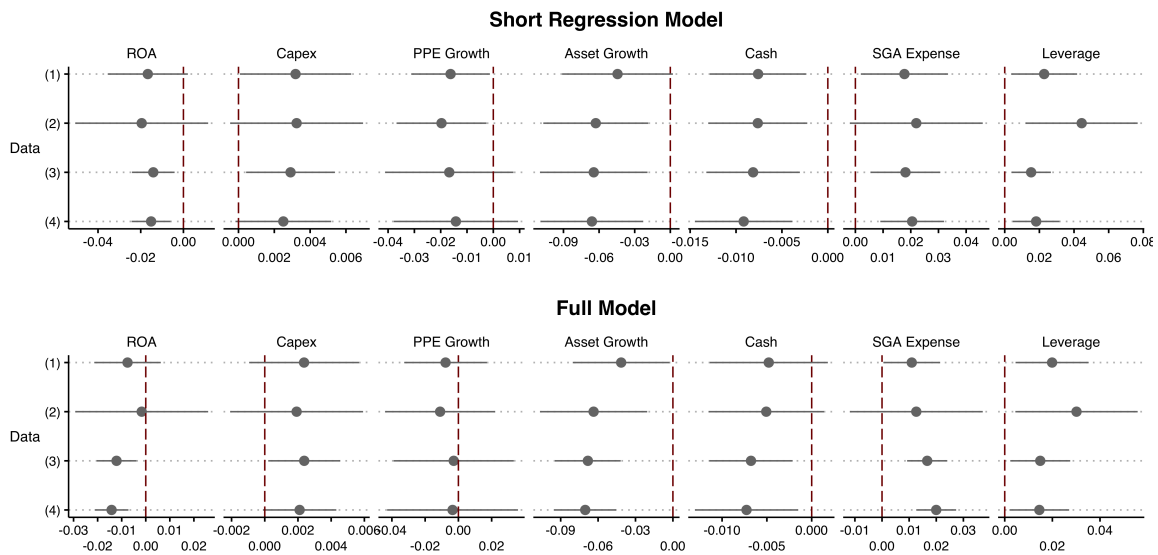
Note: Table 2 presents the DiD estimates with leverage as the outcome variable for both the short and full regression models in Table IV of [Karpoff and Wittry \(2018\)](#) using our modified datasets. Column (1) presents the results with the published data from [Karpoff and Wittry \(2018\)](#); Column (2) reports data constructed using the same code but with today's value of header information; Column (3) reports results that correct for the use of stale identifying header information; and Column (4) reports results with corrected data and which use the Fama-French 48 industry definitions and don't adjust for firms that opt out of coverage.

For illustrative purposes, Table 2 reports the differences in the DiD estimate for the short and full regression models for each dataset with firm leverage as the outcome variable. While the results do change on the margin across data constructions, the conclusions are broadly similar. The results for the full replication of Table 1 for Data (2), (3), and (4) are reported in appendix tables [A.2](#), [A.3](#), and [A.4](#).

In Figure 3 I report the estimate and 95% confidence interval for the coefficient on the presence of a business combination law statutes for each variable/specification/dataset combination. Across modeling and data choice, the DiD estimates are generally fairly close. The standard error of the estimate usually increases from Data (1) to Data (2), which is

consistent with the identifying information becoming more stale and inaccurate as we look further back into the past. There is little evidence that the data quality concerns raised by [Catan and Kahan \(2016\)](#) are driving the effect from the empirical studies. In fact, using the preferred full model from [Karpoff and Wittry \(2018\)](#), we see that after correcting for data and design issues (Data (4)), five of the seven coefficients on the business combination indicator variable in the preferred full model are significant, suggesting that the prior results may in fact be *more* robust than alleged in their paper.

**Fig. 3**  
Coefficient Estimate on Business Combination Law Across Datasets



Note: Figure 3 presents the coefficient estimate and the standard error from the basic DiD model in [Karpoff and Wittry \(2018\)](#) applied to the original and modified datasets. Row (1) presents the results with the published data from [Karpoff and Wittry \(2018\)](#); Row (2) reports data using the same code but with today's value of header information; Row (3) reports results that correct for the use of stale identifying header information; and Row (4) reports results with corrected data and which use the Fama-French 48 industry definitions and don't adjust for firms that opt out of coverage. The top panel reports the results from the short model that controls for measures of firm age and size, while the bottom panel reports the full model with controls for the legal environment.

## 5.2 Event Study Designs

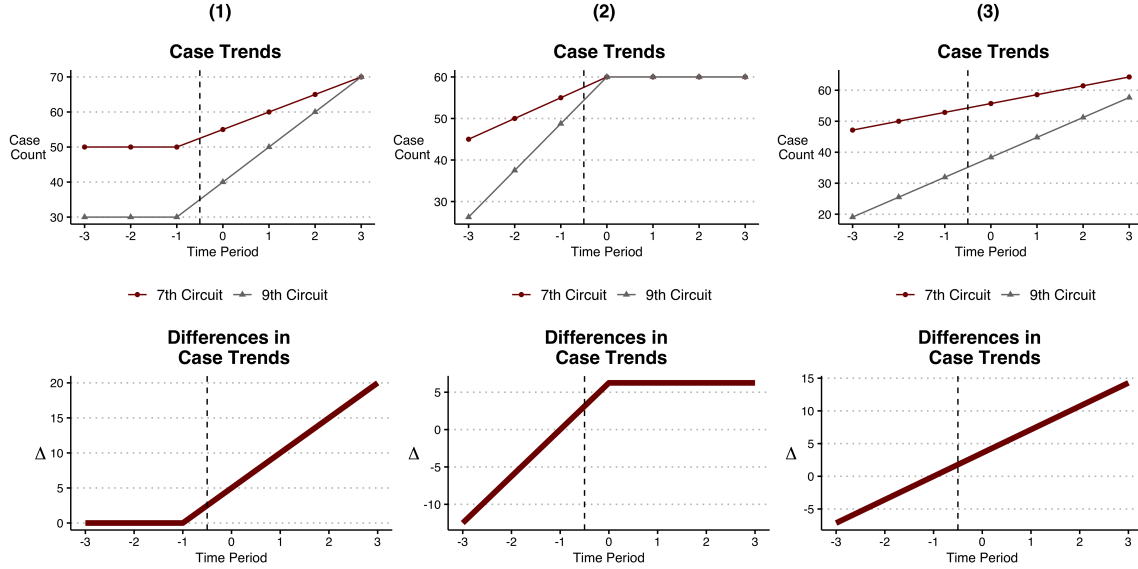
In this section I extend the analysis from the static fixed effects DiD design in Table 1 to what the applied empirical literature calls an “event study” approach (Autor, 2003). The results from the standard DiD only capture (at best) the difference between treated and untreated units before or after policy adoption. However, because analyses frequently use data with many time periods, the same average difference between post- and pre-adoption can occur in many different ways, only some of which are consistent with a causal explanation for the policy being studied. The advantage of an event study DiD is that it breaks down the average difference captured in  $\widehat{\delta^{DD}}$  from Equation 2 into the differences between treated and control units at each relative time period in relation to when treatment coverage begins.

To better understand this point, consider again the stylized example from Section 4.1, which tests the impact of a Supreme Court decision on filing rates. Assume now that we have multiple periods of data, from three years prior to three years following the decision. The top panel of Column (1) in Figure 4 reports case trends that are consistent with the causal story—the 7th and 9th Circuits had similar trends before the timing of the decision (the gray vertical line), while after the decision the 9th Circuit saw a larger increase in case filings. The bottom panel of Column (1) shows the treatment effect path recovered from an event-study DiD, reflecting a positive treatment effect that begins after the ruling.

The top panel of Column (2) shows a different pattern in case trends. Here the higher rate of increase in case filings for the 9th Circuit occurs *before* the Court issues its decision, and the treatment effect path in the bottom panel is now increasing in the periods before the ruling, but constant afterwards. Finally, Column (3) provides an example with no discontinuity, but where the two circuits simply have different trends in cases, irrespective of the Court’s ruling. Notably, each of these three examples would give *the exact same* treatment effect estimate using the standard, static DiD estimate from Equation 2, demonstrating the importance of

reporting the treatment effect path when conducting the analysis.

**Fig. 4**  
Basic Event Study DiD Example



Note: Figure 4 presents three examples of data which generate the same estimate using fixed effects DiD, but which have very different interpretations. The top panels show the number of cases over time in the two circuits, while the bottom panel shows the difference after removing circuit and time period fixed effects. The vertical line represents the period when the decision is made. In Column (1), there is the standard process one would expect for a causal estimate—the two groups (circuits) have similar trends before the treatment (the decision), but diverge afterwards. In Column (2), the entirety of the change occurs *before* the decision, which would tend to make it less credible that the decision was indeed driving the DiD estimate. Finally, in Column (3) there are constant increasing trends of different slope between circuits, with no evidence of any change around the time of the decision.

To test the robustness of the empirical results to treatment effect dynamics, I conduct event study estimates for the impact of both business combination and poison pill laws (which [Karpoff and Wittry \(2018\)](#) found to be of comparable significance to business combination statutes). For each of the laws I now estimate the modified regression model:

$$y_{it} = \alpha_i + \gamma_{lt} + \eta_{jt} + \sum_l \delta_l \mathbb{1}\{t - E_i = l\} + \Omega X_{it} + \epsilon_{it} \quad (4)$$

Here again  $y_{it}$  represents one of our seven outcome variables measured at the firm-year level, the fixed effect terms  $\alpha_i$ ,  $\gamma_{lt}$ , and  $\eta_{jt}$  are firm, headquarter state-year, and industry-year effects respectively, and  $X_{it}$  is a vector of controls that vary by model. Now, instead of just one second-generation statute variable, each law is represented by a series of indicator variables that reflect whether a firm is  $l$  years away from beginning coverage by the statute. Following [Borusyak and Jaravel \(2017\)](#) and [Sun and Abraham \(2020\)](#), I omit two relative time indicators to avoid perfect collinearity—the most negative possible value of  $l$  and  $l = -1$ —as is standard in the literature. In addition, I drop all firms that enter Compustat already covered by the statute at interest, as for these firms the relative time indicators are undefined.<sup>37</sup>

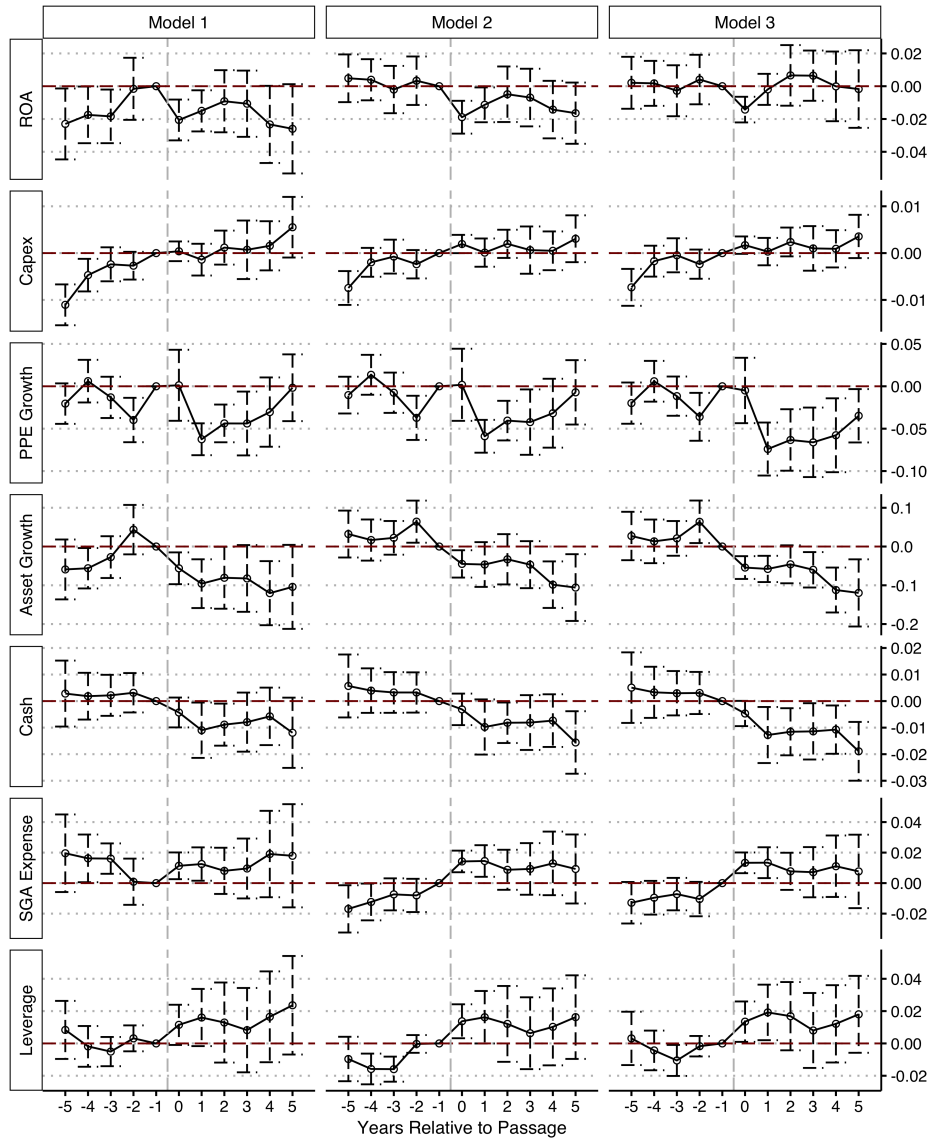
The results of this analysis for business combination statutes is presented in Figure 5. Each plot represents the event study estimates for the relative time periods ranging from five years prior to five years post adoption for each dependent variable/model combination. Model 1 is the classical DiD analysis without the inclusion of any covariates; Model 2 includes the control variables from [Karpoff and Wittry \(2018\)](#)’s short regression model; and Model 3 includes the set of controls in their full model. The dependent variable used in each plot is identified by row. Because we omit the relative time indicator for the year prior to adoption ( $l = -1$ ), the other coefficients are the deviations from the difference in that year. The circle represents the point estimate for each coefficient, and the error bars represent the 95% point-wise confidence intervals.

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<sup>37</sup>Note that these observations are already inconsistent with a standard DiD design, which requires taking averages both before and after the treatment occurs. While studies frequently keep such observations in the data, inference in such designs is more akin to a selection on observable argument, which typically requires stricter assumptions ([Imbens, 2004](#)). However, this does lead to a different sample of observations than in the original analysis. In unreported results I confirm that the estimates using the standard fixed effects DiD are very close using the same methodology as in [Karpoff and Wittry \(2018\)](#) Table IV, but dropping firms that enter Compustat covered by a BC law. Thus, the differences between the event study and static DiD models are not driven by the change in sample composition.



**Fig. 5**  
Event Study Estimates - Business Combination Statutes



Note: Figure 5 reports the event study estimates and 95% confidence interval of the impact of business combination law changes using the data and design changes described in Section 5.1. Model 1 includes only the fixed effects without any covariates, Model 2 includes the covariates in the short regression model from [Karpoff and Wittry \(2018\)](#), and Model 3 includes their “full model”.

The inferences drawn from these estimates are decidedly different from those in the static

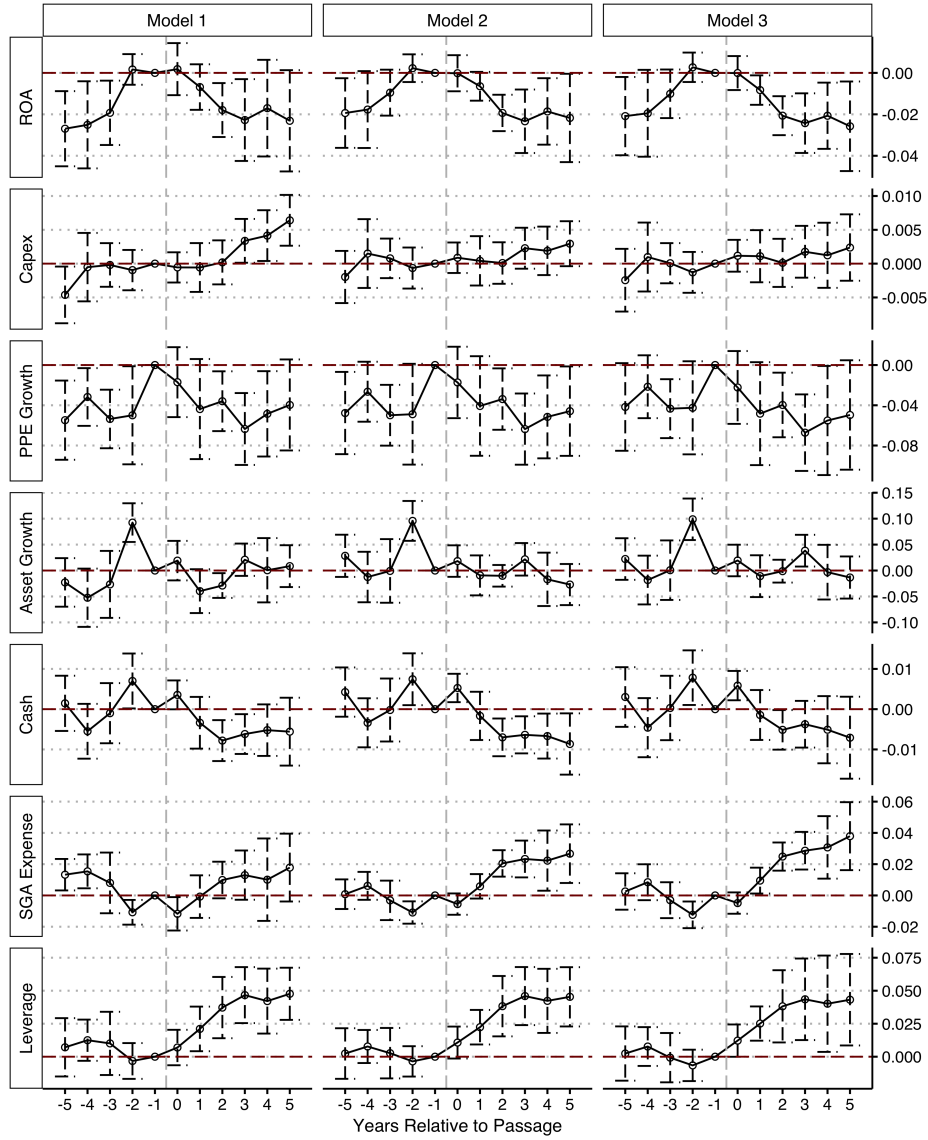
DiD estimates reported in Table 1 and Figure 3.<sup>38</sup> There is little consistent evidence of an identified treatment effect in the 11-year window around business combination law adoption for ROA or capital expenditures. There is perhaps some evidence for a decrease in PPE Growth following the adoption of business combination laws, although Karpoff and Wittry (2018) and our replication found no evidence using standard TWFE design for this outcome. For the remaining variables, some results are consistent with an effect, although in each model there is also either evidence of pre-trend differences between treatment and control firms, and/or the confidence intervals almost always include zero (the maroon line). Thus, even before addressing issues with staggered designs, assessing the dynamics of the treatment effects suggests that the prior results were less robust than reported. The same estimates using Karpoff and Wittry (2018)'s data<sup>39</sup> are reported in Figure B1 of the appendix, where there is effectively no evidence at all of a significant effect for business combination statutes on any variable once taking into consideration treatment effect dynamics.

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<sup>38</sup>The event study results using the data as reported in Karpoff and Wittry (2018) are reported in Figures B1 and B2. Using their data there is in fact little consistent evidence across any of the variables for business combination statutes.

<sup>39</sup>As a reminder, the difference between the data used in Figure 5 and the data used to produce the published estimates in Karpoff and Wittry (2018) is that my data corrects for change in state of incorporation and other header information, and uses the Fama-French industry designations rather than 3-digit SIC codes.

**Fig. 6**  
Event Study Estimates - Poison Pill Laws



Note: Figure 6 reports the event study estimates of the impact of poison pill law changes using the data and design changes described in Section 5.1. Model 1 includes only the fixed effects without any covariates, Model 2 includes the covariates in the short regression model from [Karpoff and Wittry \(2018\)](#), and Model 3 includes their “full model”.

The corresponding results for poison pill laws are reported in Figure 6. The only change made to the estimation is that, with the full model, I include controls for the presence of

a business combination law and an indicator for firms that lobby for passage of the poison pill statute (rather than for the business combination laws). The only evidence here that is consistent with a standard causal story is that poison pill laws may have increased leverage and perhaps SG&A expenses. It should be noted however that this effect—an increase in firm leverage following the entrenchment of management—is actually the *opposite* of the prediction in most agency-theoretic models.

### 5.3 Stacked Regression Designs

To this point the robustness of the prior empirical results have only been tested against standard critiques of difference-in-differences analysis. Section 5.1 shows that data and design concerns with the empirical method used in [Karpoff and Wittry \(2018\)](#) impact the results but not the inference from their paper; indeed the evidence now more strongly suggests that antitakeover measures impact corporate outcomes than before these changes were made. When event study design is used in place of standard fixed effects DiD models, the trends in differences in outcome variables complicates the causal story. Most prior results appear to be either 1) non-existent in the window surrounding the law’s adoption, when the impact would be expected to appear, 2) driven by pre-existing trend differences between firms receiving and not receiving coverage, and/or 3) substantially noisier in the relevant time window.

In the following two subsections I explore an additional concern about the use of DiD methods in the presence of staggered policy adoption. A large and growing literature in econometric theory has now established that the generalized version of DiD estimation with staggered rollout in treatment timing (e.g., the staggered adoption of an antitakeover statute across states of incorporation) is often invalid for the estimation of the usual treatment effect estimands of interest ([Athey and Imbens, 2018](#); [Strezhnev, 2018](#); [Callaway and Sant’Anna, 2020](#); [Goodman-Bacon, 2019](#); [Borusyak and Jaravel, 2017](#); [Imai and Kim, 2020](#)). [Baker et al.](#)

(2020) provide an overview of this literature, and document how the specific econometric concerns at issue substantially changes a diverse set of prior results in law, finance, and accounting.

Of most relevance for our setting, firms that already received coverage by an antitakeover statute act as the comparison units for later-treated firms when using the standard fixed effects approach, and a portion of their treatment effect will be subtracted from the DiD estimate if there is time-variation in its impact (Goodman-Bacon, 2019). This in turn biases the estimates towards zero, and, depending on the weights and effect patterns for different treatment groups, may cause the estimated effect to even be of the wrong sign. In other words, even if the adoption of antitakeover provisions was a valid exogenous shock to managerial entrenchment, and even if the true treatment effect was positive for *every* firm, the DiD estimate could be negative if the effect varies over time within a firm. A number of different methodological approaches have been advanced in the literature to address this concern, with “stacked regression” being the most similar in design and spirit to the fixed effects methods used in the literature on antitakeover provisions.<sup>40</sup>

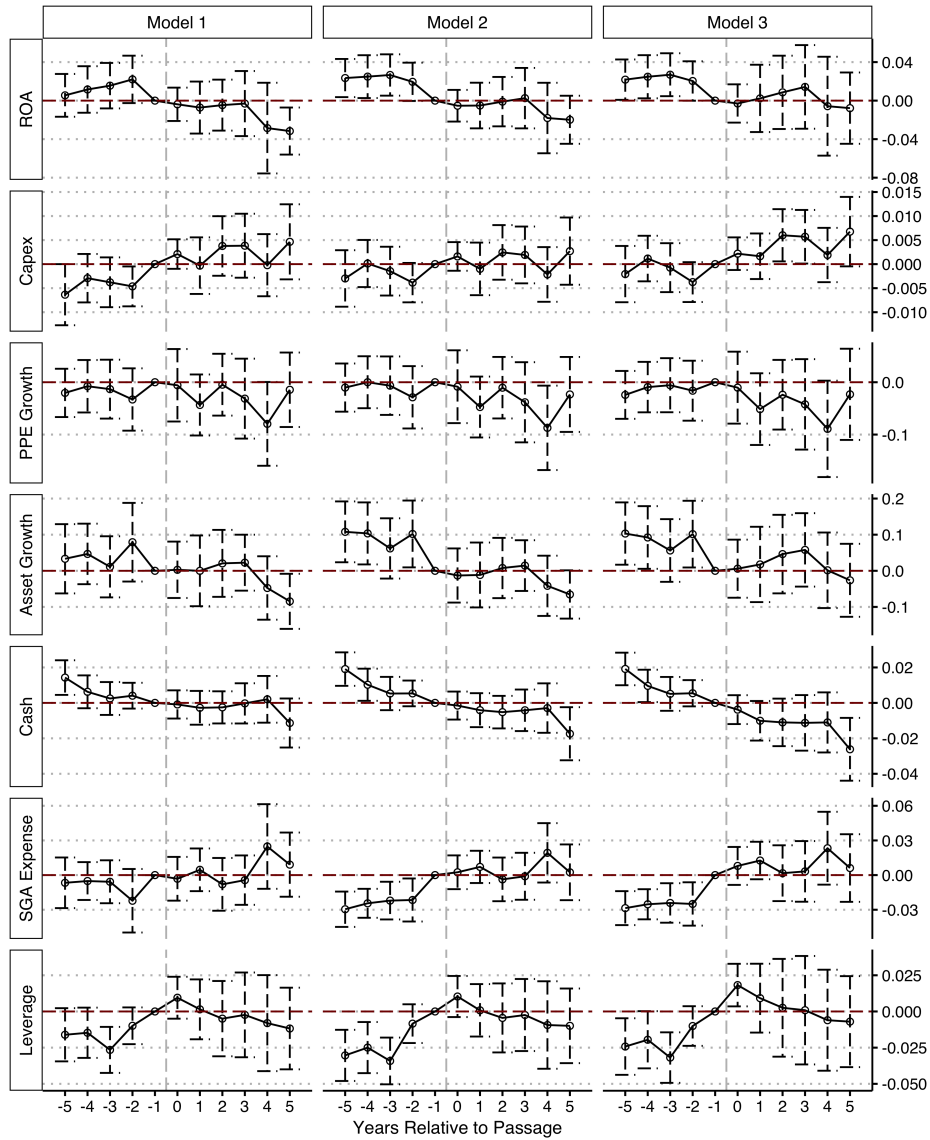
The stacked regression approach is a simple extension to the regular event study DiD design. In our setting it involves:

1. Identifying treatment cohort years where a state of incorporation adopts a statute,
2. Creating a smaller dataset that contains i) all firms incorporated in states that adopt in that cohort year, and ii) all firms that satisfy a set of conditions to be included as a valid control,
3. Restricting the years to a common window around the adoption year, here the five years prior to and the five year following adoption,
4. Stacking each of the cohort-specific datasets in relative time, and
5. Estimating the event study DiD model on this stacked dataset, making sure to interact the fixed effects with an identifier for the cohort-specific data.

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<sup>40</sup>Stacked regression is used in Cengiz, Dube, Lindner, and Zipperer (2019) and Deshpande and Li (2019).

**Fig. 7**  
Stacked Regression Event Study Estimates  
Business Combination Statutes



Note: Figure 7 reports the stacked regression event study estimates and 95% confidence interval of the impact of business combination law changes using the data and design changes described in Section 5.1. Model 1 includes only the fixed effects without any covariates, Model 2 includes the covariates in the short regression model from [Karpoff and Wittery \(2018\)](#), and Model 3 includes their “full model”.

By explicitly controlling which firms are allowed to enter as effective comparison units

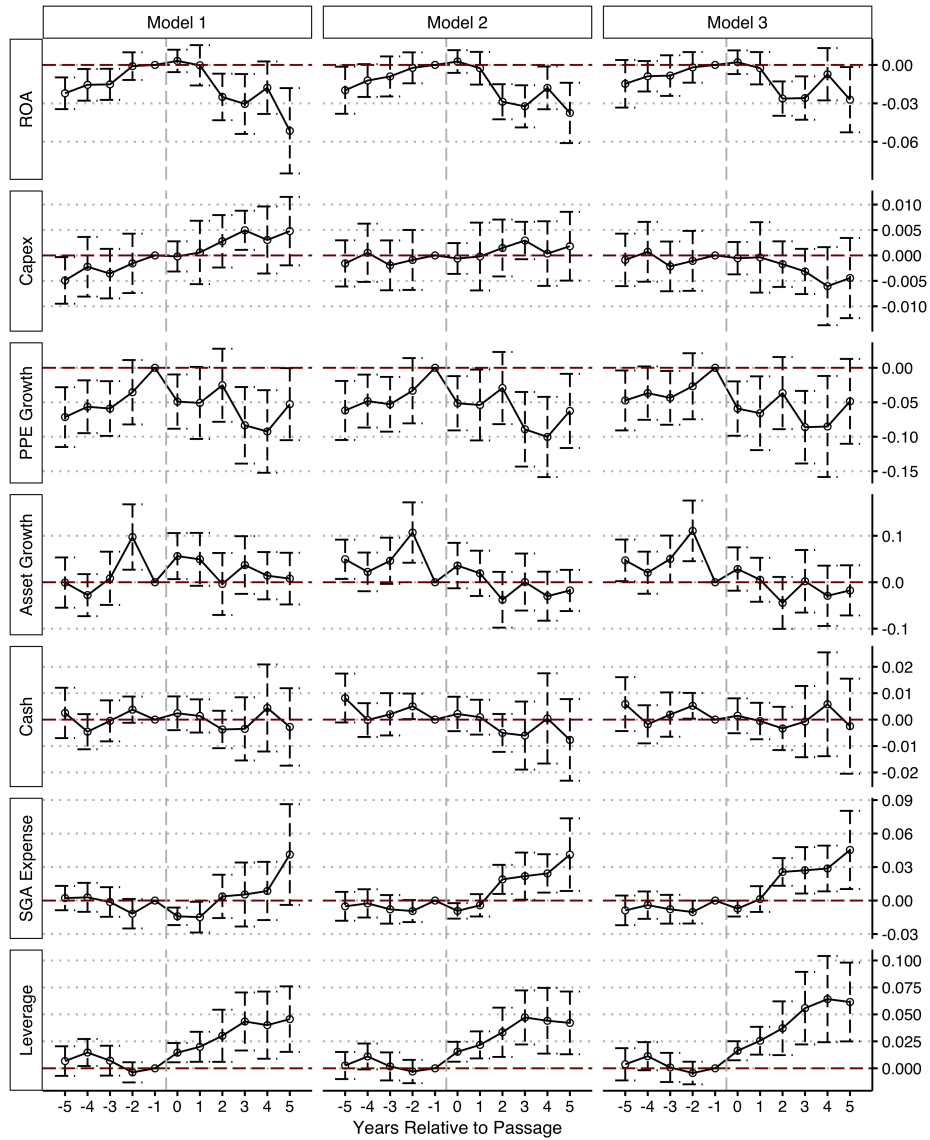
in the analysis, stacked regression models prevent prior-treated firms from being used as controls, which generates most of the bias identified in the literature. The results for the stacked event study model for the adoption of business combination statutes are presented in Figure 7. After correcting for this particular form of potential bias there is now little evidence consistent with a causal relationship between the passage of business combination statutes. Nearly all of the estimates, regardless of included control variables, are centered around zero, signifying no impact of the law, and when not the results are generally driven more by trends in the pre-adoption period.

A similar set of results is reported for the stacked regression estimates of poison pill law adoption in Figure 8.<sup>41</sup> There is some evidence consistent with an increase in both leverage and SG&A expenses for firms incorporated in states with a poison pill law, as well perhaps a short-term decrease in ROA, above and beyond what can be explained by any set of control variables used in [Karpoff and Wittry \(2018\)](#). This is not necessarily inconsistent with the legal critique, as those papers stress the primacy of the poison pill defense, and [Catan and Kahan \(2016\)](#) call for the study of poison pill validation statutes. It should be noted again, however, that at least as pertains to firm leverage, these results cut against the majority of established corporate finance theory that has considered how managerial entrenchment and corporate debt interact.

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<sup>41</sup>The equivalent plots using the data in [Karpoff and Wittry \(2018\)](#) are reported in appendix figures [B3](#) and [B4](#) for business combination and poison pill laws respectively.

**Fig. 8**  
Stacked Regression Event Study Estimates  
Poison Pill Laws



Note: Figure 8 reports the stacked regression event study estimates of the impact of poison pill law changes using the data and design changes described in Section 5.1. Model 1 includes only the fixed effects without any covariates, Model 2 includes the covariates in the short regression model from [Karpoff and Wittry \(2018\)](#), and Model 3 includes their “full model”.

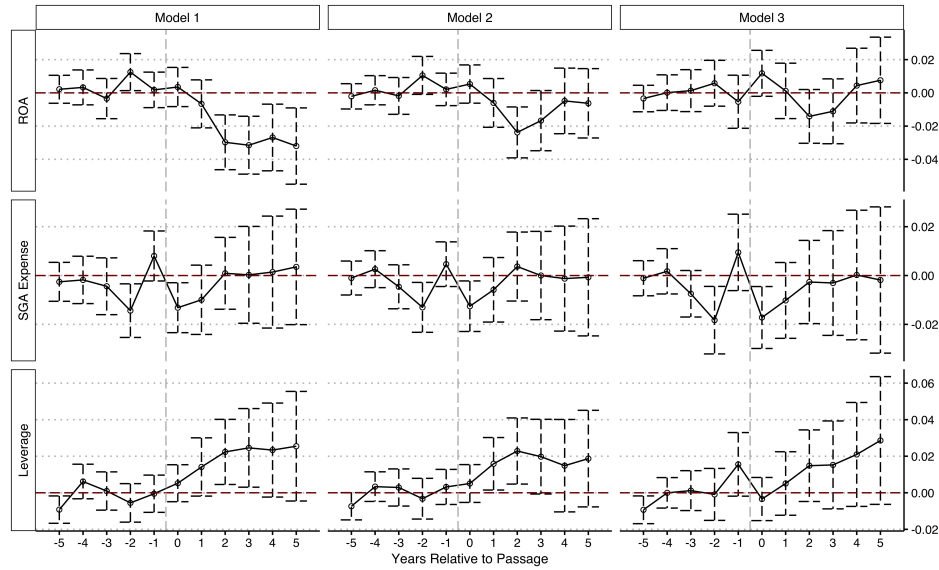


## 5.4 Callaway and Sant’Anna Estimator

Finally, I use the semi-parametric estimator from [Callaway and Sant’Anna \(2020\)](#) (CS estimator) as the last test of whether state antitakeovers are associated with changes in firm-level outcomes in a DiD framework. For brevity, I focus here only on the adoption of poison pill laws and three outcome variables—ROA, SG&A Expenses, and Leverage—where there still exists some evidence for an effect using the stacked regression approach.

The CS estimator shares certain properties with the stacked regression approach, with a few minor but important differences. Similar to stacked regressions, it also allows for a tailored decision rule on selecting control firms to ensure that prior-treated firms are not used subsequently as control units. However, it uses a straightforward aggregation approach to weight the estimates by treatment cohort size, rather than relying on the often undesirable features of regression-based weighting with fixed effects. In addition, when implementing the CS estimator I use only pre-treatment observations of the covariates, which avoids the concern of including “bad controls” in the DiD analysis ([Angrist and Pischke, 2009](#)). Both the short and full regression models in [Karpoff and Wittry \(2018\)](#) use time-varying controls—for firm size, age, and legal environment—and to the extent that the levels of these covariates are impacted by the policy adoption, could lead to mechanically biased results. A fuller explanation of the CS estimator and its implementation here is presented in Appendix E.

**Fig. 9**  
Callaway & Sant’Anna Estimates  
Poison Pill Laws



Note: Figure 9 reports the event study estimates of the impact of poison pill law changes using the estimator from Callaway and Sant’Anna (2020) and the data and design changes described in Section 5.1. Model 1 includes only the fixed effects without any covariates, Model 2 includes the covariates in the short regression model from Karpoff and Witty (2018), and Model 3 includes their “full model”.

The event study estimates from the CS estimator are reported in Figure 9 above. Using straightforward weighting of different time effects and restricting the use of confounded control variables attenuates the results. There is now little evidence of any effect of poison pill statute adoption on ROA or SG&A expenses, although the trend in leverage after passage is consistent with a small subsequent increase.

## 6 Conclusion

A series of empirical papers established a strong link between the adoption of state antitakeover statutes and a host of firm-level outcome variables, consistent with agency-

theoretic models of corporate finance. The staggered nature and supposed exogeneity of the laws proved a fruitful tool for empirical researchers looking for changes in the relative power of management and shareholders. However, a powerful critique of this research developed from corporate law scholars, who argued that the widely available “shadow” poison pill render such statutes legally and practically irrelevant. [Karpoff and Wittry \(2018\)](#) acknowledge aspects of the legal critique, but disagree with the “extreme view” that “any empirical result linking an outcome variable to the adoption of a state antitakeover law must be spurious.”

In this paper I demonstrate that the alleged inconsistency between the legal argument and the empirical results is largely a mirage. Nearly every paper in the empirical literature uses some variant of the standard fixed effect difference-in-differences design, which when used in its most simple form has been shown to suffer from severe limitations in settings similar to the antitakeover debate. Subjecting the standard approach to simple extensions—including studying the dynamics of the purported treatment effects around adoption, and implementing methods that restrict potentially confounded control firms—results in no evidence of any consistent effect of second-generation antitakeover provisions on commonly used outcome measures.

On narrow grounds, the results in this paper support the arguments provided in [Coates IV \(2000\)](#), [Klausner \(2013\)](#), and [Catan and Kahan \(2016\)](#) that antitakeover states are redundant, or vestigial, to the presence of the shadow pill. There is in reality very little empirical evidence of any effect to counter this claim, although proving a negative with empirical methods is always impossible. On the substantive merits, the conceptual rejoinder in [Karpoff and Wittry \(2018\)](#) seems porous. Even if true that managers and directors suffer career consequences when invoking a poison pill to prevent a hostile takeover, the potential defenses provided through antitakeover statutes similarly require recalcitrant management to “just say no” to a bidder’s offer. It is hard to imagine why investors would punish managers for actions that result in the exact same outcome through different provisions of our securities

laws.

These results also speak to a more general concern in fields of legal research that now frequently rely on empirical evidence to generate or buttress legal and institutional arguments. In [Baker et al. \(2020\)](#) we address the broader implications of using standard difference-in-differences analysis with staggered treatment adoption in the study of law and finance. Across a wide range of research topics we find evidence that this approach often leads to misplaced interpretations when studying the effect of changing policies. In light of the rising influence of empirical study in both legal research and policy making over the past two decades, this study reaffirms the need to match empiricism with a dedication to methodological rigour and an understanding that empirical evidence should support robust institutional knowledge, not displace it.

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## Appendix A: Tables and Figures

**TABLE A.1**  
Second-Generation State Antitakeover Laws

State	CS	BC	FP	DD	PP
Alabama					
Alaska					
Arizona	1987-07-22	1987-07-22	1987-07-22	1987-07-22	
Arkansas					
California					
Colorado					1989-03-31
Connecticut		1988-06-07	1984-06-04	1988-06-07	2003-06-26
Delaware		1988-02-02			
Florida	1987-07-02		1987-07-02	1989-06-27	1989-06-27
Georgia		1988-03-03	1985-03-27	1989-04-10	1988-04-07
Hawaii	1985-04-23			1989-06-07	1988-06-17
Idaho	1988-03-22	1988-03-22	1988-03-22	1988-03-22	1988-03-22
Illinois		1989-08-02	1985-08-23	1985-08-23	1989-08-02
Indiana	1986-03-05	1986-03-05	1986-03-05	1986-03-05	1986-03-05
Iowa		1997-05-02		1989-06-01	1989-06-01
Kansas	1988-04-14	1989-04-10			
Kentucky		1986-03-28	1984-04-09	1988-07-15	1988-07-15
Louisiana	1987-06-11		1984-07-13	1988-07-10	
Maine		1988-04-06		1985-06-21	2002-04-08
Maryland	1989-04-11	1989-04-11	1983-06-21	1999-05-13	1999-05-13
Massachusetts	1987-07-21	1989-07-18		1989-07-18	1989-07-18
Michigan	1988-03-19	1989-05-24	1984-05-24		2001-07-23
Minnesota	1984-04-25	1987-06-25	1991-05-02	1987-06-25	1995-08-01
Mississippi	1990-03-15		1985-03-29	1990-04-04	2005-04-20
Missouri	1984-06-13	1986-06-23	1986-06-23	1986-05-06	2005-08-28
Montana					
Nebraska	1988-04-08	1988-04-08		1988-04-08	
Nevada	1987-06-06	1991-06-25	1991-06-25	1991-06-25	1989-06-21
New Hampshire					
New Jersey		1986-08-05	1986-08-05	1989-02-04	1989-06-29
New Mexico				1987-04-09	
New York		1985-12-16	1985-12-16	1987-07-23	1988-12-21
North Carolina	1987-05-13		1987-04-23	1993-07-24	1989-06-08
North Dakota				1993-04-12	
Ohio	1982-11-18	1990-04-11	1990-04-11	1984-07-11	1986-11-22
Oklahoma	1987-06-24	1991-04-09			

**TABLE A.1**  
Second-Generation State Antitakeover Laws (*continued*)

State	CS	BC	FP	DD	PP
Oregon	1987-07-18	1991-04-04		1989-03-05	1989-03-05
Pennsylvania	1990-04-27	1988-03-23	1988-03-23	1990-04-27	1988-03-23
Rhode Island		1990-07-03	1990-07-03	1990-07-03	1990-07-03
South Carolina	1988-04-22	1988-04-22	1988-04-22		1998-06-09
South Dakota	1990-02-20	1990-02-20	1990-02-20	1990-02-20	1990-02-20
Tennessee	1988-03-11	1988-03-11	1988-03-11	1988-03-11	1989-05-29
Texas		1997-05-28		2003-05-29	2006-01-01
Utah	1987-05-29				1989-03-13
Vermont				1998-04-16	2008-06-06
Virginia	1989-02-22	1988-03-31	1985-03-24	1988-03-31	1990-04-02
Washington		1987-08-11	1985-05-13		1998-03-23
West Virginia					
Wisconsin	1984-04-18	1987-09-17	1984-04-18	1987-06-09	1987-09-17
Wyoming	1990-03-20	1989-03-11		1990-03-09	2009-07-01

This table lists the adoption dates for control share acquisition statutes (CS), business combination statutes (BC), fair price laws (FP), directors' duties laws (DD), and poison pill laws (PP), as reported in Table II of [Karpoff and Wittry \(2018\)](#). Overall 153 different second-generation laws were passed by a total of 43 states.

**TABLE A.2**  
Replication of DiD Results With Data (2)

	Dependent Variable													
	(1) ROA		(2) Capex		(3) PPE Growth		(4) Asset Growth		(5) Cash		(6) SGA Expense		(7) Leverage	
	Short Regres- sion	Full Model	Short Regres- sion	Full Model	Short Regres- sion	Full Model	Short Regres- sion	Full Model	Short Regres- sion	Full Model	Short Regres- sion	Full Model	Short Regres- sion	Full Model
Business combination law (BC)	-0.020 (0.016)	-0.002 (0.014)	0.003* (0.002)	0.002 (0.002)	-0.020** (0.009)	-0.011 (0.017)	-0.063*** (0.023)	-0.064*** (0.022)	-0.008*** (0.003)	-0.005 (0.003)	0.022* (0.012)	0.013 (0.013)	0.044*** (0.016)	0.030** (0.013)
First-generation law		-0.046*** (0.018)		0.002 (0.003)		-0.004 (0.015)		-0.030 (0.028)		0.002 (0.007)		0.030** (0.014)		-0.023 (0.030)
Poison Pill law (PP)		-0.022** (0.009)		0.002 (0.001)		-0.010 (0.015)		-0.032* (0.018)		-0.002 (0.003)		0.020** (0.010)		0.035*** (0.012)
Control share acquisition law (CS)		-0.034* (0.018)		0.001 (0.003)		-0.018 (0.021)		0.044 (0.028)		0.008* (0.004)		0.027* (0.014)		0.036** (0.018)
Directors' duties law (DD)		0.018 (0.017)		0.000 (0.001)		0.010 (0.013)		0.014 (0.025)		0.004 (0.004)		-0.019 (0.018)		-0.003 (0.018)
Fair price law (FP)		-0.010 (0.019)		0.001 (0.002)		-0.020 (0.013)		0.017 (0.019)		-0.003 (0.003)		-0.006 (0.017)		-0.012 (0.016)
CS x CTS		-0.004 (0.016)		0.001 (0.003)		0.009 (0.018)		-0.053* (0.027)		0.001 (0.006)		0.041*** (0.016)		0.007 (0.026)
BC x Amanda		-0.052*** (0.018)		0.003 (0.002)		-0.015 (0.035)		-0.019 (0.030)		-0.003 (0.006)		0.044*** (0.015)		0.050* (0.027)
BC x MF (motivating firms)		0.340*** (0.089)		0.012 (0.010)		0.066* (0.037)		0.234*** (0.071)		-0.048*** (0.012)		-0.248*** (0.061)		-0.185** (0.076)
Observations	88,077	88,077	87,020	87,020	79,679	79,679	80,738	80,738	88,261	88,261	81,185	81,185	88,050	88,050
R2	0.527	0.527	0.439	0.439	0.056	0.056	0.238	0.238	0.575	0.575	0.601	0.602	0.441	0.441

Note: Table A.2 replicates the estimates from Table IV of [Karpoff and Wittry \(2018\)](#) using an identical code but with current header information. The table reports results of the impact of business combination laws on seven different outcome variables, and tests for the effects of including controls for other types of states, court decisions, and motivating firms. The outcome variables are ROA, Capex, PPE growth, asset growth, cash, SGA expenses, and leverage. ROA is EBITDA divided by total assets, Capex, PPE, cash, and SGA expenses are scaled by total assets; PPE and asset growth are the percentage change in PPE and total assets, respectively; and leverage is 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. All regressions include firm, state-year, and industry-year fixed effects. Robust standard errors are clustered at the state of incorporation level. \*, \*\*, and \*\*\* denote two-tailed significance tests at the 10%, 5%, and 1% levels, respectively.

**TABLE A.3**  
Replication of DiD Results With Data (3)

	Dependent Variable													
	(1) ROA		(2) Capex		(3) PPE Growth		(4) Asset Growth		(5) Cash		(6) SGA Expense		(7) Leverage	
	Short Regres- sion	Full Model	Short Regres- sion	Full Model	Short Regres- sion	Full Model	Short Regres- sion	Full Model	Short Regres- sion	Full Model	Short Regres- sion	Full Model	Short Regres- sion	Full Model
Business combination law (BC)	-0.014*** (0.005)	-0.012*** (0.004)	0.003** (0.001)	0.002** (0.001)	-0.017 (0.012)	-0.003 (0.018)	-0.064*** (0.023)	-0.068*** (0.013)	-0.008*** (0.003)	-0.007*** (0.002)	0.018*** (0.006)	0.017*** (0.004)	0.015*** (0.006)	0.015** (0.006)
First-generation law		-0.020*** (0.007)		0.002 (0.003)		-0.009 (0.010)		0.012 (0.022)		0.003 (0.006)		0.013** (0.007)		0.003 (0.009)
Poison Pill law (PP)		-0.006 (0.004)		0.001 (0.001)		-0.017* (0.009)		0.007 (0.010)		-0.001 (0.002)		0.013*** (0.005)		0.027*** (0.007)
Control share acquisition law (CS)		-0.003 (0.009)		0.005** (0.002)		-0.021 (0.017)		0.033 (0.026)		0.006 (0.005)		0.000 (0.007)		0.023 (0.015)
Directors' duties law (DD)		-0.006 (0.007)		-0.001 (0.002)		0.041*** (0.011)		0.007 (0.020)		0.001 (0.003)		0.001 (0.006)		-0.008 (0.008)
Fair price law (FP)		-0.002 (0.007)		0.001 (0.002)		-0.017 (0.013)		0.032* (0.018)		0.003 (0.003)		0.006 (0.005)		0.006 (0.008)
CS x CTS		-0.005 (0.008)		-0.003 (0.002)		-0.012 (0.022)		-0.032 (0.025)		0.000 (0.004)		0.020** (0.008)		-0.017 (0.017)
BC x Amanda		-0.008* (0.004)		0.001 (0.002)		-0.033 (0.026)		-0.005 (0.026)		-0.003 (0.004)		0.007 (0.006)		0.000 (0.010)
BC x MF (motivating firms)		0.117*** (0.019)		0.021* (0.012)		0.032 (0.029)		0.148** (0.064)		-0.034** (0.015)		-0.035 (0.021)		-0.075** (0.037)
Observations	90,082	90,082	88,996	88,996	81,564	81,564	81,992	81,992	90,229	90,229	83,517	83,517	90,014	90,014
R2	0.657	0.657	0.450	0.450	0.074	0.074	0.296	0.296	0.593	0.593	0.760	0.760	0.521	0.522

Note: Table A.3 replicates the estimates from Table IV of [Karpoff and Wittry \(2018\)](#) using but fixing data quality issues, including the use of stale header information. The table reports results of the impact of business combination laws on seven different outcome variables, and tests for the effects of including controls for other types of states, court decisions, and motivating firms. The outcome variables are ROA, Capex, PPE growth, asset growth, cash, SGA expenses, and leverage. ROA is EBITDA divided by total assets, Capex, PPE, cash, and SGA expenses are scaled by total assets; PPE and asset growth are the percentage change in PPE and total assets, respectively; and leverage is 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. All regressions include firm, state-year, and industry-year fixed effects. Robust standard errors are clustered at the state of incorporation level. \*, \*\*, and \*\*\* denote two-tailed significance tests at the 10%, 5%, and 1% levels, respectively.

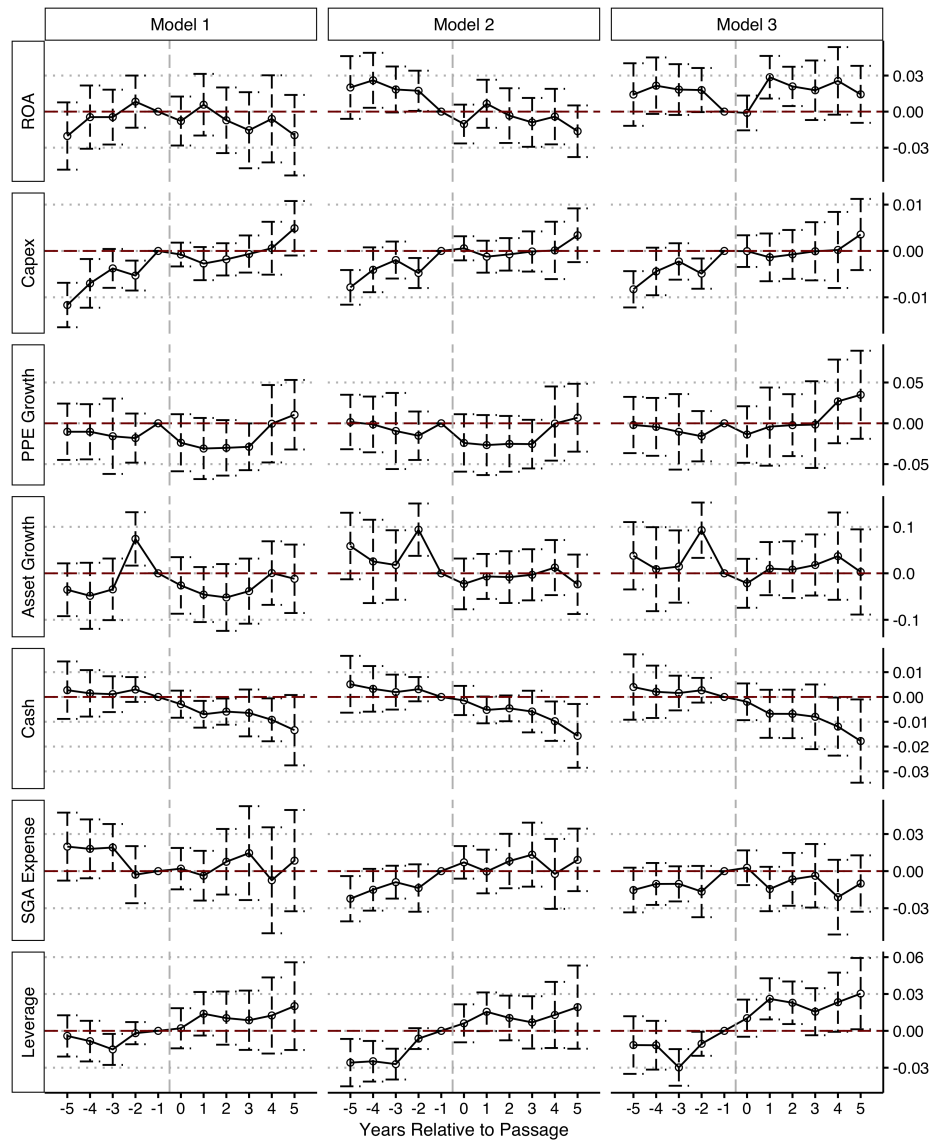
**TABLE A.4**  
Replication of DiD Results With Data (4)

	Dependent Variable													
	(1) ROA		(2) Capex		(3) PPE Growth		(4) Asset Growth		(5) Cash		(6) SGA Expense		(7) Leverage	
	Short Regres- sion	Full Model	Short Regres- sion	Full Model	Short Regres- sion	Full Model	Short Regres- sion	Full Model	Short Regres- sion	Full Model	Short Regres- sion	Full Model	Short Regres- sion	Full Model
Business combination law (BC)	-0.015*** (0.005)	-0.014*** (0.004)	0.003* (0.001)	0.002* (0.001)	-0.014 (0.012)	-0.004 (0.020)	-0.066*** (0.022)	-0.070*** (0.013)	-0.009*** (0.003)	-0.007** (0.003)	0.020*** (0.006)	0.020*** (0.004)	0.018*** (0.007)	0.015** (0.006)
First-generation law		-0.020*** (0.007)		0.002 (0.003)		-0.012 (0.012)		0.019 (0.023)		0.005 (0.005)		0.011* (0.006)		-0.001 (0.008)
Poison Pill law (PP)		-0.007* (0.003)		0.002 (0.001)		-0.014 (0.009)		0.006 (0.010)		-0.001 (0.002)		0.013** (0.005)		0.026*** (0.007)
Control share acquisition law (CS)		0.000 (0.009)		0.007*** (0.003)		-0.030 (0.020)		0.031 (0.033)		0.013** (0.006)		-0.005 (0.009)		0.006 (0.014)
Directors' duties law (DD)		-0.007 (0.006)		-0.001 (0.002)		0.043*** (0.013)		-0.003 (0.020)		0.001 (0.004)		0.001 (0.006)		-0.011 (0.007)
Fair price law (FP)		-0.002 (0.006)		0.002 (0.002)		-0.014 (0.011)		0.033 (0.020)		0.003 (0.003)		0.003 (0.005)		0.009 (0.009)
CS x CTS		-0.007 (0.008)		-0.006** (0.003)		-0.001 (0.020)		-0.027 (0.028)		-0.005 (0.004)		0.022*** (0.008)		0.000 (0.016)
BC x Amanda		-0.006 (0.005)		0.001 (0.002)		-0.025 (0.030)		-0.002 (0.029)		-0.003 (0.004)		0.005 (0.008)		0.006 (0.010)
BC x MF (motivating firms)		0.132*** (0.037)		0.013 (0.009)		0.024 (0.020)		0.146*** (0.044)		-0.036** (0.016)		-0.041 (0.040)		-0.081** (0.033)
Observations	90,082	90,082	88,996	88,996	81,564	81,564	81,992	81,992	90,229	90,229	83,517	83,517	90,014	90,014
R2	0.659	0.659	0.445	0.445	0.079	0.079	0.302	0.302	0.593	0.593	0.757	0.757	0.518	0.518

Note: Table A.4 replicates the estimates from Table IV of [Karpoff and Wittry \(2018\)](#) using but fixing data and design issues, including using less granular industry definitions. The table reports results of the impact of business combination laws on seven different outcome variables, and tests for the effects of including controls for other types of states, court decisions, and motivating firms. The outcome variables are ROA, Capex, PPE growth, asset growth, cash, SGA expenses, and leverage. ROA is EBITDA divided by total assets, Capex, PPE, cash, and SGA expenses are scaled by total assets; PPE and asset growth are the percentage change in PPE and total assets, respectively; and leverage is 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. All regressions include firm, state-year, and industry-year fixed effects. Robust standard errors are clustered at the state of incorporation level. \*, \*\*, and \*\*\* denote two-tailed significance tests at the 10%, 5%, and 1% levels, respectively.

# Appendix B: Figures

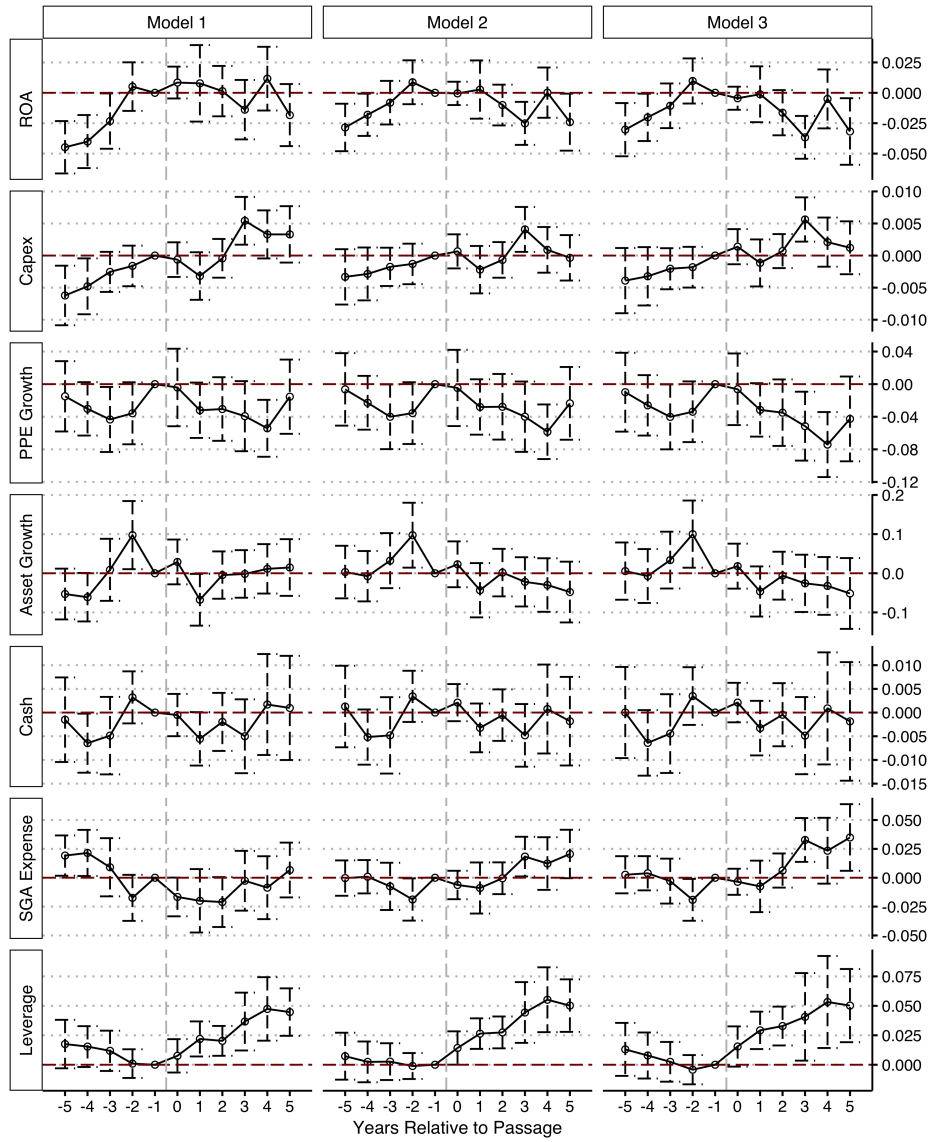
**Fig. B1**  
Event Study Estimates - Business Combination Statutes



Note: Figure B1 reports the event study estimates of the impact of business law changes using the data from [Karpoff and Wittry \(2018\)](#). Model 1 includes only the fixed effects without any covariates, Model 2 includes the covariates in the short regression model from [Karpoff and Wittry \(2018\)](#), while Model 3 includes the full model.

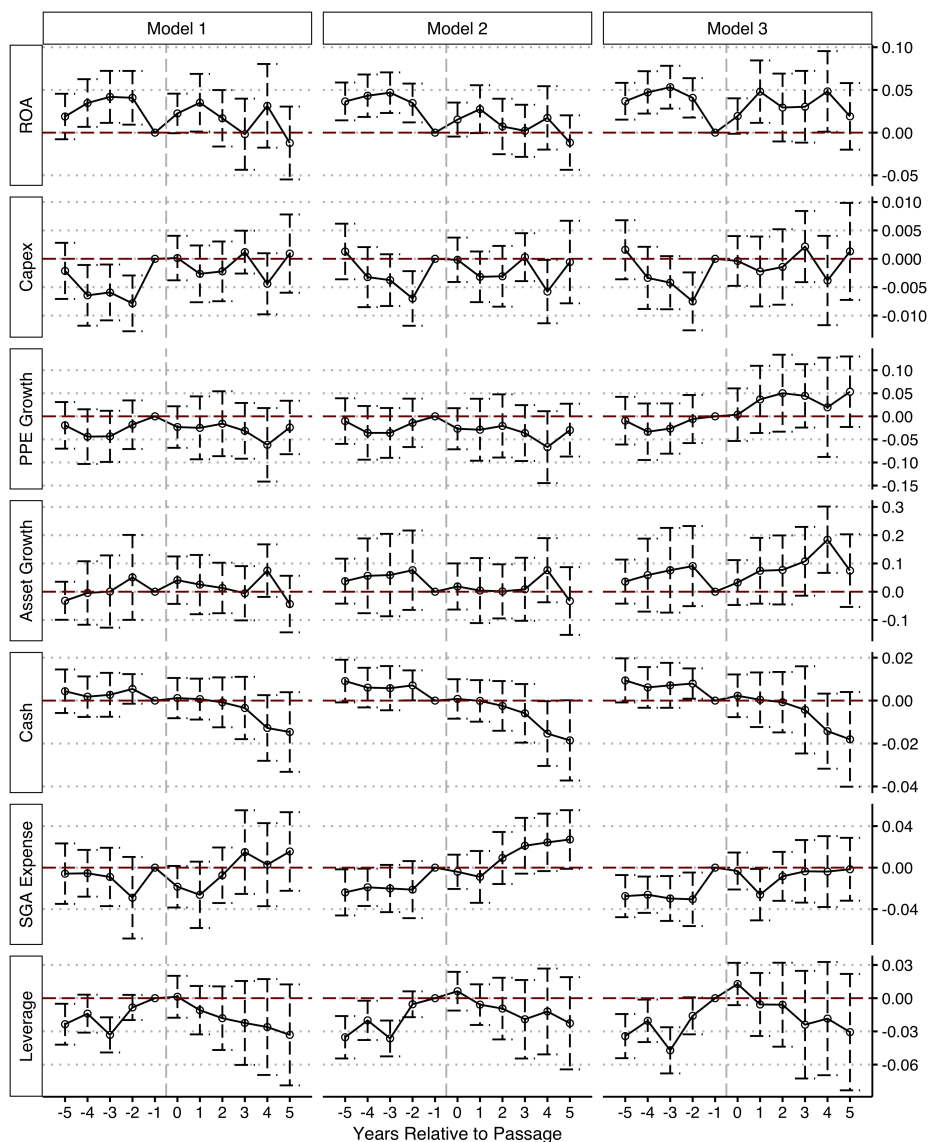


**Fig. B2**  
Event Study Estimates - Poison Pill Laws



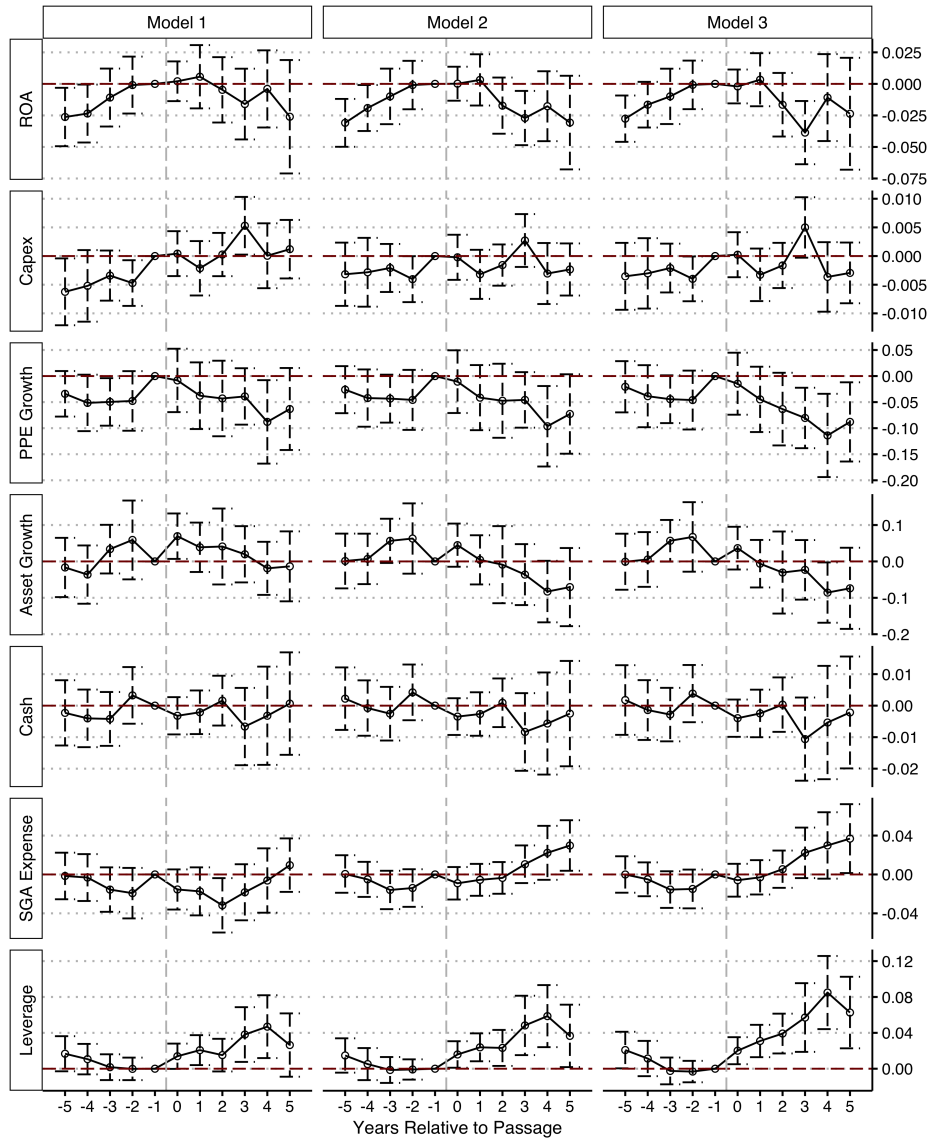
Note: Figure B2 reports the event study estimates of the impact of poison pill law changes using the data from [Karpoff and Wittry \(2018\)](#). Model 1 includes only the fixed effects without any covariates, Model 2 includes the covariates in the short regression model from [Karpoff and Wittry \(2018\)](#), while Model 3 includes the full model.

**Fig. B3**  
 Stacked Regression Event Study Estimates  
 Business Combination Statutes



Note: Figure B3 reports the stacked regression event study estimates and 95% confidence interval of the impact of business combination law changes using the data and design changes described in Section 5.1. Model 1 includes only the fixed effects without any covariates, Model 2 includes the covariates in the short regression model from Karpoff and Wittery (2018), and Model 3 includes their “full model”.

**Fig. B4**  
Stacked Regression Event Study Estimates  
Poison Pill Laws



Note: Figure B4 reports the stacked regression event study estimates and 95% confidence interval of the impact of poison pill law changes using the data and design changes described in Section 5.1. Model 1 includes only the fixed effects without any covariates, Model 2 includes the covariates in the short regression model from [Karpoff and Witter \(2018\)](#), and Model 3 includes their “full model”.

## Appendix C: Dataset Construction

In this section I detail how the datasets used in the paper are constructed. Corporate law and corporate finance papers frequently use similar publicly available data sources but “clean” the data in different and often unclear ways. For increased transparency I list in this appendix the detailed routine used to create the different datasets (besides Data (1) which came directly from Jonathan Karpoff and Michael Wittry). The code used to create the data will also be publicly posed on the code repository GitHub.

- Download the historical incorporation files from Bill McDonald [here](#) and Holger Spamann and Colby Wilkinson [here](#).

- Load the enactment dates by state from Table II of [Karpoff and Wittry \(2018\)](#).

- Download the full Compustat file from WRDS (file name = **comp.funda**), with the standard filters: *indfmt* = INDL, *datafmt* = STD, *popsrc* = D, and *consol* = C. Require that the fiscal year variable *fyear* is not missing and that firm assets (*at*) and sales (*sale*) are also non-missing and non-negative.

- For Data (3) and Data (4), clean up the SEC firm identifier (*cik*) which can change over time using the file **wrds\_cs\_names**. The file presents the *cik* by Compustat identifier *gkey* between dates *begdate* and *enddate*. Merge this into the Compustat file using the fiscal year date period variable *datadate*. Fill this variable “up” if missing so that you use the closest available value in time for missing observations, rather than the header info. Data (1) and (2) just use header info.

- Make one measure for the historical state of incorporation which is the first non-missing value from the Bill McDonald header file, the Spamann/Wilkinson header file, Compustat’s historical header information (from file **wrds\_cs\_names**), or the present header information (which comes from the file **comp.company**) based on the firm’s fiscal year date. If the value is still missing, then use the most recent available value from the McDonald file, the Spamann/Wilkinson file, or the historical header file, in that order. If still missing then use the present header information.

- Do a similar exercise for information from the header file—including headquarter state (*state*), country (*fic*), and security identifier (*cusip*).

- For industry, use Compustat’s historical industry identifier *sich* rather than the header file value *sic*. Here fill the value “down-up”. That is, assume that missing values are equal to the most recent non-missing value. For missing values before any non-missing entries (at the firm level) then fill up - using the closest non-missing value. If still missing then use the header info value.

- Drop all firms not in the United States, with missing values of state of incorporation, or incorporated outside of the 50 states (i.e. in a territory or Washington D.C.). In addition drop financial firms and utilities (SIC code between 6000 and 6999 or between 4000 and 4949), or where the industry or headquarter state is missing.

- Merge each firm-year observation into one of the 48 industry identifiers identified by Eugene Fama and Ken French, located on Ken French’s website [here](#). This will be used in Data (4) for the industry-year fixed effects.

- Create firm age variables which are equal to the natural logarithm of the fiscal year less the first fiscal year for a given firm (*age*), and the square of that value (*age2*). In addition firm size variables are the natural logarithm of firm assets *at* (*size*) and the square of that value (*size2*).

- Calculate the dependent variables as follows:

- PPE growth (*ppe*) =  $(p_t - p_{t-1})/p_{t-1}$  where  $p_t = ppent_t/at_t$ , if  $at > 0$  otherwise missing.
- Asset growth (*assetgrowth*) =  $(at_t - at_{t-1})/at_{t-1}$  if lagged assets are greater than 0, otherwise missing.
- Leverage ratio (*leverage*) =  $(dltt + dlc)/at$  if  $at > 0$  otherwise missing.
- Capital Expenditure (*capex*) =  $capx/at$  if  $at > 0$  otherwise missing.
- SG&A Expense (*sga*) =  $xsga/at$  if  $at > 0$  otherwise missing.
- Return on assets (*roa*) =  $ebitda/at$  if  $at > 0$  otherwise missing.
- Cash (*cash*) =  $che/at$  if  $at > 0$  otherwise missing.

- Winsorize all of the values for those variables at the 0.5 and 99.5 level. [Karpoff and Wittry \(2018\)](#) do this over the full dataset while I do so within fiscal year, so that the value doesn't depend on how many years you arbitrarily choose to have in your data.

- Merge in the enactment date values from Table II of [Karpoff and Wittry \(2018\)](#). Code variables (*bc*, *pp*, *cs*, *fp*, *dd*) for those laws if the firm is incorporated in a state where the law is in place. Data (3) and (4) use the fiscal year period date to do this merge, while Data (1) and (2) follow [Karpoff and Wittry \(2018\)](#) and use the year of the enactment date and the fiscal year from Compustat.

- Generate variables for the legal cases:

- *CTS* decision (*cts*) = the firm fiscal year period is after April 21, 1987 when the decision was issued.
- *Amanda* decision (*amanda*) = the firm fiscal year period is after May 24, 1989 when the decision was issued.
- *csXcts* = the firm fiscal year period is after the *CTS* decision and the firm is covered by a control share acquisition statute.
- *bcXamanda* = the firm fiscal year period is after the *Amanda* decision and the firm is covered by a business combination statute.

- Identify the firms that opt-out of the laws from the ISS risk metrics dataset (**risk.gset** and **risk.rmgovernance**). The specifics of this identification are explained in the code.

Merge into the Compustat dataset based on *cusip* and *year*. Data (1) and (2) remove these firms from coverage while Data (3) and (4) do not.

- Following [Karpoff and Wittry \(2018\)](#) remove coverage for firms incorporated in Georgia for business combination and fair price laws, and Tennessee for control share acquisition statute, because they require firms to opt-in.

- [Karpoff and Wittry \(2018\)](#) identify a set of firms (by *cusip*) that lobbied for the passage of each type of law. Create variables that are set to 1 for these firms and 0 otherwise for each type of law. Create additional variables that are equal to the interaction of these motivating firms and the periods when they have the law in place.

## Appendix D: Stacked Regression Method

In Section 5.3 I use a “stacked regression” approach to circumvent some of the pitfalls identified in the literature on difference-in-differences with panel data and staggered treatment adoption. In this appendix I more fully explain how the stacked regression estimates are implemented. For other papers that have used a stacked regression approach, see [Cengiz et al. \(2019\)](#); [Deshpande and Li \(2019\)](#); [Gormley and Matsa \(2011\)](#).

The first steps for the stacked approach is to generate stacked data:

- For each dataset and law, identify the “switch” years for each firm, which are the years when they go from not being covered by a statute to being covered.
- Make “stacked” datasets. For each unique treatment year (years when firms begin coverage by either a business combination or poison pill statute). Keep only treatment years with at least 10 treated firms.
- For each treatment year identify all firms treated in that year with full data from the year before to the year after coverage begins. Do the same thing for all control firms—i.e. they are not treated in that year and also have full coverage for three years surrounding the coverage year in question.
- Generate a sub-dataset for each treatment year which contains only observations from the identified treated and control firms for the years from five years before to five years after coverage begins. Require that control firms do not begin coverage in that relevant window, and if they do drop the observations when they are covered. If the treated firms somehow become uncovered by the statute (e.g. because they reincorporate) then also drop those uncovered observations. Generate a variable in the sub-dataset identifying the relevant treatment year.
- Generate relative time indicators for the treated firms in each sub-dataset which identify whether the fiscal year is  $t$  years from the coverage start year, for years  $t \in \{-5, \dots, -2, 0, \dots, 5\}$ .
- Create sub-datasets for each treatment year and stack them (i.e. bind them by rows).

With the stacked datasets (these are specific to the law in question—so here we have one for business combination statutes and another for poison pill statutes), estimate the event study regression model:

$$y_{itk} = \alpha_{ik} + \lambda_{ltk} + \eta_{jtk} + \sum_{l \neq -1} \delta_l \mathbb{1}\{t - E_i = l\} + \Omega X_{itk} + \epsilon_{itk}$$

The only difference between the stacked regression model and Equation 4 is that we now interact the fixed effects with an identifier for the specific sub-dataset (i.e. specific treatment year for those observations) and cluster at the state of incorporation by sub-dataset level.

Such stacking prevents the negative-weighting issues (by using past treated units to enter as controls) identified in the literature. See [Baker et al. \(2020\)](#) for confirmation that this removes the bias in simulated examples.



## Appendix E: Callaway & Sant’Anna Estimator

The estimator in [Callaway and Sant’Anna \(2020\)](#) is an alternative to standard fixed effect designs that avoids the common issues with staggered DiD, including using past-treated units as controls and undesirable attributes of OLS-based weighting that moves treatment effect estimates away from the usual estimands of interest. Their estimator is very intuitive, and accords with what most studies *allege* that their DiD estimate does in practice. In addition, it allows for multiple time periods, with treatment timing variation, and for the parallel trends assumption to only hold conditional on unit-level covariates.

The key attribute of this estimator is that it identifies granular treatment effects at the treatment-year/relative time period level, which the authors call group-time average treatment effects. Denoted  $ATT(g, t)$ , these estimates are the average treatment effect for group  $g$  at time  $t$ , which can then be aggregated to an overall estimate at relative times  $e = t - g$  to accord with the commonly used event-study designs. In addition, “the group-time average treatment effect parameters do not directly restrict heterogeneity with respect to observed covariates, the period in which units are first treated, or the evolution of treatment effects over time. As a consequence, these easy-to-interpret causal parameters can be directly used for learning about treatment effect heterogeneity, and/or to construct many other more aggregated causal parameters.”

Although the estimator allows for both inverse-probability weighted and doubly-robust methods, I use the standard outcome-regression approach, in keeping with the spirit of the standard DiD approach. In addition, I use “not-yet treated” units as potential control units. With outcome-regression models, the  $ATT(g, t)$  estimate is calculated as:

$$ATT(g, t) = \mathbb{E} \left[ \frac{G_g}{\mathbb{E}[G_g]} (Y_t - Y_{g-1} - m_{g,t}^{ny}(X)) \right]$$

where  $G_g$  is a binary variable that is equal to one if a firm is first treated in year  $g$ ,  $Y_t$  is the value of the outcome variable in year  $t$ ,  $Y_{g-1}$  is the value of the outcome variable in the year prior to treatment, and  $m_{g,t}^{ny}(X) = \mathbb{E}[Y_t - Y_{g-1} \mid X, D_t = 0, G_g = 0]$ . In words,  $m_{g,t}^{ny}(X)$  are population outcome regressions for the not yet treated group by relative time period  $t$ .

With the  $ATT(g, t)$  estimates we aggregate up to event-time (i.e. the effect of the policy adoption two years out across treatment cohort groups) by weighting each estimate by its sample share. That is  $\delta_e^* = \sum_{g \in G} w(g) \cdot ATT(g, t - g = e^*)$  where the weights  $w(g)$  sum to 1 and are equal to the number of firms with an estimate for  $e = e^*$  in group  $g$  as a percentage of all firms with an estimate for  $e = e^*$ . As noted in [Callaway and Sant’Anna \(2020\)](#), this simple combination of sub-estimates immediately rules out troubling issues due to negative weights. For example, when the effect of participating in the treatment is always positive then this aggregated parameter cannot be negative, unlike with regression-based methods. The standard errors for the estimate is calculated using a multiplier bootstrap method with the influence functions from each  $ATT(g, t)$  estimate.

While the CS estimator is open source (available on CRAN with package named **did**), unique attributes of my implementation require me to code the method by hand (in particular

firms change states of incorporation and/or states drop the law). Below I describe the exact steps used to provide the estimates in Figure 9.

- Identify a list of valid treatment years where firms go from being uncovered to covered by a poison pill statute.
- For each treatment year  $t$ , get a list of all treated units with full observations for  $t-1$  to  $t+1$ , and the same for control years. The treated units must have treatment indicators of the form  $\{0, 1, 1\}$  and the control units must be of the form  $\{0, 0, 0\}$ .
- Make a smaller dataset with just the treated and control units for years in  $\{t-5, t+5\}$ .
- Require that the treatment variable stays constant in the relative periods. For treated units the variable  $pp$  must be equal to 0 for years in  $\{t-5, t-1\}$  and 1 in  $\{t, t+5\}$  and for control units  $pp$  must be 0 for all years in  $\{t-5, t+5\}$ . Any observation that does not fit this restriction is dropped.
- For each relative time period  $e$  in  $\{-5, 5\}$ , calculate the ATT. Keep observations in year  $t-e$  and  $t-e^*$  where  $e^*$  is the reference year. For years post-treatment, the reference year is the year before treatment, while for pre-treatment observations it is the lagged year.
- Calculate the simple two period DiD ATE using the outcomes and covariates for years  $t-e$  and  $t-e^*$  and Equation 2.2 from [Sant'Anna and Zhao \(2020\)](#). Store the efficient influence functions from the estimate.
- With each treatment year/relative time period estimate, calculate the overall relative period  $\hat{e}$  which is simply the weighted average of all ATTs for each treatment group year, weighted by the number of treated firms in each group.
- Generate the standard errors for  $\hat{e}$  with a multiplier bootstrap and 1,000 iterations using the saved influence functions.

For each outcome variable this process results in a set of coefficients and standard errors for  $e \in \{-5, 5\}$  which are then plotted in Figures 9.