

# The Law and Finance of Anti-Takeover Statutes

by

Emiliano Catan\* and Marcel Kahan\*\*

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## Abstract

Lawyers and financial economists have fundamentally different views of anti-takeover statutes. While corporate lawyers and academics generally dismiss these statutes as irrelevant, economists study them empirically and find that they – and hence the threat of a takeover -- affect firm and managerial behavior. This article seeks to examine the divide between the law and the finance approach to anti-takeover statutes. We first explain why these statutes, as used by financial economists, are not a proper metric of the takeover threat facing a firm. We then review three empirical studies published in leading finance journals. For each study, we show that the results are affected by omitted variables, large scale coding errors, or improper specifications. When corrected for these problems, the associated between anti-takeover statutes and the hypothesized effect disappeared. Finally, we identify three problems – endogeneity, coding errors and omitted firm-level factors that bear on a firm’s susceptibility to anti-takeover statutes – that affect a large portion of the data used in the empirical studies and cast doubt on the results these studies arrive at. Broadly, our paper calls into question most of the understanding of the effect of takeover threat, which is based to a large extent on finance studies of anti-takeover statutes.

\* Assistant Professor of Law, New York University School of Law

\*\* George P. Lowy Professor of Law, New York University School of Law

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## Introduction

Over the last 15 years, finance scholars have developed an increasing fascination with anti-takeover statutes. Numerous articles, many published in top finance journals, have examined the effect of these statutes on performance,<sup>1</sup> leverage,<sup>2</sup> managerial stock ownership,<sup>3</sup> worker wages,<sup>4</sup> innovation,<sup>5</sup> dividend payout ratios,<sup>6</sup> bond yields,<sup>7</sup> executive pay,<sup>8</sup> cash reserves,<sup>9</sup> loan syndicate diffusion,<sup>10</sup> and the amount of employee stock in pension plans.<sup>11</sup> The popularity of these studies is not waning. Just within the last year, two new working papers on anti-takeover statutes were

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<sup>1</sup> Xavier Giroud & Holger M. Mueller, *Does Corporate Governance Matter in Competitive Industries?*, 95 J. Fin. Econ. 312 (2010); Julien Sauvagnat *Takeover Discipline and Asset Tangibility* (October 23, 2011). Available at <http://ssrn.com/abstract=1947546>.

<sup>2</sup> Gerald T. Garvey & Gordon Hanka, *Capital Structure and Corporate Control: The Effect of Antitakeover Statutes on Firm Leverage*, 54 J. Fin. 519, 526-43 (1999); Kose John & Lubomir Litov, *Managerial Entrenchment and Capital Structure: New Evidence*, 7 J. Empirical Legal Stud. 693, 695 (2010).

<sup>3</sup> Shijun Cheng, Venky Nagar & Madhar V. Rajan, *Identifying Control Motives in Managerial Ownership: Evidence from Antitakeover Legislation*, 8 Rev. Fin. Stud. 637 (2005).

<sup>4</sup> Marianne Bertrand & Sendhil Mullainathan, *Is there Discretion in Wage Setting? A Test Using Takeover Legislation*, 30 Rand J. Econ. 535 (1999); Marianne Bertrand & Sendhil Mullainathan, *Enjoying the Quiet Life? Corporate Governance and Managerial Preferences*, 111 J. Poli. Econ. 1043, 1072 (2003).

<sup>5</sup> John R. Becker-Blease, *Governance and Innovation*, 17 J. Corp. Fin. 947 (2011); Julian Atanassov, *Do Hostile Takeovers Stifle Innovation? Evidence from Antitakeover Legislation and Corporate Patenting*. 68 Journal of Finance 1097 (2013).

<sup>6</sup> Bill B. Francis, Iftexhar Hasan, Kose John & Liang Song, *Corporate Governance and Dividend Payout Policy: A Test Using Antitakeover Legislation*, 40(1) Fin. Mgmt. 83 (2011).

<sup>7</sup> Jiaping Qiu & Fan Yu, *The Market for Corporate Control and the Cost of Debt*, 93 J. Fin. Econ. 505 (2009); Bill B. Francis, Iftexhar Hasan, Kose John & Maya Waisman, *The Effect of State Antitakeover Laws on the Firm's Bondholders*, 96 J. Fin. Econ. 127 (2010).

<sup>8</sup> Marianne Bertrand & Sendhil Mullainathan, *Corporate Governance and Executive Pay: Evidence from Takeover Legislation* (Mass. Inst. Tech., Working Paper, 1999).

<sup>9</sup> Hayong Yun, *The Choice of Corporate Liquidity and Corporate Governance*, 22 Rev. Fin. Stud. 1447 (2009)

<sup>10</sup> Sreedhar T. Bharath, Sandeep Dahiya & Issam Hallak, *Do Shareholder Rights Affect Syndicate Structure? Evidence from a Natural Experiment* (August 31, 2011). Available at <http://ssrn.com/abstract=2020195>.

<sup>11</sup> Joshua D. Rauh, *Own Company Stock in Defined Contribution Pension Plans: A Takeover Defense?*, 81 J. Fin. Econ. 379 (2006).

released.<sup>12</sup>

From a legal perspective, this is very odd. Corporate lawyers and academics generally dismiss most anti-takeover statutes as irrelevant. So why do finance professors study them and why do their studies yield results?

Unlike lawyers, who study whether, how and why anti-takeover statutes offer protection against hostile acquisitions, financial economists have no intrinsic interest in anti-takeover statutes. Rather, they start from the premise that these provisions have a material impact on the prospect of a hostile takeover of the firm. Because anti-takeover statutes were adopted by different states at different times, they generate a natural experiment on their issue of real interest: whether the presence or absence of a takeover threat changes firm behavior. A finding that these statutes are associated with a change is then taken as confirmation that the statutes in fact offer anti-takeover protection.

In this article, we will examine the divide between the law and the finance approach to anti-takeover statutes. In Part I, we explain why anti-takeover statutes are not a proper metric for the degree of takeover threat. This poses the question of why finance studies of these statutes find results. In Parts II to IV, we therefore examine in greater detail three finance studies that were published in top journals. For each study, we present evidence that the results are due to omitted variables or improper specifications. When corrected for these problems, the association between anti-takeover statutes and the hypothesized effect disappears.

There are, of course, numerous finance studies of anti-takeover statutes that we do not review. It would be a Herculean, if not Sisyphean, task to examine all of these studies at the level of detail that we devoted to the three studies discussed in Parts II to IV. In Part V, we instead discuss three problems that affect most of the existing studies: endogeneity, miscodings, and failure to control for firm-level factors that affect the susceptibility of firms to anti-takeover statutes. We will show that each of these problems affect a large percentage of the observations typically used in these studies and argue that they make the results obtained by the studies unreliable.

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<sup>12</sup> Todd A. Gormley & David A. Matsa, *Playing it Safe? Managerial Preferences, Risk, and Agency Conflicts* (2014) available at [http://irrcinstitute.org/pdf/Playing-It-Safe\\_June-12-2014.pdf](http://irrcinstitute.org/pdf/Playing-It-Safe_June-12-2014.pdf). [Karpoff & Wittry]

In Part VI, we conclude by discussing the implications of our analysis for several important debates. Most basically, our analysis is consistent with the view that anti-takeover statutes do not matter after all.

Second, and most importantly, our analysis calls into doubt most of the empirical findings regarding the effect of takeover threats. Since the 1980s, scholars have debated whether an enhanced threat of a takeover acts as a disciplining device for managers<sup>13</sup> or induces short-termism.<sup>14</sup> The debate continues unabated. Earlier this year, a commission co-chaired by Larry Summers -- a renowned economist and former U.S. Treasury Secretary and Harvard president -- recommended measures to make hostile takeovers more difficult in order to combat short-termism.<sup>15</sup> The studies of how firms have responded to the adoption of anti-takeover statutes have been the principal, and (if these statutes mattered) econometrically most reliable, evidence of how firms responded to an increased takeover threat. But if these studies are based on false premises, as we argue, it turns out that we know little if anything about the form that these responses take.<sup>16</sup>

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<sup>13</sup> See, e.g., Frank H. Easterbrook & Daniel R. Fischel, *The Proper Role of a Target's Management in Responding to a Tender Offer*, 94 Harv. L. Rev. 1161, 1168-74 (1981) (arguing that hostile tender offers are an important device to reduce agency costs); Ronald J. Gilson, *A Structural Approach to Corporations: The Case Against Defensive Tactics in Tender Offers*, 33 Stan. L. Rev. 819, 841 (1981) (explaining that "it is now commonly acknowledged that the market for corporate control is an important mechanism by which management's discretion to favor itself at the expense of shareholders may be constrained"); Lucian A. Bebchuk, *The Case for Facilitating Competing Tender Offers*, 95 Harv. L. Rev. 1028, 1047 (1982) (arguing that the threat of takeovers induces managers to do more to maximize profit).

<sup>14</sup> See, e.g., Jeremy C. Stein, *Takeover Threats and Managerial Myopia*, 96 J. Pol. Econ. 61 (1988) (analyzing how myopic behavior might arise when takeover threats lead managers to seek high stock price in short term); Andrei Shleifer & Robert W. Vishny, *Equilibrium Short Horizons of Investors and Firms*, 80 Am. Econ. Rev. 148 (1990) (same); Martin Lipton, *Corporate Governance in the Age of Finance Corporatism*, 136 U. Pa. L. Rev. 1, 6-7 (1987) (arguing that takeovers induce managers to focus on short-term profits at the expense of long-term planning).

<sup>15</sup> Report of the Commission on Inclusive Prosperity at 86 (2015), available at <http://www.wlrk.com/docs/IPC-PDF-full.pdf>. A similar debate is waged today about the effect of activist hedge funds. See, e.g., Marcel Kahan & Edward Rock, *Hedge Funds in Corporate Governance and Corporate Control*, 155 U. Pa. L. Rev. 1021, 1083-91 (2007) (reviewing debate); Lucian A. Bebchuk, *The Myth that Insulating Boards Serves Long-Term Value*, 113 Colum. L. Rev. 1637 (2013) (arguing that hedge funds do not induce short-termism).

<sup>16</sup> Other forms of empirical studies include studies of takeover defenses adopted by companies and event studies on the adoption of takeover defenses. See, e.g., Jonathan Karpoff & Paul Malatesta, *The Wealth Effects of Second-Generation State Takeover Legislation*, 25 J. Fin. Econ. 291, 291 (1989) (event study on multiple anti-takeover statutes); Samuel H. Szewczyk & George P. Tsetsekos, *State Intervention in the Market for Corporate*

In addition, our analysis has wider implications about the relationship between law and empirical economics. The underlying problem in the studies of anti-takeover statutes – that empiricists have a readily available “variable” for use in their regressions, but do not pay much attention to why and how this variable would matter – is not unique. The common use of variables that share these features, we believe, reflects the incentive structure bearing on empirical economists: it is attributable to the fact that researchers can easily use such variables to churn out empirical studies even when the study is insufficiently grounded in sound theory.

Finally, the large number of published studies finding an effect of anti-takeover statutes (and the absence of published studies finding no effect), in our view, reflects publication bias. Such bias and the incentives it creates can significantly distort the direction of scientific research, inhibit self-correction and thereby slow the speed of scientific progress.

## **I. State Anti-Takeover Laws and Takeover Protection**

### **A. The Pre-Eminence of Poison Pills**

From a lawyer’s perspective, finance academics who focus on anti-takeover statutes are barking up the wrong tree. Rather than examine anti-takeover statutes, finance academics should take account of the takeover defense that really matters: the poison pill.

Poison pills work by granting, in certain events, valuable rights (hence their official name, “rights agreements”) to shareholders. The early version of pills, so-called flip-over pills, granted such rights if a raider, after acquiring stock of the company, effected a merger with an affiliate or another type of self-dealing transaction. Thus, for example, under the poison pill upheld by the Delaware Supreme Court in *Moran*, each right permitted the holder to purchase \$200 worth of stock of the hostile acquirer

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*Control: The Case of Pennsylvania Senate Bill 1310*, 31 J. Fin. Econ. 3 (1992) (event study on Pennsylvania antitakeover statute); Pandei Chintrakarn, Napatsorn Jiraporn & Pornsit Jiraporn, *The Effect of Entrenched Boards on Corporate Risk-Taking: Testing the Quiet Life Hypothesis*, 20 Applied Economics Letters 1067 (2013) (studying effect of staggered boards). These types of studies, however, do not offer reliable evidence on the actual effect of takeover threat. Studies of takeover defenses adopted by firms suffer from endogeneity problems: firms that decide to adopt takeover defenses are likely to differ systematically from firms that do not (and these differences may account for the decision to adopt a defense) and these differences make it difficult to assess the effect of a takeover threat empirically. Event studies only measure the immediate market reaction to the adoption of anti-takeover statutes. This reaction, at best, reflect what market participants anticipate about the effects of a statute at the time of adoption, as opposed what the actual effects are.

for \$100 if a merger occurred.<sup>17</sup> Flip-over pills were quickly supplemented with flip-in provisions, which grant similar rights if the raider acquires a certain percentage of company stock, even if no subsequent merger takes place. Flip-over and flip-in pills can be redeemed by the board of directors for a trivial amount, but only before the raider becomes a significant stockholder.

Poison pills are a highly effective tool to ward off a hostile raider. As summarized by Marty Lipton, “[The poison pill] is an absolute bar to a raider acquiring control ... without the approval of the company’s board of directors.”<sup>18</sup> A flip-in pill precludes a hostile acquisition through two separate mechanisms. First, a raider will not want to exceed the threshold to trigger the pill because the value of its stake would be greatly diluted by the grant of valuable right to all other shareholders. Second, even if a raider would be willing to swallow the pill, other shareholders will not want to tender their shares to the raider because they would rather hold out and exercise the rights after the pill is triggered. Since the terms of the pill, including the value of the rights, are set by the incumbent board and since pills do not require shareholder approval, the board can always fashion a pill that is sufficiently poisonous to do the trick. In fact, no single company has ever been acquired with a flip-in pill in place.<sup>19</sup> Flip-over pills function similarly, except that they do not stop a raider who is willing to acquire majority ownership and forgo a subsequent freeze-out merger.<sup>20</sup>

Because pills can be put in place at short notice, it does not matter whether a company has a pill in place when a hostile bid is made. It merely matters whether a company can adopt a pill when it needs one – whether it has a so-called “shadow pill” – and every company can do so as long as the pill is valid

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<sup>17</sup> *Moran v. Household Int’l, Inc.*, 500 A.2d 1346, 1349 (Del. 1985).

<sup>18</sup> See Martin Lipton, Wachtell, Lipton, Rosen & Katz, Memorandum to Clients (Jan. 15, 1993).

<sup>19</sup> A flip-in pill has been triggered only once, and that did not occur in the context of a hostile takeover. See [http://www.lw.com/upload/pubContent/\\_pdf/pub2563\\_1.pdf](http://www.lw.com/upload/pubContent/_pdf/pub2563_1.pdf). The pill at issue was designed to protect Selectica’s net operating losses (NOL pills), rather than protect it against a hostile bid, and was triggered by Versata Enterprises to obtain leverage in an unrelated business dispute. NOL pills have a much lower triggering threshold than regular pills, which means that the dilution suffered by the acquiring person is more limited (Versata had 6.7% of Selectica’s stock). In a hostile bid context with a similar pill, a company could have adopted a new pill with a higher trigger (say 15%) even if the first pill with the lower trigger was swallowed. Thus, the Selectica incident has no material implications for the potency of pills as a takeover defense.

<sup>20</sup> This was illustrated by James Goldsmith’s takeover of Crown Zellerbach in 1985. <http://www.nytimes.com/1985/07/26/business/goldsmith-wins-control-of-crown-zellerbach.html>

in its state of incorporation.<sup>21</sup>

Poison pills raise several questions. First, legally, are they valid in principle? Second, what are the fiduciary duty limitations on a board's refusal to redeem a pill? Third, how can pills be overcome?

The validity in principle of pills was an initial concern not just due to the novelty of the device, but also to the fact that flip-in pills discriminate among shareholders: regular shareholders receive valuable rights; the raider does not. But several 1985-86 decisions by the Delaware Supreme Court established the validity of poison pills. In *Moran* (1985),<sup>22</sup> the court upheld the use of flip-over pills (which do not involve discrimination). In *Unocal* (1985), the court sanctioned a self-tender offer that entailed a discriminatory treatment equivalent to the one in flip-in pills.<sup>23</sup> And in *Revlon* (1986), the court commented favorably on the board's use of a precursor to a flip-in pill (that discriminated between a raider and other shareholders) to get a raider to increase its offer price.<sup>24</sup>

Though the validity of pills in Delaware – the domicile for about half of all publicly traded companies – became clear in 1985 to 1986, the issue of pill validity in other states is more complex. Although no court has struck down a flip-over pill as invalid in principle, courts split on the validity of flip-in pills. Between 1986 and 1989, court decisions rendered under the laws of Colorado, Georgia, New Jersey, New York, Virginia, and Wisconsin held or strongly suggested that flip-in pills are invalid.<sup>25</sup> The basis for these decisions was that the discriminatory treatment of raiders in flip-in pills

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<sup>21</sup> See John C. Coates, *Takeover Defenses in the Shadow of the Pill: A Critique of the Scientific Evidence*, 79 Tex. L. Rev. 271 (2000).

<sup>22</sup> 500 A.2d at 1354.

<sup>23</sup> *Unocal Corp. v. Mesa Petroleum Co.*, 493 A.2d 946 (Del. 1985).

<sup>24</sup> *Revlon, Inc. v. MacAndrews & Forbes Holdings, Inc.*, 506 A.2d 173, 182 (Del.1986). Arthur Fleischer, Jr. & Alexander R. Sussman, *Takeover Defense: Mergers and Acquisitions*, at § 5.01 OVERVIEW OF THE POISON PILL [A] (“Beginning with the Delaware Supreme Court's decisions in *Household* and *Revlon*, the legal validity of standard poison pills (without deferred redemption features) became fully established for Delaware corporations.”); *id.* at § 5.06 THE LEGALITY OF THE POISON PILL [A] (“Since [*Moran*] and *Revlon*, a board's authority to adopt a standard pill under Delaware law has gone unchallenged.”)

<sup>25</sup> See, e.g., *Amalgamated Sugar Co. v. NL Industries Inc.*, 644 F.Supp. 1229 (S.D.N.Y.1986) (construing New Jersey Corporation Act); *Asarco Inc. v. Court*, 611 F.Supp. 468 (D.N.J.1985) (construing New Jersey law); *West Point–Pepperell, Inc. v. Farley Inc.*, 711 F.Supp. 1088 (N.D.Ga.1988) (construing Georgia law); *Bank of New York Co. v. Irving Bank Corp.*, 142 Misc.2d 145, 536 N.Y.S.2d 923 (N.Y.Sup.Ct.1988) (construing New York law); *R.D. Smith & Co. v. Preway Inc.*, 644 F.Supp. 868 (W.D.Wis.1986) (construing Wisconsin law); *Spinner Corp. v. Princeville Development Corp.*, Civ. No. 86–0701 (D.Haw. October 31, 1986) (construing Colorado



violated a statutory requirement that all shares of the same class be treated equally.<sup>26</sup> Court decisions under the laws of Indiana, Maine, Maryland, Michigan, Minnesota, Texas and Wisconsin have upheld flip-in pills reasoning that any discrimination entailed merely is among *shareholders*, not among *shares*.<sup>27</sup> But while the reception of flip-in pills by courts was mixed, legislatures embraced them enthusiastically. By 1989, 20 states had adopted statutes validating discriminatory pills.<sup>28</sup> This number now stands at 34.<sup>29</sup>

The fiduciary duty limitations on pills proved to be a more torturous road. The Delaware Supreme Court made clear from the outset that pills had to be employed consistent with the standards laid out in *Unocal* and *Revlon*, but what these standards required became clear only over time. An important question was whether a pill could be used merely to gain time to develop an alternative transaction or to negotiate for a better price or whether it could be used indefinitely to “just say no.” Two 1988 decisions by the Delaware Chancery court held the former, but *Time-Warner*, a 1989 decision by the Delaware Supreme court criticized these holdings and came out on the latter side.<sup>30</sup> States other than Delaware either follow Delaware law or give wider discretion to boards than Delaware does.<sup>31</sup>

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law); *Topper Acq. Corp. v. Emhart*, C.A. No. 89-00110-R (E.D. Va. 1989) (construing Virginia law).

<sup>26</sup> See, e.g., *Amalgamated Sugar*, 644 F. Supp. at 1234.

<sup>27</sup> See, e.g., *Gelco Corp. v. Coniston Partners*, 652 F.Supp. 829 (D.Minn. 1986), *aff'd* in part and vacated in part, 811 F.2d 414 (8th Cir. 1987) (Minnesota law); *Dynamics Corp. of America v. CTS Corp.*, 637 F.Supp. 406 (N.D. Ill. 1986) (Indiana law); *Harvard Indus. Inc. v. Tyson*, [1986-1987 Transfer Binder] Fed. Sec. L. Rep. (CCH) ¶ 93,064 (E.D. Mich. 1986) (Michigan law); *A Copeland Enterprises, Inc. v. Guste*, 706 F. Supp. 1283 (N.D. Tex. 1989) (Texas law); *Georgia-Pacific Corp. v. Great N. Nekoosa Corp.*, 728 F. Supp. 807, 811 (D. Me. 1990) (Maine law); *Realty Acquisition Corp. v. Property Trust of Am.*, 1989 WL 214477, at \*2 (D. Md. Oct. 27, 1989) (Maryland law); *Amanda Acquisition Corp. v. Universal Foods Corp.*, 708 F. Supp. 984, 1009 (E.D. Wis.) (applying Wisconsin law), *aff'd* on other grounds, 877 F.2d 496 (7th Cir. 1989).

<sup>28</sup> Eric S. Robinson, John C. Coates IV, and Mitchell S. Presser, *State Takeover Statutes: A Fifty State Survey* (1989) (privately published, on file with authors).

<sup>29</sup> Fleischer & Sussman, *supra* note 24, at 5.06 THE LEGALITY OF THE POISON PILL [B][2]

<sup>30</sup> A related issue is whether a company with a staggered board could continue holding a pill in place would be forced to redeem a pill after losing one round of board elections to a raider. While many commentators believe that this issue was also resolved by *Time-Warner* (in that they could), our colleague Guhan Subramanian has argued that the potency of staggered boards became clear only in the mid-1990s. See Guhan Subramanian, *Bargaining in the Shadow of Takeover Defenses*, 113 Yale L. J. 621, 627-628 (2003).

<sup>31</sup> Michal Barzuza, *The State of State Antitakeover Law*, 95 Va. L. Rev. 1973 (2009).

Because a flip-in pill that remains in place is a show-stopper, and because boards have wide discretion to use pills under *Unocal*, most M+A practitioners focused their attention on ways to overcome a pill. Here, the most popular technique became to conduct a proxy contest to oust the incumbent board while a hostile bid was pending, but before the bidder has acquired the requisite number of shares that made a pill non-redeemable by the board.<sup>32</sup> For companies without a staggered board, this technique involved only a modest delay and a modest increase in expenses.<sup>33</sup> For companies with staggered boards, the delay could be more severe. As a result, staggered boards (in conjunction with ubiquitous shadow pills) came to be seen as one of the most potent takeover defenses.<sup>34</sup>

### **B. Anti-Takeover Statutes in Light of Poison Pills**

If a pill is valid, it is easy to see how the most commonly analyzed anti-takeover statutes become irrelevant.<sup>35</sup> A flip-in pill effectively prevents a raider from becoming a major shareholder. Business combination, fair price, and control share acquisition statutes apply once a raider has become a major shareholder: business combination statutes prohibit the raider to engage in a freeze-out merger or similar transaction with the target; fair price statutes set a minimum price at which other shareholders can be frozen out; and control share acquisition statutes deny voting rights to the shares held by the raider

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<sup>32</sup> See, e.g., AT&T Declares Proxy War In Bid To Control NCR, Chicago Tribune, Dec. 17, 1990, available at [http://articles.chicagotribune.com/1990-12-17/news/9004140410\\_1\\_ncr-shareholders-spokesman-dick-gray-control-ncr](http://articles.chicagotribune.com/1990-12-17/news/9004140410_1_ncr-shareholders-spokesman-dick-gray-control-ncr).

<sup>33</sup> American Law Institute, December 2-3, 2004, Takeover Law and Practice, Theodore N. Mirvis, Wachtell, Lipton, Rosen & Katz (“if a target’s charter does not prohibit action by written consent and does not provide for a staggered board, a bidder can launch a combined tender offer/consent solicitation and take over the target as soon as consents from the holders of more than 50% of the outstanding shares are obtained. Even if its charter prohibits action by written consent and precludes stockholders from calling a special meeting, a target without a staggered board can essentially be taken over once a year: by launching a combined tender offer/proxy fight shortly before the time of the target’s annual meeting. In contrast, a target with a staggered board may well be takeover proof until the second annual meeting.”)

<sup>34</sup> Lucian A. Bebchuk, John C. Coates IV & Guhan Subramanian, *The Powerful Antitakeover Force of Staggered Boards: Theory, Evidence, and Policy*, 54 Stan. L. Rev. 887 (2002).

<sup>35</sup> Consistent with our assessment of the significance of poison pills, Cremers and Ferrell find that the G-index interactions with a “Pre-1985” dummy (the year *Moran* was decided) yields significant results, while coefficient estimates for interactions with a pre-ATS dummy are close to zero and insignificant. See Martijn Cremers & Allen Ferrell, Thirty Years of Shareholder Rights and Firm Valuation, \_\_ J. Fin. \_\_ (2014).

unless other shareholders vote to grant such rights.<sup>36</sup> But if, as a result of the flip-in pill, a raider never acquires a significant stake, any statute that deals with what a raider can do *once it becomes* a major shareholder becomes moot. Similarly, flip-over pills, which make business combinations once a raider has acquired a large stake prohibitively expensive, render business combination and fair price statutes superfluous. Control share acquisition statutes, moreover, do not even purport to offer meaningful protection against hostile bids that are opposed by the board of the target, but are favored (as most “hostile” bids are) by a majority of the target’s shareholders.

Moreover, the principal mechanism to overcome a pill – obtaining board control before acquiring a significant stake – would also work to neutralize these anti-takeover statutes. Business combination statutes, fair price statutes, and control share acquisition statutes apply only to raiders or transactions not sanctioned by the incumbent board. Thus, for example, just as a board can redeem a pill before a bidder acquires a significant stake, a board can also approve an “interested shareholder” and thus eliminate the constraints imposed by a business combination statute.<sup>37</sup>

There are a few, minor caveats to this conclusion. First, in many states, the validity of flip-in pills was unclear in the late 1980s. Court rulings over the validity of flip-in pills during this period were split.<sup>38</sup> Pill validation statutes enacted during this period<sup>39</sup> are thus clearly important, especially in the few cases where they superseded prior case law. Yet they are ignored by most finance academics.

Flip-over pills, however, were not subject to equivalent uncertainty. They do not involve

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<sup>36</sup> CITES

<sup>37</sup> See, e.g., Delaware General Corporation Law, §203(a)(1). In order to increase its chances of obtaining board control through a proxy fight, a hostile bidder may acquire a stake in the target’s shares just below the threshold that would trigger the pill, and only then launch the proxy fight. Typically, poison pills only become triggered if some acquires 15% to 20% of the firm’s outstanding shares, and courts would probably frown at poison pills that set thresholds below 10%. In the case of a handful of states’ business combination statutes, the threshold for becoming subject to the moratorium imposed by the statute is 5% or 10% of the firm’s outstanding shares. For firms incorporated in these states, the business combination statutes constrain the maximum toehold a hostile bidder can acquire before running a proxy fight. In the case of these states, business combination statutes and poison pills may complement each other. In most of the cases, however, the threshold for becoming an “interested stockholder” under the business combination statute is set at least at 15% of the firm’s shares, and hence there are no complementarities between these statutes and poison pills.

<sup>38</sup> See supra text accompanying notes 22 to 29.

<sup>39</sup> CITES

discrimination among shareholders, have been found valid in numerous opinions,<sup>40</sup> and have not been struck down by any court as invalid in principle. While there may have been some initial uncertainty over the validity of flip-over pills outside Delaware, it was lower and evaporated much more quickly than the uncertainty over flip-in pills. In any case, prior to 1987, several circuit and district courts had uniformly ruled that anti-takeover statutes were unconstitutional.<sup>41</sup> It was only in April 1987, when the United States Supreme Court reversed these rulings in *CTS v. Dynamics*,<sup>42</sup> that these statutes were widely viewed as valid.<sup>43</sup> And even in the aftermath of *CTS*, several court decisions embraced a test for the constitutionality of anti-takeover statutes under which many business combination statutes would be invalid.<sup>44</sup> This would leave just a short period when anti-takeover statutes were viewed as likely

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<sup>40</sup> In addition to the decisions upholding flip-in plans, supra note 27, which explicitly or implicitly uphold flip-over plans, flip-over plans not involving any flip-in features have been upheld by *Moran*, 500 A.2d 1346 (Delaware law) and multiple Delaware cases following *Moran*; *Horowitz v. Southwest Forest Industries*, 604 F.Supp. 1130 (D. Nev. 1985) (Nevada law); *N.V. Homes v. Ryan Homes*, Civ. No. 86-2139 (W.D. Pa. Oct. 24, 1986); and *APL Corp. v. Johnson Controls, Inc.*, No. 85 Civ. 990 (E.D.N.Y. Mar 25, 1986) (Wisconsin law).

<sup>41</sup> See, e.g., *Icahn v. Blunt*, 612 F. Supp. 1400 (W.D. Mo. 1985); *APL Limited Partnership v. Van Dusen Air*, 622 F. Supp. 1216 (D. Minn. 1985); *Dynamics Corp. v. CTS Corp.*, 637 F. Supp. 389 (N.D. Ill. 1986), aff'd, 794 F.2d 250 (7th Cir.); *Fleet Aerospace Corp. v. Holderman*, 637 F.Supp. 742, 749 (S.D. Ohio), aff'd, 796 F.2d 135 (6th Cir. 1986), vacated by 481 U.S. 1026 (1987); *Gelco Corp. v. Coniston Partners*, 811 F.2d 414, 418 (8th Cir. 1987).

<sup>42</sup> 481 U.S. 69 (1987).

<sup>43</sup> See, e.g., Fred Axley, Roberta Blum Stein & Andrew McCune, *Control Share Statutes*, 8 N. Ill. L. Rev. 237, 237 (1987) (remarking that prior to *CTS*, the ability of states to regular takeovers was viewed as “severely limited”); Richard A. Booth, *Federalism and the Market for Corporate Control*, 69 Wash. U. L. Quart. 411, 411 (1991) (“Until 1987 the growing consensus was that the market for corporate control was distinctly interstate in character, and that only Congress and the Securities and Exchange Commission (SEC or Commission) had the authority to regulate it in any comprehensive way.”)

<sup>44</sup> See, e.g., *BNS Inc. v. Koppers Co., Inc.*, 683 F.Supp. 458 (D. Del. 1988) (holding that Williams Act preempts anti-takeover statutes that do not offer raider a “meaningful opportunity for success” and that Delaware statute satisfies that standard because it contains exception for tender offers that result in raider acquiring 85% of target stock); *West Point-Pepperell, Inc. v. Farley Inc.*, 711 F.Supp. 1096 (1989) (accepting the “meaningful opportunity of success” standard and holding that the Georgia statute satisfies it because it contains an exception for tender offers that result in raider acquiring 90% of target stock); *RTE Corp. v. Mark IV Industries*, No. 88-C-378, vacated as moot, [1987-1988 Transfer Binder] Fed. Sec. L. Rep. (CCH) ¶93,789 (E.D. Wis. 1988) (finding Wisconsin statute unconstitutional). The reasoning in these cases sheds substantial doubt on the constitutionality of the bulk of business combination statutes that contain no similar exceptions. See, e.g., *New Jersey Shareholders Protection Act: Validity Questioned in Light of CTS Corp. v. Dynamics Corp. of America*, 44 Bus. Law. 141 (1988). Subsequent circuit court decisions, however, rejected the “meaningful opportunity of success” standard. See *Amanda Acquisition Corp. v. Universal Foods Corp.*, 877 F.2d 496 (7<sup>th</sup> Cir. 1989); *WLR Foods, Inc. v. Tyson Foods, Inc.*, 65 F.3d 1172, 1180 (4<sup>th</sup> Cir. 1995).

constitutional but there was significant doubt about the validity of pills.

Second, it is theoretically possible that anti-takeover statutes might nevertheless matter if a court forced a board to redeem its pill. For example, when a company's failure to redeem a pill violates the *Unocal* standard, could a board instead use Delaware's business combination statute as a defense?<sup>45</sup> While this question has not been conclusively resolved, the answer in all likelihood is "no." In all likelihood, the standard a court would apply in deciding whether a board breached its duties in failing to redeem a pill would also apply in deciding whether a board breached its duties in failing to approve a transaction under the applicable anti-takeover statute.<sup>46</sup>

Even taken together, therefore, these caveats do not amount to much. Moreover, the available evidence indicates that the standard anti-takeover statutes, without the pill, are not all that powerful. In our research,<sup>47</sup> we found seven hostile bids, where a board could not use a pill but enjoyed the protection of a standard anti-takeover statute.<sup>48</sup> In none of these bids did the anti-takeover statute stop the hostile

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<sup>45</sup> When the *Revlon* standard applies, Delaware fiduciary duty law generally does not permit a board to use a pill to favor one bidder over another. *Mills Acquisition Co. v. Macmillan, Inc.*, 559 A.2d 1261 (Del. 1989) (subjecting discrimination among bidders to heightened scrutiny if company is for sale). However, Delaware's business combination statute also does not apply in such circumstances. See Sec. 203(b)(6).

<sup>46</sup> See [http://blogs.law.harvard.edu/corpgov/files/2009/11/Critique\\_Challenge\\_to\\_Del\\_Law.PDF](http://blogs.law.harvard.edu/corpgov/files/2009/11/Critique_Challenge_to_Del_Law.PDF) (Wachtell memo opining that "in any situation where fiduciary duties might compel a board to redeem a rights plan, they would also likely compel a board to waive Section 203's waiting period.") In the recent dispute involving the validity of the pill used by Airgas, none of the briefs gave much consideration to the implications for Delaware's antitakeover statute of a ruling that the pill was invalid. See <http://www.thedeal.com/magazine/ID/038635/2011/the-strange-case-of-section-203.php>. But see Guhan Subramanian, *Delaware's Choice*, 39 Del. J. Corp. L. 1, 36 (2014) (arguing that fiduciary duty law would not require a board to provide approval under Section 203).

<sup>47</sup> Our research consisted of a review of all opinions listed in *State Takeover Statutes: A Fifty State Survey*, supra note 28, a survey produced by Wachtell Lipton in December of 1989, where a court struck down a poison pill, to determine whether the target was protected by a business combination statute at the time and, if so, the outcome of the bid, supplemented by inquiries with M&A practitioners whether they were aware of any additional bids where the target could not use a poison pill.

<sup>48</sup> Certain anti-takeover statutes retain some (albeit modest) significance whether or not pills are valid. Probably the most important of these statutes is Massachusetts', which bestowed staggered boards on all Massachusetts companies, including those that had not adopted them in their charter. Next are statutes (and court decisions) like Indiana's, which expressly provide that defensive measures taken by boards are to be evaluated under the deferential business judgment rule. More marginally significant are disgorgement statutes (adopted by Pennsylvania and Ohio) or generic constituency statutes (adopted by a large number of states). These statutes, however, have not been the focus of the empirical literature.

raider.<sup>49</sup>

Thus, as a practical matter, standard anti-takeover statutes add little to the defensive arsenal of boards. Perhaps they might have raised by a small percentage and for a short period of time the likelihood that a target could successfully defend itself against a hostile bid. From the perspective of corporate lawyers, even such a marginal impact may well be worth the effort to get a statute adopted, especially if doing so also has a reputational payoff. If flip-over pills and business combination statutes were perfect substitutes and raise the likelihood of a successful defense by, say, 15%, and if there is a 10% chance that a court may find a flip-over pill invalid (while still allowing the target's board to shield the company behind the statute), why not propose to have the statute adopted? But, in our view, it is highly unlikely that such a small (1.5%) effect, which only becomes relevant if a hostile bid is made, would result in economically significant changes in managerial or firm behavior.

### **C. Law Meets Finance: What is Wrong (and what is Right) with Economists' Treatment of Anti-Takeover Statutes**

Financial economists employ varying methods of categorizing anti-takeover protection offered by states. The most common methods are to look either exclusively at when a state adopted a business

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<sup>49</sup> In four bids (involving West-Point Pepperell, Pillsbury, Irving Bank and Moore McCormack), the hostile bidder acquired the target despite the statute (the bidder for Pillsbury appeared to have satisfied the 85% tender exception to the statute, see *Grand Metro. Pub. Ltd. Co. v. Pillsbury Co.*, 1988 WL 130637(Del.Ch.1988)); in two bids (Emhart and Irving Bank), the target was sold to a third party that offered a higher price than the hostile bidder; one bid (Interco) was withdrawn while the target's appeal of the ruling requiring the redemption of the pill was still pending. See Emhart, B&D to Merge, *Tulsa World*, Mar. 20, 1989 (reporting the Emhart was acquired by Black and Decker which offered a higher price than Topper); Pillsbury Agrees to Takeover, *Star Tribune*, Dec. 19, 1988 (reporting that Pillsbury agreed to be acquired by Grand Met); West Point-Pepperell OKs \$3 billion bid from Farley, *Austin American Statesman*, Feb. 24, 1989; Nina Andrews, Southdown Will Buy Moore McCormack, *N.Y. Times*, Apr. 7, 1988, available at <http://www.nytimes.com/1988/04/07/business/company-news-southdown-will-buy-moore-mccormack.html>; Irving Bank Deal Completed, *N. Y. Times*, Jan. 2, 1989, available at <http://www.nytimes.com/1989/01/02/business/irving-bank-deal-completed.html> (reporting that Irving Bank was merged into Bank of New York); Appeal Moot in Interco Case, *N. Y. Times*, Nov. 19, 1988, available at <http://www.nytimes.com/1988/11/19/business/appeal-moot-in-interco-case.html> (reporting that Rales brothers had withdrawn offer while appeal pending); Rales Extend Tender Offer, Threaten to Withdraw it After Deadline, *AP News Archive*, Nov. 13, 1988, available at <http://www.apnewsarchive.com/1988/Rales-Extend-Tender-Offer-Threaten-To-Withdraw-It-After-Deadline/id-e89d93a04146d3ea545afac57f40ffc7> (reporting that Rales brothers threatened to withdraw bid unless target provided confidential information or entered into negotiations and that only barrier to completion of buyout was target's poison pill).

combination statute,<sup>50</sup> at when a state adopted the first of a set of statutes (usually business combination, control share, and fair price),<sup>51</sup> or at how many different types of statutes a state has adopted (with business combination, fair price, control share acquisition, constituency, and pill validation statutes being the types commonly considered).<sup>52</sup>

From a lawyer's perspective, these categorizations are nonsensical. They result in a gross mischaracterization of Delaware –a state that typically accounts for about half of the firm observations in the studies– as either having changed from a pro- to an anti-takeover state when it adopted its 1988 business combination statute or as being largely pro-takeover because it has only a single statute. This characterization ignores the centrality of case law on poison pills in Delaware and the fact that pills moot most other statutes.

Because pills have been valid in Delaware since 1985, the 1988 statute had a negligible effect on a target's ability to resist a hostile bid.<sup>53</sup> Rather, the most important legal developments for Delaware in 1988 were two opinions from the Chancery Court that imposed severe constraints on the use of the poison pill. These decisions caused Marty Lipton from Wachtell, Lipton, Rosen & Katz, one of the most prominent takeover defense lawyers of his generation, to send a memo to all firm clients describing these cases as “a dagger aimed at the hearts of all Delaware Corporations” and advising that they might have to consider reincorporating in a different state.<sup>54</sup> The fact that Delaware had passed its anti-takeover law a few months before these cases were decided – which, according to the coding used by many finance papers, is the *only* relevant event in Delaware takeover law in the entire 1980-2000 time

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<sup>50</sup> See, e.g., Bertrand & Mullainathan, *supra* note 4; Giroud & Mueller, *supra* note 1.

<sup>51</sup> See, e.g., Garvey & Hanka, *supra* note 2.

<sup>52</sup> See, e.g., Francis et al. *supra* note 7. A notable exception is a recent working paper by Karpoff and Wittry that considers business combination statutes, control share acquisition statutes, pill validation statutes, director duty statutes, and fair price statutes separately and controls for certain legal decisions. Jonathan M. Karpoff & Michael D. Wittry, Test Identification with Legal Changes: The Case of State Antitakeover Laws, available at [http://papers.ssrn.com/sol3/papers.cfm?abstract\\_id=2493913](http://papers.ssrn.com/sol3/papers.cfm?abstract_id=2493913).

<sup>53</sup> <http://www.thedeal.com/magazine/ID/038635/2011/the-strange-case-of-section-203.php>

88163338&pubNum=162&originationContext=document&transitionType=DocumentItem&contextData=(sc.Search)"; *City Capital Assocs. Ltd. v. Interco, Inc.*, 551 A.2d 787 (Del. Ch. 1988); *Grand Metro. Pub. Ltd. Co. v. Pillsbury Co.*, 558 A.2d 1049 (Del.Ch.1988).

<sup>54</sup> See Jeffrey N. Gordon, *Corporations, Markets, and Courts*, 91 Colum. L. Rev. 1931, 1959 n 95 (1991) (quoting from the letter).

period – did not play into his analysis at all.<sup>55</sup>

For states other than Delaware, studies that focus on business combination statutes have several problems. Most importantly, studies do not start from a valid theory on how anti-takeover statutes affect the target's *marginal* ability to defend itself.<sup>56</sup> Thus, the studies usually do not take account of the fact that targets in states where pills are valid have a high ability to defend themselves against takeovers even if the state has not adopted any anti-takeover statute.<sup>57</sup> For fair price and control share statutes, the studies ignore whether companies had adopted fair price charter provisions which offer protection similar to these statutes. Finally, many studies ignore the high degree of uncertainty over the validity of anti-takeover statutes prior to 1987 and all fail to account for the decline in uncertainty over the validity of both flip-over and flip-in pill in states without pill validation statutes.

The studies that add up the total number of statutes adopted are even more problematic. Four of the five types of statutes cover overlapping territory. As explained, pill validation statutes make business combination, fair price, and control share acquisition statutes moot; similarly, business combination statutes render the other two types largely irrelevant, and fair price and control share acquisition statutes overlap in that both mostly restrain coercive bids.<sup>58</sup>

One state that deserves particular mention is California. California is often singled out as the

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<sup>55</sup> In neither of the two takeover battles did Delaware's anti-takeover statute block the bid after the target board was forced to redeem the pill. See also *supra* note 49.

<sup>56</sup> As Karpoff and Wittry have pointed out, the claimed rationale for focusing on business combination statutes – that these statutes have been shown in event studies to have the largest impact on stock prices – is not supported by the empirical evidence, which shows that poison pill laws are associated with a larger impact on stock prices. See Karpoff & Wittry, *supra* note 52, at 8.

<sup>57</sup> These situations are by no means unusual. Thirty one states adopted a business combination statute at some point before 1995. Four states –Iowa, Nevada, Ohio, and Oregon– adopted a pill validation statute before adopting a business combination statute. Eight states –Idaho, Illinois, Indiana, Massachusetts, Pennsylvania, Rhode Island, South Dakota, and Wisconsin– adopted their business combination and their pill validation statutes at the same time. Three states –Georgia, Tennessee, and Washington– adopted a pill validation statute one year after adopting a business combination statute; finally, six states –Colorado, Florida, Hawaii, Mississippi, North Carolina, and Utah– adopted a pill validation statute and never adopted a business combination statute. Four states – Delaware, Michigan, Nevada, Pennsylvania – had case law upholding pills that preceded the state's business combination statute; a fifth state, Texas, had such case law and did not enact a statute before 1995.

<sup>58</sup> See Marcel Kahan, *The Demand for Corporate Law: Statutory Flexibility, Judicial Quality, or Takeover Protection?*, 22 J. L. Econ. & Org. 340 (2006).



only major state that has not adopted any anti-takeover statute. California definitely stands out, though not necessarily for that reason. It expressly prohibits discrimination among *shareholders* (a provision which casts unique doubt on the validity of flip-in poison pills);<sup>59</sup> it prohibited staggered boards until 1989 for all firms<sup>60</sup> and continues to prohibit them for firms that are not “listed”;<sup>61</sup> it prohibits a “for cause” standard for director removal, even for companies with a staggered board;<sup>62</sup> and it permits holders of 10% of the shares to call a special meeting (a right that cannot be narrowed in the company’s charter). In combination, these latter provisions make it so easy to replace a board (by calling a special meeting and removing a majority of the board) that they render the typical defensive devices (which must, of course, be approved and maintained by the board) less important. Even if California had adopted the standard anti-takeover statutes,<sup>63</sup> they could have easily been overcome by replacing the board. In other words, California is and has always been uniquely takeover-friendly, but for reasons other than the failure to adopt anti-takeover statutes.<sup>64</sup>

We believe that these problems make it very difficult, if not entirely impossible, to separate statistically the effect of takeover law from contemporaneous economic changes.<sup>65</sup> The econometric

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<sup>59</sup> California General Corporation Law, Section 203.

<sup>60</sup> *Id.*, Section 301.

<sup>61</sup> *Id.*, Section 301.5, added by Stats. 1989, c. 876, § 2. Listed firms include only firms with outstanding shares listed on the New York Stock Exchange, the NYSE Amex, the NASDAQ Global Market, or the NASDAQ Capital Market. *Id.*

<sup>62</sup> California General Corporation Law, Section 303. Removal of directors of companies at staggered boards, however, is subject to a higher voting requirement.

<sup>63</sup> In addition, California has, and had for the entire periods commonly analyzed, a unique anti-freeze-out statute that can also inhibit hostile tender offers. This statute prohibits cash-out mergers of minority shareholders of a company by a majority shareholder unless either all shareholders of the company consent, the majority shareholder owns at least 90% of the company’s stock, or the California Commissioner of Corporations approves the fairness of the merger. See California General Corporation Law, Sections 1101 and 1101.1. (For certain companies, different California officials must render approval.) Given that this provision can be overcome by just acquiring short of a majority of shares, it is probably not an effective anti-takeover device.

<sup>64</sup> Because these provisions have been part of California law for long periods of time, they pre-date the incorporation decisions of many, if not most, California companies. To that extent, the (lack of) protection they offer should be treated as endogenous, just like the presence and absence of anti-takeover charter provisions in endogenous, and California companies should be eliminated from the sample.

<sup>65</sup> We thus disagree with the assessment by Karpoff and Wittry, who criticize finance studies of anti-takeover provisions for their failure to account for the judicial and statutory validation of poison pills, that there are “simple

basis of the finance studies, and the factor that permits these studies to differentiate between anti-takeover statutes and economic changes, is that states adopted these statutes at different times. But if poison pills make anti-takeover statutes moot, and if other states are believed to follow the lead of Delaware law – as they did for flip-over pills -- then the validation of poison pill in *Moran* and subsequent Delaware cases on the use of pills affected all firms at the same time (albeit with potentially different intensities). But then, if firms, say, reduce their leverage in 1986, one cannot tell whether they reduced their leverage because the *Moran* decision in 1985 boosted their ability to resist a takeover or because of some other economic change that occurred in 1985. Anti-takeover statutes would thus only be relevant to the extent that they go beyond pills or are enacted in a state where a pill is not valid. And while some statutes fit this bill, they tend to affect only a small number of firms, they tend to relate only a few years of observation per firm, and they tend to entail only small changes in the ability to resist a bid.<sup>66</sup>

#### **D. The Effect of Anti-Takeover Statutes on Takeovers**

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data-oriented solutions to address” these problems. See Karpoff & Wittry, *supra* note 52, at 2. Karpoff and Wittry include in their regressions a “poison pill law” dummy that controls for pill validation statutes and, for Delaware firms, for the *Moran* decision. *Id.* at 17, 40. While this is a step in the right direction, it is for several reasons inadequate. First, it fails to account for the decline in uncertainty over the validity of both flip-over and flip-in pill in states without pill validation statutes (including in states other than Delaware with case law validating pills). Second, even assuming that pills are invalid outside Delaware absent a statute, it fails to account for the interaction of takeover defenses. Thus, for example, by adding separate controls for poison pill laws and business combination statutes but no control for the interaction of these statutes, the regression format assumes that business combination statutes have the same marginal effect whether or not pills are valid. However, as we discussed, there is no valid theoretical basis for this assumption.

<sup>66</sup> Examples of such statutes are statutes enacted prior to 1985, pill validation statutes that overturn case law invalidating flip-in pills, statutes that provide for a more lenient standard of review of anti-takeover defenses than the standard used in Delaware, or the Massachusetts statute that legislatively imposed staggered boards on all Massachusetts companies. While there are event studies analyzing the effect of some of these statutes on stock prices (see, e.g., Jonathan M. Karpoff & Paul H. Malatesta, *The Wealth Effects of Second-Generation State Takeover Legislation*, 25 *J. Fin. Econ.* 291 (1989) (pill validation statutes); Robert Daines, *Classified Boards and Corporate Control: Takeover Defenses After the Pill*, Working Paper 2011 (Massachusetts law)), we are aware of only one study that isolates the effects of such statutes on managerial or firm behavior and that study finds no robust effects for the statutes at issue. See Karpoff & Wittry, *supra* note 52, at 40-41 (reporting mixed results on the effect of first-generation anti-takeover laws on the number and citation of patents).

Unsurprisingly from the legal perspective, but problematically from the perspective of finance studies, there is no substantial evidence that anti-takeover statutes had a material effect on hostile bids in the post-1985 area, after the Delaware Supreme court upheld a flip-over pill in *Moran*. Thus, for example, in the most prominent study, Robert Comment and William Schwert find no evidence that control share acquisition or business combination statutes reduce the frequency of takeover bids.<sup>67</sup>

Finance scholars who study how these statutes affect firms grudgingly acknowledge the findings of Comment and Schwert, but often cite to two other studies that, so they claim, arrive at contrary results. One is a note by Jo Hackl and Rosa Testani published in the 1988 *Yale Law Journal*.<sup>68</sup> But Hackl and Testani's article contains no control variables and examines the 1981 to 1986 period, which predates the advent of poison pills.<sup>69</sup> The second is an article by Schwert from 2000.<sup>70</sup> Schwert's 2000 article contains no data at all comparing states with and states without anti-takeover statutes. He merely speculates, in a footnote, that the shift away from hostile transactions after 1991 "probably reflects the effects of antitakeover devices, such as poison pills and state antitakeover laws."<sup>71</sup>

A recent working paper by Cain, McKeon and Davidoff examines the impact of multiple types of anti-takeover statutes as well major court decisions on takeover.<sup>72</sup> In their most elaborate set of regressions, they find that fair price and control share acquisition had no significant effect on hostile acquisitions; the effect of business combination was negative, but non-robust. As Cain et al. caution, however, the value-weighted percentage of firms covered by business combination statutes jumps from 0% in 1984 to 95% in 1990, so that the variable through which they proxy for the incidence of business

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<sup>67</sup> Robert Comment & G. William Schwert, *Poison or Placebo? Evidence on the Deterrence and Wealth effects of modern antitakeover measures*, 39 *J. Fin. Econ.* 3 (1995).

<sup>68</sup> Jo Watson Hackl & Rosa Anna Testani, *Second Generation State Takeover Statutes and Shareholder Wealth: An Empirical Study*, 97 *Yale L. J.* 1193 (1988).

<sup>69</sup> *Id.* at 1212 (stating that authors examined offers made between June 1, 1981 and December 31, 1986).

<sup>70</sup> See, e.g., Cheng et al., *supra* note 3, at 641. Francis et al. *supra* note 7, at 130, Qiu & Yu, *supra* note 7, at fn. 14 (Schwert "*shows* that takeover rates indeed declined as a result of poison pills and state antitakeover laws" [emphasis added]).

<sup>71</sup> William Schwert, *Hostility in Takeovers: In the Eyes of the Beholder?*, 55 *J. Fin.* 2599, 2609 (2000).

<sup>72</sup> Matthew Cain, Stephen McKeon and Steven Davidoff Solomon, *Do Takeover Laws Matter? Evidence from Five Decades of Hostile Takeovers* (2014), available at [http://papers.ssrn.com/sol3/papers.cfm?abstract\\_id=2517513](http://papers.ssrn.com/sol3/papers.cfm?abstract_id=2517513)

combination statutes is very close to being a dummy that switches from zero to one in the late 1980s. Moreover, Cain et al.'s regressions do not control for overall changes in M&A activity over time (e.g. by including year dummies), and other studies suggest that friendly takeover activity declined substantially in the early 1990s.<sup>73</sup> It is thus not clear whether the estimates derived by Cain et al. are attributable to the effect of the anti-takeover statutes or to the changes in overall takeover activity over time.

### **E. Anti-Takeover Statutes and Real Effects**

The treatment of anti-takeover statutes in the finance literature is highly deficient because it fails to grasp the actual effects of anti-takeover statutes on a target's ability to defend itself. Put differently, the relationship between the measures of anti-takeover protection used by finance scholars studying anti-takeover statutes and the actual level of anti-takeover protection provided by state law for a generic firm is highly attenuated and noisy.

In many of the studies, this attenuated relationship is aggravated, in varying degrees, by several other factors. First, most finance studies of anti-takeover statutes do not consider relevant firm-level anti-takeover measures. In particular, three types of firm-level provisions are often relevant: fair price charter provisions (which resemble fair price statutes); high managerial stock ownership; and structural measures that make it more difficult for a raider to replace a majority of the target board (including staggered boards and provisions on shareholder rights to call a special meeting or act by written consent).

Assuming that anti-takeover statutes matter in the abstract, these firm-level provisions can either function as substitutes for or complements to anti-takeover statutes. For example, managerial control of a majority of the voting stock, either through high equity ownership or through ownership of a stock with high voting rights, on its own precludes a hostile bid and is thus a substitute for anti-takeover statutes.<sup>74</sup> Similarly, fair price charter provisions are a substitute for fair price statutes.<sup>75</sup> On the other

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<sup>73</sup> See Schwert, *supra* note 71, at 2607; Martijn Cremers & Allen Ferrell, *Thirty Years of Shareholder Rights Stock Returns* 8 (2012), available at [http://papers.ssrn.com/sol3/papers.cfm?abstract\\_id=2020471](http://papers.ssrn.com/sol3/papers.cfm?abstract_id=2020471).

<sup>74</sup> The protection offered by the present state of incorporation would be irrelevant even if managers planned to dispose of a substantial block of shares. If managers have voting control, they could use it to have the company

hand, staggered boards make business combination statutes and poison pills more effective because the way to overcome these defenses is to replace a board majority. They thus function as complements to these statutes. A failure to control for firm-level anti-takeover protection means that the relationship between the measures of anti-takeover protection used by finance scholars studying anti-takeover statutes and the level of anti-takeover protection for actual firms is even more attenuated and noisy.<sup>76</sup>

Second, finance studies suffer from varying degrees of coding errors. These errors arise from issues like the failure to control for companies that changed their state of incorporation,<sup>77</sup> from errors in the years in which anti-takeover statutes were adopted or became effective,<sup>78</sup> or from including in the analysis entities that are not subject to these statutes (e.g., because they are limited partnerships rather than corporations, or because they are not publicly traded corporations).<sup>79</sup>

Third, in some of the studies, the link between anti-takeover protection and the variable of interest would seem to be not all that strong. Surely a story can be told why takeover protection would be associated with, say, diffusion of lenders in loan syndicates.<sup>80</sup> But our prior would be that the relationship is not all that tight.

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reincorporate into a state that offers greater statutory anti-takeover protection if they so desired.

<sup>75</sup> See also Karpoff & Wittry, *supra* note 52, at 12-13 (criticizing studies for failure to take account of firm-level defenses and pointing to evidence from event studies that shows that anti-takeover statutes only had statistically significant effects on stock prices for firms without pre-existing firm-level defenses).

<sup>76</sup> Though firm-level provisions are endogenous, this is no justification for not including a control for the presence of these provisions before 1985, when the wave of second-generation anti-takeover statutes began. Firm-level provisions clearly affect the relative impact of statutes on firms. Fair price statutes, for example, have less of an impact, and should generate less of an effect, on firms with fair price charter provisions than on firms without such provisions. A study on the effect of such statutes would thus be bolstered by a finding that this effect is limited to firms that had not already adopted a fair price charter provision, and would be undermined by a contrary finding that the effect is similar for both firms with and firms without a charter provision.

<sup>77</sup> See, e.g., Bertrand & Mullainathan, *supra* note 4 (explaining why authors fail to control for reincorporations).

<sup>78</sup> See, e.g., *infra* text accompanying notes 86 to 89 (describing errors in Garvey and Hanka study); Bertrand & Mullainathan, *supra* note 4, at 552 (giving 1989 as enactment date of Pennsylvania business combination statute, while statute was enacted in March of 1988, effective immediately); *infra* note 104 (coding errors in Cheng et al. study).

<sup>79</sup> See *infra* note 109 (raising concerns about inclusion of entities other than public corporations in Qiu and Yu study).

<sup>80</sup> Bharath et al. *supra* note 10.

Given all that noise, we are somewhat surprised that so many studies find statistically significant and, in many cases, economically meaningful relationships between the adoption of anti-takeover statutes and firm behavior. It is, of course, possible, that we are wrong. Perhaps, contrary to our legal analysis, anti-takeover statutes have a much stronger relationship with actual takeover protection; perhaps the relationship between takeover protection and firm behavior is much stronger than we would have thought; and perhaps the relationship between these statutes and firm behavior is so robust that it remains strong despite the coding errors and the failure to account for firm-level defenses. Another possibility is that, against the odds, the authors of the studies got lucky. A third possibility is that there is something else that is going on that explains the results of the studies.

In the next three Parts, we review three studies of anti-takeover statutes. We picked these studies because they were published in top finance journals and because we were able to get access to most of the variables used by the authors in their analysis. The goal of our review is to shed more light on which of these possibilities accounts for the results found in these studies.

## **II. Anti-Takeover Statutes and Leverage**

One of the earlier articles on the effect of anti-takeover statutes (ATS) is Gerald Garvey and Gordon Hanka's study of the effect of these statutes on firm leverage.<sup>81</sup> The starting point of their paper is the view that leverage can keep managers on their toes. Managers, in turn, would prefer to issue less debt than shareholders desire. Since anti-takeover statutes are thought to make hostile takeover discipline less stringent, the argument then goes, managers of firms subject to ATS are likely to reduce the amount of leverage in their firms' capital structure.<sup>82</sup>

Garvey and Hanka's data consists of annual observations for 1200 publicly-traded firms over the 1982-1993 period. They construct their main explanatory variable, the "Protected dummy", as a dummy that switches from zero to one in the year after the firm's state of incorporation adopted an ATS.<sup>83</sup> All

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<sup>81</sup> Garvey & Hanka, *supra* note 2.

<sup>82</sup> *Id.* at 519-20.

<sup>83</sup> Although the paper is not entirely clear about which kind of statutes counts, it seems to include control share acquisition, business combination, and constituency statutes, and may or may not include fair price statutes. *Id.* at 522.

their regressions control for several standard firm characteristics (e.g., return on assets, stock returns, and book value of assets, all during the previous year). They estimate a linear specification in which the dependent variable is the change in leverage experienced by the firm in the year at issue. Their main result (which we analyze in much more detail below) is that the estimated coefficient for the Protected dummy is -0.013. Garvey and Hanka interpret this as an indication that, in each year after the firm's state of incorporation adopted an ATS, firms subject to the statute, on average, reduced their leverage by 1.3 percentage points relative to other firms not subject to an ATS.<sup>84</sup>

We will focus on two issues that we believe undermine the conclusions Garvey and Hanka attempt to draw from their study: coding problems and the way Garvey and Hanka control for time trends in leverage unrelated to the adoption of ATS.<sup>85</sup>

### *1. Coding Errors*

Coding problems are pervasive throughout Garvey and Hanka's paper. First, the authors wrongly claim that the business combination statutes adopted by Delaware and Pennsylvania only took effect in 1990.<sup>86</sup> In fact, Delaware's and Pennsylvania's statutes took effect in December 1987 and March 1988.<sup>87</sup>

Second, the authors have a peculiar way of dealing with states that had adopted anti-takeover laws prior to the *CTS* decision. They suggest, correctly, that these laws were of doubtful constitutionality

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<sup>84</sup> Id. at 522-530.

<sup>85</sup> Other scholars have found that Garvey and Hanka's results are not robust to alternative sample constructions. John & Litov, *supra* note 2.

<sup>86</sup> Garvey & Hanka, *supra* note 2, at 522.

<sup>87</sup> The authors state that their results continue to hold when they restrict the "Antitakeover sample" (i.e., the set of observations that correspond to firms incorporated in states that eventually adopted an anti-takeover statute) to firms incorporated in Delaware, and they view this as evidence of the robustness of their main results (Id. at 523). However, given that they miscoded the year in which Delaware adopted its statute, the "robustness check" is actually self-defeating, since it suggests that their estimations are picking the impact of a shock that differentially affected Delaware-incorporated firms after 1990. This conjecture is consistent with Figure 1 in their paper, which suggests that average leverage for Delaware-incorporated firms started to drop (relative to firms incorporated in the Antitakeover states) starting in 1990 (even though Delaware adopted its business combination statute in February of 1988). In addition, the result that Delaware-incorporated firms started reducing their leverage in 1990 is consistent with evidence we discuss in Section III about a credit-crunch that affected firms issuing speculative bonds after 1989.

and exclude firms incorporated in states that passed such laws before 1987.<sup>88</sup> Yet they do include firms from states, such as Minnesota, Ohio, New Jersey, and Virginia, that had adopted an ATS before *CTS* and then adopted *another* ATS after *CTS*.<sup>89</sup> The rationale, we presume, is that while the pre-*CTS* statute was invalid, the post-*CTS* statute was valid. This, of course, misconstrues the impact of *CTS*. Even if a statute was held to be unconstitutional by a lower court prior to *CTS*, these rulings did not erase the statute. Once *CTS* was decided, pre-*CTS* statutes were presumptively constitutional and firms incorporated in such states became subject to a valid anti-takeover law *immediately*, and not only at some later point when the state enacted a subsequent statute.

Third, the authors confine their analysis to business combination, control share and constituency statutes. They ignore states that had adopted pill validation statutes, which are at least as important. We estimate that these three coding errors results in a miscoding of the Protected dummy in, respectively, 70%, 16%, and 3%, of the firms in their sample.<sup>90</sup>

## 2. *Changes in Leverage over Time*

Moving beyond the coding errors, a further problem in Garvey and Hanka's analysis relates to the way they try to control for leverage trends over time unrelated to antitakeover statutes. To explain this problem, we have to take a brief detour to discuss the statistical technique known as "difference-in-differences."

Assume that one wanted to estimate the average causal effect of the adoption of a statute on some variable Y (e.g., the change in leverage, in the case of Garvey and Hanka's paper). Assume furthermore that one had data about this variable Y for firms incorporated in Maryland and California over several years, say 1986 to 1991. Finally, assume that Maryland adopted an ATS in 1989, while California did not adopt any ATS.

There are several ways one could try to estimate the average causal impact of the statute. One

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<sup>88</sup> Id. at 522, 523.

<sup>89</sup> Id. at 524 (showing inclusion of Minnesota, Ohio, and New Jersey, and Virginia). The authors list only 10 specific states, so this problem may affect other states as well.

<sup>90</sup> Our calculation of the incidence of miscodings is derived from table 2 in Garvey and Hanka's paper, which identifies 10 states and the number of firms each contributed to the sample. Our percentage calculation is the number of firms in a miscoded state divided by the aggregate number of firms in all 10 identified states.



could use data from 1990 and 1991, compare the average of Y for Maryland and California firms, and ascribe the difference to the impact of the statute. A problem with this approach is that the average of Y for Maryland and California firms may differ (and have long differed) for other reasons. Alternatively, one could use data from Maryland alone, compare the average of Y for Maryland firms for 1986-89 with the average for 1990-91, and ascribe the differences in averages to the impact of the statute. In this case, the problem is that there may have been some shock other than the adoption of the statute that may have caused the averages of Y in these periods to differ.

The “difference-in-differences” technique combines the previous two approaches. It first calculates the difference in the average of Y for Maryland firms between periods 1986-89 and 1990-91. It then does the same thing, but using the sample of California firms. Finally, it uses the latter difference in averages as a measure of the aggregate shock suffered by California firms (which were by hypothesis not affected by the adoption of the statute of interest) and assumes that Maryland firms suffered a similar aggregate shock, and that the only factor that *differentially* affected Maryland firms in the latter period is that Maryland adopted the statute of interest. Under that assumption, one can estimate the average causal effect of the statute adopted by Maryland by simply subtracting the difference in averages for California firms from the difference in averages for Maryland firms (hence the name “difference-in-differences”).<sup>91</sup>

To implement this technique, one regresses the variable Y against a constant, a “Maryland dummy” (which takes the value of 1 for the observations of Maryland firms and zero for California firms), a “period 2 dummy” (which takes a value of 1 for all observations corresponding to the years 1990 and 1991, and zero for the years 1986 to 1989), and a “statute” dummy (which takes a value of 1 for all observations affected by the statute –i.e., Maryland firms for 1990 and 1991–, and zero otherwise). The first two dummies take care, respectively, of the fact that firms from Maryland and firms from California may differ systematically (regardless of period) and firms may differ systematically between the period 1986-89 on one hand and the period 1990-91 on the other (regardless of where they are incorporated); and the coefficient for the “statute” dummy captures exactly the

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<sup>91</sup> Of course, this technique is not a silver bullet, since its assumptions may not really hold. An obvious situation where it would not hold would be a case in which there was some local shock that affected firms in Maryland (but not firms in California) in 1990 and 1991. The difference-in-differences technique would not allow one to disentangle the direct effect of this local shock from the effect of the statute, and a hasty interpretation of the results would lead one to think that the consequences of the local shock were actually consequences of the statute.

difference-in-differences described above. The technique can also be refined by using a separate dummy variable for each year (so-called “year fixed effects”) and, if more than two states are involved, by using a separate dummy variable for each state (so-called “state fixed effects”) and, of course, by adding further controls (e.g., the book value of the firm's assets or the stock return for the firm in the relevant year) as independent variables.

Garvey and Hanka's main estimates employ a coarse multi-state difference-in-differences specification.<sup>92</sup> Just like the “statute” variable in the difference-in-differences approach we discussed above, the variable “Protected” is 1 for the firms incorporated in a state that has adopted an ATS in any year after the statute was enacted (and 0 otherwise). Instead of including a separate dummy for each state, Garvey and Hanka use the coarser method of lumping together all firms in any state that at some time adopted and all firms in states that never adopted an ATS: for the former, the “State” dummy is equal to one on every observation; for the latter, the State dummy always is 0.

When it comes to controlling for changes over time, however, Garvey and Hanka depart from the differences-in-differences approach. They do include a dummy variable called “Time.” For firms in states that never adopted an ATS (control states), that variable takes the value of 1 in 1988 and thereafter (and is 0 otherwise). But for firms in states that did adopt an ATS, Time takes the value of 1 only in the year after the ATS adoption (and is 0 beforehand). Thus, for Maryland firms and the years 1988 and 1989, the Time variable would be 0; but for California firms for these years, the Time variable would be 1. Because the Time variable switches in different years in control states and in any ATS state that adopts a statute after 1987, the variable does not control for overall changes in leverage over time.<sup>93</sup>

To see the effect of the peculiar construction of the Time variable, assume that there are 2 firms, Firm A, incorporated in Maryland, and Firm B, incorporated in California, and that Table 1 below gives the value of the variable Y for the years 1986 to 1991. The value of Y is, in each year, identical for Firm

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<sup>92</sup> This regression is reported in column 3 of Table III of the paper. Garvey & Hanka, *supra* note 2, at 529.

<sup>93</sup> Garvey and Hanka attempt to control for shocks that occurred in a given industry and year by including as an independent variable the average change in leverage experienced by firms in the same industry and year as the firm in the observation at hand. As demonstrated by Gormley and Matsa, that is an inadequate way to control for the industry-year shocks, and including that independent variable may lead to more biased estimates than the ones would obtain if one simply omitted the control altogether. See Todd A. Gormley & David A. Matsa, *Common Errors: How to (and Not to) Control for Unobserved Heterogeneity*, 27 *Rev. Fin. Stud.* 617 (2013). By the same token, there is no reason to expect that variable to control for time trends.

A and B. This, therefore, represents a scenario in which the business combination statute had no impact and firms in both states experienced identical annual shocks. If one used this data to estimate a regression of Y against the Protected dummy, the State dummy and the Period 2 dummy, the estimate one would recover for the coefficient of the Protected dummy would be zero.<sup>94</sup> This is exactly what one would expect to recover from a difference-in-differences analysis. But if one instead used this data to estimate a regression of Y against the Protected, State, and Time dummies as defined by Garvey and Hanka, the estimate of the coefficient for Protected would have a value 1.<sup>95</sup> In other words, even though the adoption of the business combination statute was completely irrelevant, the estimate of the coefficient from BC would seem to suggest otherwise. The intuition behind this result is straightforward: by including Time (instead of Period 2) as a control, one is using the observations of Firm B for years 1988-1991 to construct the counterfactual of the outcome experienced by Firm A in 1990-1991. That is to say, one is comparing apples to oranges.

Table 1: Example of Difference-in-Differences Methodology

	1986	1987	1988	1989	1990	1991
Firm A	1	1	0	0	1	1
Firm B	1	1	0	0	1	1

To determine whether the results reported by Garvey and Hanka would hold in a more proper difference-in-differences analysis, we constructed a sample that replicates that of Garvey and Hanka.<sup>96</sup> We then estimated a proper difference-in-differences specification with separate dummy variables for each state (state fixed effects) instead of the State dummy and separate dummy variables for each year

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<sup>94</sup> The coefficient for State would have an estimate of 0, the coefficient for Period 2 would have an estimate of 0.5, and the estimate for the constant would be 0.5.

<sup>95</sup> The coefficient for State would have an estimate of -0.5, the coefficient for Time would have an estimate of -.5, and the estimate for the constant would be 1.

<sup>96</sup> Although we were not able to exactly replicate the sample sizes and the average ratio of long-term debt reported by Garvey and Hanka, our replication of their main regression yielded estimates for the coefficients of the Protected and Time variables that were extremely close (in size and significance levels) to those reported by Garvey and Hanka when we employed their coding and used the Time variable they constructed. See *infra* Table 2.

(year fixed effects) instead of the Time dummy.<sup>97</sup> The estimate for the coefficient of Protected dropped to -.0036, and stopped being statistically significant.<sup>98</sup> (When, in addition, we corrected the miscodings described, our results remained essentially unchanged.)

**Table 2:** Garvey and Hanka Replication: Leverage and Anti-Takeover Statutes

	Protected	Time
Garvey and Hanka, Table III, col. 3	-0.013*** (0.004)	0.0093*** (0.004)
Our replication, same methodology	-0.013*** (0.004)	0.009*** (0.004)
State Dummy + Year Fixed Effects	-0.0035 (0.0022)	
State Fixed effects + Year Fixed Effects <sup>§</sup>	-0.0036 (0.0023)	
State Fixed Effects + Year Fixed Effects, coding corrected <sup>§</sup>	-0.0043 (0.0028)	

Note: Standard errors (in parentheses) are corrected for clustering at the firm level. <sup>§</sup> Since regressions with year fixed effects do not have a single equivalent to the Time dummy, no equivalent values can be reported.

Virtually all of the other tests reported by Garvey and Hanka are robustness checks that also include this peculiar Time dummy as a control.<sup>99</sup> Hence, the estimates for the coefficient of the Protected dummy in Garvey and Hanka’s regressions do *not* capture the impact of the ATS. When

<sup>97</sup> The standard errors we report for our estimations were calculated using errors clustered at the firm level. Using White-robust standard errors that do not allow for any kind of clustering yields similar results.

<sup>98</sup> John and Litov estimate a similar specification and report that their estimate for the coefficient of Protected equals -0.003, and is also insignificant at conventional levels. John & Litov, *supra* note 2, at 732

<sup>99</sup> The only regressions that do not include the Time dummy are regressions estimated using either data from only the 1983-1986 period or data from only the 1990-1993 period. Garvey & Hanka, *supra* note 2, at 529. The results of the latter regressions indicate that firms incorporated in states that did adopt anti-takeover statutes decreased their leverage in the 1990 to 1993 period, relative to firms incorporated in states that never adopted anti-takeover statutes. This, however, does not present much evidence for Garvey and Hanka’s hypothesis. For one, there is a significant time gap between the adoption of ATS and the leverage changes found by Garvey and Hanka. Moreover, these regressions, by design, lack even the coarse controls for state (the State dummy) employed in the regressions with the Time dummy. The implicit assumption underlying the estimates is thus that leverage of firms incorporated in different states should have, but for the adoption of ATS (and other controls), followed the same trend. But Garvey and Hanka’s results for the 1983-86 period indicate that firms incorporated in control states significantly increased their leverage relative to firms in ATS states in the period *predating* the adoption of ATS. Thus, their own results contradict the assumption that, but for ATS, leverage trends across states would have been equivalent and show that firms incorporated in Control states do not constitute a proper control group.

corrected for coding errors and properly specified, there is no evidence for an association between ATS and leverage changes.

### III. Anti-Takeover Statutes and Managerial Stock Ownership

The next paper we are examining, *Identifying Control Motives in Managerial Ownership: Evidence from Antitakeover Legislation* by Shijun Cheng, Venky Nagar and Madhav Rajan,<sup>100</sup> examines the relationship between anti-takeover statutes and managerial stock ownership.<sup>101</sup> Starting from the premise that these statutes are effective in deterring takeovers, the authors argue that, after their adoption, “managers do not need to hold as many shares as before to ensure their control.”<sup>102</sup> Their main hypothesis is therefore that the passage of these laws is associated with a decline in managerial stock ownership. In a series of regressions, using a sample of 587 large, publicly traded firms, which they follow throughout the 1984-1991 period, they find a negative and significant association between the adoption of an anti-takeover statute and the fraction of the firms’ shares owned by the firms’ managers and directors.

The main body of the paper contains two types of tests: panel regressions and firm-level regressions. For the panel regressions, the authors first transform the main dependent variable of interest, the percentage of shares of the firm owned by directors and officers, into  $\ln(1 + \text{Director/Officer Stockholdings})$ .<sup>103</sup> They then run a series of regressions, including controls for year, industry, and various firm-level characteristics, and a variable, *AfterLaw*, which equals one for a given firm in a given year if the firm’s state of incorporation had adopted its first ATS by the end of the previous year.<sup>104</sup> In

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<sup>100</sup> Cheng et al. supra note 3.

<sup>101</sup> They focus on the impact of the first of fair price, control share and business combination statutes that was adopted by a given state during their sample period. Id. at 640-642.

<sup>102</sup> Id. at 641.

<sup>103</sup> The variables *Director/officer Stockholdings* and *CEO Stockholdings* measure the percent of shares in the firm owned by directors and officers, and by the CEO, respectively. Such transformations are conventional in regressions.

<sup>104</sup> Cheng et al. incorrectly code some of the years in which states adopted their first anti-takeover statute. For example, the authors code Ohio as having adopted its first statute in 1990, when it adopted a control share statute in 1982, and Washington as having adopted its first statute in 1990, when it adopted a fair price statute in 1985.

these regressions, the estimate of the coefficient of the *AfterLaw* dummy is negative and statistically significant. Cheng et al. interpret this result as evidence that managerial stockholdings dropped after a firm becomes subject to an ATS.

The panel regressions, however, suffer from a serious methodological flaw. They do not control for the possibility that the firms that became subject to an ATS *always* had a lower director and officer (D&O) ownership than the firms incorporated in states that never adopted an ATS.<sup>105</sup> Consider, for example, two firms, Circle K, incorporated in Texas, a state that never adopted an ATS; and Eastman Kodak, incorporated in New Jersey, which adopted its first ATS in 1986. Throughout the 1984 to 1991 period, Circle K had high managerial ownership (say, 20% a year) and Eastman Kodak had low ownership (say, 0.1%). The way Cheng et al. look at the data, the average managerial ownership level in years where a firm was subject to an ATS was 0.1%, compared to 14.6% for years where a firm was not subject to an ATS.<sup>106</sup> But, of course, this difference cannot be attributed to New Jersey's adoption of an ATS in 1986 since neither firm, in the example, had a change in its managerial ownership. Rather it derives from the fact that the firm in the state without ATS had higher ownership than the firm in the state with an ATS throughout the whole sample period.

To determine whether and how this flaw affected the results derived by Cheng et al, we obtained ownership data from the same database of director and managerial ownership.<sup>107</sup> We were able to match

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By the same token, Cheng et al.'s coding assumes that Florida firms never became subject to an ATS, when in fact Florida adopted both a control share acquisition statute and a fair price statute in 1987. *Id.* at 646. Throughout this section we follow Cheng et al.'s coding for whether a given state had adopted an ATS by a given year. However, the results we report are not qualitatively different from the ones we recovered when we correctly coded the variable that describes whether each state had already adopted its first ATS in a given year.

<sup>105</sup> That is, they do not control for state fixed effects.

<sup>106</sup> The average for years where a firm was subject to an ATS would be the average of the 1987 to 1991 for Eastman Kodak and the average for years where a firm was not subject to an ATS would be the average of 1984 to 1991 for Circle K and 1984 to 1986 for Eastman Kodak.

<sup>107</sup> The ownership data, which relates to 792 firms, was kindly shared with us by David Yermack. We attempted to recover the state of incorporation of each of the 792 firms in Yermack's sample in multiple ways: first of all, we searched the different volumes published by the Investor Responsibility Research Center during the late 1980s and the 1990s. Second, we searched the firms' SEC filings from the second half of the 1980s using the SEC Online database in Westlaw. In both cases, we used the firms' names as the matching criterion. This process allowed us to recover the state of incorporation of 764 out of the 792 firms. After discarding 43 firms that reincorporated during the sample period, one firm that was incorporated in Puerto Rico, one firm that was incorporated in Panama, and 9 federally chartered institutions, we ended up with a sample of 710 firms.

710 firms with a state of incorporation.<sup>108</sup> The data indicate that, throughout the entire 1984-1991 period, average D&O ownership was systematically higher among the firms incorporated in states that never adopted an ATS than among firms that eventually became subject to an ATS. That is illustrated by Figure 1.<sup>109</sup> Moreover, the figure shows that average D&O ownership *increased* after 1988 among the firms in states that eventually adopted an ATS statute (ATS states) while it *decreased* among the firms incorporated in control states. Given that more than half of the firms in the sample became subject to an ATS for the first time in 1988 or thereafter, this seems at odds with Cheng et al.'s hypothesis.

We then ran regressions using a similar set of control variables as did Cheng et al. Employing the same methodology, we find, as did Cheng et al., a significantly negative coefficient for the *AfterLaw* variable. However, when we added an additional control for the state of incorporation (state fixed effects), which addresses the methodological flaw we discuss, the coefficient for the *AfterLaw* turned

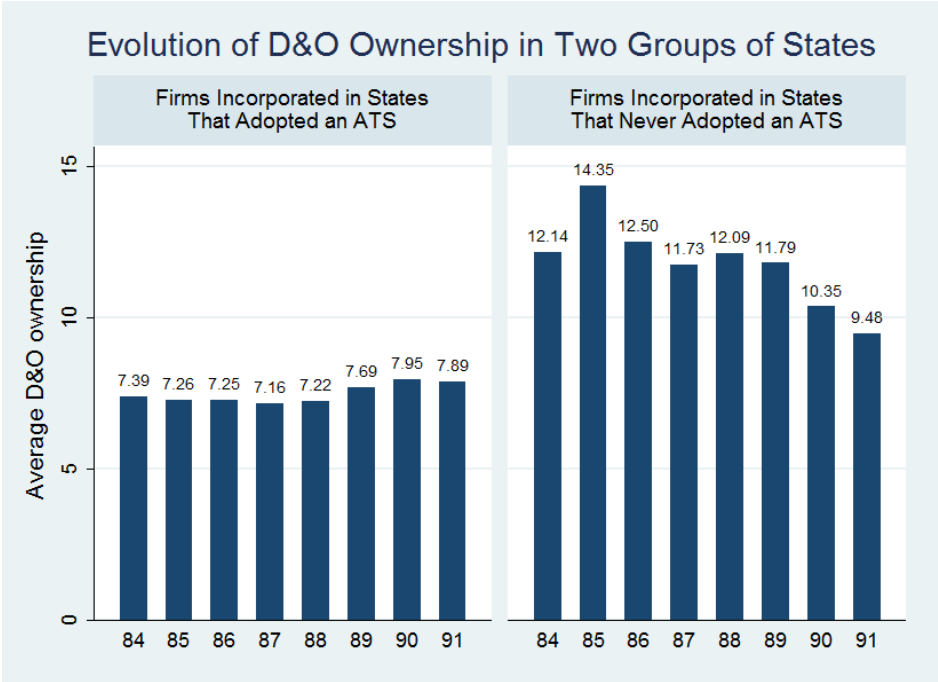
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<sup>108</sup> Our sample of firms is somewhat larger than Cheng et al.'s. They report that they were able to recover the state of incorporation for only 587 unique firms (p. 645). They do not indicate what criterion they followed to match the firms in Yermack's database with the databases from which they retrieved information about state of incorporation and we do not know why they were unable to match as many firms with a state of incorporation as we did. In any case, our sample resembles theirs in the distribution of firms across states of incorporation, in the mean and median ownership by officer/directors and by CEOs, and in other descriptive statistics. For example, mean (median) stock holdings by directors and officers was 7.855% (2.600%) in our sample and 8.163% (2.800%) in the Cheng et al. sample. Id. Table 2. The samples differ materially only with respect to the 75<sup>th</sup> percentile of the two stockholding variables, which were 8.7% and 0.68% for director/officers and CEOs, respectively, in our sample, and 18.5% and 3.74% in the Cheng et al. sample. The percentiles we recover for these variables are very close to the ones one would observe by analyzing the raw data for all 792 firms provided by Yermack.

<sup>109</sup> Yermack's data description indicates that, for the case of firms with dual-class share structures, he calculates the D&O ownership of the relevant firm by looking at the total fraction of *shares* owned by directors and officers. If one is interested in studying how shareholdings can substitute for statutory takeover protection, one should really focus on the fraction of *votes* owned by directors and officers. Given that in many cases dual-class share structures involve one class of stock with ten votes per share, dual class shares often render the firms takeover-proof, even if the D&O ownership variable, as coded, would suggest otherwise. For example, the proxy statement filed in 1989 by New York Times Co. indicates that the firm had approximately 78.5 million class A shares and 440 thousand class B shares outstanding; directors and officers owned 19 million class A shares, and 84% of the class B shares; class B shareholders had the power to elect 9 out of the 14 members of the board of directors. The firm, thus, was takeover-proof. However, D&O ownership in Yermack's database is reported as approximately 25%. By the same token, utilities are typically subject to state-level regulations that render them takeover-proof. Arguably, Cheng et al. should have excluded these types of firms from their sample. However, they do not report that they excluded any kind of firm from the original database provided by Yermack. Hence, neither do we. In any event, all of the results we report continue to hold in a qualitatively similar way if we discard utilities and firms with dual-class share structures.

(insignificantly) positive.<sup>110</sup> Put differently, after controlling for the fact that firms in states that adopted ATS had lower D&O ownership *in the years preceding adoption* than did firms in states that did not adopt ATS, the relation between D&O ownership and ATS evaporates.

Figure 1: Evolution of D&O Ownership



To their credit, Cheng et al. acknowledge the shortcomings of the panel regressions. They therefore proceed with a series of statistical analyses at the firm level that do not suffer from the methodological issues discussed above. In those analyses, Cheng et al. focus on the firms incorporated in states that eventually adopted an ATS statute, and study, for each firm, how the average percentage of shares owned by directors and officers changed between the years when the firm had not yet become subject to an ATS and the years in which the firm was already subject to an ATS.<sup>111</sup>

<sup>110</sup> The estimate of the coefficient for *AfterLaw* changed from -0.057 (significant at the 10% level) to 0.033 (insignificant). The estimate of the coefficient in the Cheng et al. regressions was -0.108 (significant at the 5% level). Id. at 651.

<sup>111</sup> Since this analysis focuses on changes in ownership for a given firm, this result cannot be ascribed to secular differences between the firms that were never subject to ATS and those that at some point became subject to one (or to the fact that the composition of firms in the different groups of firm changed due to entries and exits).



Cheng et al. convey the main result of this analysis in their Table 9. In their analysis, Cheng et al. do not use the actual difference in percentage ownership but the difference in a logarithmic transformation of the ownership percentage. We will refer to this variable as the “*transformed change in ownership*”.<sup>112</sup> They report that the mean value of the *transformed change in ownership* is -0.157 and that this mean is significantly different from zero at the 1% level.<sup>113</sup> In subsequent multivariate analysis with additional controls, they obtain similar results.

Table 3 below shows our replication of the analysis of Cheng et al. The second row of the table follows the same methodology as Table 9 in Cheng et al. In our sample,<sup>114</sup> we obtain values for the transformed ownership change (including a statistically significant decline in the mean value) similar to those reported by Cheng et al. But unlike Cheng et al. we also examine the mean and deciles of the *untransformed* change in ownership: for any given firm, the average of the ownership percentages in the pre-adoption years minus the average of the ownership percentages in the post-adoption years. The mean of that variable is -0.012 percentage points, meaning that, on average, ownership declined by about 1/100 of 1%, a drop that is economically trivial and statistically insignificant.<sup>115</sup> Basically, average ownership did not change at all in firms that became subject to an ATS.<sup>116</sup>

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<sup>112</sup> This variable is constructed as follows: for each firm that eventually became subject to an ATS, they calculate the average D&O stockholdings over the years during which the firm had still not become subject to an ATS, and the average D&O stockholdings over the years during which the firm was already subject to an ATS. They then subtract the first expression from the second, to recover, for each firm, a measure of the average change in D&O stockholdings between the “pre-treatment” years and the “post-treatment” years (call this measure “*average % change*”). They then construct the variable they use in their analysis as the sign of *average % change* times  $\ln(1 + \text{absolute value of average \% change})$ .

<sup>113</sup> Although Cheng et al. do not interpret these results, one possible way to understand them is the following: calculating the average of the transformed change in ownership is equivalent to calculating a weighted average of the *untransformed* change in ownership (in which changes in ownership of larger magnitude are assigned weights lower than those assigned to changes that are closer to zero in absolute value).

<sup>114</sup> Our sample for the firm-level tests, as Cheng et al.’s, is smaller than the respective sample for the panel regressions since it only includes firms from states that adopted a statute during the period of analysis.

<sup>115</sup> In unreported results, we performed a similar analysis as that of the second row of table 3, but using  $\ln(1 + D\&O \text{ stockholdings})$  –instead of *D&O stockholdings*– to construct the measures of average pre-treatment and average post-treatment D&O ownership. This transformed measure of ownership is the same that Cheng et al. use in the regressions they estimate in section 3 of the paper. The results we obtained were qualitatively similar to those of the third row of table 3.

<sup>116</sup> There is a second reason why the results reported in Table 9 of Cheng et al. probably overstate the change in D&O ownership experienced by the firms that became subject to an ATS. In using all the years before the firms

Table 3: Change in Ownership After ATS Adoption

Variable	N	Mean	Mean (p-value)	10 <sup>th</sup> Pctle	20 <sup>th</sup> Pctle	30 <sup>th</sup> Pctle	40 <sup>th</sup> Pctle	50 <sup>th</sup> Pctle	60 <sup>th</sup> Pctle	70 <sup>th</sup> Pctle	80 <sup>th</sup> Pctle	90 <sup>th</sup> Pctle
Cheng et al. Table 9 Transformed Change in Ownership	467	-.157	.006	-1.74	-1.09	-.626	-.281	-.033	.072	.288	.629	1.316
Our replication – same methodology and variable	610	-.095	.044	-1.57	-.977	-.531	-.203	-.017	.071	.240	.580	1.295
Change in % ownership (untransformed)	610	-.012	.964	-3.82	-1.66	-.700	-.225	-.017	.073	.271	.787	2.650

The table above,<sup>117</sup> of course, does not control for additional reasons why D&O ownership in a firm may have changed. In particular, it does not control for secular changes in ownership over time. We therefore ran a series of regressions including controls for firm and year fixed effects. This methodology, like the one employed by Cheng et al., is designed to tease out the factors that are related to a change in D&O ownership in a particular firm. Year fixed effects, however, are a more effective, and more conventional, way to control for ownership changes over time that are unrelated to anti-takeover statutes than the method used by Cheng et al.<sup>118</sup>

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became subject to the ATS to calculate the average “pre-treatment” D&O ownership, Cheng et al. implicitly assume that D&O ownership was stable in the years leading to the adoption of the ATS. Our look at the data suggests that that was not the case. Instead, ownership seems to have been trending downward even before the firms became subject to the statutes. We redid the calculations involved in table 3 using only the year immediately prior to the adoption of the first ATS to generate the “pre-treatment” baseline for each firm. In that case, the average of the transformed change in ownership became much smaller in magnitude (instead of -.095, as in table 3, it became -0.008, statistically insignificant at conventional levels).

<sup>117</sup> The fact that the average change in D&O ownership in the third row of Table 3 has a negative sign is not inconsistent with figure 1, which suggests that average D&O ownership increased slightly after 1988 (the year when a majority of the sample became subject to an ATS). The evolution of ownership depicted by Figure 1 is likely to be driven by entries and exits of firms from the sample, while the regression performs a “within-firm” analysis.

<sup>118</sup> Cheng et al. use a different methodology in the regressions they report in Tables 10 and 11. In those regressions, the dependent variable is the one described supra, note 112, and control variables are changes in the firm’s average market value, leverage, etc., experienced by the firm between the years when the firm was still not subject to an ATS and the years in which the firm was already subject to an ATS. To control for secular time

Table 4 summarizes the results. In specifications 1 and 2, the dependent variable is the fraction of shares owned by directors and officers; in specifications 3 and 4, it is the transformed ownership variable Cheng et al. use in their panel regressions. Specifications 2 and 4 include, in addition to firm and year fixed effects, firm-level controls like the ones included by Cheng et al. in their panel regressions.<sup>119</sup> The estimate of interest is that of the coefficient of the *AfterLaw* dummy. Notably, in each specification, the coefficient is statistically indistinguishable from 0, thus providing no evidence that the statutes are associated with a change in ownership. (For example, the point estimate of 0.112 for the *AfterLaw* coefficient in specification 1 indicates that, after a firm becomes subject to an ATS, D&O ownership tends to increase by approximately 0.1 percentage points, an increase that is statistically insignificant).

Table 4: Change in Ownership Regressions

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trends in ownership, they add as a control a variable (“*ownership trend*”) that proxies for the average change in D&O ownership experienced by the firms incorporated in the states that never adopted an ATS (the “control states”) during the relevant period. (The “relevant period” depends on the state of incorporation of the firm in the observation of interest. For example, if the observation corresponds to a Delaware firm, Cheng et al.’s ownership trend variable is a measure of the change in average director and officer ownership for firms in the control states between 1989-1991 and 1986-1988. (p. 662).) This attempt to control for secular trends suffers from multiple flaws. First of all, in order for the firms from the “control states” to be an adequate control, they should be comparable (in terms of size, industry, etc.) to those in the “treated states”. Our own look at the data suggests that this is not the case. Moreover, even if one disregards the concerns about bias, there is no way in which the regressions would allow one to test whether the change in D&O ownership experienced by the treated firms was significantly different from the one experienced by the firms in the control group. In a nutshell, the problem is that the regression does not “know” whether the variable that reflects the trend of the dependent variable is a very precise or a very noisy estimate of the evolution of average ownership among the firms in the control group. This problem is particularly significant because, according to the paper’s coding, only 35 firms did not become subject to any such statute during the sample period (and data for all these 35 firms may not even be available for their regressions). While the predicted value of the coefficient for the “ownership trend” variable is plus one, the estimate for that coefficient in Cheng et al.’s regressions is always negative, and often quite large in magnitude (even if noisily estimated). This suggests that, on average, even if the ATS had not been adopted, ownership trends in the two groups of firms would have moved in opposite directions. Consequently, the “control group” employed by Cheng et al. is unsatisfactory.

The appropriate way to tackle the concern about secular trends is to exploit the panel structure of the database, and (as we do) run a regression using a sample that includes both the firms that at some point became subject to an ATS and those that never became subject to one. Ownership trends can be controlled for by including year fixed effects. In addition, the panel structure allows one to control for differences in secular trends in ownership by including state fixed effects (or, even better, firm fixed effects, which also ensure that results are not simply driven by the fact that some firms enter or exit the sample).

<sup>119</sup> The estimates of the coefficients for those controls are unreported to preserve space.

	(1)	(2)	(3)	(4)
AfterLaw	0.112 (0.36)	0.327 (1.13)	0.009 (0.40)	0.028 (0.19)
N	5391	4780	5391	4780
Firm FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Other Firm Controls	N	Y	N	Y

Note: t-statistics (in parentheses) are corrected for error clustering at the firm level. Other firm controls are the same controls used by Cheng et al in the second column of Table 4.

All these results suggest that the findings reported by Cheng et al. are driven by methodological shortcomings in their analyses. When one analyzes the evolution of stock ownership more carefully, there is no evidence that directors and officers reduced their shareholdings once their firms became subject to an anti-takeover statute.

#### IV. Anti-Takeover Statutes and Bond Yields

The last article examining anti-takeover statutes that we examine is *The Market for Corporate Control and the Cost of Debt* by Jiaping Qiu and Fan Yu.<sup>120</sup> Qiu and Yu examine the relationship between business combination statutes and bond yields and conclude that these statutes are associated with a significant increase in yields.<sup>121</sup>

Qiu and Yu construct a yearly panel that spans the 1976-1995 period and includes yield data for bonds issued by approximately 700 individual firms.<sup>122</sup> The dependent variable in their regressions is the average yield spread over treasuries calculated over all of the outstanding bonds for the given firm in the relevant year. Controls in the regressions include year fixed effects; bond characteristics (e.g., the bond's

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<sup>120</sup> Qiu & Yu, supra note 7.

<sup>121</sup> These results are in tension with Garvey and Hanka's result that ATS are associated with a decrease in leverage. Generally, a decrease in leverage should result in decline in yields as debt becomes less risky. We are grateful to Zohar Goshen for alerting us about this contradiction.

<sup>122</sup> Qiu and Yu obtain bond yield information from the University of Houston's Fixed Income Database. Id. at 508.

duration and credit rating);<sup>123</sup> firm characteristics (e.g., profitability and leverage); and variables that attempt to control for shocks common to all firms operating in the same industry and year, and shocks common to all firms operating in the same location and year. Moreover, because Qiu and Yu employ firm fixed effects, their regressions are structured to show how bond prices for a particular firm changed over time.<sup>124</sup>

One of Qiu and Yu's main results is that the adoption of a business combination statute is associated with an increase in yield spreads for speculative-grade bonds.<sup>125</sup> Specifically, while they find no evidence of a significant increase in the yield spread for bonds that are rated investment grade, they find an increase of over 114 basis points for speculative-grade (a.k.a. junk) bonds.<sup>126</sup> For an average junk bond with 5 (respectively, 10) years to maturity that was traded at par before the increase in spread, an increase in spread of 114 basis points would be associated with a drop in price of approximately 5 percent (respectively, 8 percent).<sup>127</sup> Such an increase is enormous! One would have thought that a statute

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<sup>123</sup> For firms with more than one bond outstanding during a given year, the variables that control for bond characteristics (e.g., credit rating, duration) are defined for the relevant firm and year as the average of the respective bond-level variables across all bonds outstanding for that firm and year. *Id.* at 508.

<sup>124</sup> In other respects, the article is more problematic. Qiu and Yu appear to use annual prices in their regression but do not explain how these prices are derived from the monthly pricing data in the Fixed Income Database. *Id.* at 508. They also do not explain whether any data were not included in the analysis other than due to winsorizing all variables at the 1% level. If no other selection criteria were used, the sample presumably contains a large number of financials and utilities among the issuers and a large number of bonds issued by corporations that are not publicly traded or entities that are not corporations, for which business combination statutes are not relevant. Including financials and utilities is problematic because those firms tend to be subject to federal regulation, and their takeover is governed by rules that depend on the state where they operate (see, e.g., Robert M. Daines, *Does Delaware Law Improve Firm Value?*, 62 *Journal of Financial Economics* 525, 530 (2001)). Consistently with our belief that they may have included non-public firms, the Fixed Income Database includes many bonds issued by privately held firms, and none of the main specifications estimated in the paper includes controls that would only be available for publicly traded firms. In robustness check regressions including a control for institutional blockholdings, the sample size declines by 45%, suggesting that this variable was not obtainable for 45% of the sample, as would be the case for non-public firms. Table 9. *Id.* at 518. It is thus possible that many of the firms in Qiu and Yu's sample should not really be part of the sample.

<sup>125</sup> Qiu and Yu also conclude that the adoption of business combination statutes is associated with an increase in yield spreads for bonds issued by firms operating in concentrated industries. *Id.* at 513. Their analysis of the relation of business combination laws and competition raises issues that we do not address in this paper.

<sup>126</sup> *Id.* at 507.

<sup>127</sup> These estimates are based on our replication of the sample employed by Qiu and Yu. The average yield spread among junk bonds in our replication sample during 1986-1988 (that is, the period before

that caused such a price change would have attracted significant contemporary attention.

Qiu and Yu attribute their results to the “co-insurance effect”: the possibility that an acquirer’s strong financial position can make the repayment of the target’s debt safer. Business combination statutes, by making acquisitions less likely, would then reduce bond prices by reducing the likelihood of acquisitions that generate a co-insurance effect. In support, they cite a study by Billett, King and Mauer that finds that the price of junk bonds increased by 4.3% when their firm was acquired.<sup>128</sup> But Billett, King and Mauer explicitly exclude leveraged buyouts, which are associated with a decline in bond values,<sup>129</sup> from their sample. Their results thus overstate the average effect of all acquisition on bond values.

Most crucially, however, the Billett, King and Mauer study relates to the effect of *actual* acquisitions. The adoption of a business combination statute would have a much smaller effect, equal to the effect of actual acquisitions times the difference in likelihood that a firm is acquired *if it is* subject to a statute and *if it is not* subject to a statute. This difference is small: many firms would not receive any acquisition offer to start with; many offers are not opposed by management and thus not affected by a business combination statute;<sup>130</sup> and even with respect to hostile offers, the presence or absence of a statute is at most one of several factors that bears on the offer’s success. It thus makes no sense that an (at most) somewhat reduced prospect of a 4.3% increase in junk bond prices would account for an increase in yield of over 114 basis points. Something else must be going on.

We believe that this something else is the melt-down in the junk bond market after 1988. As

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most firms became subject to a business combination statute following Qiu & Yu’s coding) was approximately 5.5%.

<sup>128</sup> Matthew Billett, Tao-Hsien Dolly King, and David C. Mauer, *Bondholder Wealth Effects in Mergers and Acquisitions: New Evidence from the 1980s and 1990s*, 59 J. Fin. 107 (2004). When looking only at hostile acquisitions, the average effect drops to 3.2%.

<sup>129</sup> See, e.g., Paul Asquith & Thierry A. Wizman, *Event Risk, Covenants, and Bondholder Returns in Leveraged Buyouts*, 27 J. Fin. Econ. 195, 202-03 (1990) (finding losses of 2.8% from LBOs); Arthur Warga & Ivo Welch, *Bondholder Losses in Leveraged Buyouts*, 6 Rev. Fin. Stud. 959, 962 (1993) (losses of 6%).

<sup>130</sup> Even at its peak, the percentage of firms subject to hostile M&A activity in a given year in a sample of firms collected by Cremers and Ferrell did not exceed 0.5%. See Cremers & Ferrell, *supra* note 73, at 8. By the same token, Cain et al. report that, at any year during 1980-1995, the fraction of firms acquired by a hostile bidder never exceeded 0.25% of all publicly traded firms. Cain et al., *supra* note 72, at 12.

relayed by Robert Comment and William Schwert, “the junk bond market crashed in September 1989 when Campeau, which had become a major issuer of (non-Drexel) junk bonds, revealed the extent of its liquidity crisis and when UAL failed to secure buyout financing.”<sup>131</sup> Other contributing factors, according to Comment and Schwert, were the demise of Drexel Burnham Lambert in 1990 and the passage of federal legislation penalizing savings and loans for holding junk bonds in August 1989.<sup>132</sup> Finally, the United States experienced a recession between July 1990 and March 1991.<sup>133</sup> Junk bond issuers are particularly likely to be negatively affected by recessions, as the cash flows they rely on to repay their debt are likely to diminish. As a consequence, the average default rates for junk bonds during 1990-1992 were dramatically higher than their average default rates over the preceding decade.<sup>134</sup>

Figure 2 below depicts a time series of the yield spreads for portfolios of bonds of different rating categories -relative to the yield of a portfolio of AAA bonds- between July 1988 and July 1995.<sup>135</sup> As Figure 2 shows, the spread for investment-grade (AA- to BBB-rated) portfolios remained stable at between 30 and 130 basis points throughout most of the period. The spread for junk (BB- and B-rated) bonds moved in lockstep with the other spreads during late 1988 and early 1989. However, beginning at around March 1989, the spread for junk bonds began to drift away substantially from the spread for investment grade bonds. The difference in spreads between the two groups peaked during January 1991, and then began to drop, so that by mid-1992 the average spreads of all bond categories were, again,

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<sup>131</sup> Comment & Schwert, *supra* note 67, at 9.

<sup>132</sup> Comment and Schwert’s *ex post* analysis is consistent with the way the press evaluated the events as they unfolded. See, e.g., Anise C. Wallace, 'Junk Bond' Prices Fall Sharply, N.Y. Times, Apr. 14, 1989, available at <http://www.nytimes.com/1989/04/14/business/junk-bond-prices-fall-sharply.html?pagewanted=2&src=pm> (describing a trading day in which the average price of junk bonds dropped approximately 2 percent as “chaotic” and “a panic market”; arguing that the turmoil was driven by events related to the investigation of Drexel Burnham by the federal government; and noting that several savings banks were selling their portfolios of junk bonds because they expected being taken over by federal regulators). Our cursory review of news articles describing the junk bond market between 1988 and 1991 did not produce any evidence that the adoption of state anti-takeover statutes was perceived as a cause of the turmoil in that market.

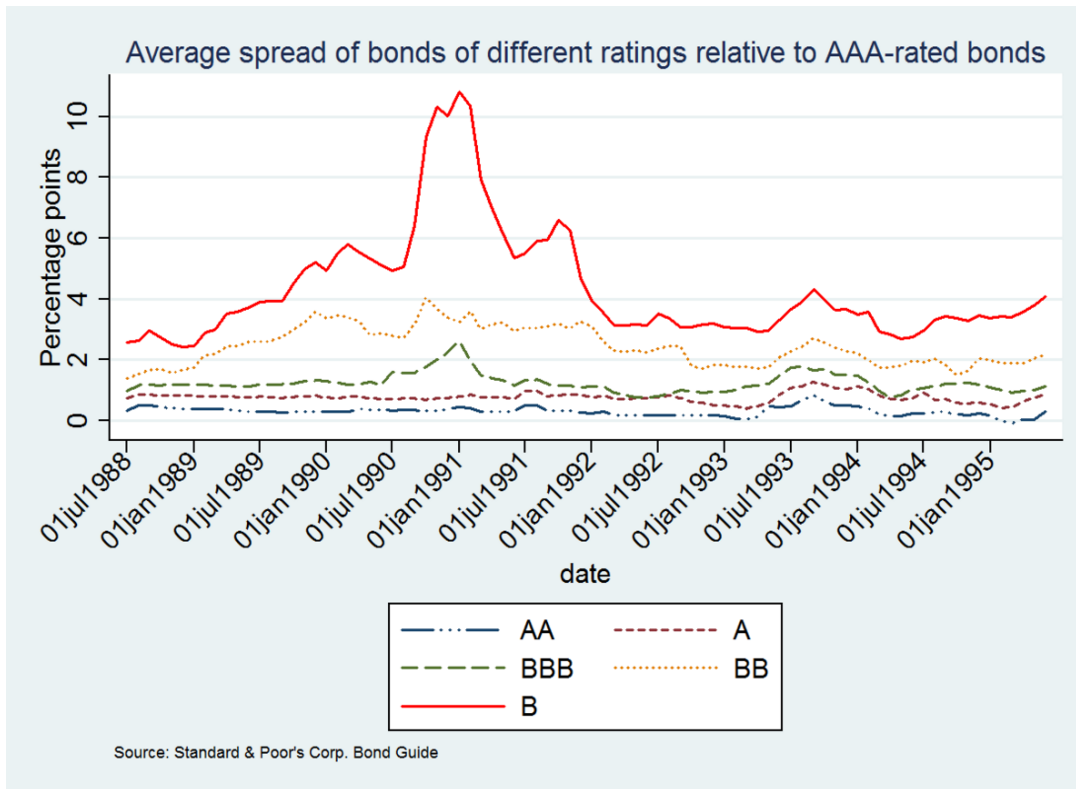
<sup>133</sup> <http://www.nber.org/cycles.html>

<sup>134</sup> Jean Helwege & Paul Kleiman, *Understanding Aggregate Default Rates of High Yield Bonds*, 2 Current Issues in Econ. & Fin. 6 (1996).

<sup>135</sup> The figure was constructed using data from the Standard & Poor's Corporation Bond Guides.

moving in lockstep.<sup>136</sup>

Figure 2



According to Qiu and Yu's coding, 57% of the sample firms were incorporated in states that adopted a business combination statute in 1988 and another 14% in states that adopted a statute in 1989. Hence, the steep increase in the spreads faced by junk-bonds in 1989-1991 raises serious omitted variable bias concerns: much of the impact that the paper ascribes to the statutes may simply be due to the fact that the adoption of those statutes coincided with the shocks to the bond market and that these shocks are not adequately controlled for.

With this potential explanation in mind, let us take a closer look at the regressions in the Qiu and Yu paper. In the regressions that use all the observations in their full sample,<sup>137</sup> explanatory variables

<sup>136</sup> Figure 2 only depicts the average spreads for bonds rated B or higher. The spike experienced by bonds with lower ratings was even more extreme and Qiu and Yu report that their sample includes bonds rated all the way down to D. Qiu & Yu, *supra* note 7, at fn 7.

<sup>137</sup> See Qiu & Yu, *supra* note 7, at 515 (Table 6, column 3).



include a dummy for whether the firm is incorporated in a state that has adopted a business combination law in the prior year or before, the bond credit rating, year fixed effects, several other control variables not relevant to the issues we discuss, as well as the variable *BC\*Speculative* that takes the value of 1 if the bond is rated junk and the issuer is incorporated in a state that has passed a business combination law by the relevant year (and zero otherwise). It is for this *BC\*Speculative* variable that the high estimate is obtained.

The functional form in these regressions posits that the relationship between credit rating and yield spread is both linear and stable over time. For example, based on the coefficients reported in Table 6 (column 3), each one-step reduction in credit rating is associated with an increased yield of 12 basis points, whether the rating decreases from AA to AA- or from BBB- to BB+ or whether that decrease occurred in 1976 or 1992. The linear and stable relationship between credit rating and yield spread is a constraint imposed by the regression format, not a result of the regression. To the extent that, in actuality, variations in rating at different times do not have the same effect on the yield spread, the regression will not be able to adjust for this and will instead report an average effect. As shown in Figure 2, the yield spread for junk bonds substantially widens right around the time firms became subject to business combination statutes.<sup>138</sup> When the yield spread on junk bonds (but not on investment grade bonds) rises in 1989 and thereafter, this rise may therefore push up the estimate for the coefficient of the variable *BC\*Speculative Grade*.<sup>139</sup>

To test our hypothesis that Qiu and Yu's estimate reflects the collapse in the junk bond market that occurred at about the same time as the wave of business combination statutes and is not controlled for in their regressions, we replicated their study using the data and data sources that Qiu and Yu

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<sup>138</sup> The inclusion of variables for business combination laws or year fixed effects does not change this picture. The year fixed effects simply allow the yields of all observations belonging to a given year to move in tandem, regardless of the bond rating or the state of incorporation of the issuer (and since over 85% of the observations in the sample are investment grade bonds, the fixed effects will largely reflect the average shock to the spread of those bonds relative to the baseline year). The business combination dummy allows the yields of all observations belonging to firms incorporated in a state that has already adopted a business combination statute to move in tandem, regardless of the bond rating, or the particular year as of which the observation is dated (as long as the state at issue has adopted a business combination statute by then). But neither these nor other variables control for secular changes in the yield spread between different rating categories, like the ones discussed above.

<sup>139</sup> The melt-down hypothesis is also consistent with the dynamics described by Qiu and Yu in Table 4 (column 3), where they find that the impact of the BC statutes on spreads is only observed starting in the year *after* the statutes are enacted. Id. at 512.

describe in their article. When we estimated a specification using Qiu and Yu's methodology, we obtained similar results: the enactment of business combination statutes was associated with no significant change in the yield of investment-grade bonds, but with a steep and statistically significant increase in the yield of speculative grade bonds. But when we removed the constraint that the relationship between credit rating and yield be linear and stable over time, the result disappeared.<sup>140</sup>

In sum, the conclusions Qiu and Yu draw from their results – that business combination statutes account for the very large increase in yield spread for junk bonds -- are theoretically highly implausible.

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<sup>140</sup> In these regressions, we used year-rating fixed effects instead of the year and rating dummies used Qiu and Yu. By using this more granular specification, we are effectively comparing a bond issued by a firm that became subject to a BC statute with another bond of the same credit rating and in the same year issued by a firm that did not become subject to a BC statute. We used these granular fixed effects as controls in several specifications, including one using the same set of controls and Qiu and Yu. The estimates of the coefficients for *BC\*Speculative* and the sum of the estimates of the coefficients for *BC* and *BC\*Speculative* were insignificantly different from 0 in each specification. In some specifications, the estimate of the coefficient for *BC* was positive and significant, suggesting that BC statutes are associated with an increase in the yield for investment-grade bonds. This result, however, was not robust; nor would an increase in the yield for investment-grade bonds as a result of reduced takeover risk be predicted either by the co-insurance effect nor by the alternative hypothesis that takeovers are associated with a decline in bond values due to increased leverage. See Francis et al, *supra* note 7; see also *supra* note 129 (studies finding the bond values declined after leveraged buyouts).

The melt-down in the junk bond market, however, does not by itself explain why Qiu and Yu find a significant increase in the yield spread even in a separate regression that includes only junk bonds. Qiu & Yu, *supra* note 7, at 515 (Table 6, column 2). If the collapse of the junk bond market merely increased the spread between junk bonds and investment grade bonds, this effect would be controlled by year fixed effects in a regression estimated using only junk bonds. In replicating Qiu and Yu's result for the junk-bond only regressions, we did not obtain significant results whether we used their methodology or a methodology that permits the yield spread to vary across years and between categories. The results obtained by Qiu and Yu could be due to the fact that, during the credit market crash, spreads increased much more steeply for the junk bonds with the lowest ratings.

In particular, the junk bonds issued by Delaware-incorporated firms were of systematically lower rating than those issued by firms incorporated in states that never adopted a business combination statute. Moreover, Qiu and Yu report that all of their variables are winsorized at the 1% level. *Id.* at 509. Although this is a standard practice, it is problematic for the regressions they estimate using a subsample that consists exclusively of junk bonds. By definition, junk bonds are outliers in terms of their credit rating. Since the sample of junk bonds is approximately 13 percent of the total sample (Table 6), it follows that winsorizing 1 percent of the entire sample is equivalent to winsorizing approximately 8 percent of the observations of the sample that consists exclusively of junk bonds.

Instead, we suggest that the association between yield spreads and business combination statutes that Qiu and Yu describe is driven by omitted variable bias: a massive contemporaneous shock to the credit market, for whose impact Qiu and Yu's regressions do not adequately control, that increased the yield spreads for junk bonds. When we replicate Qiu and Yu's regression in a manner that controls for this impact, the association between the statutes and junk bond yields disappears.

## **V. General Problems in Existing Finance Studies**

In Parts II and IV, we have examined three finance studies. There are, of course, many other studies of anti-takeover statutes that we have not reviewed. In this Part, we will discuss three problems that, to our knowledge, affect all the studies on how anti-takeover statutes affect behavior. These problems, together with our argument regarding the relevance of these studies from a legal perspective, make us very doubtful that the results derived in these studies are causally attributable to anti-takeover statutes.

First, we explain why, *assuming that finance studies are correct about anti-takeover laws*, finance studies would suffer from a substantial endogeneity problem that affects the interpretation of their results. What we mean by "correct about anti-takeover laws" is that there is significant variation in the degree of protection offered by the laws and that this variation induces significant differences in the way managers and firms act.

Second, we discuss coding problems that affect even the best of the existing finance studies. These problems relate to miscodings of the year in which a state adopted a statute, to miscodings of the state in which a firm was incorporated, and to the failure to take proper account of the effect of poison pill validity.

Third, we discuss the omission of firm-level factors that ought to affect how firms respond to changes in anti-takeover laws. These factors include board ownership of shares that enable a firm to resist a hostile takeover attempt regardless of the level of anti-takeover protection provided by standard laws; board ownership of shares that, through the incentives it provides, severs the link between the anti-takeover laws and managerial and firm behavior; and firm-level defenses that make standard anti-takeover statutes largely irrelevant.

## A. Endogeneity

The premise underlying the finance studies of anti-takeover statutes is that these statutes are exogenous: which firms are subject to a statute, and when they become subject, is determined quasi-randomly, and is not “chosen” by a firm. As imagined by these finance studies, firms incorporate in a certain state before they know whether (and when) the state will adopt an ATS, some states then decide to adopt a statute, and firms are stuck with the decision made by the state.

This story, alas, is wrong. For one, firms can change their state of incorporation once they know which states have adopted anti-takeover statutes. Second, anti-takeover statutes affect a firm’s initial incorporation state.

These possibilities generate severe problems for the finance studies on anti-takeover statutes. Empirical scholars almost always obtain the information to define each firm’s state of incorporation from Compustat. However, the variable that Compustat uses to define where a firm is incorporated is set to be equal to the firm’s current state of incorporation. For example, if firm X reincorporated from Delaware to Nevada in 1990, and one downloaded the Compustat database after 1990, Compustat would provide no indication that the firm was not always incorporated in Nevada. Thus, finance studies look at where firms were incorporated many years after the passage of anti-takeover statutes, sometime (depending on the study) between 1995 and today. But where a firm is incorporated in 1995, or 2015, is endogenous: determined by choices made by firms, rather than by “random” decisions by states to adopt statutes in the late 1980s. And if the state of incorporation is endogenously determined, then it is improper to ascribe the results associated with the adoption of an ATS to the statute, rather than to the factors determining that firm choice.

### 1. Reincorporations

Finance scholars are aware that companies can move their state of incorporation. They ignore reincorporations, relying on an article by Marianne Bertrand and Sendhil Mullainathan that reports that only 3 companies in a sample of 200 reincorporated during a 20 year period.<sup>141</sup> Reincorporations, however, are substantially more frequent than Bertrand and Mullainathan found. Moreover, actual reincorporations are not the relevant metric.

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<sup>141</sup> Cite at 1053.

### a. Actual Reincorporations

To measure the quantitative impact of the problems we raise in this Part, we constructed a sample as follows: we started with the whole set of firms that appear in the CRSP-Compustat database over 1976-1995. We excluded firms whose *gvkey* Compustat identifier appears more than once in any given year —our inspection of CRSP data strongly suggests that these are firms that have more than one publicly traded class of stock, and hence are arguably at a much lower risk of being taken over by a hostile bidder.<sup>142</sup> We then excluded financials (firms for which the first digit of the primary SIC code was 6), utilities (firms for which the first two digits of the primary SIC code was 49), firms that went public after 1985 (see discussion below), and firms which did not appear in the database in each of the years 1985 to 1990.<sup>143</sup> This left us with 2412 unique firms (our “restricted sample”).

To determine the frequency of reincorporations, we obtained data on where firms were incorporated in 1989 from Compact Disclosure, and on where the firms were incorporated in the mid-1990s from SEC Analytics.<sup>144</sup> We supplemented these data with searches in Moody’s manual for reincorporations in the years 1985 to 1988. Out of the 2412 firms in our restricted sample, approximately 13% had changed their state of incorporations between 1985 – the year the first business combination statute was adopted – and the mid-1990s.

As to firms that reincorporated after some states had adopted an anti-takeover statute, the state of incorporation is clearly endogenous. These firms, at the time of reincorporation, could have either intentionally chosen, or intentionally avoided, a state with an anti-takeover statute. To the extent that other finance studies use later Compustat vintages to determine the state of incorporation, and assume that firms did not incorporate, or include fair price and control share acquisition statutes, which were adopted as early as 1983 and 1982, the percentage of firms that reincorporated will only be higher.

### b. Non-Reincorporations

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<sup>142</sup> Cite to Gompers, Metrick & Ishii, *Extreme Governance*

<sup>143</sup> Although this last filter biases our sample towards oversampling survivors, firms that did not appear in the sample during 1985-1990 will also have a lesser weight on the estimation of the impact of anti-takeover statutes, most of which were adopted during that period.

<sup>144</sup> The coverage of the SEC Analytics database gradually increases over time, starting in 1994, and becomes almost completely comprehensive by 1996. For each firm, we recover the firm’s state of incorporation as of the firm’s earliest occurrence in the database.

Firms that did not reincorporate, on some level, also made a choice: to remain in their state of incorporation. For any given firm that, say, is happy with the anti-takeover protection provided by its incorporation state and therefore chooses not to reincorporate, the incorporation state is as endogenous as for a firm that changes the state of reincorporation to obtain the desired level of anti-takeover protection. In our view, if anti-takeover statutes mattered as much as finance scholars claim, a good claim can be made that the 1995 incorporation state is endogenous for all firms.

To examine this issue in more depth, one needs to examine why most firms did not reincorporate. One possibility is that the differences in the level of anti-takeover protection between most states were relatively minor. This would be consistent with our assessment, but not with the premise of the finance studies of anti-takeover statutes.

A second possibility is that reincorporations are very costly. Reincorporations, however, are pretty cheap. Bernie Black, for example, estimated in 1990 that the costs of reincorporation for a company with 100,000 shareholders are \$40,000 to \$80,000.<sup>145</sup> Fees in that magnitude are hardly sufficient to prevent a firm from reincorporating. Moreover, it is hard to see why managers would, for example, care much reincorporation costs but react to the passage of anti-takeover studies by, as found by Bertrand and Mullainathan, paying their workers higher wages or permitting a decline in productivity and profitability.<sup>146</sup>

A third possibility is that the board of directors and a majority of shareholders cannot agree to move to a different state, thus creating a stalemate. To reincorporate, a publicly traded company merges with a wholly owned subsidiary that is incorporated in a different state. This merger requires approval by both the board and holders of a majority (or sometimes a supermajority) of the outstanding shares

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<sup>145</sup> Bernard S. Black, *Is Corporate Law Trivial? A Political and Economic Analysis*, 84 *Nw. U. L. Rev.* 542, 558 (1990). Romano, *CITA*, estimates fees to be higher, but Romano's estimate includes New York Stock Exchange listing fees, which were changed in 1987; SEC registration fees, which do not arise in a pure reincorporation; and the capitalized value of higher franchise taxes, which are relevant only for a reincorporation to Delaware. See Black at 587 – 588.

<sup>146</sup> See *supra* note \_\_\_. Put differently, if an anti-takeover statute generates economically meaningful effects that can be picked up in finance studies, then relatively modest costs of reincorporation should present no barrier to firms' changing their state of incorporations. Even if the costs of reincorporating were orders of magnitude higher, as long as they are not prohibitive, the state of incorporation will be endogenous for companies where the decision-makers care most about these statutes.

entitled to vote. If boards, say, systematically preferred a state that offers greater anti-takeover protection, and shareholders a state with less, firms would be stuck in the state in which it is incorporated.

Such a dynamic probably explains the failure of firms to adopt or eliminate staggered boards — an important anti-takeover device — in the years around 2000.<sup>147</sup> In the latter part of the 1990s, shareholders in many companies became unwilling to approve new staggered boards, but boards remained unwilling to eliminate staggered boards that were already in place.

But the situation in the 1980s was different. Shareholders regularly approved staggered boards and other anti-takeover devices. Thus, of companies without a staggered board in 1978, almost 50% had adopted one by 1990, with most of the adoptions taking place after 1982. During the 1980s, companies also regularly adopted anti-takeover “fair price” charter amendments. According to IRRC data, by 1987, 158 of 424 Fortune 500 companies had such provisions.<sup>148</sup> One-hundred-eleven of these 158 companies are in our dataset. Of these, 91% had adopted the charter amendment between 1983 and 1986. In only 5% of those companies did officers and the board own more than 30% of the company’s stock; in 64%, officers and the board owned less than 5%. In the same vein, Cremers et al follow a large sample of publicly traded firms over 1978-1990, and document that more than half of the firms that did not have a staggered board in 1980 had adopted such a structure by the end of the decade.<sup>149</sup> In a substantial fraction of companies, shareholders were thus willing to vote for anti-takeover devices — and would presumptively have been willing to vote for a reincorporation into a state with stronger takeover protection.

In some companies, we can be highly confident that obtaining shareholder approval would not have been a barrier to reincorporating. These are companies where the board controlled a substantial fraction of the shareholder vote. We could recover from Compact Disclosure the fraction of shares owned by directors and officers (D&O) as of 1989 for a total of 1809 of the 2412 firms of our restricted

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<sup>147</sup> Cite to Cremers, Litov and Sepe (2014), Fig. 1, available at [http://papers.ssrn.com/sol3/papers.cfm?abstract\\_id=2364165](http://papers.ssrn.com/sol3/papers.cfm?abstract_id=2364165)

<sup>148</sup> Virginia Rosenbaum, *Takeover Defenses: Profiles of the Fortune 500* (1987).

<sup>149</sup> Cite to Cremers et al, *supra* note 147, Fig. 2.

sample.<sup>150</sup> Out of those 1809 firms there were 542 firms (30.0%) in which D&O owned at least 30% of the firm's shares.<sup>151</sup>

## 2. Initial Incorporations

Even if publicly traded firms could not change their state of incorporation, a substantial endogeneity problem would remain. The problem is most clear for firms that went public after the first adoption of an anti-takeover statute that is studied. When these firms went public, they could have opted for a state that had adopted the statute at issue or opted for a state that, at least at present, had not adopted the statute.<sup>152</sup>

Moreover, as we discussed above, California has long-standing laws that, until 1989, prohibited staggered boards and that continue to require that companies permit shareholders to remove shareholders directors without cause and to call a special meeting. Companies that wanted to provide for staggered boards, director removal for cause only, and no shareholder right to call a special meeting – all provisions that make takeovers more difficult – could incorporate in other states. At least for companies headquartered in California, for which a California incorporation would be a natural option, the state of incorporation reflects a choice between a state offering laws facilitating takeovers and states that are neutral or anti-takeover even for firms that went public prior to the advent of second-generation anti-takeover statutes.

The table below provides some data on the numbers of firms in our sample affected by one or the other endogeneity concerns. In total, 39.5% of our sample is affected by at least one of these problems. This highly conservative estimate ignores firms that adopted staggered boards or that have a controlling shareholder and assumes that no firms for which we presently lack the requisite information has adopted

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<sup>150</sup> Although using the D&O ownership as of 1989 is less than ideal, we believe on the basis of David Yermack's data about ownership throughout 1984-1991 that D&O ownership is very stable within firm over the sample period.

<sup>151</sup> Similarly, when two companies with relatively equal size engage in a stock merger, they have significant discretion to chose which state the resulting company will be incorporated in. Although shareholders must approve these transactions, the shareholders vote is dominated by the economics of the merger transaction, not by the governance arrangements. See Lucian A. Bebchuk & Ehud Kamar, Bundling and Entrenchment, 123 Harv. L. Rev. 1551 (2010). Even in the 1995 to 2007 period, where shareholders were much more resistant to anti-takeover measures, companies were able to use such mergers to maintain and increase their takeover protection. *Id.* at \_\_\_.

<sup>152</sup> Reincorporations prior to an IPO are common, cheap, and do not involve conflicts with public shareholders.



a fair price charter provisions or has board ownership in excess of 30%. In the samples used in the finance studies, which (unlike ours) includes companies that went public after 1985, this percentage is in all likelihood substantially higher.

**Table 5: Fraction of firms suffering from various sources of endogeneity**

	Number	Percent
Total Restricted Sample Size	2412	100
Reincorporations 1985-1995	307	12.7
Companies with Board and Exec. Officer ownership > 30% [of 1809]	542	22.4
Companies based in California	345	14.3
Sum of categories (without overlaps) excluding estimates	952	39.5

#### B. Coding for Anti-Takeover Protection Supplied by Law

We already noted that some finance papers contain significant problems in the coding of anti-takeover protection. In this section, we want to generalize our criticism. To do so, we examine the coding methodology employed in by Bertrand and Mullainathan in their article *Enjoying the Quiet Life*.<sup>153</sup> We chose this article because it contains among the fewest coding errors, because it is widely cited, and because its general methodology has been followed by several other papers.

Coding problems fall into three categories. Miscoding when a statute was adopted or became effective; miscoding where an entity was incorporated during the relevant period; and ignoring other relevant anti-takeover provisions supplied by law.

Bertrand and Mullainathan focus on business combination statutes and make almost no mistakes in the year such statutes were adopted. However, they ascribe a wrong year to the Connecticut, Kentucky and Pennsylvania statutes and omit the 1991 Oregon statute. In our sample, that results in the miscoding of the variable that measures when a firm became subject to a business combination statute

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<sup>153</sup> See supra note 4.

for approximately 5% of the firms.

Bertrand and Mullainathan also treat the state in which an entity was incorporated in 1995 as the state in which it was incorporated during the entire sample period. However, a significant number of firms reincorporated between 1985 and 1995. In addition, some firms were partnerships during this period and thus not subject to anti-takeover statutes. We treat such instances as miscodings if they result in a firm becoming subject to a business combination statute in a year different from the one based on their 1995 incorporation.<sup>154</sup> Papers subsequent to Bertrand and Mullainathan generally rely on later vintages of incorporation data and thus contain more severe coding errors.

**Table 6: Firms affected by miscodings or alternative measures of takeover protection**

	I Wrong Year	II Wrong State	III Pill Statutes and DE	IV Pill Statutes and Case Law
% of firms in sample	5	4 <sup>155</sup>	61	68
% of firms in never BCS states	4	2	44	60
% of firms in eventually BCS states	5	4	64	70
% of firms in eventually BCS states (excluding Delaware)	10	2	21	34

Finally, we analyze the effect that a more proper treatment of poison pills would have on the Bertrand and Mullainathan coding. In column III, we examine the effect of recoding anti-takeover

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<sup>154</sup> In addition, we recode Comsat, a DC firm which is takeover proof as a result of federal law.

<sup>155</sup> The reason why the fraction of firms in sample for which reincorporations led to a miscoding of the BC dummy is only 4 percent (while the fraction of firms that reincorporated is 13 percent) is that most of the reincorporations were into Delaware after Moran was decided, but before Delaware had adopted its business combination statute. A majority of the firms that reincorporated were originally incorporated in states that had not adopted a business combination statute at the time of the reincorporation (most notably, an exodus of firms originally incorporated in California explains almost half of the reincorporations into Delaware).

protection assuming that pills are valid only if endorsed by statute or the state supreme court, that business combination statutes were regarded as valid throughout, and that pills and business combination statutes offer substantially similar anti-takeover protection. (In column IV, we examine the effect of recoding anti-takeover protection assuming that pills are valid whenever any court has passed on their validity on the law of the respective state.) We regard this as a likely understatement of the relative importance of pills as it ignores the substantial uncertainty over the validity of business combination statutes prior to *CTS*, that no court has found that flip-over pills were invalid, and that flip-in pills offer substantially more protection than business combination statutes.

As noted above, since Delaware validated poison pills three years before it adopted its business combination statute, coding firms as if they had become subject to takeover protection only after their state of incorporation adopted a business combination results in massive differences in the coding of the degree of protection afforded by state laws. The impact of miscodings is not limited to Delaware-incorporated firms, however. If one takes into account states that validated pills through either statute or case law, a very large fraction of the firms incorporated in states that never adopted a business combination statute were authorized to adopt a poison pill starting in some point in the late 1980s. By the same token, approximately one third of the firms incorporated in states (other than Delaware) that adopted a business combination statute were already authorized by their state of incorporation to adopt a poison pill by the time the firms became subject to a business combination law.

### C. Firm Level Considerations

There are strong reasons to believe that firms will differ in their response to a change in state-supplied anti-takeover protection. First, firms may have firm-level defenses that may make the state-supplied anti-takeover protection largely irrelevant. Second, management may have incentives that make the marginal effects of the takeover threat largely irrelevant.

We regard large stockholding by the board as particularly important in these respects. First, it provides a defense against a hostile takeover. Second, it provides significant incentives to increase the value of the equity held by the board. To the extent, for example, that the takeover threat induces management not to indulge in the quiet life (as argued by some commentators) or to pursue short-termism at the expense of generating long-term value (as argued by others), large shareholdings by the

board should produce significant counter-incentives. Reasonable minds may differ as to when board shareholdings become large enough to significantly reduce the threat of a hostile takeover or to overpower the incentives created by the takeover threat. We regard, respectively, a 30% and a 20% ownership stake as reasonable cutoffs. That is, once the board owns at least 30% of the company's stock, we would regard the protection afforded by that ownership as so significant that the additional protection offered by anti-takeover statutes is not material. And once the board owns at least 20% of the company's stock, we would regard the incentives provided by that ownership as so significant that the additional incentives generated by a lack of an anti-takeover statute are not material.<sup>156</sup> As noted above, 542 (approximately 30%) of the firms in our restricted sample for which we could recover director and officer ownership data reported that their directors and officers owned at least 30 percent of the firm's shares. Forty-four percent of the firms for which we could recover ownership data report that at least 20 percent of the firm's shares were owned by the firm's directors and officers.

The effect of director and officer ownership does not simply attenuate the impact of anti-takeover statutes across the board. It also renders the firms incorporated in states that never adopted a business combination statute a very poor counterfactual for those incorporated in states that eventually adopted such a statute. Among the first group of firms, the median director and officer ownership was approximately 27.2 percent, and the fraction of firms in which directors and officers held more than 30 percent of the shares was 46.6%. By comparison, among firms incorporated in states that eventually adopted a business combination statute, the median director and officer ownership was only 15.2 percent, and the fraction of firms whose directors and officers held more than 30 percent of the shares was 27.3 percent.

Director and officer ownership, however, is not the only dimension along which the firms incorporated in different states differ. Firms incorporated in states that never adopted a business combination statute are dramatically smaller than their peers. In our restricted, sample, the average (median) book value of assets reported for 1985 by the firms that never became subject to a business

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<sup>156</sup> To be sure, even for a firm with large board ownership, anti-takeover provisions may be important to the extent that they induce the board to reduce its ownership stake after the passage of these provisions. This, indeed, is the thesis tested by Cheng et al. and reviewed in Part III. However, Cheng does not find an economically significant effect of ATS on board ownership and, in our replication, the effect is neither economically nor statistically significant. Moreover, if an anti-takeover statute were to induce a board to reduce its ownership stake, this would have substantial bearing on the interpretation of the results of the finance studies.

combination statute was \$232 million (\$22.2 million), while the respective values for firms that eventually became subject to a business combination statute were \$908 million (\$67.4 million).

These differences (and possible multiple other, observable or unobservable differences) imply that firms in the states that never adopted a business combination statute and those incorporated in states that eventually adopted such statutes were likely to respond to aggregate shocks in very different ways. Using one group of firms as an input to construct the counterfactual for the other, thus, is likely to generate biased estimates of the impact of business combination statutes.

## **VI. Implications**

In this article, we presented our legal argument why most anti-takeover statutes had no or only a minimal impact on the ability of a target to resist a hostile bid. We then reviewed three studies, each published in a top finance journal, that claim to have found a statistical association between anti-takeover statutes and real economic effects and attributed these effects to the statutes acting as effective anti-takeover devices. For each of these studies, we have shown that the main results are due either to the omission of important control variables or to methodological flaws. We have also identified significant endogeneity, coding, and omitted variable problems that affect the other articles in this literature.

We started this article by pointing to a divide among scholars in their view of anti-takeover statutes. Legal scholars tend to dismiss them as barely relevant, while empirical finance scholars study them and find that they have significant effects. One contribution of this article is thus to show that the empirical results generated by finance scholars may be due to factors other than the causal effect of anti-takeover statutes.

But this article has important implications that go beyond adjudicating a debate between different branches of the academia. Most importantly, it calls into doubt much of the perceived empirical knowledge about the real economic effects of a takeover threat.

Starting in the 1980s, theorists took different positions on what these effects might be. One set of scholars argued that the threat of a takeover acts as a beneficial disciplining device that induces

managers to act in the interest of shareholders.<sup>157</sup> Another set of scholars argued that the threat of a takeover induces an excessive short-term focus by management and thereby lowers long-term shareholder value.<sup>158</sup> Yet others have suggested that the takeover threat leads management to take actions that benefit shareholders, but harm other constituents, and may therefore not enhance overall social value.<sup>159</sup>

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<sup>157</sup> Easterbrook & Fischel, *supra* note 13 (arguing that hostile tender offers are an important device to reduce agency costs); Gilson, *supra* note 13, at 841 (explaining that "it is now commonly acknowledged that the market for corporate control is an important mechanism by which management's discretion to favor itself at the expense of shareholders may be constrained"); Bebchuk, *supra* note 13, at 1047 (noting that the threat of takeovers induces managers to do more to maximize profit); Arthur Oesterle, *Delaware's Takeover Statute: Of Chills, Pills, Standstills and Who Gets Iced*, 13 Del. J. Corp. L. 879, 897 (1988) (arguing that "bootstrappers [raiders] may provide the best curative for lazy, inept, or self-interested managers"); Alfred Rappaport, *The Staying Power of the Public Corporation*, Harv. Bus. Rev., Jan.-Feb. 1990, at 96, 100 (explaining that the market for corporate control "represents the most effective check on management autonomy ever devised"); Elliott J. Weiss, *Economic Analysis, Corporate Law, and the ALI Corporate Governance Project*, 70 Cornell L. Rev. 1, 27 (1984) (arguing that "the market for corporate control in general, and tender offers in particular, are the most important disciplinary factors in the corporate governance system"). See also *Edgar v. Mite Corp.*, 457 U.S. 624, 643 (1982) (arguing that "[t]he effects of [inhibiting takeovers] are substantial. . . . The reallocation of economic resources to their highest valued use, a process which can improve efficiency and competition, is hindered. The incentive the tender offer mechanism provides incumbent management to perform well so that stock prices remain high is reduced."). The origins of this position date to Henry G. Manne, *Mergers and the Market for Corporate Control*, 73 J. Pol. Econ. 110, 113 (1965) (arguing that takeover threats encourage efficient management).

<sup>158</sup> Jeremy C. Stein, *Efficient Capital Markets, Inefficient Firms: A Model of Myopic Corporate Behavior*, 104 Q.J. Econ. 655 (1989) (developing model explaining why, in presence of asymmetric information, managers may behave myopically even when faced with rational stock market); Stein, *supra* note 14 (analyzing how myopic behavior might arise when takeover threats lead managers to seek high stock price in short term); Lucian A. Bebchuk & Lars A. Stole, *Do Short-Term Objectives Lead to Under or Overinvestment in Long-Term Projects?*, 48 J. Fin. 719 (1993) (model in which takeover threat can induce inefficiencies); Shleifer & Vishny, *supra* note 14; Lipton, *supra* note 14, at 6-7 (takeovers focus on short-term profits at the expense of long-term planning); Thomas L. Hazen, *The Short-Term/Long-Term Dichotomy and Investment Theory: Implications for Securities Market Regulation and for Corporate Law*, 70 N.C. L. Rev. 137, 205-206 (1991) (concluding that short-term planning has been overly emphasized by corporate investors and managers); P.F. Drucker, *Corporate Takeovers—What Is To Be Done*, 82 Pub. Interest 3 (1986); Lynn A. Stout, *Do Antitakeover Defenses Decrease Shareholder Wealth? The Ex Post/Ex Ante Valuation Problem*, 55 Stan. L. Rev. 845 (2002) (anti-takeover provisions encourage non-shareholder groups to make extra-contractual investments in corporate team production).

<sup>159</sup> See, e.g., Andrei Shleifer & Lawrence H. Summers, *Breach of Trust in Hostile Takeovers*, in CORPORATE TAKEOVERS: CAUSES AND CONSEQUENCES 33 (Alan J. Auerbach ed., 1988) (expropriation from labor); Mark L. Mitchell & J. Harold Mulherin, *The Stock Price Response to Pension Terminations and the Relation of Terminations with Corporate Takeovers*, 18 Fin. Mgmt. 41 (1989).

Takeovers and takeover defenses continue to generate significant controversy. Earlier this year, for example, a blue-ribbon commission co-chaired by Larry Summers -- a renowned economist and former U.S. Treasury Secretary and Harvard president -- endorsed a limitation on voting rights for short-term shareholders to make hostile takeovers more difficult. This, it is argued, would help combat excessive short-termism.<sup>160</sup> At the same time, under pressure of shareholder rights advocates, most large companies that used to have staggered boards decided to move to annual elections of the entire boards, thereby facilitating hostile takeovers.<sup>161</sup>

The long-standing debate over the effects of hostile takeovers has a curious parallel in a more recent one, with many of the same partisans and rehashing many of the same arguments, about the effect of activism by hedge funds.<sup>162</sup> Thus, a recent memo by Wachtell, Lipton claims that an article by Professors Allaire and Dauphin refutes evidence presented in an article by Professors Bebchuk, Brav and Jiang that hedge fund activism leads to improved operating performance by targeted companies.<sup>163</sup>

To empirically test the hypotheses about the effect of takeover threat, one would ideally want to compare two sets of firms -- one set which faces a sudden increase (or decrease) in the takeover threat and other set where the takeover threat is stable -- and compare how they perform. This is the rationale behind many of the studies on anti-takeover statutes. Thus, for example, the study by Garvey and Hanka, which we reviewed, concludes that firms that become subject to an ATS (posited to reflect a reduction in the takeover threat) increase managerial slack, consistent with the hypothesis that the takeover threat keeps managers on their toes. And a recent article by Julian Atanassov concludes that firms that become subject to an ATS experience a decline in innovation, a finding at odds with the hypothesis that the takeover threat induces short-termism.<sup>164</sup> But if the passage of an anti-takeover statute does not change the takeover threat facing a firm, as we claim in this article, these studies would have no bearing on how takeover threat affects behavior.

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<sup>160</sup> Report of the Commission on Inclusive Prosperity at 142, 159 (2015), available at <http://www.wlrk.com/docs/IPC-PDF-full.pdf>.

<sup>161</sup> See [http://dealbook.nytimes.com/2015/01/05/an-unusual-boardroom-battle-in-academia/?\\_r=0](http://dealbook.nytimes.com/2015/01/05/an-unusual-boardroom-battle-in-academia/?_r=0).

<sup>162</sup> See, e.g., Kahan & Rock, *supra* note 15, at 1083-91 (reviewing debate); Bebchuk, *supra* note 15

<sup>163</sup> Wachtell, Lipton, Rosen & Katz, Still No Valid Evidence that Attacks by Hedge Funds are Long-Term Beneficial to Corporations, their Shareholders or the American Economy, Jan. 20, 2015.

<sup>164</sup> See Atanassov, *supra* note 5.

To be sure, there are reasons other than anti-takeover statutes why firms differ in their susceptibility to a takeover. For example, firms with staggered boards or controlling shareholders are less subject to a takeover threat than firms without staggered boards and with dispersed shareholders. However, sorting firms along these metrics does not make for a reliable study of the effect of a takeover threat because any anti-takeover device that is adopted at the firm level is endogenous. That is, the very fact that one set of firms adopted the device and another set did not shows that these sets of firms differ; and it may be the underlying reason that accounted for the decision to adopt the anti-takeover device, rather than the anti-takeover device itself, that also accounts for the difference in other firm actions. By contrast, since anti-takeover statutes are adopted by states rather than individual firms, finance scholars have regarded them as exogenous.<sup>165</sup> But if these statutes do not have an impact on the takeover threat, or if (re)incorporation decisions render a firm's exposure to them endogenous, the single best source of unconfounded evidence for how the takeover threat affects real behavior becomes useless. As we see it, four decades of studying the effect of a takeover threat have yielded little knowledge. Rather than pouring even more energy into empirical studies of anti-takeover statutes, scholars should develop a different approach.<sup>166</sup>

Our findings also have some farther-reaching implications. The use by empirical scholars of anti-takeover statutes as a variable in their analyses, despite the lack of a well-grounded understanding of how these statutes function in actuality, reflects broader problems. There seem to be a number of law-related variables that lack coherent theoretical grounding but are frequently used by empiricists. Top of

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<sup>165</sup> See Karpoff & Wittry, *supra* note 52, at 18-19 (arguing that anti-takeover laws should be viewed as endogenous for at least 46 firms that promoted the passage of these laws).

<sup>166</sup> Another popular approach are event studies related to the enactment of anti-takeover statutes and to major legal opinions. Event studies on anti-takeover statutes have produced mixed results, with most studies either finding no significant effects or small negative effects. See Sanjai Bhagat & Roberta Romano, *Event Studies and the Law: Part II - Empirical Studies of Corporate Law*, 4 *Amer. L. & Econ. Rev.* 380 (2002). In legislative event studies, it is often difficult to identify the precise event dates. In event studies of legal opinions, it is often difficult to separate the legal event from other contemporaneous market-moving events. Even event studies that are able to develop an identification strategy that overcomes this problem (see, e.g., Alma Cohen & Charles C.Y. Wang, *How Do Staggered Boards Affect Shareholder Value? Evidence from a Natural Experiment*, \_\_ *J. Fin. Econ.* \_\_ (analyzing Airgas rulings which had disparate effects on companies with staggered boards depending on the timing of a company's annual meeting and finding that evidence consistent with the view that staggered boards reduce stock price)) at most measure the market's expectation of the effect of a legal event.



the list is the widely-used GIM governance/takeover index. Lucian Bebchuk, Alma Cohen and Allen Ferrell have shown that institutional investors care little or not at all about 18 of the 24 elements in the GIM index. They propose, as an alternative, an index based on the 6 factors that attract significant opposition by institutional investors.<sup>167</sup> Similarly, Michael Klausner has recently argued that the index contains elements that are irrelevant for all companies and elements that are irrelevant for a subset of companies. Even to the extent that the index captures useful variables, he explains, empiricists have not understood the underlying governance mechanisms and have misinterpreted their results.<sup>168</sup> Misinterpretations of this sort have a long pedigree. Already 15 years ago, John Coates had argued that economists widely misinterpret the import of a company adopting a poison pill.<sup>169</sup>

Another contemporary example of the use of theoretically spurious variables is the claim, as expressed in a *Journal of Financial Economics* article, that there were “mid-1990 case law changes in Delaware [that] affected only Delaware firms with staggered boards.”<sup>170</sup> While this notion, which forms the basis for several empirical articles,<sup>171</sup> derives from an article by Harvard Law Professor Guhan Subramanian, Subramanian never points to any case changes in Delaware law, – for the simple reason that there were none. Rather, he merely notes that there were three companies with staggered boards, each incorporated in Delaware, that successfully resisted takeovers. While this may well have affected market perception of the potency of staggered boards as an anti-takeover device, it is unclear why it would have had a materially differential impact on Delaware firms with staggered boards, as posited by finance scholars.

To our mind, what all of these instances have in common is that they reflect the generation of a variable – anti-takeover statutes, GIM Index, pill adoptions, the combination of having a staggered board and being incorporated in Delaware after the mid-1990s – that is easily available and exhibits significant cross-sectional and time-series variations that allow for an interesting statistical analysis.

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<sup>167</sup> Lucian Bebchuk, Alma Cohen & Allen Ferrell, *What Matters in Corporate Governance?*, 22 *Review of Financial Studies* 783-827 (2009).

<sup>168</sup> Michael Klausner, *Fact and Fiction in Corporate Law and Governance*, 35 *Stan. L. Rev.* 1325, 1368 (2013).

<sup>169</sup> See Coates, *supra* note 21.

<sup>170</sup> Rauh, *supra* note 11, at 399.

<sup>171</sup> See, e.g., Angie Low, *Managerial Risk-Taking Behavior and Equity-Based Compensation*, 92 *J. Fin. Econ.* 470 (2009); Yun, *supra* note 9.

Empiricists can use these variables, often in different permutations, in their tests to check how they relate a large set of potential outcomes – leverage, wages, patents, dividends, and so on -- employing various methodologies and adding various sets of controls.

Naturally, empiricists do not take kindly to the idea that such a neat tool should not be used, especially if that view is held by scholars in a different discipline who do not act as referees for their articles and who have little impact on their professional reputation. Put differently, just like managers suffer from agency costs that distort behavior, academics (finance but also law, and us included) have incentives that can distort behavior. And for empiricists, one of the potential distortions is to embrace variables that can be used for interesting empirical studies, and pay little heed to arguments that the variable has no theoretical validity.

Incentives also interact with spurious variables in another way. If anti-takeover statutes are, in fact, largely irrelevant, how is it that there are numerous studies that find statistically significant relations between these statutes and real effects, but few if any published studies that find no statistically significant relationship? We believe that the answer is rooted in the publication bias of empirical journals and the incentives that such bias generates for researchers. As any empirical scholar can confirm, it is much easier to get an article published that finds statistically significant results than to get an article published that finds no such results. The Economist, in a recent feature article, characterized publication bias as “pervasive” and counted it as a principal reason why many published research findings are false.<sup>172</sup>

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<sup>172</sup> Trouble at the Lab, The Economist, Oct. 19, 2013.