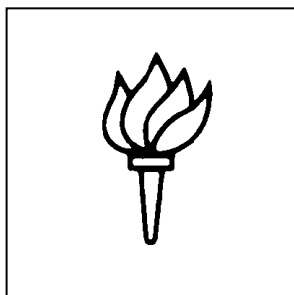# NEW YORK UNIVERSITY SCHOOL OF LAW

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The Law and Finance of Anti-Takeover Statutes

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Anti-Takeover Statutes

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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 bridge the divide between the law and the finance approach to antitakeover 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. Our paper calls into doubt most of the understanding of the effect of takeover threat, which is based to a large extent on studies of antitakeover statutes.

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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 bridge 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. Our paper calls into doubt most of the understanding of the effect of takeover threat, which is based to a large extent on studies of anti-takeover statutes.

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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,[1] leverage,[2] managerial stock ownership,[3] worker wages,[4] innovation,[5] dividend payout ratios,[6] bond yields,[7] executive pay,[8] cash reserves,[9] loan syndicate diffusion,[10] and the amount of employee stock in pension plans.[11] The popularity of these studies is not waning. Just last year, the Journal of Finance published an article on anti-takeover statutes and patents[12] and earlier this year, a new paper on anti-takeover statutes and diversifying acquisitions

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[1] 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.

[2] 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).

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

[4] 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).

[5] 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).

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

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

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

[9] Hayong Yun, *The Choice of Corporate Liquidity and Corporate Governance*, 22 Rev. Fin. Stud. 1447 (2009)

[10] 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.

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

[12] Atanassov, supra note 5.

was released.[13]

From a legal perspective, this is very odd. Corporate lawyers and academics generally dismiss most anti-takeover statutes as irrelevant. So why are finance professors studying 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. Because anti-takeover statutes were adopted by different states at different times, they generate a natural experiment on the issue of real interest to financial economists: 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 try to bridge 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 takeover threat. This poses the question of why finance studies of these statutes find results. In Parts II to IV, we therefore examine three studies in greater detail. For each study, we present evidence that the results are due to omitted variables, large scale coding errors, or improper specifications. When corrected for these problems, the association between anti-takeover statutes and the hypothesized effect disappears.

In Part V, 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 and suggests that the results of other studies reporting a statistically significant and economically meaningful relationship between firm behavior and anti-takeover statutes may also be due to other factors or caused by shortcomings similar to the ones in the studies we analyzed.

Second, and most importantly, our analysis calls into doubt most of our understanding of 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[14] or induces short-termism.[15] A similar debate is

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[13] 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

[14] 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*

waged today about the effect of activist hedge funds.[16] 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.[17]

In addition, our analysis has wider implications about the relationship between law and empirical economics more generally. 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

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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).

[15] 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).

[16] 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).

[17] 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-takoever statutes); Samuel H. Szewczyk & George P. Tsetsekos, *State Intervention in the Market for Corporate 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 an 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 form firms that do not (and these differences 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.

economists: it is attributable to the fact that researchers can easily use such variables to churn out empirical studies rather than to the fact that they are theoretically well grounded.

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 and 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 for $100 if a merger occurred.[18] Flip-over pills were quickly replaced by flip-in pills, 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."[19] 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

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[18] *Moran v. Household Int'l, Inc.*, 500 A.2d 1346, 1349 (Del. 1985).

[19] See Martin Lipton, Wachtell, Lipton, Rosen & Katz, Memorandum to Clients (Jan. 15, 1993).

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.[20] 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.[21]

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 in its state of incorporation.[22]

Poison pills raise several questions. First, 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 a pill 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),[23] 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.[24] 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.[25]

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[20] 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. 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 in 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.

[21] 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

[22] *See* John C. Coates, *Takeover Defenses in the Shadow of the Pill: A Critique of the Scientific Evidence*, 79 Tex. L. Rev. 271 (2000).

[23] 500 A.2d at 1354.

[24] *Unocal Corp. v. Mesa Petroleum Co.,* 493 A.2d 946 (Del. 1985).

[25] *Revlon, Inc. v. MacAndrews & Forbes Holdings, Inc.*, 506 A.2d 173, 182 (Del.1986). Arthur Fleischer, Jr. &

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.[26] The basis for these decisions was that the discriminatory treatment of raiders in flip-in pills violated a statutory requirement that all shares of the same class be treated equally.[27] 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*.[28] 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.[29] This number now stands at 34.[30]

The fiduciary duty limitations on pills proved to be a more torturous road. The Delaware

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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.")

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

[27] See, e.g., *Amalgamated Sugar,* 644 F. Supp. at 1234.

[28] 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).

[29] 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).

[30] Fleischer & Sussman, supra note 25, at 5.06 THE LEGALITY OF THE POISON PILL [B][2]

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.[31] States other than Delaware either follow Delaware law or give wider discretion to boards than Delaware does.[32]

Because flip-in pills are a show-stopper, and because boards had 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.[33] For companies without a staggered board, this technique involved only a modest delay and a modest increase in expenses.[34] 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.[35]

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[31] 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).

[32] Michal Barzuza, *The State of State Antitakeover Law*, 95 Va. L. Rev. 1973 (2009).

[33] 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.

[34] 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.")

[35] 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).

B. Anti-Takeover Statutes in Light of Poison Pills

If a pill is valid, it is easy to see how many of the most common anti-takeover statutes become irrelevant. A flip-in pill effectively prevents a raider from becoming a major shareholder. As a result, business combination statutes, fair price statutes, and control share acquisition statutes, which deal with what a raider can do *once it becomes* a major shareholder, are moot. Similarly, flip-over pills, which regulate business combinations involving a major shareholder, 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 like 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.[36]

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.[37] Pill validation statutes enacted during this period are thus clearly important, especially in the few cases where they superseded prior case law. Yet they are generally ignored by finance academics.

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

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[36] 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.

[37] See supra text accompanying notes 23 to 30.

discrimination among shareholders, have been found valid in numerous opinions,[38] 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.[39] It was only in April 1987, when the United States Supreme Court reversed these rulings in *CTS v. Dynamics*,[40] that these statutes were widely viewed as valid.[41] 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.[42] This leaves for any state at most a short period where there was significant doubt about the validity of pills but anti-takeover statutes were viewed as likely constitutional.

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[38] In addition to the decisions upholding flip-in plans, supra note 28, 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).

[39] 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), affd. 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).

[40] 481 U.S. 69 (1987).

[41] 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.")

[42] 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 (7th Cir. 1989); *WLR Foods, Inc. v. Tyson Foods, Inc.*, 65 F.3d 1172, 1180 (4th Cir. 1995).

Second, it is theoretically conceivable that anti-takeover statutes would matter if a court forces a board to redeem its pill. For example, when a company is for sale and the *Revlon* standard applies, Delaware fiduciary duty law generally does not permit a board to use a pill to favor one bidder over another.[43] Could a board instead use Delaware's business combination statute to favor one bidder over another? 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 refusing 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.[44] Tellingly, even though transactions governed by *Revlon* are common, to the best of our knowledge, no board in the entire history of hostile takeovers in the United States has ever tried to hide behind an anti-takeover statute once its fiduciary duties required it to redeem a pill.

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,[45] we found six bids (excluding cases governed by *Revlon*), where a board could not use a pill but enjoyed the protection of a standard anti-takeover statute.[46] In none of these six bids did the

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[43] *Mills Acquisition Co. v. Macmillan, Inc.*, 559 A.2d 1261 (Del. 1989) (subjecting discrimination among bidders to heightened scrutiny if company is for sale).

[44]See 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).

[45] Our research consisted of a review of all opinions listed in *State Takeover Statutes: A Fifty State Survey*, supra note 29, 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.

[46] 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.

anti-takeover statute stop the hostile raider.[47] And as mentioned before, in transactions governed by *Revlon*, we are likewise unaware of any bid that failed due to an anti-takeover statute.

Thus, as a practical matter, anti-takeover statutes add little to the defensive arsenal of boards. Perhaps they raised the likelihood that a target could successfully defend itself against a hostile bid by a small percentage for a short period of time. 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 are 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 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

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[47] 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 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).

by states. The most common methods are to look either exclusively at whether (and when) a state adopted a business combination statute,[48] at when a state adopted the first of a set of statutes (usually business combination, control share, and fair price),[49] 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).[50]

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.[51] 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.[52] 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 period – did not play into his analysis.[53]

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[48] See, e.g., Bertrand & Mullainathan, supra note 4; Giroud & Mueller, supra note 1.

[49] See, e.g., Garney & Hanka, supra note 2.

[50] See, e.g, Francis et al. supra note 7.

[51] http://www.thedeal.com/magazine/ID/038635/2011/the-strange-case-of-section-203.php

88163338&pubNum=162&originationContext=document&transitionType=DocumentItem&contextData=(sc.Se arch)"; *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).

[52] See Jeffrey N. Gordon, *Corporations, Markets, and Courts*, 91 Colum. L. Rev. 1931, 1959 n 95 (1991) (quoting from the letter).

[53] 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 47.

For states other than Delaware, studies that focus on business combination statutes have several problems. Most importantly, most studies do not start from a valid theory on how anti-takeover statutes affect the target's *marginal* ability to defend itself. 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.[54] 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 almost 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.[55]

One state that deserves particular mention is California. California is often singled out as the 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);[56] it prohibited staggered boards until 1989 for all firms[57] and continues to prohibit them for firms that are not "listed";[58] it prohibits a "for

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[54] 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.

[55] See Marcel Kahan, *The Demand for Corporate Law: Statutory Flexibility, Judicial Quality, or Takeover Protection?*, 22 J. L. Econ. & Org. 340 (2006).

[56] California General Corporation Law, Section 203.

[57] Id., Section 301.

[58] Id., Section 301.5, added by Stats. 1989, c. 876, § 2. Listed firms include only firms with outstanding shares

cause" standard for removal, even for companies with a staggered board;[59] 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,[60] 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.[61]

D. The Effect of Anti-Takeover Statutes on Takeovers

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.[62]

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

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listed on the New York Stock Exchange, the NYSE Amex, the NASDAQ Global Market, or the NASDAQ Capital Market. Id.

[59] California General Corporation Law, Section 303. Removal of directors of companies at staggered boards, however, is subject to a higher voting requirement.

[60] 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.

[61] 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.

[62] 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).

contrary results. One is a student note by Jo Hackl and Rosa Testani published in the 1988 Yale Law Journal.[63] But Hackl and Testani's article, which does not include any statistical analysis and contains no other control variables, examines the 1981 to 1986 period, which predates the advent of poison pills.[64] The second is an article by Schwert from 2000.[65] Schwert's 2000 article contains no data at all comparing states with anti-takeover statutes and states without them. 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."[66]

E. Anti-Takeover Statutes and Real Effects

From a lawyer's perspective, the treatment of anti-takeover statutes in the finance literature is highly deficient. 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

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[63] Jo Watson Hackl & Rosa Anna Testani, *Second Generation State Takeover Statutes and Shareholder Wealth: An Empirical Study*, 97 Yale L. J. 1193 (1988).

[64] Id. at 1212 (stating that authors examined offers made between June 1, 1981 and December 31, 1986).

[65] 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]).

[66] William Schwert, *Hostility in Takeovers: In the Eyes of the Beholder?*, 55 J. Fin. 2599, 2609 (2000).

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.[67] Similarly, fair price charter provisions are a substitute for fair price statutes. On the other 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.[68]

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,[69] from errors in the years in which anti-takeover statutes were adopted or became effective,[70] 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).[71]

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

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

[68] 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.

[69] See, e.g., Bertrand & Mullainathan, supra note 4 (explaining why authors fail to control for reincorporations).

[70] See, e.g, infra text accompanying notes 78 to 81 (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 99 (coding errors in Cheng et al. study).

[71] See infra note 104 (raising concerns about inclusion of entities other than public corporations in Qiu and Yu study).

be associated with, say, diffusion of lenders in loan syndicates.[72] But our prior would be that the relationship is not all that tight.

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.[73] 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 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.[74]

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

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[72] Bharath et al. supra note 10.

[73] Garvey & Hanka, supra note 2.

[74] Id. at 519-20.

ATS.[75] All 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.[76]

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.[77]

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.[78] In fact, both Delaware's and Pennsylvania's statutes took effect in the first quarter of 1988.[79]

Second, the authors have a peculiar way of dealing with states that had adopted anti-takeover

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[75] 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.

[76] Id. at 522-530.

[77] Other scholars have found that Garvey and Hanka's results are not robust to alternative sample constructions. John & Litov, supra note 2.

[78] Garvey & Hanka, supra note 2, at 522.

[79] 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.

laws prior to the *CTS* decision. They suggest, correctly, that these laws were of doubtful constitutionality and exclude firms incorporated in states that passed such laws before 1987.[80] 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*.[81] 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.[82]

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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[80] Id. at 522, 523.

[81] 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.

[82] 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").[83]

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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[83] 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.[84] 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.[85]

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

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[84] This regression is reported in column 3 of Table III of the paper. Garvey & Hanka, supra note 2, at 529.

[85] 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.

for Firm 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.[86] 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.[87] 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.[88] 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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[86] 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.

[87] 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.

[88] 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.[89] The estimate for the coefficient of Protected dropped to -.0036, and stopped being statistically significant.[90] (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	-.0035 (0.0022)	
State Fixed effects + Year Fixed Effects[§]	-0.0036 (0.0023)	
State Fixed Effects + Year Fixed Effects, coding corrected[§]	-0.0043 (0.0028)	

Note: Standard errors (in parentheses) are corrected for clustering at the firm level. [§] 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.[91] 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 corrected for coding errors and properly specified, there is no evidence for an association between ATS

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[89] 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.

[90] 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

[91] 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.

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,[92] examines the relationship between anti-takeover statutes and managerial stock ownership.[93] 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."[94] 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.

Let us start with considering the basis for the hypothesis tested by Cheng et al. The authors implicitly argue that greater share ownership by managers has a *lesser* impact on the ability to defend against a takeover when the company is incorporated in a state that has adopted an ATS than when a company is incorporated in a state that has not. This goes beyond the claim that these laws make takeovers harder. It is a claim about the importance of share ownership *given the presence or absence of such a law*.

But this claim seems spurious with regard to control share acquisition statutes, which are part of the set of statutes analyzed by Cheng et al.[95] Control share acquisition statutes require a hostile bidder

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[92] Cheng et al. supra note 3.

[93] 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.

[94] Id. at 641.

[95] Fair price statutes are more complicated. They apply to a freeze-out merger that follows the completion of a hostile tender offer and require that the raider either pay a "fair price" (usually no less than the price the raider paid in the tender offer that preceded the merger) or obtain the approval of a supermajority of shares (typically 80%) and of disinterested shares (typically 2/3) to complete the merger. Even if pills were not valid, fair price statutes would offer little protection against non-coercive bids, where a raider pays as much in the freeze-out merger as it did in the tender offer to acquire a majority stake, and to that extent should not affect the managerial stock ownership incentives. But obtaining the requisite shareholder approval, which is an alternative to paying a "fair price," may affect managerial stock ownership incentives. Alas, the direction of the effect is unclear.

to obtain an affirmative vote of the other shareholders in order to be able to vote its shares.[96] If anything, managerial stock ownership is more important for firms subject to control share acquisition statutes than for others because higher managerial ownership makes it less likely that the raider will obtain the requisite vote.[97]

Setting aside the unclear theoretical link between some of the ATS and the authors' hypothesis, we turn to Cheng et al.'s empirical tests. The main body of the paper contains two types of tests: so-called 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+Director/Officer Stockholdings)*.[98] 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.[99] In these regressions, the estimate of the

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Consider for example a managerial team with a 5% ownership stake. Without a fair price statute, such a stake would not be a significant impediment to a hostile bid. However, with a fair price statute, the shareholder requirement prong –to the extent it is relevant– may be a reason to maintain or increase the stake, rather than to reduce it as posited by Cheng et al.

[96] Absent a pill, these statutes may also impose a short delay on the completion of a hostile tender offer.

[97] The authors are perhaps aware of the lack of theoretical grounding of their hypothesis. They thus say that they "take no a priori stand on the effectiveness of the different form of laws" and suggest that "passage of subsequent laws in a given state [may be] eased by the enactment of the first law." Id. at 647. It is unclear what the authors mean to argue. Most firms in their sample became subject to the statutes at a single date and no subsequent laws were adopted at a later point. In any case, even if a manager thought that, say, the passage of a control share statute raised the probability that the state would eventually also adopt a business combination statute, there is no reason to believe that the manager would start to reduce her stockholdings after the control share statute was passed unless the control share statute, in itself, reduced the benefit of maintaining her equity position.

[98] 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.

[99] 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. 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.

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.[100] Consider, for example, two firms, Circle K, incorporated in Texas, a state that never adopted at 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.[101] 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.[102] We were able to match 710 firms with a state of incorporation.[103] The data indicate that, throughout the entire

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[100] That is, they do not control for state fixed effects.

[101] 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.

[102] 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.

[103] 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%

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.[104] 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.
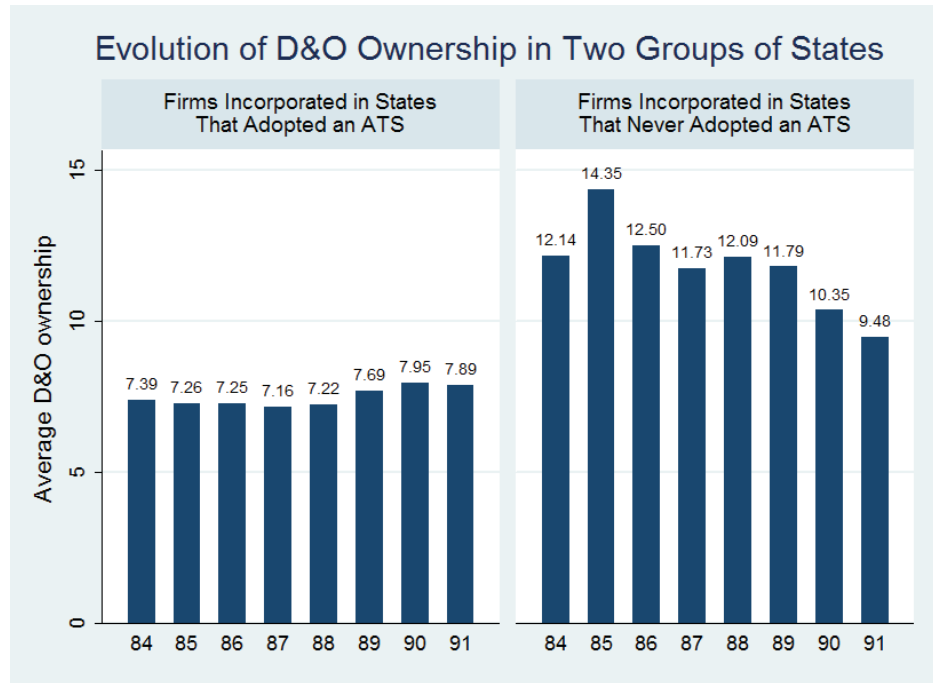
We then ran regressions using a similar set of control variables as did Cheng et al. Employing the same methodology, we find, as did Cheng et al., a significantly negative coefficient for the *AfterLaw* variable. However, when we added an additional control for the state of incorporation (state fixed effects), which addresses the methodological flaw we discuss, the coefficient for the *AfterLaw* turned (insignificantly) positive.[105] 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.

(2.800%) in the Cheng et al. sample. Id. Table 2. The samples differ materially only with respect to the 75th 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.

[104] 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.

[105] 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.

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.[106]

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*".[107] They report that the mean value of *transformed change in ownership* is -0.157 and

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[106] 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).

[107] 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

that this mean is statistically significantly different from zero at the 1% level.[108] 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,[109] 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.[110] Basically, average ownership did not change at all in firms that became subject to an ATS.[111]

"*average % change*"). They then construct the variable they use in their analysis as the sign of *average % change* times *ln(1+absolute value of average % change)*.

[108] 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).

[109] 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.

[110] In unreported results, we performed a similar analysis as that of the second row of table 3, but using *ln(1+ D&O 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.

[111] 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 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).

Variable	N	Mean	Mean (p-value)	10th Pctle	20th Pctle	30th Pctle	40th Pctle	50th Pctle	60th Pctle	70th Pctle	80th Pctle	90th 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,[112] 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.[113]

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[112] 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.

[113] 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 107, 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 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.

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.[114] 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

	(1)	(2)	(3)	(4)
AfterLaw	0.112	0.327	0.009	0.028
	(0.36)	(1.13)	(0.40)	(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.

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).

[114] The estimates of the coefficients for those controls are unreported to preserve space.

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.[115] 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.

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.[116] 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 duration and credit rating);[117] 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.[118]

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[115] Qiu & Yu, supra note 7.

[116] Qiu and Yu obtain bond yield information from the University of Houston's Fixed Income Database. Id. at 508.

[117] 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.

[118] 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,

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.[119] Specifically, while they find no evidence of a significant increase in yield spread for bonds that are rated investment grade, they find an increase of over 114 basis point for speculative-grade (a.k.a. junk) bonds.[120] 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).[121] Such an increase is enormous! One would have thought that a statute 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.[122] But Billett, King and Mauer explicitly exclude leveraged buyouts, which are associated with a decline in bond values,[123] from their sample. Their results thus overstate the average effect of all acquisition on

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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.

[119] 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.

[120] Id. at 507.

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

[122] 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%.

[123] See, e.g., Paul Asquith & Thierry A. Wizman, *Event Risk, Covenants, and Bondholder Returns in Leveraged*

bond values.

Most crucially, however, the Billet, 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 will not receive any acquisition offer to start with; many offers are not opposed by management and thus not affected by a business combination statute; and even with respect to hostile offers, the presence or absence of a statute is clearly not the sole determinative factor of 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 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."[124] 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.[125] Finally, the United States experienced a recession between July 1990 and March 1991.[126] 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

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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%).
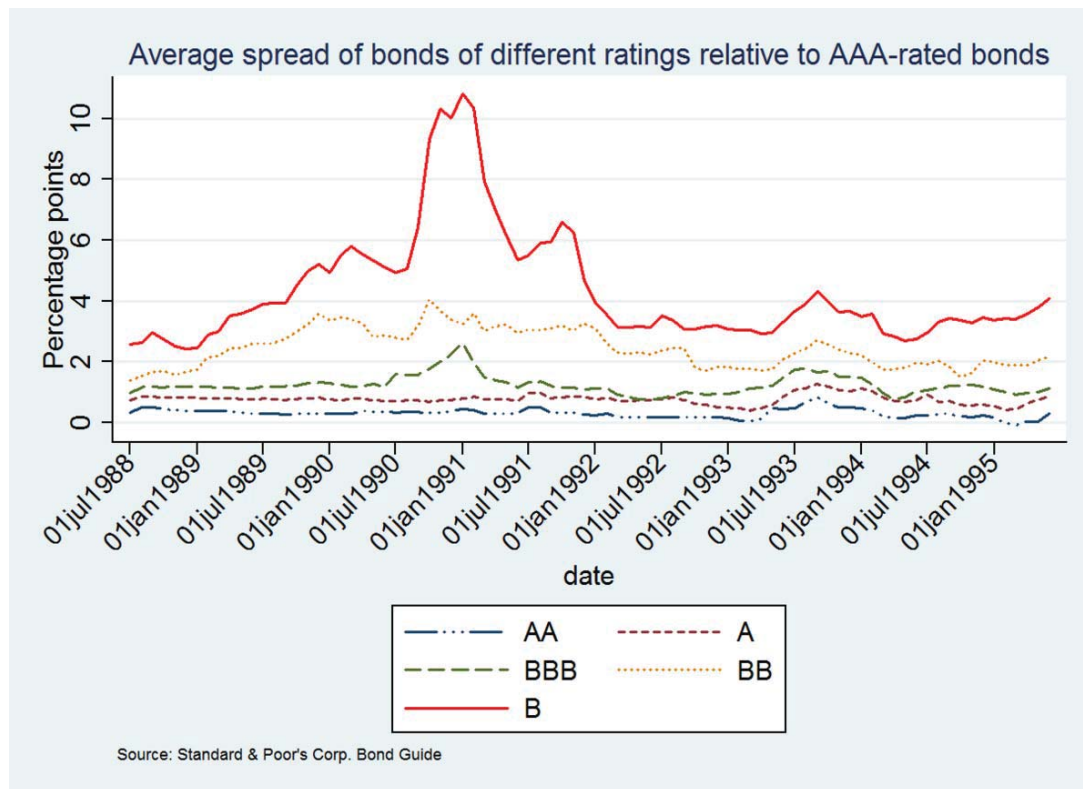
[124] Comment & Schwert, supra note 62, at 9.

[125] 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.

[126] http://www.nber.org/cycles.html

decale.[127]

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.[128] 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, moving in lockstep.[129]

Figure 2

Source: Standard & Poor's Corp. Bond Guide

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[127] Jean Helwege & Paul Kleiman, *Understanding Aggregate Default Rates of High Yield Bonds*, 2 Current Issues in Econ. & Fin. 6 (1996).

[128] The figure was constructed using data from the Standard & Poor's Corporation Bond Guides.

[129] 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.

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,[130] explanatory variables 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.[131] When the yield spread on junk bonds (but not on

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[130] See Qiu & Yu, supra note 7, at 515 (Table 6, column 3).

[131] 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.

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*.[132]

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 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.[133]

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[132] 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.

[133] 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 123 (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. As a result, the yield spread among junk bonds (e.g., between bonds rated B and those rated BB) widened. The specification estimated by Qiu and Yu only allows for an association between credit rating and spreads that is linear and stable over time. Hence, their estimate of the impact of the business combination statutes would be biased if the junk bonds issued by firms incorporated in the states adopting the statutes had systematically *lower* credit ratings that the junk bonds issued by firms incorporated in the states that never adopted a business combination statute.

Our analysis of the Fixed Income Database suggests that this was indeed the case. In our replication sample, the 10th, 25th, 50th, 75th and 90th percentile of the distribution of (unwinsorized) credit ratings across junk bond issuers incorporated in states that eventually adopted a business combination statute were always lower (i.e., represented a worse credit rating) than the corresponding percentile among junk bond issuers incorporated in states that never adopted a business combination statute. 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 and those issued by firms incorporated in other states that at some point adopted a business combinations statute. This suggests that, even in a subsample that consists exclusively of junk bonds, the comparisons made by Qiu and Yu's specifications are not *apples-to-apples*. 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 sample that consists exclusively of junk bonds. By definition, junk bonds are outliers in terms of their credit rating. This implies that one of the 1% extreme tails of the credit distribution in the paper's full sample consists of exclusively of junk bonds. 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. Our examination of the Fixed Income Database suggests that the winsorized observations consisted disproportionately of firms incorporated in Delaware. This further exacerbates the potential bias arising from the fact that, by only controlling for credit rating in a linear fashion, their regressions fail to compare one junk bond with another junk bond of equivalent credit rating.

In addition to the concerns about internal validity already raised, the junk-bonds only regression raises external validity concerns. First, the results for the effect of BC statutes are derived from a small amount of firms. To be sure, the overall regression results are based on 529 observations. But each firm in the paper's full sample contributes on average 5.6 yearly observations to the regression. So, if the junk bonds do not differ systematically from the investment grade bonds in the sample, the database used to estimate the junk-bond only regressions includes fewer than 100 firms. The small number of firms is especially troublesome because many of the firms with the lowest-rated bonds are likely to have received a low rating because the issuer became insolvent (and one would expect –as our look at our replication sample confirms- insolvencies to have been particularly prevalent during or after the 1990-91 recession). Moreover, unless a firm had junk bonds outstanding both before and after the firm's state of incorporation adopted its BC statute, those firm's junk bonds do not contribute to the estimate for the effect of a BC statute on yield spread, but only to the estimate of various control variables. In fact, Qiu and Yu report elsewhere, only 35.7% of the observations are derived from firms that had bonds outstanding both before and after the adoption of a BC law. Id. at 521 (Table 11, columns 1 and 5). Moreover, Qiu and Yu attempt to control for shocks that affect all firms in a given industry and year, and shocks affecting all firms in a given location and year by following a standard practice in this strand of literature. For each firm-year observation, they proxy for those shocks by taking the average yield spread over all other firms operating in the same industry and year (or the same location and year, respectively) as the firm at issue, and then use those two averages as control variables. Table 4. This construction requires at least two observations in each industry-year and at least two observations in the same state of location-year. This means that, although the regression in column 2 of Table 6 reports using 529 observations, the number of observations *effectively used* to identify the coefficients of interest is likely to be much lower (especially since the sample spans 20 years, and Qiu and Yu report using the three-digit SIC code to define a firm's industry, which implies that they may have more than one hundred different industries). Unfortunately, we do not know how much lower. Even if the number of observations is enough for them to get statistically significant results, those results are likely to be based on such a small number of firms that the conclusions reached within the sample are hard to extrapolate out of sample.

Finally, the fact that the procedure Qiu and Yu follow in their attempt to control for common shocks is standard does not make it right. Gormley and Matsa, supra note 85, show that attempting to control for industry-year of location-year common shocks in this way can lead to estimation biases that are worse than the biases that would arise if one simply failed to control for these common shocks altogether. These concerns affect each of the

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. 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. 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.

There are, of course, many other finance studies that have found a statistical association between anti-takeover statutes and real economic effects that we have not examined. We do not know whether these studies suffer from similar flaws. Still, in light of the legal arguments we have presented, and in light of the fact that we were able to debunk the results of each of the finance studies that we examined closely, we believe that we have presented a strong case that the anti-takeover statutes examined in these studies are, in fact, immaterial and that they do not make a good metric of the anti-takeover threat facing a firm.

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 yield important insights. One contribution of this article is thus to show that, despite the empirical results generated by finance scholars, the legal view of anti-takeover statutes may be right after all.

specifications estimated by Qiu and Yu in their paper.

But our article has several major implications that go beyond adjudicating a debate between different branches of the academia. Most importantly, it calls into doubt much of the empirical knowledge about the real economic effects of a takeover threat. Starting in the 1980s, theorists have taken different positions on what these effects are. One set of scholars has argued that the threat of a takeover acts as a beneficial disciplining device that induces managers to act in the interest of shareholders.[134] Another set has argued that the threat of a takeover induces an excessive short-term focus on management and thereby lowers long-term shareholder value.[135] 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.[136] A similar debate, with many of the

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[134] Easterbrook & Fischel, supra note 14 (arguing that hostile tender offers are an important device to reduce agency costs); Gilson, supra note 14, 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 14, 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).

[135] 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 15 (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 15; Lipton, supra note 15, 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).

[136] See, e.g., Andrei Shleifer & Lawrence H. Summers, Breach o/Trust in Hostile Takeovers, in CORPORATE

same partisans, is currently being waged about the effect of activism by hedge funds and similar shareholders.[137]

To empirically test these hypotheses, 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 react. 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.[138] 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 have no bearing on how a takeover threat affects behavior.

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. For example, firms with controlling shareholders may act differently from other firms for reasons unrelated to takeovers. More generally, any anti-takeover device that is adopted at the firm level – like a staggered board – 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, they are "exogenous" and do not suffer from this problem. Thus, if these statutes do not have an impact on the takeover threat, the single best source of unconfounded evidence for how the takeover threat affects real behavior becomes useless. As we see

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).

[137] See, e.g., Kahan & Rock, supra note 16, at 1083-91 (reviewing debate); Bebchuk, supra note 16.

[138] See Atanassov, supra note 12.

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.

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 theory of how these statutes function, reflects broader problems. There seem to be a number of law-related variables that lack theoretical grounding but are frequently used by empiricists. Top of the list is the widely-used GIM governance/takeover index. As our colleague Michael Klausner has recently argued, the index contains elements that are irrelevant for all companies, elements that are irrelevant for a subset of companies, and elements that have no impact on takeovers and reflect good governance practices. And even to the extent that the index captures useful variables, he explains, some empiricists have not understood the underlying governance mechanisms and have misinterpreted their results.[139] Earlier, John Coates had argued that economists widely misinterpret the import of a company adopting a poison pill.[140]

Another 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."[141] While this notion, which forms the basis for several empirical articles,[142] 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.

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[139] Michael Klausner, *Fact and Fiction in Corporate Law and Governance*, 35 Stan. L. Rev. 1325, 1368 (2013).

[140] See Coates, supra note 22.

[141] Rauh, supra note 11, at 399.

[142] See, e.g., Angie Low, *Managerial Risk-Taking Behavior and Equity-Based Compensation*, 92 J. Fin. Econ. 470 (2009); Yun, supra note 9.

To our mind, what all of these instances have in common is that they generate 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. Empiricists can use these variables, often in different permutations, in their tests to check how they relate a large set of potential effects – 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 effect, 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.[143]

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[143] Trouble at the Lab, The Economist, Oct. 19, 2013.

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