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The Law and Finance of Anti-Takeover Statutes

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October 2014

Emiliano M. Catan
New York University School of Law and ECGI
Marcel Kahan
New York University School of Law and ECGI

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Abstract

Over the last 15 years, numerous finance articles have examined the effect of anti-takeover statutes on firm and managerial behavior. In this article, we evaluate these studies from a theoretical-legal and an empirical-finance perspective. To assess the impact on an anti-takeover statute from a theoretical perspective, one has to evaluate how the statute affects the ability of a firm to defend itself in light of the other defenses already available to a firm. But given gaps in the protection they afford and the availability of other, more powerful, takeover defenses - specifically, poison pills - standard antitakeover statutes do not materially increase a company's ability to resist a hostile takeover bid. From the empirical side, the finance studies omit important control variables, use improper specifications, contain errors in when states adopted statutes and which companies such statutes cover, and suffer from selection bias and endogeneity. These problems render the empirical results derived by these studies unreliable. Indeed, we are able to replicate several of the empirical studies we criticize and to show that the results in none of these studies withstand closer scrutiny. Our article has important implications, in particular for the debate over whether an increased threat of a takeover acts as a disciplining device or induces short-termism. The finance studies we criticize have supplied most of the empirical evidence in this debate. But if, as we argue, these studies suffer from serious flaws, the bulk of our understanding of the effects of a takeover threat has to be reassessed. 2

Emiliano M. Catan

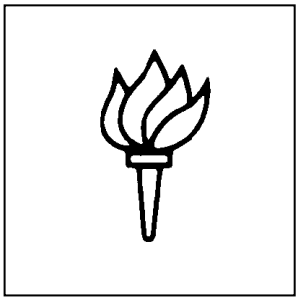
New York University School of Law

Marcel Kahan

New York University School of Law

NEW YORK UNIVERSITY SCHOOL OF LAW

NYU Center for Law, Economics and Organization



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The Law and Finance of Anti-Takeover Statutes

by

Emiliano M. Catan* and Marcel Kahan**

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

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

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Introduction

Over the last 15 years, finance scholars have developed an increasing fascination with anti-takeover statutes. Numerous articles, many published in top finance journals, have examined the effect of these statutes on performance,[1] leverage,[2] managerial stock ownership,[3] worker wages,[4] innovation,[5] dividend payout ratios,[6] bond yields[7] and the cost of bank loans,[8] executive pay,[9] cash reserves,[10] earnings quality[11] and loss recognition,[12] loan syndicate diffusion,[13] and the amount of employee stock in pension

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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. Pol. Econ. 1043 (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] Maya Waisman, *Product Market Competition and the Cost of Bank Loans: Evidence from State Antitakeover Laws*, 37 J. Banking & Econ. 4721 (2013).

[9] Marianne Bertrand & Sendhil Mullainathan, *Corporate Governance and Executive Pay: Evidence from Takeover Legislation* (Mass. Inst. Tech., Working Paper, 1999); Shijun Cheng & Raffi J. Indjejikian, *The Market for Corporate Control and CEO Compensation: Complements or Substitutes*, 26 Comtemporary Acc. Research 701 (2009; Shijun Cheng & Raffi J. Indjejikian, *Managerial Influence and CEO Performance Incentives*, 29 Int'l. Rev. L. & Econ. 115 (2009)

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

[11] Sudarshan Jayaraman & Lakshmanan Shivakumar, *Agency-Based Demand for Convervatism: Evidence from State Adoption of Antitakoever Laws*, 18 Rev. Acc. Stud. 95 (2013).

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plans.[14] The popularity of these studies is not waning. Just within the last year, two new working papers on anti-takeover statutes were released.[15]

From a legal perspective, this is very odd. Finance scholars focus predominantly on three kinds of anti-takeover statutes: business combination statutes, fair price statutes, and control share acquisition statutes. Corporate lawyers and academics generally dismiss these anti-takeover statutes as irrelevant. So why do finance studies of these statutes yield results?

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

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

There are, of course, numerous finance studies of anti-takeover statutes that we do not review. It

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[12] Yijang Zhao & Kung H. Chen, *Earnings Quality Effect of State Antitakeover Statutes*, J. Acc. & Public Policy 92 (2009).

[13] 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.

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

[15] 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; Jonathan M. Karpoff & Michael D. Wittry, Test Identification with Legal Changes: The Case of State Antitakeover Laws, available at http://papers.ssrn.com/sol3/papers.cfm?abstract_id=2493913 (replicating study of anti-takeover provisions and patents).

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would be a Herculean, if not Sisyphean, task to examine all of these studies at the level of detail that we devoted to the three studies. In Parts III to V, we instead discuss three problems that affect most of the existing studies: miscodings, failure to account for managerial share ownership, and selection bias. We will show that each of these problems affects a large percentage of the observations typically used in, and biases the results obtained by, these studies.

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

Second, and most importantly, our analysis calls into doubt most of the empirical findings regarding the effect of an increased threat of takeovers. Since the 1980s, scholars have debated whether an enhanced threat of a takeover acts as a disciplining device for managers[16] or induces short-termism.[17] The debate continues unabated. Earlier this year, a commission co-chaired by Larry Summers -- a renowned economist and former U.S. Treasury Secretary and Harvard president -- recommended measures to make hostile takeovers more difficult in order to combat short-termism.[18] The studies of how firms have responded to the adoption of anti-takeover statutes have been the principal, and (if these statutes functioned as posited) econometrically most reliable, evidence of how firms responded to an increased threat of takeovers. But if these studies are based on false premises and their estimates are biased, as we argue, it turns out that we know little if anything about the form that these responses take.

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

[17] 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).

[18] Report of the Commission on Inclusive Prosperity at 86 (2015), available at http://www.wlrk.com/docs/IPC-PDF-full.pdf.

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

I. State Anti-Takeover Laws and Takeover Protection

To assess the impact on an anti-takeover statute, one has to evaluate how the statute affects the ability of a firm to defend itself in light of the other defenses already available to a firm. As we explain in this Part, even in the absence of standard anti-takeover statues, a company could use a poison pill to defend itself against a hostile bid. Because poison pills are equally or more effective than standard anti-takeover statutes, and because most firms already had the legal ability to employ a pill at the time standard anti-takeover statutes were enacted, the enactment of these statutes added little, if anything, to the defensive arsenal of most firms. Moreover, in the few instances of hostile takeover bids for firms that were protected by a statute, but that could not employ a poison pill, the statute did not prevent the hostile takeover.

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

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for $100 if a merger occurred.[19] Flip-over pills were quickly supplemented with flip-in provisions, which grant similar rights if the raider acquires a certain percentage of company stock, even if no subsequent merger takes place. Flip-over and flip-in pills can be redeemed by the board of directors for a trivial amount, but only before the raider becomes a significant stockholder.

Poison pills are a highly effective tool to ward off a hostile raider. As summarized by Marty Lipton, "[The poison pill] is an absolute bar to a raider acquiring control … without the approval of the company's board of directors."[20] 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 rights to all other shareholders. Second, even if a raider would be willing to swallow the pill, other shareholders will not want to tender their shares to the raider because they would rather hold out and exercise the rights after the pill is triggered. Since the terms of the pill, including the value of the rights, are set by the incumbent board and since pills do not require shareholder approval, the board can always fashion a pill that is sufficiently poisonous to do the trick. In fact, no single company has ever been acquired with a flip-in pill in place.[21] 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.[22]

Because pills can be put in place on short notice, it does not matter whether a company has a pill 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.[23]

Poison pills raise several questions. First, legally, are they valid in principle? Second, what are

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

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

[21] 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 (noting that pill at issue was designed to protect Selectica's net operating losses, rather than protect it against a hostile bid, and was triggered by Versata Enterprises to obtain leverage in an unrelated business dispute).

[22] 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

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

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the fiduciary duty limitations on a board's refusal to redeem a pill? Third, how can pills be overcome?

The validity in principle of pills was an initial concern not just due to the novelty of the device, but also to the fact that flip-in pills discriminate among shareholders: regular shareholders receive valuable rights; the raider does not. But several 1985-86 decisions by the Delaware Supreme Court established the validity of poison pills. In *Moran* (1985),[24] 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.[25] 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.[26]

Although the validity of pills in Delaware – the domicile for about half of all publicly traded companies – became clear in 1985, the issue of pill validity in other states is more complex. Although no court 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.[27] 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.[28] Court decisions under the laws of Indiana, Maine, Maryland, Michigan, Minnesota, Texas and Wisconsin upheld flip-in pills reasoning

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[24] 500 A.2d at 1354.

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

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

[27] See, e.g., *Amalgamated Sugar Co. v. NL Industries Inc.*, 644 F.Supp. 1229 (S.D.N.Y.1986) (New Jersey law); *Asarco Inc. v. Court*, 611 F.Supp. 468 (D.N.J.1985) (same); *West Point–Pepperell, Inc. v. Farley Inc.*, 711 F.Supp. 1088 (N.D.Ga.1988) (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) (New York law); *R.D. Smith & Co. v. Preway Inc*., 644 F.Supp. 868 (W.D.Wis.1986) (Wisconsin law); *Spinner Corp. v. Princeville Development Corp.*, Civ. No. 86–0701 (D.Haw., 1986) (Colorado law); *Topper Acq. Corp. v. Emhart,* C.A. No. 89-00110-R (E.D. Va. 1989) (Virginia law).

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

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that any discrimination entailed merely is among *shareholders*, not among *shares*.[29] But while the reception of flip-in pills by courts was mixed, legislatures embraced them enthusiastically. By 1990, 24 states (including all states where courts had invalidated flip-in pills) had adopted statutes validating discriminatory pills.[30] This number now stands at 34.[31]

The fiduciary duty limitations on pills proved to be a more torturous road. The Delaware Supreme Court made clear from the outset that pills had to be employed consistent with the standards laid out in *Unocal* and *Revlon*, but what these standards required became clear only over time. An important question was whether a pill could be used merely to gain time to develop an alternative transaction or 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,[32] but *Time-Warner*,[33] a 1989 decision by the Delaware Supreme court criticized these holdings and came out on the latter side. States other than Delaware either follow Delaware law or give wider discretion to boards than Delaware does.[34]

Because a flip-in pill that remains in place is a show-stopper, and because boards have wide discretion to use pills under *Unocal*, most M&A practitioners focused their attention on ways to overcome a pill. Here, the most popular technique became to conduct a proxy contest to oust the incumbent board while a hostile bid was pending, but before the bidder has acquired the requisite

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[29] 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.) (Wisconsin law), aff'd on other grounds, 877 F.2d 496 (7th Cir. 1989).

[30] 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).

[31] Fleischer & Sussman, supra note 26, at 5.06 THE LEGALITY OF THE POISON PILL [B][2]

[32] *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).

[33] *Paramount Communications, Inc. v. Time, Inc.*, 571 A.2d 1140 (Del. 1989).

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

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number of shares that made a pill non-redeemable by the board.[35] For companies without a staggered board, this technique involved only a modest delay and a modest increase in expenses.[36] 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.[37]

B. Anti-Takeover Statutes in Light of Poison Pills

If a pill is valid, it is easy to see how the most commonly analyzed anti-takeover statutes become irrelevant.[38] A flip-in pill effectively prevents a raider from becoming a major shareholder. Business combination, fair price, and control share acquisition statutes apply once a raider has become a major shareholder: business combination statutes prohibit the raider from engaging in a freeze-out merger or similar transaction with the target; fair price statutes set a minimum price at which other shareholders can be frozen out; and control share acquisition statutes deny voting rights to the shares held by the raider unless other shareholders vote to grant such rights.[39] But if, as a result of the flip-in pill, a raider never acquires a significant stake, any statute that deals with what a raider can do *once it becomes* a major shareholder becomes moot. Similarly, flip-over pills, which make business combinations once a

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[35] 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.

[36] 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.")

[37] 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).

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

[39] See William J. Carney, Mergers and Acquisitions 463-464 (Foundation Press 2011)

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raider has acquired a large stake prohibitively expensive, render business combination and fair price statutes superfluous. Control share acquisition statutes, moreover, do not even purport to offer meaningful protection against hostile bids that are opposed by the board of the target, but are favored (as most "hostile" bids are) by a majority of the target's shareholders.

Moreover, the principal mechanism to overcome a pill – obtaining board control before acquiring a significant stake – would also work to neutralize these anti-takeover statutes. Business combination statutes, fair price statutes, and control share acquisition statutes apply only to raiders or transactions not sanctioned by the incumbent board. Thus, for example, just as a board can redeem a pill before a bidder acquires a significant stake, a board can also approve an "interested shareholder" and thus eliminate the constraints imposed by a business combination statute.[40]

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.[41] Pill validation statutes enacted during this period are thus important, especially in the few cases where they superseded prior case law. Yet they are ignored by most finance academics.[42]

Flip-over pills, however, were not subject to equivalent uncertainty. They do not involve discrimination among shareholders, have been found valid in numerous opinions, [43] and have not been

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[40] See, e.g., Delaware General Corporation Law, §203(a)(1). Likewise, most control share acquisition statutes permit the board to adopt a by-law that renders the statute inapplicable. See, e.g., Mass. Gen. Laws., Ch. 110D, §2(c). Other statutes exempt control share acquisitions that are effected through a merger with the target corporation. See, e.g., N. Car. Gen. Stat., §55-9A-01(b)(3)(e) (exempting acquisitions pursuant agreements to which the covered corporation is a party).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 someone acquires 10% to 20% of the firm's outstanding shares. In a few business combination statutes, the threshold for becoming subject to the moratorium imposed by the statute is 5% 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.

[41] See supra text accompanying notes 24 to 31.

[42] Exceptions include Gormley & Matsa, supra note 15, Karpoff & Wittry, supra note 15, and Francis et al., supra note 8.

[43] In addition to the decisions upholding flip-in plans, supra note 29, 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,

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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.[44] It was only in April 1987, when the United States Supreme Court reversed these rulings in *CTS v. Dynamics*,[45] that these statutes were widely viewed as valid.[46] 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.[47] This would leave just a short period when anti-takeover statutes were viewed as likely constitutional, but there was significant doubt about the validity of pills.

Second, it is theoretically possible that anti-takeover statutes might nevertheless matter if a court forced a board to redeem its pill. For example, when a company's failure to redeem a pill violates the

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1986) (Pennsylvania law); and *APL Corp. v. Johnson Controls, Inc.*, No. 85 Civ. 990 (E.D.N.Y. Mar 25, 1986) (Wisconsin law).

[44] 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); see also Guhan Subramanian, *Delaware's Choice*, 39 Del. J. Corp. L. 1 (2014) (reviewing case law).

[45] 481 U.S. 69 (1987).

[46] 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 regulate 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.")

[47] 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, e.g., *Amanda Acquisition Corp. v. Universal Foods Corp*., 877 F.2d 496 (7th Cir. 1989).

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Unocal standard, could a board instead use Delaware's business combination statute as a defense?[48] While this question has not been conclusively resolved – it was not raised in the few cases where a court forced a board to redeem a pill – the answer in all likelihood is "no." The standard a court would apply in deciding whether a board breached its duties in failing to redeem a pill should also apply in deciding whether a board breached its duties in failing to approve a transaction under the applicable anti-takeover statute.[49]

Moreover, standard anti-takeover statutes, without the pill, are not all that powerful. Business combination statutes, for example, neither block a hostile takeover nor prevent a raider, after acquiring control, from having the target sell assets, having the target incur debt, having the target make cash or in-kind distributions to its shareholders, or selling the target to a third party. Rather, they mostly restrict the raider's ability to obtain full ownership through a freeze-out merger of minority shareholders and similar self-dealing transactions.[50] And even these restrictions often do not apply if the raider acquires sufficient shares in the tender offer.[51]

Indeed, in our research,[52] we found seven hostile bids where a board could not use a pill but

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

[49] 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.") Consistent with this assessment, 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 Subramanian, supra note 44, at 36 (arguing that fiduciary duty law would not require a board to provide approval under Section 203).

[50] See, e.g., Delaware General Corporation Law, Section 203(c)(3) (definition of business combination).

[51] See, e.g., id., Section 203(a)(2) (exception for instances where interested shareholder owns 85% of stock).

[52] Our research consisted of a review of all opinions listed in *State Takeover Statutes: A Fifty State Survey*, supra note 30, 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.

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enjoyed the protection of a standard anti-takeover statute.[53] In none of these bids did the anti-takeover statute stop the hostile raider.[54] Nor is there any substantial evidence that standard anti-takeover statutes deterred hostile bids in the post-1985 area, after the Delaware Supreme Court decided *Moran* and *Unocal*,[55] or that the adoption of standard anti-takeover statutes after the validity of poison pills was

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[53] 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. See generally Barzuza, supra note 34. 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.

[54] 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).

[55] 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. 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). Finance studies of anti-takeover statutes sometimes cites to two studies for contrary results. See, e.g., Cheng et al., supra note 3, at 641; Francis et al. supra note 8, at 130, Qiu & Yu, supra note 8, at fn. 14. One is a 1988 note by Hackl and Testani. Jo Watson Hackl & Rosa Anna Testani, *Second Generation State Takeover Statutes and Shareholder Wealth: An Empirical Study*, 97 Yale L. J. 1193 (1988). But Hackl and Testani's article examines the 1981 to 1986 period, which predates the advent of poison pills. Id. at 1212 (stating that authors examined offers made between June 1, 1981 and December 31, 1986). The second is an article by Schwert from 2000. Schwert's 2000 article contains no relevant data and 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." William Schwert, *Hostility in Takeovers: In the Eyes of*

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established affected the stock price of firms that became subject to such statutes.[56]

Thus, as a practical matter, standard anti-takeover statutes add little to the defensive arsenal of boards. Perhaps they might have raised by a small percentage and for a short period of time the likelihood that a target could successfully defend itself against a hostile bid. Or perhaps they served as a contingency device, in case the S.E.C. were to adopt a rule constraining the use of poison pills.[57] 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

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the Beholder?, 55 J. Fin. 2599, 2609 (2000). A recent working paper by Cain, McKeon and Davidoff also examines the impact of multiple types of anti-takeover statutes as well major court decisions on takeover activity. Matthew Cain, Stephen McKeon and Steven Davidoff Solomon, *Do Takeover Laws Matter? Evidence from Five Decades of Hostile Takeovers* (2014), available at http://papers.ssrn.com/sol3/papers.cfm?abstract_id=2517513. Cain et al. report that fair price and control share acquisition had no significant effect on hostile acquisitions; that the effect of business combination was negative, but non-robust; and that poison pills facilitated hostile takeovers. (Id., Tbl. 8). Since Cain et al.'s regressions do not control for secular changes in M&A activity over time (e.g. by including year dummies), these results likely reflect the spike in overall (friendly and hostile) takeover activity in the mid-1980s, when pills became nationally valid (as coded by Cain et al.) and the decline in overall (friendly and hostile) takeover activity around 1990 (shortly after Delaware adopted its business combination statute), which is generally attributed to the recession and increased financing costs. See Schwert at 2607; Martijn Cremers & Allen Ferrell, Thirty Years of Shareholder Rights and Stock Returns 8 (2012), available at http://papers.ssrn.com/sol3/papers.cfm?abstract_id=2020471.

[56] Two event studies examining the adoption of the Delaware business combination statute, the only single standard statute study involving a state where the validity of pills had been established, find no statistically significant effect on stock prices of Delaware firms. Jonathan M. Karpoff & Paul H. Malatesta, *The Wealth Effects of Second-Generation State Takeover Legislation*, 25 J. Fin. Econ. 291 (1989) (finding no significant effects for adoption of Delaware statute); John S. Jahera, Jr. & William Pugh, *State Takeover Legislation: The Case of Delaware*, 7 J. L. Econ. & Org. 410 (1991) (finding no consistent evidence that takeover statute affected shareholder wealth). Several studies of the 1990 Pennsylvania anti-takeover statute report statistically significant declines in stock prices. See Roberta Romano, THE GENIUS OF AMERICAN CORPORATE LAW 60 – 75 (AEI Press 1993) (presenting summary of event studies). While the 1990 Pennsylvania statute post-dates Pennsylvania's adoption of a pill validation statute, the 1990 statute was non-standard: it contained unusual provisions on the disgorgement of profits obtained by a raider and on the fiduciary duty standard applicable in the takeover context. These provisions, unlike those provisions in standard control share, business combination, or fair price statutes that are the subject of the studies we critique, strengthen marginal defenses even in the presence of a poison pill. It should also be noted that event studies relating to multiple statutes include not only statutes for states where the validity of pills had not yet been established, but also statutes, including fair price statutes and the New York business combination statute, where the legislative event dates precede the Delaware Supreme Court decision in *Moran*.

[57] Though the S.E.C. never even proposed such a rule, it issued a concept release dealing with pills in 1986. See Concept Release on Takeovers and Contests for Corporate Control, Exchange Act Release No. 34-23486, 36 SEC Dckt 230 (July 31, 1986)

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combination statutes were perfect substitutes and raise the likelihood of a successful defense by, say, 15%, and if there is a 10% chance that a court may force a board to redeem a pill (while still allowing the target's board to shield the company behind the statute), why not propose to have the statute adopted? But 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. What is Wrong with Economists' Treatment of Anti-Takeover Statutes

Financial economists employ varying methods of categorizing anti-takeover protection offered by states. The most common methods are to look either at when a state adopted a business combination statute,[58] at when a state adopted the first of a set of statutes (usually business combination, control share, and fair price),[59] 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).[60]

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. 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 poison

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

[59] See, e.g., Garvey & Hanka, supra note 2.

[60] See, e.g., Francis et al. supra note 8. A notable exception is a recent working paper by Karpoff and Wittry that considers business combination statutes, control share acquisition statutes, pill validation statutes, director duty statutes, and fair price statutes separately and controls for certain legal decisions. See Karpoff & Wittry, supra note 15.

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pills.[61] 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.[62] 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 at all.

For states other than Delaware, studies that focus on business combination statutes have several problems. Most importantly, studies do not start from a valid theory on how anti-takeover statutes affect the target's *marginal* ability to defend itself.[63] 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.[64] For fair price and control share statutes, the studies ignore whether companies had adopted fair price charter provisions which offer protection similar to these statutes. Finally, many studies ignore the high degree of uncertainty over the validity of anti-takeover statutes prior to 1987 and all fail to account for the decline in uncertainty over the validity of both flip-over and flip-in pill in states without pill validation statutes.

The studies that add up the total number of statutes adopted are even more problematic. Four of the five types of statutes cover overlapping territory. As explained, pill validation statutes make business

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[61] *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). 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 54.

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

[63] As Karpoff and Wittry have pointed out, the claimed rationale for focusing on business combination statutes – that these statutes have been shown in event studies to have the largest impact on stock prices – is not supported by the empirical evidence, which shows that poison pill laws are associated with a larger impact on stock prices. See Karpoff & Wittry, supra note 15, at 8.

[64] These situations are by no means unusual. Thirty one states adopted a business combination statute at some point before 1995. Four states adopted a pill validation statute before adopting a business combination statute. Eight states adopted their business combination and their pill validation statutes at the same time. Six states adopted a pill validation statute and never adopted a business combination statute. Five states had case law upholding pills that preceded the state's business combination statute.

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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 front-end loaded, coercive bids.[65]

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*[66] (a provision which casts unique doubt on the validity of flip-in poison pills);[67] it prohibited staggered boards until 1989 for all firms[68] and continues to prohibit them for firms that are not "listed";[69] it prohibits a "for cause" standard for director removal, even for companies with a staggered board;[70] 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).[71] 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 be approved and maintained by the board) less important. Even if California had adopted the standard anti-takeover statutes, 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.

We believe that these problems make it very difficult, if not entirely impossible, to separate statistically the effect of takeover law from contemporaneous economic changes.[72] The econometric

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

[66] California General Corporation Law, Section 203.

[67] See Guhan Subramanian, *Bargaining in the Shadow of Takeover Defenses*, 113 Yale L. J. 621, 628-629 (2003).

[68] California General Corporation Law, Section 301.

[69] Id., Section 301.5, added by Stats. 1989, c. 876, § 2. Listed firms include only firms with outstanding shares listed on the New York Stock Exchange, the NYSE Amex, the NASDAQ Global Market, or the NASDAQ Capital Market. Id.

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

[71] California General Corporation Law, Section 600(d).

[72] These problems cannot be adequately addressed by merely adding a "poison pill law" dummy that controls for pill validation statutes and, for Delaware firms, for the *Moran* decision, to the regressions. Cf. Karpoff & Wittry,

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basis of the finance studies, and the factor that permits these studies to differentiate between anti-takeover statutes and economic changes, is that states adopted these statutes at different times. But if poison pills make anti-takeover statutes moot, and if other states are believed to follow the lead of Delaware law – as they did for flip-over pills -- then the validation of poison pill in *Moran* and subsequent Delaware cases on the use of pills affected all firms at the same time (albeit with potentially different intensities). But then, if firms, say, reduce their leverage in 1986, one cannot tell whether they reduced their leverage because the *Moran* decision in 1985 boosted their ability to resist a takeover or because of some other economic change that occurred in 1985. Anti-takeover statutes would thus only be relevant to the extent that they go beyond pills or are enacted in a state where a pill is not valid. And while some statutes fit this bill, they tend to affect only a small number of firms, they tend to relate only a few years of observation per firm, and they tend to entail only small changes in the ability to resist a bid.[73]

D. Anti-Takeover Statutes and Real Effects

The way financial economists approach takeover defenses results in a highly distorted view of the takeover protections supplied by state law. Distortions arise for three reasons. First, financial

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supra note 15, at 17, 40 (using this approach). While this is a step in the right direction, it fails to account for the decline in uncertainty over the validity of both flip-over and flip-in pill in states without pill validation statutes (including in states other than Delaware with case law validating pills). Even assuming that pills are invalid outside Delaware absent a statute, it fails to account for the interaction of takeover defenses. Thus, for example, by adding separate controls for poison pill laws and business combination statutes but no control for the interaction of these statutes, the regression format assumes that business combination statutes have the same marginal effect whether or not pills are valid. As we discussed, this reflects a misunderstanding of how these statutes operate.

[73] Examples of such statutes are statutes enacted prior to *Moran*, pill validation statutes that overturn case law invalidating flip-in pills, statutes that provide for a more lenient standard of review of anti-takeover defenses than the standard used in Delaware, or the Massachusetts statute that legislatively imposed staggered boards on all Massachusetts companies. While there are event studies analyzing the effect of some of these statutes on stock prices (see, e.g., Jonathan M. Karpoff & Paul H. Malatesta, *The Wealth Effects of Second-Generation State Takeover Legislation*, 25 J. Fin. Econ. 291 (1989) (pill validation statutes); Robert Daines, Classified Boards and Corporate Control: Takeover Defenses After the Pill, Working Paper 2011 (Massachusetts law)), we are aware of only one study that isolates the effects of such statutes on managerial or firm behavior and that study finds no robust effects for the statutes at issue. See Karpoff & Wittry, supra note 15, at 40-41 (reporting mixed results on the effect of first-generation anti-takeover laws on the number and citation of patents).

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economists make mistakes regarding the coverage of statutes. They generally miss some relevant statutes entirely; ascribe the wrong year of adoption for some statutes they include; and assume that all firms incorporated in a state become subject to the statute even though some statutes impose apply only to a subset of those firms (such as firms that are also headquartered in the state or have a minimum number of shareholders who are residents of the state). Second, most finance studies assume that firms rarely change their state of incorporation and thus ascribe the wrong domicile to some firms.[74] Third, as discussed before, the studies ignore the interaction and overlap among takeover defenses and, in particular, the significant impact of poison pills on a firm's ability to resist a takeover.

To quantify the impact of these problems, we constructed a sample of 2391 firms that were publicly traded in 1985.[75] We then examined the years in which these firms would have been treated as having become subject to anti-takeover protection by Marianne Bertrand and Sendhil Mullainathan in their article *Enjoying the Quiet Life.*[76] We chose this article because it contains among the fewest errors[77] and because its general methodology (the article focuses exclusively on business combination statutes) and coding have been followed by several other papers.[78]

In our comparison sample, the methodology used by Bertrand and Mullainathan would have

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[74] See, e.g., Bertrand & Mullainathan, supra note 4. For one of the few exceptions, see Cheng et al, supra note 3.

[75] We constructed our comparison sample as follows: we started with the whole set of firms that appear in the CRSP-Compustat database over 1976-1995. We excluded firms whose *gvkey* Compustat identifier appears more than once in any given year -–our inspection of CRSP data strongly suggests that these are firms that have more than one publicly traded class of stock, and hence are arguably at a much lower risk of being taken over by a hostile bidder. Paul Gompers, Joy Ishii & Andrew Metrick, *Extreme Governance: An Analysis of Dual Class Firms in the United States*, 23 Rev. Fin Stud. 1051 (2010). We then excluded financials (firms for which the first digit of the primary SIC code was 6), utilities (firms for which the first two digits of the primary SIC code was 49), firms that went public after 1985 (see discussion below), and firms which did not appear in the database in each of the years 1985 to 1990. Although this last filter biases our sample towards oversampling survivors, firms that did not appear in the sample during 1985-1990 will also have a lesser weight on the estimation of the impact of anti-takeover statutes, most of which were adopted during that period. Finally, we excluded firms that were not incorporated in one of the 50 states or DC or that were limited partnerships in 1985.

[76] See supra note 4.

[77] For examples of papers with more severe errors, see, e.g., infra notes __ and 130.

[78] See, e.g., Atanassov, supra note 5; Qiu & Yu, supra note 7, at 509 (following methodology and using coding of Bertrand and Mullainathan); Giroud & Mueller, supra note 1, at 315 (same); Sauvagnat, supra note 1, at 15 (same) Jayaraman & Shivakumar, supra note 11, at 105 (same); Zhao & Chen, supra note **Error! Bookmark not defined.**, at 96, 101 (same).

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resulted in ascribing a wrong year of becoming subject to anti-takeover statutes for 10.5% of the firms.[79] If we further take into account the effects of poison pills -- and assume, conservatively, that pills (whether flip-in or flip-over) are only valid if endorsed by statute or the state supreme court, that flip-in pills do not offer any stronger protection than business combination statutes, and that there was no doubt about the validity of business combination statutes prior to 1987 -- the rate of error increases to 66% of the firms in our comparison sample.

Bertrand and Mullainathan and most other studies of anti-takeover statutes employ a differences-in-differences methodology. This methodology, which we describe in more detail below, involves a series of pairwise comparisons between a "treated" and a "control" firm. A firm is "treated" between one year and another if it was not subject to an anti-takeover statute in the first year, and was subject to the statute in the second one. "Control" firms are firms that were either subject to the anti-takeover statute in both years or not subject to the anti-takeover statute in either of those two years. If one corrects Bertrand and Mullainathan's categorizations for the mistakes we have identified and for the effect of poison pills, 63% of the comparisons under Bertrand and Mullainathan's coding would be incorrect. That is, in almost two thirds of the pairings which, under Bertrand and Mullainathan's coding, involve a treatment and a control firm, either neither firm was treated, both firms were treated, or the firm considered by Bertrand and Mullainathan's categorizations as treated was in fact a control firm and the firm considered by Bertrand and Mullainathan's categorizations as control was in fact treated.

In sum, because the finance literature fails to grasp the actual effects of anti-takeover statutes on a target's ability to defend itself, and makes some mistakes on top of that, 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. The relationship between the measures of anti-takeover protection used by finance scholars and the actual susceptibility to a takeover given state law, firm specific defenses, and the overall economic

[79] The sources of error include the fact that Bertrand and Mullainathan ascribe a wrong year to the Connecticut, Kentucky and Pennsylvania statutes, omit the 1991 Oregon business combination statute, do not take account of the fact the New York, New Jersey and Missouri business combination statutes were initially applicable only to firms that had their principal place of business in the state, and do not control for changes in the state of incorporation between 1985 and 1995. See, e.g., Ch. 915, 1985 N.Y. Laws 3454 at 3457 (adopting New York statute). In addition, some firms were partnerships during this period and thus not subject to anti-takeover statutes. Papers subsequent to Bertrand and Mullainathan generally rely on even later incorporation data to proxy for firms' historic states of incorporation, and thus contain more severe coding errors.

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and industry environment in which a firm operates is even weaker. Given this attenuated and noisy relationship, how is it that so many studies find statistically significant and, in many cases, economically meaningful relationships between the adoption of anti-takeover statutes and firm behavior? In the following Parts, we will try to shed light on this question.

II. Scrutinizing Studies on Anti-Takeover Statutes

In this Part and in the Appendix, 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 analyses. As we will show, in each of the studies an omitted variable or an improper specification accounts for the statistical association between the anti-takeover statutes at issue and the outcome variable. When corrected for these problems, the association disappears.

The goal of this review is to rebut the argument that anti-takeover statutes must matter, because if they did not, finance studies would not be able to find that their passage is associated with any change in managerial or firm behavior. In Parts III to V, we will supplement our specific critique of the three studies with a more general critique that identifies flaws present in all of the finance studies in this genre.

In this Part, we review *Identifying Control Motives in Managerial Ownership: Evidence from Antitakeover Legislation* by Shijun Cheng, Venky Nagar and Madhav Rajan.[80] The Appendix contains our reviews of *Capital Structure and Corporate Control: The Effect of Antitakeover Statutes on Firm Leverage* and *The Market for Corporate Control and the Cost of Debt.*

Identifying Control Motives in Managerial Ownership examines the relationship between three types of anti-takeover statutes – fair price, control share and business combination statutes – and managerial stock ownership. Starting from the premise that these statutes are effective in deterring takeovers, the authors argue that, after one of the statutes is adopted, "managers do not need to hold as many shares as before to ensure their control."[81] Their main hypothesis is therefore that the passage of

[80] Cheng et al. supra note 3.

[81] Id. at 641.

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these laws is associated with a decline in managerial stock ownership. In a series of regressions, using a sample of 587 large, publicly traded firms, which they follow throughout the 1984-1991 period, they find a negative and significant association between the adoption of an anti-takeover statute and the fraction of the firms' shares owned by the firms' managers and directors.

The main body of the paper contains two types of tests: panel regressions and firm-level regressions. For the panel regressions, the authors first transform the main dependent variable of interest, the percentage of shares of the firm owned by directors and officers, into *ln(1+Director/Officer Stockholdings)*.[82] They then run a series of regressions, including controls for year, industry, and various firm-level characteristics. The explanatory variable of interest is *AfterLaw*, which equals one for a given firm in a given year if the firm's state of incorporation had adopted its first anti-takeover statute (ATS) by the end of the previous year.[83] In these regressions, the estimate of the coefficient of the *AfterLaw* dummy is negative and statistically significant. Cheng et al. interpret this result as evidence that managerial stockholdings dropped after a firm became subject to an ATS.

The panel regressions, however, suffer from a 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.[84] Consider, for example, two firms, Circle K, incorporated in Texas, a state that did not adopt 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% every 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

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[82] 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.

[83] 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. 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.

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

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subject to an ATS.[85] But, of course, this difference cannot be attributed to New Jersey's adoption of an ATS in 1986 since neither firm, in the example, experienced any 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. [86] We were able to match 710 firms with a state of incorporation.[87] The data indicate that, throughout the entire 1984-1991 period, average D&O ownership was systematically higher among the firms incorporated in states that never adopted an ATS than among firms that eventually became subject to an ATS. That is illustrated by Figure 1.

We then ran regressions using a similar set of control variables as did Cheng et al. To confirm that our replication is proper, we first use the same methodology as Cheng et al. do. 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.[88] Put differently, after

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[85] 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.

[86] 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 by searching the different volumes published by the Investor Responsibility Research Center during the late 1980s and the 1990s and the firms' SEC filings from the second half of the 1980s using the SEC Online database in Westlaw. This process allowed us to recover the state of incorporation of 764 out of the 792 firms. After discarding 54 firms that reincorporated during the sample period or were not incorporated in one of the states or D.C., we ended up with a sample of 710 firms.

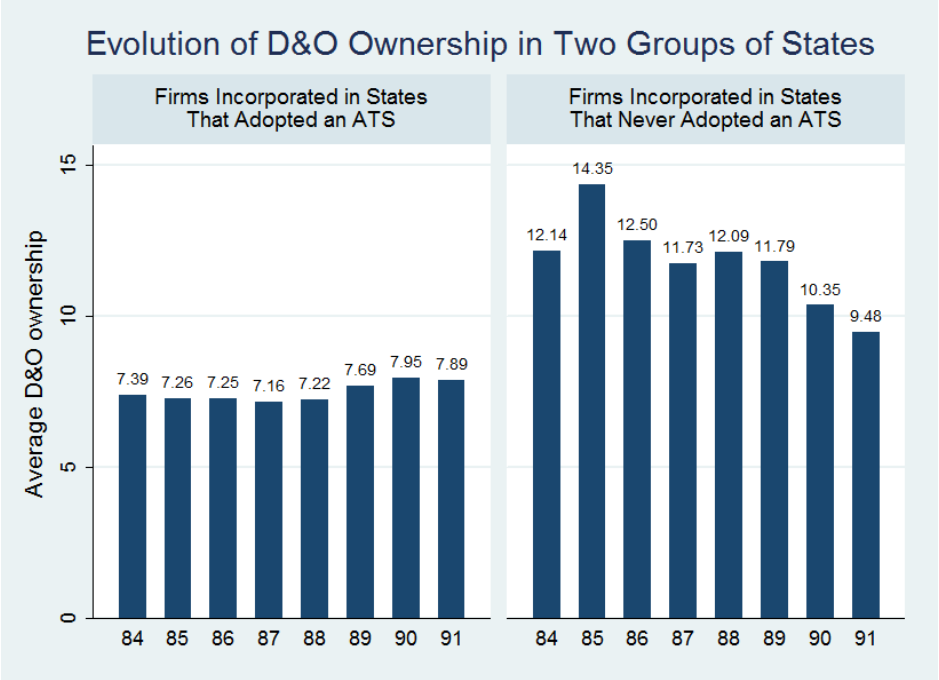
[87] Our sample of firms is somewhat larger than Cheng et al.'s who were able to recover the state of incorporation for only 587 unique firms. They do not indicate what criterion they followed to match the firms in Yermack's database with the databases from which they retrieved information about state 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.

[88] 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.

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controlling for the fact that firms in states that adopted ATS had lower D&O ownership *in the years preceding adoption* than did firms in states that did not adopt ATS, the relation between D&O ownership and ATS evaporates.

Figure 1: Evolution of D&O Ownership



Cheng et al. are aware of 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 (ATS states), 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.[89]

Cheng et al. convey the main result of this analysis in their Table 9. Their Table 9, however, does not report the actual difference in percentage ownership, but the difference in a logarithmic

[89] 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).

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transformation of the ownership percentage. We will refer to this variable as the "*transformed change in ownership*".[90] Cheng et al. report that the mean value of the *transformed change in ownership* is -0.157 and that this mean is significantly different from zero at the 1% level. In subsequent multivariate analysis with additional controls, they obtain similar results.

Table 1 below shows our replication of the analysis of Cheng et al.[91] Again, we start by employing the same methodology as Cheng et al. As shown in the second row of the table, doing so yields 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 post-adoption years minus the average of the ownership percentages in the pre-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.[92] Basically, average ownership did not change at all in firms that became subject to an ATS.[93]

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[90] This variable is constructed as follows: for each firm that eventually became subject to an ATS, they calculate the average D&O stockholdings over the years during which the firm had still not become subject to an ATS, and the average D&O stockholdings over the years during which the firm was already subject to an ATS. They then subtract the first expression from the second, to recover, for each firm, a measure of the average change in D&O stockholdings between the "pre-treatment" years and the "post-treatment" years (call this measure "*average % change"*). They then construct the variable they use in their analysis as the sign of *average % change* times *ln(1+absolute value of average % change)*. See Cheng et al., supra note 3, at 660.

[91] Our sample for the firm-level tests, as Cheng et al.'s, is smaller than the number of unique firms in the sample for the panel regressions since it only includes firms incorporated in states that adopted a statute during the period of analysis.

[92] In unreported results, we performed a similar analysis as that of the second row of table 1, 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 their panel regressions. The results we obtained were qualitatively similar to those of the third row of Table 1.

[93] There is a second reason why the results reported in Table 9 of Cheng et al. 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 ownership had been trending downward before the firms became subject to the statutes. We redid the calculations involved in table 1 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

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Table 1: Change in Ownership After ATS Adoption

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
Actual Change in % ownership (untransformed)	610	-.012	.964	-3.82	-1.66	-.700	-.225	-.017	.073	.271	.787	2.650

The table above,[94] 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.[95] 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.[96]

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smaller in magnitude (-0.008 instead of -.095, as in Table 1) and statistically insignificant.

[94] The fact that the average change in D&O ownership in the third row of Table 1 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.

[95] The review of the Garvey and Hanka paper in the Appendix provides an explanation of the fixed effects methodology.

[96] 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 90, 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

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Table 2 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.[97] 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).

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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. Most importantly, the regression does not "know" whether the variable that reflects the trend of the dependent variable is a very precise or a very noisy estimate of the evolution of average ownership among the firms in the control group. This problem is particularly significant because, according to the paper's coding, only 35 firms did not become subject to any such statute during the sample period (and data for all these 35 firms may not even be available for their regressions). While the predicted value of the coefficient for the "ownership trend" variable is plus one, the estimate for that coefficient in Cheng et al.'s regressions is always negative, and often quite large in magnitude (even if noisily estimated). This suggests that, on average, even if the ATS had not been adopted, ownership trends in the two groups of firms would have moved in opposite directions. Consequently, the "control group" employed by Cheng et al. is unsatisfactory.

The appropriate way to tackle the concern about secular trends is to exploit the panel structure of the database, and (as we do) run a regression using a sample that includes both the firms that at some point became subject to an ATS and those that never became subject to one. Ownership trends can be controlled for by including year fixed effects. In addition, the panel structure allows one to control for secular differences in ownership across states 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).

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

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Table 2: Change in Ownership Regressions

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

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

All these results suggest that the findings reported by Cheng et al. are driven by methodological shortcomings in their analyses. When one analyzes the evolution of stock ownership more carefully, there is no evidence that directors and officers reduced their shareholdings once their firms became subject to an anti-takeover statute. As we show in the Appendix, a closer review of the other two papers yields similar conclusions. When corrected for omitted variables and misspecifications, the postulated relationship between anti-takeover statutes and, respectively, leverage and bond yields disintegrates.

III. Categorization Problems

In Part II and the Appendix, we examine three finance studies and show that their finding of a link between anti-takeover statutes and, respectively, managerial ownership, leverage and bond prices can be explained by the failure to control for key variables or by misspecifications. There are, of course, many other studies of anti-takeover statutes that we do not review in such a detailed way. In Parts III to V, we will discuss three problems that, to our knowledge, affect all the studies of anti-takeover statutes. These problems, together with our argument in Part II that anti-takeover statutes did not materially increase a target's ability to defend itself, make us very doubtful that the results derived in these studies are causally attributable to anti-takeover statutes.

As mentioned before, categorization problems affect even the best of the existing finance studies. These problems relate to errors regarding the year in which a state adopted a statute, to errors regarding the state in which a firm was incorporated, and to errors as to which firms were covered by a state's anti-

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takeover statute. These errors, together with a very conservative assessment of the effect of poison pills, would have generated a massive mismeasurement in whether and when firms became subject to takeover protection.[98]

The errors we identify cannot be dismissed as noise that merely results in less accurate regression estimates. Rather, they have systematic (non-random) effects on the categorization of firms that render any results generated unreliable. In this Part, we discuss some of these systematic effects.[99] The next two Parts will address biases generated by the failure to take account of managerial ownership and by the endogeneity of the state of incorporation, respectively.

The various categorization errors have systematic effects because the firms affected by these errors are not randomly selected. Assigning the wrong year to when an anti-takeover statute was adopted, omitting a statute entirely, or ignoring pill validation statutes or case law clearly establishing the validity of poison pills generally affects *all* firms incorporated in a specific state. Similarly, errors regarding the state in which a firm was incorporated and which firms were covered by a state's anti-takeover statute affects specific types of firms: firms that decided to reincorporate or firms that are headquartered outside their state of incorporation.

But firms incorporated in a specific state, firms that reincorporated, or firms headquartered outside their state of incorporation are not a random selection of firms. For example, outside of Delaware and Nevada, most firms incorporated in a state also have their principal place of business in the state. Firms incorporated in a certain state thus resemble each other in their geographic location and sometimes in other ways, such as the industries in which they operate.

We provide a few specific examples that illustrate some of the systematic effects of the categorization errors.

- Under Bertrand and Mullainathan's categorization, 331 of the 2391 firms in our comparison sample were incorporated in states that did not adopt an anti-takeover

[98] See supra Section I. D.

[99] Papers in this strand of literature systematically overlook another potential source of estimation bias. During the second half of the 1980s almost all states adopted provisions enabling corporations to include in their articles of incorporation a clause that limits the liability of directors for violations of the duty of care. See, e.g., Delaware General Corporation Law, Sec. 102(b)(7).

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statute. However, almost one half of these firms (152) were incorporated in states that did adopt either a business combination or a pill validation statute. These firms are substantially smaller (their median book value of assets as of 1985 was $18 million) than the firms in the comparison sample (median assets $55 million).[100]

- Under Bertrand and Mullainathan's categorization, 76 firms in the comparison sample became "treated" between 1989 and 1990, mostly because Ohio passed its business combination statute in 1990. Since Ohio had adopted a pill validation statute in 1986, we regard these firms as having become subject to takeover protection four years earlier. Of the 72 firms that are wrongly categorized, 68 firms (94%) were headquartered in Ohio. Overall, Ohio-headquartered firms constitute 4.8% of our comparison sample.
- Under Bertrand and Mullainathan's categorization, 184 firms (all incorporated in New York) in our comparison sample were "treated" between 1985 and 1986 (as New York adopted its business combination statute). However, 52 of these firms were headquartered outside of New York and thus did not become subject to the statute. These 52 firms were larger (median assets $103 million) and had lower median board and managerial ownership (10.8%) than the firms incorporated in New York that were also headquartered in New York (median assets $33 million; median D&O ownership 18.9%).[101]

Because economic shocks may have differential effects on large firms, firms with low board and managerial ownership, firms located in a specific area, and so on, these systematic errors affect the regression estimates more severely than measurement errors that are simply random. For example, if

[100] In a Mann-Whitney test, the difference in median values between those 152 firms and the other firms in the Comparison Sample is significant at the 1% level.

[101] The managerial ownership data corresponds to year 1989 (see footnote 104 below and corresponding text). In a Mann-Whitney test, the difference in the median value of assets between the New York-incorporated firms headquartered in New York and the New York-incorporated firms headquartered elsewhere is significant at the 5% level. The Mann-Whitney test comparing the median director and officer ownership between these two groups yields a p-value of .11 (this is unsurprising, since the ownership data is only available for 39 out of the 52 firms headquartered out of New York)

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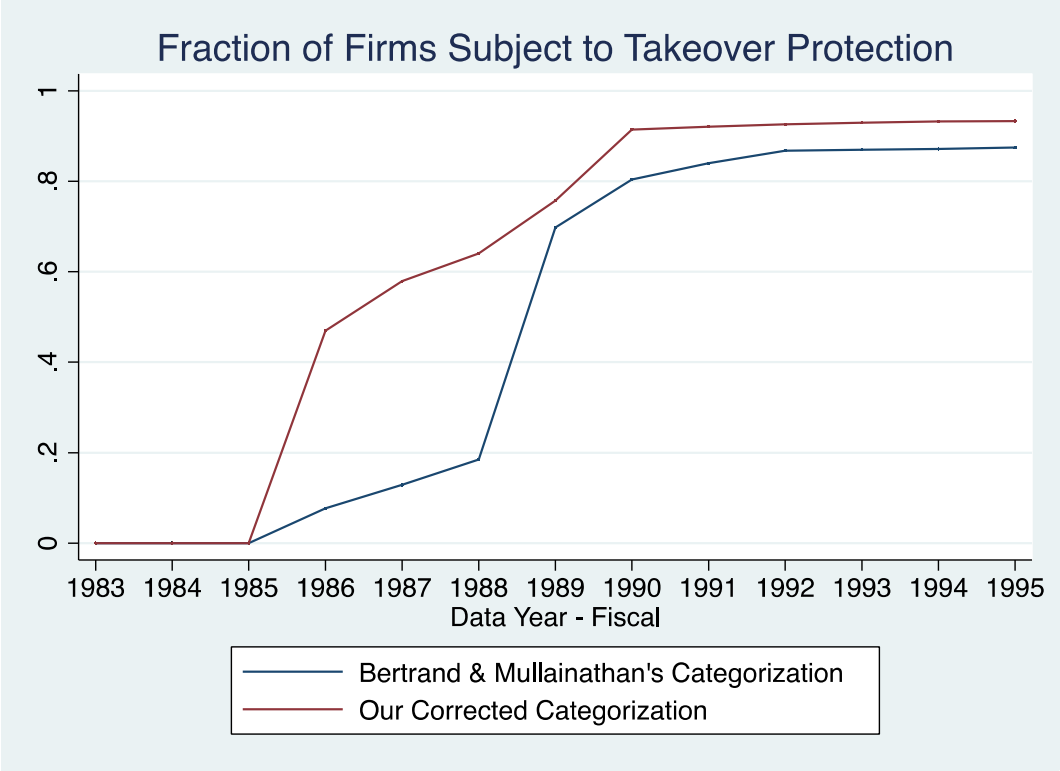
Ohio firms suffered an economic shock at around 1990, the impact of the shock would be reflected in the estimate for the anti-takeover statute variable.[102]

Moreover, the categorization errors result in firms being regarded as becoming subject to takeover protection at a systematically later date. Figure 2 below describes the fraction of firms in our comparison sample subject to takeover protection at the beginning of each year, under the methodology used by Bertrand and Mullainathan and under our methodology. The differences are stark. The fraction of firms subject to protection rises from 7.7% to 47% for 1986 and from 18.5% to 64% for 1988. Under our methodology, only 0.7% of firms become subject to takeover protection during 1990 or 1991 and only 7.5% of firms do not become subject to takeover protection at all during the standard study period (1976 to 1995); under Bertrand and Mullainathan's, the respective percentages are 5.2% and 13.9%. Importantly, under our categorization, the timeframe over which the bulk of firms become subject to takeover protection is much more compressed: within 5 years, over 90% of firms are covered by a statute. This compressed timeframe would generally increase the likelihood that regression estimates will be tainted by omitted variable bias, as the estimates will be more prone to ascribing to takeover protection the effects of concurrent economic shocks that had differential effects on treated and control firms.

[102] Although some of these potential sources of bias could be mitigated by adding multiple layers of highly granular fixed effects (e.g., a location-by-year fixed effect, an industry-by-year fixed effect, a size-quintile-by-year fixed effect, an insider-ownership-quintile-by-year fixed effect), the amount of fixed effects one would need to include would be massive. We know of no published paper that gets even close to doing so.

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Figure 2



IV. Managerial Share Ownership

There are strong reasons to believe that firms with different levels of board and managerial share ownership ("inside ownership") will differ in their response to a change in state-supplied anti-takeover protection. First, high inside ownership provides a defense against a hostile takeover. Second, it provides significant incentives to increase the value of the equity held by the board and management. To the extent, for example, that a decrease in the takeover threat induces management to run the firm less efficiently (as argued by some commentators) or that an increase in the takeover threat induces management to pursue short-termism at the expense of generating long-term value (as argued by others), large inside ownership should produce significant counter-incentives. Reasonable minds may differ as to when inside ownership is large enough to significantly reduce the threat of a hostile takeover or to overpower the incentives created by the takeover threat. We regard, respectively, a 30% and a 20% ownership stake as reasonable cutoffs. That is, once the board and upper management owns at least 30% of the company's stock, we would regard the protection against a hostile takeover afforded by that ownership as so significant that the additional protection offered by other anti-takeover devices is not material. And once the board and upper management owns at least 20% of the company's stock, we

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would regard the incentives provided by that ownership as so significant that the additional incentives generated by the presence or absence of other anti-takeover devices are not material.[103]

We were able to recover information on inside ownership for 1807 firms in our comparison sample. In 543 of these firms (30.0%), directors and executive officers owned at least 30 percent of the firm's shares. In 44 percent of the firms for which we could recover ownership data, inside ownership exceeded 20 percent of the firm's shares.[104]

The effect of director and officer ownership does not simply attenuate the impact of anti-takeover statutes across the board. It also renders the firms incorporated in states that never adopted a business combination statute a very poor counterfactual for those incorporated in states that eventually adopted such a statute. Among the first group of firms, the median director and officer ownership was approximately 28.2 percent, and the fraction of firms in which directors and officers held more than 30 percent of the shares was 47.6 percent. By comparison, among firms incorporated in states that eventually adopted a business combination statute, the median director and officer ownership was only 15.2 percent, and the fraction of firms whose directors and officers held more than 30 percent of the shares was 27.5 percent.[105] Firms incorporated in states that never adopted a business combination statute are also dramatically smaller than their peers. In our comparison sample, the average (median) book value of assets reported for 1985 by the firms that never became subject to a business combination statute was $190.6 million ($20.7 million),[106] while the respective values for firms that eventually

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

[104] Our ownership data is from 1989. Although this is less than ideal, we believe on the basis of David Yermack's data about ownership throughout 1984-1991 that D&O ownership is very stable within firm over the sample period.

[105] In a Mann-Whitney test, the difference in the median director and officer ownership between the two groups was significant at the 1% level

[106] Firms that never became subject to either a business combination or a pill validation statute constitute an even worse comparison group. Median D&O ownership was 32% (significantly different from the median ownership among the remaining firms in the Comparison Sample at the 1% level) and D&O ownership exceeded 30% in 53% of the firms. Average (median) book value of assets as of 1985 was $137M ($21.9M). The average (median)

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became subject to a business combination statute were $903 million ($65 million).[107]

These differences (and possibly multiple other, observable or unobservable, differences) imply that firms in states that never adopted a business combination statute and those in states that eventually adopted such statutes were likely to respond to aggregate shocks in very different ways. Consider, for example, the potential impact of the 1990-91 recession.[108] Should one presume that firms where insiders own 28% of the shares respond similarly to the downturn – in terms of reducing expenses, maintaining long-term investments, changing leverage, selling assets, and so on – as firms where insiders own a much lower stake? We think not. But finance studies, by failing to control for ownership stake, implicitly make this assumptions. By using one group of firms – those incorporated in states that never adopted a business combination statute -- as an input to construct the counterfactual for the other, even though those groups consist of very different types of firms, these studies are likely to derive biased estimates of the impact of business combination statutes.

V. Selection Bias and Endogeneity

The premise underlying the finance studies of anti-takeover statutes is that these statutes are exogenous: which firms are subject to a statute, and when they become subject, is determined quasi-randomly, and is not "chosen" by a firm (and hence not endogenous). As imagined by these finance studies, firms incorporate in a certain state before they know whether (and when) the state will adopt an ATS; some states then decide to adopt a statute; and firms are stuck with the decision made by the state. Bertrand and Mullainathan thus explain that "[anti-takeover statutes] avoid the endogeneity problem to the extent that they are passed by states and not endogenously driven by firm-specific conditions. Unlike firm-specific takeover defenses, laws are not passed on a firm-by-firm basis."[109]

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value of assets was significantly lower from the median value among the remaining firms in the Comparison Sample at the 1% (5%) level.

[107] The differences in median and mean value of assets between firms that eventually became subject to a business combination statute and those that never became subject to such a statute were both significant at the 1% level.

[108] See http://www.nber.org/cycles.html

[109] Bertrand & Mullainathan, supra note 9, at 1045.

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That the protection afforded by anti-takeover statutes is not endogenous is central for the design of these studies. Consider, as an analogy, an experimental drug that has the potential for alleviating a serious illness, but also entails severe side effects. A study where patients are randomly assigned to the drug and a placebo resembles a finance study where the protection afforded by anti-takeover statutes *is not* endogenous. A study where patients can choose whether to receive the drug resembles a finance study where the protection afforded by anti-takeover statutes *is* endogenous. But then, because patients who chose to receive the drug despite the side effects are likely to differ systematically from patients who chose to forego the drug – they may be more ill, or they may be better able to withstand the side effects – comparing how patients who receive the drug fare relative to patients who do not may reflect the difference in patients rather than the effect of the drug.

As we show in this Part, the premise that the protection afforded by anti-takeover statutes is exogenous is wrong. It is wrong for three reasons: First, firms can reincorporate. Second, even prior to the adoptions of the first anti-takeover statute, one state – California – had legal rules that differed from other states in the level of takeover protection they afforded. And third, finance studies oddly include in their sample firms that went public *after* the first anti-takeover statutes were adopted, and thus chose between going public in a state that had adopted a statute or one that had not.[110]

A. Reincorporations

Empirical scholars almost always obtain the information to define each firm's state of incorporation from Compustat. Compustat, however, keeps track only of a firm's current state of incorporation, not of where a firm was incorporated in prior years. Finance studies thus look at where a firm was incorporated many years after the passage of anti-takeover statutes, sometime (depending on the study) between 1995 and today. But where a firm was incorporated in 1995, or 2015, is endogenous: determined by choices made by firms – whether or not to reincorporate – rather than by "random"

[110] Other scholars have noted that several states adopted their anti-takeover statutes at the behest of particular firms (or groups of firms). See, e.g., Roberta Romano, *The Political Economy of Takeover Statutes*, 73 Va. L. Rev. 111 (1987); Karpoff & Malatesta, supra note 73, at 305. If firms lobbied for these statutes in response to shocks, and shocks are correlated across firms incorporated in the same state, then the estimates of the impact of those statutes could be tainted by omitted variables bias.

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decisions by states to adopt statutes in the late 1980s.[111]

To determine the frequency of reincorporations, we obtained data on where firms were incorporated in 1989 from Compact Disclosure, and on where the firms were incorporated in the mid-1990s from SEC Analytics.[112] We supplemented these data with searches in Moody's manuals for reincorporations in the years 1985 to 1988, and with searches of SEC filings from the 1980s and early 1990s available in Thomson One Banker. Out of the 2391 firms in our comparison sample, we identified approximately 12.2% that changed their state of incorporations between 1985 – the year the first business combination statute was adopted – and the mid-1990s.

As to firms that reincorporated after some states had adopted an anti-takeover statute, the state of incorporation is clearly endogenous. These firms, at the time of reincorporation, could have either intentionally chosen, or intentionally avoided, a state with an anti-takeover statute.

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

Notably, in the 1985 to 1995 period, firms faced few barriers to reincorporating. Reincorporations costs were low, $40,000 to $80,000 for a company with 100,000 shareholders according to a contemporary estimate by Bernard Black.[113] And shareholders, who regularly approved

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[111] Finance scholars are aware that companies can move their state of incorporation. They ignore reincorporations, relying on an article by Bertrand and Mullainathan that reports that only 3 companies in a sample of 200 reincorporated during a 20 year period. Id. at 1053. As we discuss, reincorporations are substantially more frequent than Bertrand and Mullainathan found.

[112] The coverage of the SEC Analytics database gradually increases over time, starting in 1994, and becomes almost completely comprehensive by 1996. For each firm, we recover the firm's state of incorporation as of the firm's earliest occurrence in the database.

[113] Bernard S. Black, *Is Corporate Law Trivial? A Political and Economic Analysis*, 84 Nw. U. L. Rev. 542, 558 (1990). If an anti-takeover statutes generated economically meaningful effects that can be picked up in finance studies, then relatively modest costs of reincorporation should present no barrier to firms' changing their state of incorporations. But even if the costs of reincorporating were orders of magnitude higher, as long as they are not prohibitive, the state of incorporation will be endogenous for companies where the decision-makers care most

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all kinds of anti-takeover devices such as staggered boards and anti-takeover "fair price" charter amendments during that period, would have been unlikely to balk at a proposed reincorporation.[114]

Given these low barriers, one may wonder why only 12% of firms reincorporated. To us, the answer is straightforward: with the exception of California, there were no substantial differences in the level of takeover protection afforded by states. And indeed, firms that reincorporated out of California account for the bulk of the reincorporation activity in the 1985 to 1995 period. But for finance scholars, the fact that only 12.2% of firms reincorporated, and that most reincorporations were not from a state without an anti-takeover statute to one with (or vice versa), remains a puzzle that they fail to address.

B. California

As discussed in Part I, California has long-standing laws that, until 1989, prohibited staggered boards and that continue to require that companies permit shareholders to remove directors without cause and to call a special meeting. Companies that wanted to provide for staggered boards, director removal for cause only, and no shareholder right to call a special meeting – all provisions that make takeovers more difficult – needed to incorporate in another state. At least for companies headquartered in California – 15% of the firms in our comparison sample -- for which a California incorporation would be a natural option, the state of incorporation thus reflects a choice between a state offering laws facilitating takeovers and states that are neutral or anti-takeover. Even without regard to the ability to reincorporate post-1985, the state of incorporation for all California headquartered firms should thus be viewed as endogenous.

C. Firms That Became Public After 1985

Though finance studies place great importance of the supposed exogeneity of anti-takeover

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about these statutes.

[114] According to IRRC data, by 1987, 158 of 424 Fortune 500 companies had "fair-price" provisions, most of which were adopted in the 1980s. Virginia Rosenbaum, Takeover Defenses: Profiles of the Fortune 500 (1987). In addition, more than half of publicly traded firms that did not have a staggered board in 1980 had adopted such a structure by 1990. See Martijn Cremers, Lubomir Litov & Simone Sepe, Staggered Boards and Firm Value, Revisited, Fig. 2 (available at http://papers.ssrn.com/sol3/papers.cfm?abstract_id=2364165).

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statutes, the sample they use in their regressions includes firms that become public after the first anti-takeover statutes were passed. Because it is very easy to change a state of reincorporation before a firm becomes publicly traded – transaction costs are almost nil and obtaining the requisite shareholder vote is straightforward – these firms could have opted to go public in a state that has already adopted the statute at issue or opted to go public in a state that has not adopted the statute (but may in the future).

Because our comparison sample is limited to firms that were public in each year between 1985 and 1990, firms that became public after 1985 are by design excluded. But our review of the CRSP-Compustat database, a principal database of public firms used by finance scholars to construct their samples, indicates that firms that became public after 1985 constitute a substantial fraction of the firms used in the finance studies. For example, more than half of the firms that were public in 1995 first appeared in the database after 1985.

Because of these endogeneity problems, even if the statistical link between anti-takeover statutes and firm behavior were to hold up in some of the finance studies, one could not infer that any change in the takeover threat entailed by these statutes *caused* the change in firm behavior. Instead, it would be equally plausible that firms sorted themselves into whether or not they want to be subject to an anti-takeover statute and that the factors affecting this firm choice are responsible for the change in firm or managerial behavior.

VI. Implications and Conclusion

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 reviewed in detail three empirical studies and showed that their main results are due either to the omission of important control variables or to methodological flaws. Finally, we have identified significant problems – miscategorization, failure to control for inside ownership, and selection bias -- that affect the other articles in this literature.

We started this article by pointing to a divide among scholars in their view of anti-takeover statutes. Legal scholars tend to dismiss them as barely relevant, while empirical finance find that they

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have significant effects. One contribution of this article is thus to show that the empirical results generated by finance scholars may be due to factors other than the causal effect of anti-takeover statutes.

But this article has important implications that go beyond anti-takeover statutes. Most importantly, it calls into doubt much of the perceived empirical knowledge about the real economic effects of a change in the threat of a takeover.

Starting in the 1980s, theorists took different positions on what these effects might be. One set of scholars argued that the threat of a takeover acts as a beneficial disciplining device that induces managers to act in the interest of shareholders.[115] Another set of scholars argued that the threat of a takeover induces an excessive short-term focus by management and thereby lowers long-term shareholder value.[116] Yet others have suggested that the takeover threat may lead management to take

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[115] Easterbrook & Fischel, supra note 16 (arguing that hostile tender offers are an important device to reduce agency costs); Gilson, supra note 16, 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 16, 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).

[116] 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 17 (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 17; Lipton, supra note 17, 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*

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actions that benefit shareholders, but harm other constituents, and may therefore not enhance overall social value.[117]

Takeovers and takeover defenses continue to generate significant controversy. Earlier this year, for example, a blue-ribbon commission co-chaired by Larry Summers -- a renowned economist and former U.S. Treasury Secretary and Harvard president – endorsed a limitation on voting rights for short-term shareholders to make hostile takeovers more difficult. This, it is argued, would help combat excessive short-termism.[118] At the same time, under pressure of shareholder rights advocates and institutional investors, most large companies that used to have staggered boards decided to move to annual elections of the entire boards, thereby facilitating hostile takeovers.[119] Moreover, the long-standing debate over the effects of hostile takeovers has a curious parallel in a more recent one, with many of the same partisans and rehashing many of the same arguments, about the effect of activism by hedge funds.[120]

To empirically test the hypotheses about the effect of a change in the threat of takeovers, one would ideally want to compare two sets of firms – one set which faces a sudden increase (or decrease) in the takeover threat and other set where the takeover threat is stable – and compare how they perform. This is the rationale behind many of the studies on anti-takeover statutes. Thus, for example, the study

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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).

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

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

[119] See http://dealbook.nytimes.com/2015/01/05/an-unusual-boardroom-battle-in-academia/?_r=0.

[120] A similar debate is waged today about the effect of activist hedge funds. See, e.g., Marcel Kahan & Edward Rock, *Hedge Funds in Corporate Governance and Corporate Control*, 155 U. Pa. L. Rev. 1021, 1083-91 (2007) (reviewing debate); Lucian A. Bebchuk, *The Myth that Insulating Boards Serves Long-Term Value*, 113 Colum. L. Rev. 1637 (2013) (arguing that hedge funds do not induce short-termism); Wachtell, Lipton, Rosen & Katz, Still No Valid Evidence that Attacks by Hedge Funds are Long-Term Beneficial to Corporations, their Shareholders or the American Economy, Jan. 20, 2015 (disputing evidence that hedge fund activism leads to improved operating performance by targeted companies).

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by Garvey and Hanka, which we review in the Appendix, and the study by Bertrand and Mullainathan, to which we refer in Parts III and V, conclude 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.

But if these statutes do not have an impact on the takeover threat, or if (re)incorporation decisions render a firm's exposure to them endogenous, the single best source of unconfounded evidence for how the takeover threat affects real behavior becomes useless. As we see it, four decades of studying the effect of a takeover threat have yielded little knowledge. Rather than pouring even more energy into empirical studies of anti-takeover statutes, scholars should develop a different approach.[121]

Our findings also have some farther-reaching implications. The use by empirical scholars of anti-takeover statutes to construct the main explanatory variable in their analyses, despite the lack of a well-grounded understanding of how these statutes function in actuality, reflects broader problems. There seem to be a number of law-related variables that lack coherent theoretical grounding but are frequently used by empiricists. Top of the list is the widely-used GIM governance/takeover index. Lucian Bebchuk, Alma Cohen and Allen Ferrell have shown that institutional investors care little or not at all about 18 of the 24 elements in the GIM index. They propose, as an alternative, an index based on the six factors that attract significant opposition by institutional investors.[122] Similarly, Michael Klausner has recently

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[121] Another popular approach are event studies related to the enactment of anti-takeover statutes and to major legal opinions. See, e.g., Karpoff & Malatesta, supra note 73 (event study on multiple anti-takeover statutes). Event studies on anti-takeover statutes have produced mixed results, with most studies either finding no significant effects or small negative effects. See Sanjai Bhagat & Roberta Romano, *Event Studies and the Law: Part II - Empirical Studies of Corporate Law*, 4 Amer. L. & Econ. Rev. 380 (2002). In legislative event studies, it is often difficult to identify the precise event dates. In event studies of legal opinions, it is often difficult to separate the legal event from other contemporaneous market-moving events. Event studies that develop an identification strategy that overcomes this problem (see, e.g., Alma Cohen & Charles C.Y. Wang, *How Do Staggered Boards Affect Shareholder Value? Evidence from a Natural Experiment*, __ J. Fin. Econ. __ (analyzing *Airgas* rulings which had disparate effects on companies with staggered boards depending on the timing of a company's annual meeting and finding that evidence consistent with the view that staggered boards reduce stock price)) at most measure the market's expectation of the effect of a legal event.

[122] Lucian Bebchuk, Alma Cohen & Allen Ferrell, *What Matters in Corporate Governance?*, 22 Rev. Fin. Stud. 783 (2009).

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argued that the GIM index contains elements that are irrelevant for all companies and elements that are irrelevant for a subset of companies. Even to the extent that the index captures useful variables, he explains, empiricists have not understood the underlying governance mechanisms and have misinterpreted their results.[123] Misinterpretations of this sort have a long pedigree. Already 15 years ago, John Coates had argued that economists widely misinterpret the import of a company adopting a poison pill.[124]

To our mind, what all of these instances have in common is that they reflect the generation of a variable – anti-takeover statutes, GIM Index, pill adoptions – 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 to a large set of potential outcomes – leverage, wages, patents, dividends, and so on -- employing various methodologies and adding differing 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 easily employed in an empirical test, and pay little heed to arguments that the variable has no theoretical validity.

During the past couple of decades, scholarship in the intersection of law and finance has become increasingly prevalent. This interdisciplinary approach promises to yield a significantly better understanding of corporate law and corporate governance. To do that, however, scholarship has to both employ a proper methodology and be based on a proper understanding of legal institutions. To put it more bluntly, it is high time for finance scholars to pay more attention to the "law" in "law and finance".

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

[124] See Coates, supra note 23.

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Appendix

In this Appendix, we discuss two additional finance studies: *Capital Structure and Corporate Control: The Effect of Antitakeover Statutes on Firm Leverage*, published in the Journal of Finance, by Gerald Garvey and Gordon Hanka and *The Market for Corporate Control and the Cost of Debt*, published in the Journal of Financial Economics, by Jiaping Qiu and Fan Yu.

The starting point of *Capital Structure and Corporate Control: The Effect of Antitakeover Statutes on Firm Leverage*[125] is the view that leverage can keep managers on their toes. Managers, in turn, would prefer to issue less debt than shareholders desire. Since anti-takeover statutes are thought to make hostile takeover discipline less stringent, the argument then goes, managers of firms subject to ATS are likely to reduce the amount of leverage in their firms' capital structure.[126]

Garvey and Hanka's data consist of annual observations for 1200 publicly-traded firms over the 1982-1993 period.[127] They construct their main explanatory variable, the "Protected dummy", as a dummy that switches from zero to one in the year after the firm's state of incorporation adopted an ATS.[128] 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 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.[129]

The main problem in Garvey and Hanka's analysis relates to the way they try to control for

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[125] Garvey & Hanka, supra note 2.

[126] Id. at 519-20.

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

[128] 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.

[129] Id. at 522-530.

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leverage trends over time unrelated to antitakeover statutes.[130] 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 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

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[130] In addition, Garvey and Hanka's paper has pervasive coding errors. First, the authors wrongly claim that the business combination statutes adopted by Delaware and Pennsylvania only took effect in 1990. See Garvey & Hanka, supra note 2, at 522. In fact, Delaware's and Pennsylvania's statutes took effect in December 1987 and March 1988, respectively. See Karpoff & Wittry, supra note 15, at 37-38. Second, the authors have a peculiar way of dealing with states that had adopted anti-takeover laws prior to the *CTS* decision. They suggest, correctly, that these laws were of doubtful constitutionality and exclude firms incorporated in states that passed such laws before 1987. Garvey & Hanka, supra note 2, at 522, 523. 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*. Id. at. 524. 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 ignore pill validation statutes, which are at least as important as the statutes they analyze. We estimate that these three coding errors results in a miscoding of the Protected dummy in, respectively, 70% and 16%, and 3%, of the firms in their sample.

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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").[131]

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 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.[132] 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

[131] 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.

[132] Garvey & Hanka, supra note 2, at 529 (Tbl. III, col. 3).

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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.[133]

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 A1 below gives the value of the variable Y for the years 1986 to 1991. The value of Y is, in each year, identical for Firm 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.[134] 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.[135] In other words, even though

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[133] 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.

[134] 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.

[135] 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.

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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 A1: 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.[136] 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 (year fixed effects) instead of the Time dummy.[137] The estimate for the coefficient of Protected dropped to -.0036, and stopped being statistically significant.[138] (When, in addition, we corrected the miscodings described in footnote 130, our results remained essentially unchanged.)

[136] 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 A2.

[137] 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.

[138] 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.

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Table A2: Garvey and Hanka Replication: Leverage and Anti-Takeover Statutes

	Protected	Time
Garvey and Hanka, Table III, col. 3	-0.013*** (0.004)	0.0093*** (0.004)
Our replication, same methodology	-0.013*** (0.004)	0.009*** (0.004)
State Dummy + Year Fixed Effects	-0.0035 (0.0022)	
State Fixed effects + Year Fixed Effects[§]	-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.[139] 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 and leverage changes.

Jiaping Qiu and Fan Yu's article *The Market for Corporate Control and the Cost of Debt,*[140] examines the relationship between business combination statutes and bond yields and concludes that these statutes are associated with a significant increase in yields.[141] Qiu and Yu construct a yearly panel that spans the 1976-1995 period and includes yield data for bonds issued by approximately 700

[139] 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. These regressions, by design, lack even the coarse controls for state (the State dummy) employed in the regressions with the Time dummy. Moreover, 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.

[140] Qiu & Yu, supra note 8.

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

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individual firms.[142] 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);[143] 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.[144]

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.[145] Specifically, while they find no evidence of a significant increase in the yield spread for bonds that are rated investment grade, they find an increase of over 114 basis points for speculative-grade (a.k.a. junk) bonds.[146] 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).[147]

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

[143] 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.

[144] 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. In addition, their sample may include a large number of financials and utilities among the issuers and a large number of bonds issued by corporations that are not publicly traded or entities that are not corporations, for which business combination statutes are not relevant. Including financials and utilities is problematic because those firms tend to be subject to federal regulation, and their takeover is governed by rules that depend on the state where they operate (see, e.g., Robert M. Daines, *Does Delaware Law Improve Firm Value?*, 62 J. Fin. Econ. 525, 530 (2001).).

[145] 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.

[146] Id. at 507.

[147] 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%.

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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.[148] But Billett, King and Mauer explicitly exclude leveraged buyouts, which are associated with a decline in bond values,[149] from their sample. Their results thus overstate the average effect of all acquisition on 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 by a stronger firm *if it is* subject to a statute and *if it is not* subject to a statute. This difference is small: many firms would not receive any acquisition offer to start with; many offers are not made by financially stronger firms; many offers are not opposed by management and thus not affected by a business combination statute;[150] and even with respect to hostile offers by financially stronger firms, the presence or absence of a statute is at most one of several factors that bear on the offer's success. It thus makes no sense that an (at most) somewhat reduced prospect of a 4.3% increase in junk bond prices would account for an increase in yield of over 114 basis points. Something else must be going on.

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

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[148] 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%.

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

[150] Even at its peak, the percentage of firms subject to hostile M&A activity in a given year in a sample of firms collected by Cremers and Ferrell did not exceed 0.5%. See Cremers & Ferrell, supra note, at 8. By the same token, Cain et al. report that, at any year during 1980-1995, the fraction of firms acquired by a hostile bidder never exceeded 0.25% of all publicly traded firms. Cain et al., supra note 56, at 12.

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liquidity crisis and when UAL failed to secure buyout financing."[151] 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 associations for holding junk bonds in August 1989.[152] Finally, the United States experienced a recession between July 1990 and March 1991.[153] Junk bond issuers are particularly likely to be negatively affected by recessions, as the cash flows they rely on to repay their debt are likely to diminish. As a consequence, the average default rates for junk bonds during 1990-1992 were dramatically higher than their average default rates over the preceding decade.[154]

Figure A1 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.[155] As Figure A1 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.[156]

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[151] Comment & Schwert, supra note, at 9.

[152] 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.

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

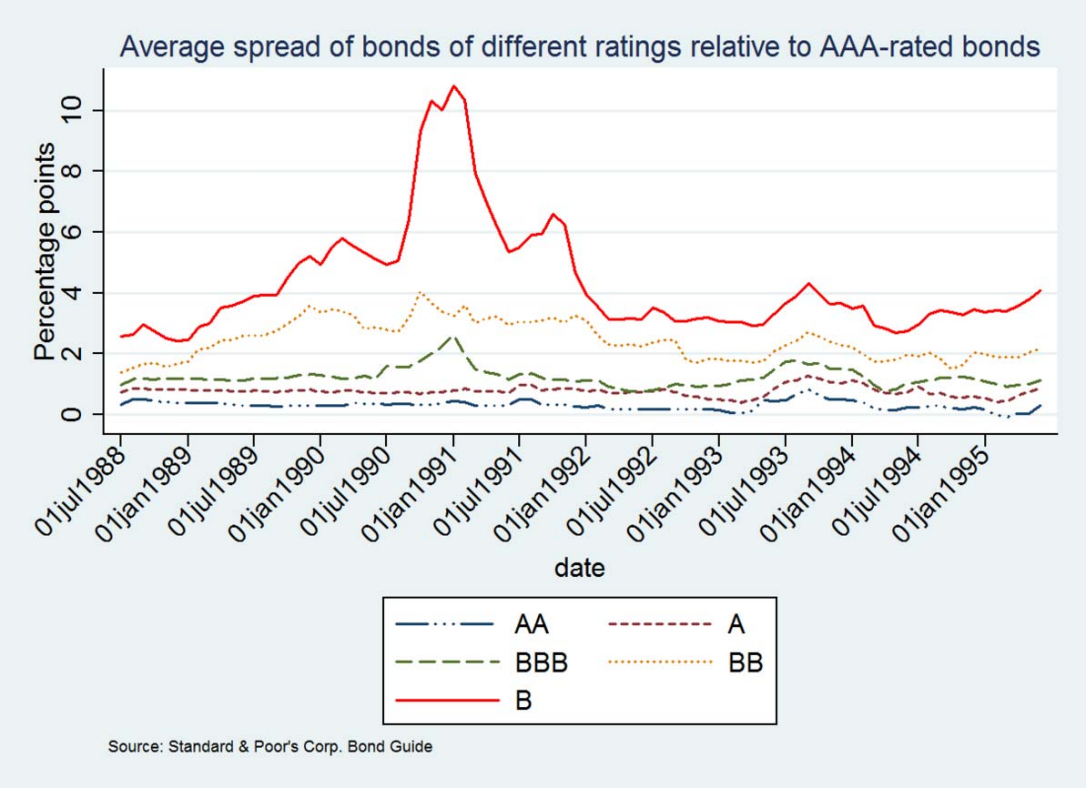
[154] Jean Helwege & Paul Kleiman, *Understanding Aggregate Default Rates of High Yield Bonds*, 2 Current Issues in Econ. & Fin. 6 (1996).

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

[156] Figure A1 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 8, at fn 7.

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Figure A1



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,[157] 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

[157] See Qiu & Yu, supra note 8, at 515 (Table 6, column 3).

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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 A1, the yield spread for junk bonds substantially widens right around the time firms became subject to business combination statutes.[158] When the yield spread on junk bonds (but not on investment grade bonds) rises in 1989 and thereafter, this rise may therefore push up the estimate for the coefficient of the variable *BC*Speculative Grade*.

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.[159]

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[158] 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.

[159] In these regressions, we used year-rating fixed effects instead of the year and rating dummies used Qiu and Yu.

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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.

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 or by the alternative hypothesis that takeovers are associated with a decline in bond values due to increased leverage. See Francis et al, supra note 8; see also supra note 149 (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 8, 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.

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