



ARTICLE

The Law and Finance of Antitakeover Statutes

Emiliano M. Catan* & Marcel Kahan**

Abstract. Over the last fifteen years, numerous finance articles have examined the effect of antitakeover statutes (ATs) 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 of an antitakeover 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 the firm. The finance studies, by failing to take account of how antitakeover defenses interact and how they function in practice, are based on fundamentally flawed assumptions about the additional protection afforded by antitakeover statutes. In particular, because most firms have access to 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 coding the year in which states adopted statutes and the companies such statutes cover, and suffer from selection bias and endogeneity. These problems render the empirical results derived by these studies unreliable. Indeed, this Article replicates several of these empirical studies in order to show that their results do not withstand closer scrutiny.

This Article has important implications for the debate over whether an increased threat of takeovers acts as a disciplining device or induces short-termism. The finance studies

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

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criticized herein have been taken as supplying the single best source of unconfounded empirical evidence in this debate. But if, as this Article will show, these studies suffer from serious flaws, much of the perceived empirical knowledge about the real economic effects of a change in the threat of takeovers has to be reassessed. In the end, decades of empirical studies have yielded little empirical knowledge.

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Introduction

Over the last fifteen years, finance scholars have developed an increasing fascination with antitakeover 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 yields⁷ and the cost of bank loans,⁸ executive pay,⁹ cash reserves,¹⁰ earnings quality¹¹ and loss recognition,¹² loan

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1. See, e.g., Xavier Giroud & Holger M. Mueller, *Does Corporate Governance Matter in Competitive Industries?*, 95 J. FIN. ECON. 312 (2010); Julien Sauvagnat, *Corporate Governance and Asset Tangibility* (Oct. 2013) (unpublished manuscript), <http://ssrn.com/abstract=1947546>.
 2. See, e.g., Gerald T. Garvey & Gordon Hanka, *Capital Structure and Corporate Control: The Effect of Antitakeover Statutes on Firm Leverage*, 54 J. FIN. 519 (1999); Kose John & Lubomir Litov, *Managerial Entrenchment and Capital Structure: New Evidence*, 7 J. EMPIRICAL LEGAL STUD. 693 (2010).
 3. See, e.g., Shijun Cheng et al., *Identifying Control Motives in Managerial Ownership: Evidence from Antitakeover Legislation*, 18 REV. FIN. STUD. 637 (2004).
 4. See, e.g., Marianne Bertrand & Sendhil Mullainathan, *Enjoying the Quiet Life?: Corporate Governance and Managerial Preferences*, 111 J. POL. ECON. 1043 (2003) [hereinafter Bertrand & Mullainathan, *Quiet Life*]; Marianne Bertrand & Sendhil Mullainathan, *Is There Discretion in Wage Setting?: A Test Using Takeover Legislation*, 30 RAND J. ECON. 535 (1999) [hereinafter Bertrand & Mullainathan, *Discretion*].
 5. See, e.g., Julian Atanassov, *Do Hostile Takeovers Stifle Innovation?: Evidence from Antitakeover Legislation and Corporate Patenting*, 68 J. FIN. 1097 (2013); John R. Becker-Blease, *Governance and Innovation*, 17 J. CORP. FIN. 947 (2011).
 6. See, e.g., Bill B. Francis et al., *Corporate Governance and Dividend Payout Policy: A Test Using Antitakeover Legislation*, 40 FIN. MGMT. 83 (2011).
 7. See, e.g., Bill B. Francis et al., *The Effect of State Antitakeover Laws on the Firm's Bondholders*, 96 J. FIN. ECON. 127 (2010); Jiaping Qiu & Fan Yu, *The Market for Corporate Control and the Cost of Debt*, 93 J. FIN. ECON. 505 (2009).
 8. See, e.g., Maya Waisman, *Product Market Competition and the Cost of Bank Loans: Evidence from State Antitakeover Laws*, 37 J. BANKING & FIN. 4721 (2013).
 9. See, e.g., Shijun Cheng & Raffi Indjejikian, *Managerial Influence and CEO Performance Incentives*, 29 INT'L REV. L. & ECON. 115 (2009); Shijun Cheng & Raffi J. Indjejikian, *The Market for Corporate Control and CEO Compensation: Complements or Substitutes?*, 26 CONTEMP. ACCT. RES. 701 (2009); Marianne Bertrand & Sendhil Mullainathan, *Corporate Governance and Executive Pay: Evidence from Takeover Legislation* (Nov. 29, 1999) (unpublished manuscript), <http://faculty.chicagobooth.edu/Marianne.bertrand/research/papers/execcomp.pdf>.
 10. See, e.g., Hayong Yun, *The Choice of Corporate Liquidity and Corporate Governance*, 22 REV. FIN. STUD. 1447 (2009).
 11. See, e.g., Yijiang Zhao & Kung H. Chen, *Earnings Quality Effect of State Antitakeover Statutes*, 28 J. ACCT. & PUB. POL'Y 92 (2009).
 12. See, e.g., Sudarshan Jayaraman & Lakshmanan Shivakumar, *Agency-Based Demand for Conservatism: Evidence from State Adoption of Antitakeover Laws*, 18 REV. ACCT. STUD. 95 (2013).

syndicate diffusion,¹³ and the amount of employee stock in pension plans.¹⁴ The popularity of these studies is not waning. Since we started working on this Article, two new draft papers on antitakeover statutes were released.¹⁵

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

Unlike lawyers, who study whether, how, and why antitakeover statutes offer protection against hostile acquisitions, financial economists start from the premise that these provisions have a material impact on the prospect of a hostile takeover of the firm. Because antitakeover statutes were adopted by different states at different times, they are thought to generate a natural experiment on the 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 takeover protection.

This Article examines the divide between the law and the finance approaches to antitakeover statutes. Our analysis is consistent with the view that antitakeover statutes do not matter after all. This has important implications for the ongoing policy controversy over takeovers. Since the 1980s, scholars have debated whether an enhanced threat of a takeover acts as a disciplining device for managers¹⁶ or induces short-termism.¹⁷ The debate

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13. See, e.g., Sreedhar T. Bharath et al., *Do Shareholder Rights Affect Syndicate Structure?: Evidence from a Natural Experiment* (Aug. 31, 2011) (unpublished manuscript), <http://ssrn.com/abstract=2020195>.
 14. See, e.g., 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* (Dec. 18, 2015) (unpublished manuscript), <http://ssrn.com/abstract=2465632>; Jonathan M. Karpoff & Michael D. Wittry, *Institutional and Political Economy Considerations in Natural Experiments: The Case of State Antitakeover Laws* (June 30, 2015) (unpublished manuscript), <http://ssrn.com/abstract=2493913> (replicating a study of antitakeover provisions and patents and analyzing whether—after controlling for a host of factors—business combination statutes are associated with different measures of firm operating performance and various features of the firms' capital structure).
 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 for reducing 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) (“[I]t 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, Comment, *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).

continues unabated. In 2015, 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 antitakeover 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.

Part I explains why antitakeover statutes are not a proper metric for the degree of takeover threat. This raises the question of why finance studies of these statutes find results. To better understand the discrepancy between the theoretical-legal predictions and the empirical findings, Part II and the Appendix examine in greater detail three finance studies. Part II and the Appendix show that the results generated in each of these studies stem from omitted variables or improper specifications. When corrected for these problems, the association between antitakeover statutes and the hypothesized effect disappears.

There are, of course, numerous finance studies of antitakeover statutes that we do not review. To show that the difficulties in the finance studies go beyond the three studies discussed in Part II and the Appendix, we therefore discuss in Parts III through V three problems that affect most of the existing studies: miscodings, failure to account for managerial share ownership, and selection bias. We show that each of these problems affects a large percentage of the observations typically used in these studies and potentially biases the studies' estimates.

The Conclusion discusses the implications of this analysis. As noted, our analysis calls into question most of the empirical findings regarding the effect of an increased threat of takeovers. Despite numerous papers on antitakeover statutes, our empirical understanding of the effects of takeover remains highly limited.

In addition, this analysis has wider implications about the relationship between law and empirical economics. The underlying problem in the studies of antitakeover statutes—that empiricists have a readily available explanatory

17. See, e.g., Martin Lipton, *Corporate Governance in the Age of Finance Corporatism*, 136 U. PA. L. REV. 1 (1987) (arguing that takeovers induce managers to focus on short-term profits at the expense of long-term planning); Andrei Shleifer & Robert W. Vishny, *Equilibrium Short Horizons of Investors and Firms*, 80 AM. ECON. REV. (PAPERS & PROC.) 148 (1990) (arguing that short horizons of arbitrageurs can lead to short horizons of corporate managers); 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 a high stock price in the short term).

18. CTR. FOR AM. PROGRESS, REPORT OF THE COMMISSION ON INCLUSIVE PROSPERITY 86 (2015), <http://www.wlrk.com/docs/IPC-PDF-full.pdf>.

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 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. To counteract these incentives, editors and referees of finance publications need to become more attentive to complexities in the legal and institutional arrangements that these papers try to study.

I. State Antitakeover Laws and Takeover Protection

To assess the impact of an antitakeover statute (ATS), one must evaluate how the statute affects the ability of a firm to defend itself in light of the other defenses already available to the firm. As we explain in this Part, even in the absence of standard ATSs, a company can use a poison pill to defend itself against a hostile bid. Because poison pills are equally or more effective than standard ATSs, and because most firms already had the legal ability to employ a pill at the time states enacted standard ATSs, 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 could not employ a poison pill, the statute did not prevent the hostile takeover.¹⁹ Whether a firm is incorporated in a state that has adopted one or another standard ATS is thus not a proper way of evaluating the takeover threat facing the firm.

A. The Preeminence of Poison Pills

From a lawyer's perspective, finance academics who focus on ATSs are barking up the wrong tree. Rather than examine ATSs, 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 plans.”²⁰ 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 v. Household International, Inc.*, each right permitted the holder to purchase \$200 worth of stock of the hostile acquirer for \$100 if a merger occurred.²¹ Flip-over pills were quickly supplemented with flip-in provisions, which grant similar rights if the raider acquires a certain

19. See *infra* notes 59-61 and accompanying text.

20. ARTHUR FLEISCHER, JR. ET AL., TAKEOVER DEFENSE: MERGERS AND ACQUISITIONS § 5.01[A] (West 2016).

21. 500 A.2d 1346, 1349, 1357 (Del. 1985).

percentage of company stock, even if no subsequent merger takes place.²² 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.²³

Poison pills are highly effective tools with which to ward off a hostile raider. As summarized by Martin Lipton: “[The poison pill] is an absolute bar to a raider acquiring control . . . without the approval of the company’s board of directors.”²⁴ 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 deter a hostile takeover. In fact, no company has ever been acquired when a flip-in pill was in place.²⁵ Flip-over pills function similarly, except that they do not stop a raider who is willing to acquire majority ownership and forgo a subsequent freezeout merger.²⁶

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—a so-called “shadow pill”—and

22. FLEISCHER ET AL., *supra* note 20, § 5.01[B][1] (“The flip-over feature of the current pill is largely an anachronistic carryover from the early form of the pill that was considered in *Household* (although potentially useful in states where the legal status of the discriminatory flip-in is still in doubt). Today, it is the flip-in which accounts for the pill’s effectiveness.” (footnote omitted)).

23. *Id.* (“The target board retains the right to eliminate the plan, by amendment or redemption, prior to an unapproved acquisition of the specified percentage of the target’s voting stock.”).

24. See Joseph A. Grundfest, *Just Vote No: A Minimalist Strategy for Dealing with Barbarians Inside the Gates*, 45 STAN. L. REV. 857, 859 n.4 (1993) (quoting Memorandum from Martin Lipton, Wachtell, Lipton, Rosen & Katz, to Clients (Jan. 15, 1993)).

25. A flip-in pill has been triggered only once, and that did not occur in the context of a hostile takeover. See Mark D. Gerstein et al., Latham & Watkins, LLP, *Lessons from the First Triggering of a Modern Poison Pill: Selectica, Inc. v. Versata Enterprises, Inc.*, 1-5 (Mar. 2009), http://www.lw.com/upload/pubContent/_pdf/pub2563_1.pdf (noting that the pill at issue was designed to protect Selectica’s net operating losses, rather than to protect it against a hostile bid, and was triggered by Versata Enterprises to obtain leverage in an unrelated business dispute).

26. This was illustrated by James Goldsmith’s takeover of Crown Zellerbach in 1985. See Jonathan P. Hicks, *Goldsmith Wins Control of Crown Zellerbach*, N.Y. TIMES (July 26, 1985), <http://nyti.ms/1OQMVlz>.

every company can do so as long as the pill is valid in its state of incorporation.²⁷

The validity of pills was an initial concern due not only 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 decisions in 1985 and 1986 by the Delaware Supreme Court established the validity of poison pills. In *Moran*, the court upheld the use of flip-over pills (which do not involve discrimination).²⁸ In *Unocal Corp. v. Mesa Petroleum Co.*, the court sanctioned a self-tender offer that involved discriminatory treatment equivalent to that caused by flip-in pills.²⁹ And in *Revlon, Inc. v. MacAndrews & Forbes Holdings, Inc.*, the court commented favorably on the board's use of a precursor to a flip-in pill—which discriminated between a raider and other shareholders—to induce a raider to increase its offer price.³⁰

Although the validity of pills in Delaware—the domicile for about half of all publicly traded companies³¹—became clear in 1985, the issue of pill validity in other states was more complex. While 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 were invalid.³² The basis for these decisions was that the discriminatory treatment of raiders caused by flip-in pills violated a statutory requirement that all shares of the same class be treated equally.³³ On the other

27. See John C. Coates IV, *Takeover Defenses in the Shadow of the Pill: A Critique of the Scientific Evidence*, 79 TEX. L. REV. 271, 286-91 (2000).

28. *Moran v. Household Int'l, Inc.*, 500 A.2d 1346, 1354, 1357 (Del. 1985).

29. See 493 A.2d 946, 957-58 (Del. 1985).

30. See 506 A.2d 173, 183 (Del. 1986); see also FLEISCHER ET AL., *supra* note 20, § 5.01[A] (“Beginning with the Delaware Supreme Court’s decisions in [*Moran*] and *Revlon*, the legal validity of standard poison pills (without deferred redemption features) became fully established for Delaware corporations.” (footnotes omitted)); *id.* § 5.06[A] (“Since [*Moran*] and *Revlon*, a board’s authority to adopt a standard pill under Delaware law has gone unchallenged.”).

31. Bertrand & Mullainathan, *Quiet Life*, *supra* note 4, at 1070.

32. See, e.g., *Topper Acquisition Corp. v. Emhart Corp.*, Civ. A. No. 89-00110-R, 1989 WL 513034, at *7-8 (E.D. Va. Mar. 23, 1989) (applying Virginia law); *W. Point-Pepperell, Inc. v. Farley Inc.*, 711 F. Supp. 1088, 1094-95 (N.D. Ga. 1988) (applying Georgia law); *Spinner Corp. v. Princeville Dev. Corp.*, Civ. No. 86-0701, 1986 BL 11, at *1 (D. Haw. Oct. 31, 1986) (applying Colorado law); *Amalgamated Sugar Co. v. NL Indus., Inc.*, 644 F. Supp. 1229, 1234 (S.D.N.Y. 1986) (applying New Jersey law); *R.D. Smith & Co. v. Preway Inc.*, 644 F. Supp. 868, 874-75 (W.D. Wis. 1986) (applying Wisconsin law); *Asarco Inc. v. Court*, 611 F. Supp. 468, 477-80 (D.N.J. 1985) (applying New Jersey law); *Bank of N.Y. Co. v. Irving Bank Corp.*, 536 N.Y.S.2d 923, 925-26 (N.Y. Sup. Ct. 1988) (applying New York law).

33. See, e.g., *Amalgamated Sugar*, 644 F. Supp. at 1234.

hand, court decisions under the laws of Indiana, Maine, Maryland, Michigan, Minnesota, Texas, and Wisconsin upheld flip-in pills. The basic rationale of the cases upholding flip-in pills is that any discrimination entailed is merely among stockholders, not among shares.³⁴ Yet while the reception of flip-in pills in the judiciary was mixed, legislatures embraced them enthusiastically. By 1990, twenty-four states (including all states where courts had invalidated flip-in pills) had adopted statutes validating discriminatory pills.³⁵ This number now stands at thirty-four.³⁶

However, even when a pill is valid in principle, a board must comply with its fiduciary duties in using the pill. 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*: that a defensive device must be reasonable in relation to the threat posed by a bid, and that, once a sale or breakup of the company is inevitable, the board must try to obtain the best price for the shareholders.³⁷ But what these standards required became clear only over time. An important question was whether a pill could be used merely as a delay tactic that afforded the board more 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 suggested the former,³⁸ but *Paramount Communications v. Time Inc.*, a 1989 decision by the Delaware Supreme Court, criticized these holdings and came out on the latter side.³⁹

34. FLEISCHER ET AL., *supra* note 20, § 5.06[B]; *see, e.g.*, *Ga.-Pac. Corp. v. Great N. Nekoosa Corp.*, 728 F. Supp. 807, 809-12 (D. Me. 1990) (applying Maine law); *Realty Acquisition Corp. v. Prop. Tr. of Am.*, Civ. No. JH-89-2503, 1989 WL 214477, at *2 (D. Md. Oct. 27, 1989) (applying Maryland law); *Amanda Acquisition Corp. v. Universal Foods Corp.*, 708 F. Supp. 984, 1008 (E.D. Wis.) (applying Wisconsin law), *aff'd on other grounds*, 877 F.2d 496 (7th Cir. 1989); *A. Copeland Enters. v. Guste*, 706 F. Supp. 1283, 1289-92 (W.D. Tex. 1989) (applying Texas law); *Harvard Indus., Inc. v. Tyson*, No. 86-CV-74639-DT, 1986 WL 36295, at *1 (E.D. Mich. Nov. 25, 1986) (applying Michigan law); *Gelco Corp. v. Coniston Partners*, 652 F. Supp. 829, 847-48 (D. Minn. 1986) (applying Minnesota law), *aff'd in part and vacated in part*, 811 F.2d 414 (8th Cir. 1987); *Dynamics Corp. of Am. v. CTS Corp.*, 637 F. Supp. 406, 407-09 (N.D. Ill.) (applying Indiana law), *aff'd*, 794 F.2d 250 (7th Cir. 1986), *rev'd on other grounds*, 481 U.S. 69 (1987).

35. Memorandum from John C. Coates & Mitchell S. Presser, Wachtell, Lipton, Rosen & Katz, to All Attorneys, *State Takeover Statutes: A Fifty State Survey* (Dec. 15, 1989) (on file with authors).

36. FLEISCHER ET AL., *supra* note 20, § 5.06[B][2].

37. *See Revlon, Inc. v. MacAndrews & Forbes Holdings, Inc.*, 506 A.2d 173, 183 (Del. 1986); *Unocal Corp. v. Mesa Petroleum Co.*, 493 A.2d 946, 955 (Del. 1985).

38. *See Grand Metro. Pub. Ltd. v. Pillsbury Co.*, 558 A.2d 1049, 1059-60 (Del. Ch. 1988) (forcing the board to redeem the pill to permit shareholders to accept an uncoercive cash tender offer that the board considered inadequate); *City Capital Assocs. v. Interco Inc.*, 551 A.2d 787, 798 (Del. Ch. 1988) (stating that once the period of negotiations and the development of alternatives has ended, the legitimate role of a pill has, in most instances, been satisfied).

39. 571 A.2d 1140, 1150, 1152-53 (Del. 1989).

States other than Delaware have either followed Delaware law or given wider discretion to boards.⁴⁰

Because a flip-in pill that remains in place is a showstopper and because boards have wide discretion to use pills under *Unocal*, most M&A practitioners have focused their attention on ways to overcome a pill. Here, the most popular technique has become conducting a proxy contest to oust the incumbent board while a hostile bid is pending but before the bidder has acquired the requisite number of shares that make a pill nonredeemable by the board.⁴¹ For companies without a staggered board, this technique involved only a modest delay and a modest increase in expenses.⁴² 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.⁴³

B. ATs in Light of Poison Pills

If a pill is valid, it is easy to see how the most commonly analyzed ATs become irrelevant.⁴⁴ 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 freezeout merger or similar transaction with the target, fair price statutes set a minimum

40. See Michal Barzuza, *The State of State Antitakeover Law*, 95 VA. L. REV. 1973, 2029, 2038-39 (2009).

41. See, e.g., Pamela Sherrod, *AT&T Declares Proxy War in Bid to Control NCR*, CHI. TRIB. (Dec. 17, 1990), http://articles.chicagotribune.com/1990-12-17/news/9004140410_1_ncr-shareholders-spokesman-dick-gray-control-ncr.

42. WACHTELL, LIPTON, ROSEN & KATZ, TAKEOVER LAW AND PRACTICE 129 (rev. ed. 2015), <http://www.wlrk.com/files/2015/TakeoverLawandPracticeGuide.pdf> (“[I]f a target’s charter does not prohibit action by written consent and the target does not have a staggered board, a bidder for a Delaware corporation generally can launch a combined tender offer/consent solicitation and take over the target’s board as soon as consents from the holders of more than 50% of the outstanding shares are obtained. Even if the target’s charter prohibits action by written consent and precludes shareholders from calling a special meeting, a target without a staggered board can essentially be taken over in under a year by launching a combined tender offer/proxy fight shortly before the deadline to run a proxy fight at the target’s annual meeting. In contrast, a target with a staggered board may be able to resist a takeover unless a bidder successfully wages a proxy fight over two consecutive annual meetings.”).

43. Lucian Arye Bebchuk et al., *The Powerful Antitakeover Force of Staggered Boards: Theory, Evidence, and Policy*, 54 STAN. L. REV. 887, 890 (2002).

44. Consistent with our assessment of the significance of poison pills, Cremers and Ferrell find that the *G-Index* interactions with the *pre-1985* dummy variable (1985 is the year *Moran* was decided) yield significant results, while coefficient estimates for interactions with the *pre-ATS* dummy are close to zero and insignificant. See Martijn Cremers & Allen Ferrell, *Thirty Years of Shareholder Rights and Firm Value*, 69 J. FIN. 1167, 1184, 1185 tbl.4 (2014).

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.⁴⁵ But if, as a result of the flip-in pill, a raider never acquires a significant stake, any statute that deals with what a raider can do *once it becomes* a major shareholder becomes moot. Similarly, flip-over pills, which make business combinations once a raider has acquired a large stake prohibitively expensive, render business combination and fair price statutes superfluous. Control share acquisition statutes, moreover, do not even purport to offer meaningful protection against hostile bids that are opposed by the board of the target but are favored (as most “hostile” bids are) by a majority of the target’s shareholders.

Moreover, the principal mechanism to overcome a pill—obtaining board control before acquiring a significant stake—would also work to neutralize these ATs. 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 authorize someone to become an “interested shareholder” and thus eliminate the constraints imposed by a business combination statute.⁴⁶

There are a few minor caveats to this conclusion. First, in many states, the validity of flip-in pills was unclear during the late 1980s. Court rulings over the validity of flip-in pills during this period were split.⁴⁷ 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.⁴⁸

45. See WILLIAM J. CARNEY, *MERGERS AND ACQUISITIONS: CASES AND MATERIALS* 463-64 (3d ed. 2011).

46. See, e.g., DEL. CODE ANN. tit. 8, § 203(a)(1) (2015). Likewise, most control share acquisition statutes permit the board to adopt a bylaw that renders the statute inapplicable. See, e.g., MASS. GEN. LAWS ch. 110D, § 2(c) (2015). Other statutes exempt control share acquisitions that are effected through a merger with the target corporation. See, e.g., N.C. GEN. STAT. § 55-9A-01(b)(3)(e) (2015) (exempting acquisitions pursuant to 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 become triggered only if someone acquires ten to twenty percent of the firm’s outstanding shares. See Julian Velasco, *The Enduring Illegitimacy of the Poison Pill*, 27 J. CORP. L. 381, 409 (2002). In a few business combination statutes, the threshold for becoming subject to the moratorium imposed by the statute is five percent of the firm’s outstanding shares. See, e.g., MASS. GEN. LAWS ch. 110F, § 3 (2015). For firms subject to these statutes, the business combination statutes constrain the maximum toehold a hostile bidder can acquire before running a proxy fight.

47. See *supra* text accompanying notes 28-36.

48. Exceptions include Francis et al., *supra* note 7, at 128; Gormley & Matsa, *supra* note 15, at 2; and Karpoff & Wittry, *supra* note 15, at 15.

Flip-over pills, however, were not subject to equivalent uncertainty. They do not involve discrimination among shareholders, have been found valid in numerous opinions,⁴⁹ and have not been struck down by any court as invalid in principle. While there may have been some initial uncertainty over the validity of flip-over pills outside Delaware, it was less intense 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 ATs were unconstitutional under the Commerce Clause.⁵⁰ It was only in April 1987, when the United States Supreme Court reversed these rulings in *CTS Corp. v. Dynamics Corp. of America*,⁵¹ that these statutes were widely viewed as valid.⁵² And even in the aftermath of *CTS*, several court decisions embraced a test for the constitutionality of ATs under which many business combination statutes would be invalid.⁵³ This would leave just a short period when ATs were

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49. In addition to the decisions upholding flip-in plans, *see supra* note 34, which explicitly or implicitly uphold flip-over plans, flip-over plans not involving any flip-in features have been upheld by *Moran v. Household International, Inc.*, 500 A.2d 1356, 1357 (Del. 1985) (applying Delaware law); multiple Delaware cases following *Moran*; *APL Corp. v. Johnson Controls, Inc.*, No. 85-C-990, 1985 U.S. Dist. LEXIS 21442, at *3-4 (E.D.N.Y. Mar. 3, 1986) (applying Wisconsin law); and *Horwitz v. Southwest Forest Indus., Inc.*, 604 F. Supp. 1130, 1136 (D. Nev. 1985) (applying Nevada law).
50. *See, e.g.*, *Fleet Aerospace Corp. v. Holderman*, 796 F.2d 135, 139 (6th Cir. 1986), *vacated and remanded sub nom. Ohio v. Fleet Aerospace Corp.*, 481 U.S. 1026 (1987); *Dynamics Corp. of Am. v. CTS Corp.*, 794 F.2d 250, 265 (7th Cir. 1986), *rev'd*, 481 U.S. 69 (1987); *APL Ltd. v. Van Dusen Air, Inc.*, 622 F. Supp. 1216, 1224-25 (D. Minn.), *vacated as moot*, No. 85-5346 (8th Cir. Nov. 26, 1985); *Icahn v. Blunt*, 612 F. Supp. 1400, 1421 (W.D. Mo. 1985), *abrogated by Dynamics Corp.*, 481 U.S. 69; *see also* Guhan Subramanian, *Delaware's Choice*, 39 DEL. J. CORP. L. 1, 20-23 (2014) (reviewing case law).
51. *See* 481 U.S. at 94.
52. *See, e.g.*, Fred Axley et al., *Control Share Statutes*, 8 N. ILL. U. L. REV. 237, 237 (1988) (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. Q. 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 . . . had the authority to regulate it in any comprehensive way.").
53. *See, e.g.*, *W. Point-Pepperell, Inc. v. Farley Inc.*, 711 F. Supp. 1096, 1102, 1105 (N.D. Ga. 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 a raider acquiring ninety percent of target stock (quoting *BNS Inc. v. Koppers Co.*, 683 F. Supp. 458, 469 (D. Del. 1988))); *BNS Inc.*, 683 F. Supp. at 469-71 (holding that the Williams Act preempts ATs that do not offer raider a "meaningful opportunity for success" and that Delaware statute satisfies that standard because it contains an exception for tender offers that result in raider acquiring eighty-five percent of target stock); *RTE Corp. v. Mark IV Indus., Inc.*, Civ. A. No. 88-C-378, 1988 WL 75453, at *3 (E.D. Wis. May 6, 1988) (finding Wisconsin statute unconstitutional). The reasoning in these cases casts substantial doubt on the constitutionality of the bulk of business combination statutes that contain no similar exceptions. *See* Michael H. Hurwitz, *New Jersey Shareholders Protection Act: Validity Questioned in Light of CTS Corp. v. Dynamics Corp. of America*, 44 BUS. LAW. 141, 156 (1988). Subsequent circuit court decisions, however, rejected the "meaningful opportunity of success" standard. *See, e.g.*,
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viewed as likely constitutional, but there was significant doubt about the validity of pills.

Second, it is theoretically possible that ATs might nevertheless matter if a court forced a board to redeem its pill. For example, when a company's failure to redeem a pill violates the *Unocal* standard, could a board instead use Delaware's business combination statute as a defense?⁵⁴ While this question has not been conclusively resolved—indeed, it was never raised in the few cases where courts forced boards to redeem a pill⁵⁵—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 ATs.⁵⁶

Moreover, standard ATs, without the pill, are not all that powerful. Business combination statutes, for example, do not block a hostile takeover; do not prevent a raider, after acquiring control, from having the target sell assets, incur debt, or make cash or in-kind distributions to its shareholders; and do not inhibit the sale of the target to a third party. Rather, they mostly restrict the raider's ability to obtain full ownership through a freezeout merger of minority shareholders and similar self-dealing transactions.⁵⁷ And even these restrictions often do not apply if the raider acquires sufficient shares in the tender offer.⁵⁸

Amanda Acquisition Corp. v. Universal Foods Corp., 877 F.2d 496, 508-09 (7th Cir. 1989).

54. 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. See *Mills Acquisition Co. v. Macmillan, Inc.*, 559 A.2d 1261, 1264-65 (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 DEL. CODE ANN. tit. 8, § 203(b)(6) (2015).
55. See, e.g., *City Capital Assocs. v. Interco Inc.*, 551 A.2d 787 (Del. Ch. 1988).
56. See Memorandum from Eric S. Robinson & Ryan A. McLeod, Wachtell, Lipton, Rosen & Katz, *Flawed Academic Challenge to Constitutionality of Delaware's Antitakeover Law* (Sept. 29, 2009), http://corpgov.law.harvard.edu/wp-content/uploads/2009/11/Critique_Challenge_to_Del_Law.PDF (“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 of a ruling invalidating the pill to Delaware's antitakeover statute. See David Marcus, *The Strange Case of Section 203*, CORP. CONTROL ALERT, Apr. 2011, at 10, 11. But see Subramanian, *supra* note 50, at 36 (arguing that fiduciary duty law would not require a board to provide approval under section 203).
57. See, e.g., DEL. CODE ANN. tit. 8, § 203(c)(3) (defining “[b]usiness combination”).
58. See, e.g., *id.* § 203(a)(2) (delineating an exception for instances where interested shareholder owns eighty-five percent of stock).

Indeed, in our research,⁵⁹ we found six hostile bids where a board could not use a pill but enjoyed the protection of a standard ATS.⁶⁰ In none of these bids did the ATS stop the hostile raider.⁶¹ Nor is there any substantial evidence that

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59. Our research consisted of a review of all opinions where a court struck down a poison pill listed in a survey produced by Wachtell Lipton in December of 1989. See Memorandum from John C. Coates & Mitchell S. Presser, *supra* note 35. For each of these opinions, we determined whether a 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.
60. Certain ATSs retain some (albeit modest) significance whether or not pills are valid. Probably the most important of these statutes is that of Massachusetts, which bestowed staggered boards on all Massachusetts publicly traded companies, including those that had not adopted them in their charters. MASS. GEN. LAWS ch. 156B, § 50A (2015). 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 Barzuza, *supra* note 40, at 1995-96 (discussing statutes). More marginally significant are disgorgement statutes, which provide that failed hostile bidders must disgorge any profits they may have reaped by selling their shares (adopted by Ohio and Pennsylvania), see OHIO REV. CODE ANN. § 1707.043 (2015); 15 PA. CONS. STAT. § 2571 (2015), or generic constituency statutes, which permit or mandate that directors take into account the effect of their decisions on other constituencies above and beyond shareholders (adopted by a large number of states), see Barzuza, *supra* note 40. None of these statutes, however, has been the focus of the empirical literature we analyze.
61. In four bids, the hostile bidder acquired the target despite the statute. The targets in these bids were West Point-Pepperell, Irving Bank, Moore McCormack, and Pillsbury. See Nina Andrews, *Southdown Will Buy Moore McCormack*, N.Y. TIMES (Apr. 7, 1988), <http://nyti.ms/1OR3bD8> (announcing that Moore McCormack was purchased by Southdown); *Irving Bank Deal Completed*, N.Y. TIMES (Jan. 2, 1989), <http://nyti.ms/1OR362g> (reporting that Irving Bank was merged into the Bank of New York); Pamela Sherrod & Charles Storch, *Cutting Pepperell's Family Ties: Farley Snatches Textile Maker from Its Defiant Roots*, CHI. TRIB. (Feb. 26, 1989), http://articles.chicagotribune.com/1989-02-26/business/8903080456_1_west-point-pepperell-william-f-farley-lanier (reporting that West Point-Pepperell was acquired by the Chicago industrialist William F. Farley); *Pillsbury Agrees to Takeover*, DESERET NEWS (Dec. 19, 1988, 12:00 AM MST), <http://www.deseretnews.com/article/27374/pillsbury> (reporting that Pillsbury agreed to be acquired by Grand Metropolitan). (The bidder for Pillsbury appeared to have satisfied the eighty-five percent tender exception to the statute. See *Grand Metro. PLC v. Pillsbury Co.*, Civ. A. No. 10319, 1988 WL 130637, at *1 (Del. Ch. Nov. 22, 1988).) In a fifth bid, the target (Emhart) was sold to a third party that offered a higher price than the hostile bidder. *Emhart, B&D to Merge*, TULSA WORLD (Mar. 20, 1989, 12:00 AM), http://www.tulsaworld.com/news/emhart-b-d-to-merge/article_80910e9f-1f7d-59df-bbb2-6762a482c627.html (reporting that Emhart was acquired by Black & Decker, which offered a higher price than Topper). The sixth bid, for Interco, was withdrawn while the target's appeal of the ruling requiring the redemption of the pill was still pending. See *Appeal Moot in Interco Case*, N.Y. TIMES (Nov. 19, 1988), <http://nyti.ms/23vmpWH> (reporting that the Rales brothers had withdrawn their offer while an appeal was pending); *Rales Extend Tender Offer, Threaten to Withdraw It After Deadline*, ASSOCIATED PRESS (Nov. 13, 1988, 2:57 PM ET), <http://www.apnewsarchive.com/1988/Rales-Extend-Tender-Offer-Threaten-To-Withdraw-It-After-Deadline/id-e89d93a04146d3ea54afac57f40ffc7> (reporting that the Rales brothers threatened to withdraw their bid unless Interco provided confidential information or entered into

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standard ATs deterred hostile bids in the post-1985 era, after the Delaware Supreme Court decided *Moran* and *Unocal*,⁶² or that the adoption of standard ATs after the validity of poison pills was established affected the stock price of firms that became subject to such statutes.⁶³

negotiations and that the only barrier to the completion of the buyout was Interco's poison pill).

62. 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, 37 (1995). Finance studies of ATs sometimes cite to two studies for contrary results. See, e.g., Cheng et al., *supra* note 3, at 641 n.4; Francis et al., *supra* note 7, at 130; Qiu & Yu, *supra* note 7, at 514 n.14. One study is a 1988 note by Hackl and Testani. Jo Watson Hackl & Rosa Anna Testani, Note, *Second Generation State Takeover Statutes and Shareholder Wealth: An Empirical Study*, 97 YALE L.J. 1193 (1988). But Hackl and Testani's note examines the 1981 to 1986 period, which predates the advent of poison pills. See *id.* at 1212 (stating that authors examined offers made between June 1, 1981 and December 31, 1986). The second study is an article by G. William Schwert from 2000, which 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." G. William Schwert, *Hostility in Takeovers: In the Eyes of the Beholder?*, 55 J. FIN. 2599, 2609 n.1 (2000). A recent draft paper by Cain, McKeon, and Davidoff also examines the impact of multiple types of ATs, as well as major court decisions on takeover activity. Matthew Cain et al., *Do Takeover Laws Matter?: Evidence from Five Decades of Hostile Takeovers* (Oct. 2014) (unpublished manuscript), <https://www.sec.gov/dera/staff-papers/working-papers/dera-wp-takeover-laws.pdf>. Cain et al. report that fair price and control share acquisition states had no significant effect on hostile acquisitions; that the effect of business combination states was negative, but nonrobust; and that poison pills facilitated hostile takeovers. See *id.* at 43 tbl.8. Since Cain et al.'s regressions do not control for secular changes in M&A activity over time (for example, by including year dummy variables), 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 that began in mid-1990 and increased financing costs. See Comment & Schwert, *supra*, at 8 (suggesting that the drop in takeover activity at the end of the 1980s was driven by the recession and the collapse of the junk bond market).

63. Several event studies examine the effect of antitakeover statutes on stock prices. Two event studies examining the adoption of the Delaware business combination statute, the only single standard statute studies involving a state where the validity of pills had been established, find no statistically significant effect on stock prices of Delaware firms. See John S. Jahera, Jr. & William Pugh, *State Takeover Legislation: The Case of Delaware*, 7 J.L. ECON. & ORG. 410, 411 (1991) (finding no consistent evidence that takeover statute affected shareholder wealth); Jonathan M. Karpoff & Paul H. Malatesta, *The Wealth Effects of Second-Generation State Takeover Legislation*, 25 J. FIN. ECON. 291, 315 (1989) (finding no significant effects for adoption of Delaware statute).

Several studies of the 1990 Pennsylvania ATS report statistically significant declines in stock prices. See ROBERTA ROMANO, *THE GENIUS OF AMERICAN CORPORATE LAW 68-70* (Christopher C. DeMuth & Jonathan R. Macey eds., 1993), https://www.aei.org/wp-content/uploads/2014/07/-the-genius-of-american-corporate-law_162946985222.pdf (presenting summary of event studies). While the 1990 Pennsylvania statute postdates

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Thus, as a practical matter, standard ATs 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 SEC were to adopt a rule constraining the use of poison pills.⁶⁴ From the perspective of corporate lawyers, even such a marginal impact may be worth the effort to get such a statute adopted, especially if doing so also has a reputational payoff. If flip-over pills and business combination statutes were perfect substitutes and raised the likelihood of a successful defense by, say, fifteen percent, and if there were a ten percent 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 ATs

Financial economists employ varying methods for categorizing takeover protection offered at the state level. The most common methods involve coding when a state adopted a business combination statute,⁶⁵ when a state adopted the first of a set of statutes (usually business combination, control share, and fair price),⁶⁶ or how many different types of statutes a state has adopted (with business combination, fair price, control share acquisition,

Pennsylvania's adoption of a pill validation statute, the 1990 statute was nonstandard: it contained unusual provisions on the disgorgement of profits obtained by a raider and on the fiduciary duty standard applicable in the takeover context. 15 PA. CONS. STAT. § 1715(d); see Act of Dec. 21, 1988, No. 177, sec. 103, § 2513, 1988 Pa. Laws 1444, 1611 (codified as amended at 15 PA. CONS. STAT. § 2513 (2015)); Act of Apr. 27, 1990, No. 36, sec. 6, §§ 2561-67, 1990 Pa. Laws 129, 138-48 (codified as amended at 15 PA. CONS. STAT. §§ 2561-67). These provisions, unlike the 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 adopted at a time when the validity of pills had not yet been established, but also statutes for which the legislative event dates precede the Delaware Supreme Court decision in *Moran* in November 1985. See, e.g., Karpoff & Malatesta, *supra*, at 292 (indicating that their event studies focus on the effect of antitakeover statutes enacted between 1982 and 1987).

64. Though the SEC 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 Docket 230 (July 31, 1986).

65. See, e.g., Bertrand & Mullainathan, *Discretion*, *supra* note 4, at 544; Giroud & Mueller, *supra* note 1, at 316.

66. See, e.g., Garvey & Hanka, *supra* note 2, at 524.

constituency, and pill validation statutes being the types commonly considered).⁶⁷

From a lawyer's perspective, these categorizations are nonsensical. They result in a gross mischaracterization of the law in Delaware—a state that typically accounts for about half of the firm observations in the studies. The standard finance methodologies characterize Delaware as either having changed from a pro- to an antitakeover state when it adopted its 1988 business combination statute or as being largely protakeover because it has only a single statute. These characterizations ignore the centrality of the 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 Delaware Chancery Court that imposed severe constraints on the use of poison pills.⁶⁸ These decisions caused Martin 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.⁶⁹ The fact that Delaware had passed its antitakeover 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 ATs affect the target's *marginal* ability to defend itself.⁷⁰ Thus, the studies usually do not take account of the fact that targets in states where pills are valid have a great ability to defend themselves against takeovers

67. See, e.g., Francis et al., *supra* note 7, at 133. A notable exception is a recent draft 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, at 21.

68. Grand Metro. Pub. Co. v. Pillsbury Co., 558 A.2d 1049, 1060 (Del. Ch. 1988); City Capital Assocs. v. Interco, Inc., 551 A.2d 787, 799-800 (Del. Ch. 1988). In neither of the two takeover battles did Delaware's ATS block the bid after the target board was forced to redeem the pill. See *supra* note 61.

69. See Jeffrey N. Gordon, *Corporations, Markets, and Courts*, 91 COLUM. L. REV. 1931, 1959 n.95 (1991) (quoting Memorandum from Wachtell, Lipton, Rosen & Katz to Clients, The *Interco* Case (Nov. 3, 1988) (on file with the *Columbia Law Review*)).

70. 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.

even if the state has not adopted any ATS.⁷¹ 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.⁷² Finally, many studies ignore the high degree of uncertainty over the validity of ATSs prior to 1987,⁷³ and all fail to account for the decline in uncertainty over the validity of both flip-over and flip-in pills in states without pill validation statutes.⁷⁴

Studies that merely add up the total number of statutes adopted are even more problematic. Four of the five types of statutes cover overlapping territory. As explained, pill validation statutes make business combination, fair price, and control share acquisition statutes moot; similarly, business combination statutes render the other two types largely irrelevant, and fair price and control share acquisition statutes overlap in that both mostly restrain front-end loaded, coercive bids.⁷⁵

One state that deserves particular mention is California. California is often singled out as the only major state that has not adopted any ATS. California stands out, though not necessarily for that reason. It expressly prohibits discrimination among *shareholders* in the absence of explicit shareholder authorization⁷⁶ (a provision which casts unique doubt on the validity of flip-in poison pills);⁷⁷ it prohibited staggered boards for all firms until 1989⁷⁸ and continues to prohibit them for firms that are not “listed”;⁷⁹ it prohibits a “for cause” standard for director removal, even for companies with a staggered

71. These situations are by no means unusual. Thirty-one states adopted a business combination statute at some point before 1995. Three 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 did not adopt a business combination statute before 1995. Five states had case law upholding pills that preceded the state’s business combination statute. See FLEISCHER ET AL., *supra* note 20, at § 5.06[B][1]; Karpoff & Wittry, *supra* note 15, at 39-40; *supra* notes 34, 49.

72. See, e.g., Cheng et al., *supra* note 3; Garvey & Hanka, *supra* note 2.

73. See, e.g., Atanassov, *supra* note 5.

74. See, e.g., Bertrand & Mullainathan, *Quiet Life*, *supra* note 4; Qiu & Yu, *supra* note 7.

75. See Marcel Kahan, *The Demand for Corporate Law: Statutory Flexibility, Judicial Quality, or Takeover Protection?*, 22 J.L. ECON. & ORG. 340, 345-46 (2006).

76. CAL. CORP. CODE § 203 (West 2015).

77. See Guhan Subramanian, *Bargaining in the Shadow of Takeover Defenses*, 113 YALE L.J. 621, 628-29 (2003).

78. Sanjai Bhagat & Roberta Romano, *Empirical Studies of Corporate Law*, in 2 HANDBOOK OF LAW AND ECONOMICS 945, 974 n.15 (A. Mitchell Polinsky & Steven Shavell eds., 2007); see Act of Sept. 26, 1989, ch. 876, secs. 1-2, §§ 301-301.5, 1989 Cal. Stat. 2872, 2872-74 (codified as amended at CAL. CORP. CODE §§ 301-301.5 (West 2015)).

79. CAL. CORP. CODE § 301.5. Listed firms include only those with outstanding shares listed on the New York Stock Exchange, the NYSE Amex, the NASDAQ Global Market, or the NASDAQ Capital Market. *Id.* § 301.5(d).

board,⁸⁰ and it permits holders of ten percent of a company's shares to call a special meeting (a right that cannot be narrowed in the company's charter).⁸¹ 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 ATs, 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 ATs.

These problems make it very difficult, if not entirely impossible, to statistically separate the effect of takeover law from contemporaneous economic changes.⁸² Finance studies can differentiate between ATs and contemporaneous economic changes because states adopted these statutes at different times (and some states never adopted such statutes). But if poison pills make ATs 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 the 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, reduced their leverage in 1986, one cannot tell whether they did so because the 1985 *Moran* decision boosted their ability to resist a takeover or because of some other economic change that occurred in 1985. ATs would, therefore, only be relevant to the extent that they go beyond pills or are enacted in a state where a pill is not valid. While some statutes fit this bill, they tend to affect only a small number of firms, relate only a few years of observation per firm, and entail only small changes in the ability to resist a bid.⁸³

80. *Id.* § 303. Removal of directors of companies with staggered boards, however, is subject to a higher voting requirement. *Id.*

81. *Id.* § 600(d).

82. These problems cannot be adequately addressed by merely adding to the regressions the dummy variable *poison pill law* that controls for pill validation statutes, and, for Delaware firms, the *Moran* decision. *But see* Karpoff & Wittry, *supra* note 15, at 39-40 (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 pills 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, by adding separate controls for poison pill laws and business combination statutes but no control for the interaction of these statutes, the estimated specification 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.

83. Examples of such statutes are those 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 antitakeover defenses than the standard ones used in Delaware, or the Massachusetts statute that legislatively imposed staggered boards on all Massachusetts public companies. Several event studies analyze the effect of some of these statutes on stock prices. *See, e.g.,* Karpoff & Malatesta, *supra* note 63, at 299, 310-11 (analyzing pill validation statutes); Robert Daines, *Do Classified Boards Affect Firm*

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D. ATs 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 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, or assume that all firms incorporated in a state become subject to the statute even though some statutes apply only to a subset of those firms (such as those 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 almost never change their state of incorporation and thus ascribe the wrong domicile to some firms.⁸⁴ Third, as discussed previously, 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 1985.⁸⁵ We then examined the years in which these firms would have been treated as having become subject to takeover protection by Marianne Bertrand and Sendhil Mullainathan in their article *Enjoying the Quiet Life*.⁸⁶ We chose this article because it contains among

Value?: Takeover Defenses After the Poison Pill 2-3, 11-16 (unpublished manuscript) (on file with authors) (examining Massachusetts law). We are aware, however, 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 3, 44 (reporting mixed results on the effect of first-generation antitakeover laws on the number and citation of patents and that the statistical association between the adoption of business combination statutes and both several measures of operating performance and multiple features of capital structure is not robust to the inclusion of controls for a series of legal factors).

84. See, e.g., Bertrand & Mullainathan, *Quiet Life*, *supra* note 4, at 1053. For one of the few exceptions, see Cheng et al., *supra* note 3, at 645.

85. We constructed our comparison sample as follows: we started with the whole set of firms that appear in the CRSP-Compustat database in any year between 1976 and 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 A. Gompers et al., *Extreme Governance: An Analysis of Dual-Class Firms in the United States*, 23 REV. FIN. STUD. 1051, 1052 (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 were 49), firms that went public after 1985, see *infra* Part V.C, and firms that did not appear in the database in each of the years from 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 ATs, most of which were adopted during that period. Finally, we excluded firms that were not incorporated in one of the fifty states or the District of Columbia or that were limited partnerships in 1985.

86. Bertrand & Mullainathan, *Quiet Life*, *supra* note 4.

the fewest errors⁸⁷ and because its general methodology (the article focuses exclusively on business combination statutes) and coding have been followed by several other papers.⁸⁸

In our comparison sample, the coding methodology used by Bertrand and Mullainathan resulted in attributing an incorrect year of becoming subject to ATSs to 10.5% of firms.⁸⁹ Further accounting for the effects of poison pills—and assuming, conservatively, that pills (whether flip-in or flip-over) are only valid if endorsed by statute or a state supreme court, that flip-in pills do not offer any stronger protection than business combination statutes, and that there is 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 ATSs employ a differences-in-differences methodology. This methodology 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 ATS in the first year but was subject to the statute in the second year. “Control” firms are those that were either subject to the ATS in both years or not subject to the ATS in either year. If one corrects Bertrand and Mullainathan’s categorizations for the mistakes we have identified and for the effect of poison pills, sixty-three percent of the comparisons under Bertrand and Mullainathan’s coding would be incorrect: In almost two-thirds of the pairings that, using 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 treated by Bertrand and Mullainathan’s coding algorithm was in fact a control firm, while the supposed control firm was in fact treated.

Because the finance literature fails to grasp the actual effects of ATSs on a target firm’s ability to defend itself—and makes some additional mistakes—the relationship between the measures of takeover protection used by finance scholars studying ATSs and the actual level of takeover protection provided by

87. For examples of papers with more severe errors, see note 153 below.

88. See, e.g., Atanassov, *supra* note 5, at 1099-1102 (following the methodology and using the coding of Bertrand and Mullainathan); Giroud & Mueller, *supra* note 1, at 314-15 (same); Jayaraman & Shivakumar, *supra* 12, at 105 (same); Qiu & Yu, *supra* note 7, at 509 (same); Zhao & Chen, *supra* note 12, at 96, 101 (same); Sauvagnat, *supra* note 1, at 14-15 (same).

89. The sources of error include Bertrand and Mullainathan ascribing the wrong year to the Connecticut, Kentucky, and Pennsylvania statutes, omitting the 1991 Oregon business combination statute, not taking account of the fact that 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 not controlling for changes in law in states of incorporation between 1985 and 1995. In addition, some firms were partnerships during this period and thus not subject to ATSs. 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.

state law for a generic firm is highly attenuated and noisy. The relationship between the measures of takeover protection used by finance scholars and the actual susceptibility to a takeover given state law, firm-specific defenses, and the overall economic 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 ATSs and firm behavior? In the following Parts, we will try to shed light on this question.

II. Scrutinizing Studies on ATSs

This Part reviews and deconstructs an ATS study. The Appendix provides an analogous review of two other studies. These studies were selected because they were published in leading finance journals and because we were able to obtain most of the variables used by the authors in their analyses. As this Part and the Appendix will show, in each of the studies an omitted variable or an improper specification accounts for the statistical association between the ATSs 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 ATSs 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, IV, and V below, this Article supplements our specific critique of the three studies with a more general critique identifying flaws present in all of the finance studies of this genre.

This Part reviews *Identifying Control Motives in Managerial Ownership: Evidence from Antitakeover Legislation* by Shijun Cheng, Venky Nagar, and Madhav V. Rajan.⁹⁰ The Appendix contains our reviews of *Capital Structure and Corporate Control: The Effect of Antitakeover Statutes on Firm Leverage*⁹¹ and *The Market for Corporate Control and the Cost of Debt*.⁹²

Identifying Control Motives in Managerial Ownership examines the relationship between three types of ATSs—fair price, control share, and business combination statutes—and managerial stock ownership.⁹³ 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.”⁹⁴ Their main hypothesis is that the passage of these laws is associated with a decline in

90. Cheng et al., *supra* note 3.

91. Garvey & Hanka, *supra* note 2.

92. Qiu & Yu, *supra* note 7.

93. Cheng et al., *supra* note 3, at 640-41.

94. *Id.* at 641.

managerial stock ownership.⁹⁵ 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 ATS and the fraction of the firm's shares owned by the firm's managers and directors (which we represent with the variable *D&O ownership*).⁹⁶ These results, the authors claim, demonstrate the significant role that control considerations play in managers' stockholding decisions.⁹⁷

In their main tests, Cheng et al. focus on the firms incorporated in states that eventually adopted an ATS (which we will refer to as "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.⁹⁸

Cheng et al. convey the result of this analysis in their table 9. However, table 9 does not report the actual difference in percentage ownership but rather the difference in a logarithmic transformation of the ownership percentage.⁹⁹ We will refer to this variable as *transformed change in ownership*.¹⁰⁰ Cheng et al. report that the mean value of *transformed change in ownership* is -0.157 and that this mean is significantly different from zero at the 1% level.¹⁰¹ In subsequent multivariate analyses with additional controls, they obtain similar results.¹⁰²

95. *Id.*

96. *Id.* at 649-64.

97. *Id.* at 639.

98. 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 changed due to entries and exits). *Id.* at 659-62.

99. *Id.* at 661 tbl.9.

100. This variable is constructed as follows: For each firm that eventually became subject to an ATS, they calculate the average *D&O ownership* over the years during which the firm had still not become subject to an ATS and the average *D&O ownership* over the years during which the firm was 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 ownership* between the "pretreatment" years and the "posttreatment" years (call this measure "*average % change*"). They then construct the variable they use in their analysis as the sign of *average % change* times $\ln(1 + \text{absolute value of average \% change})$. See *id.* at 660.

101. *Id.* at 660-61, 661 tbl.9.

102. *Id.* at 660-65. Cheng et al. attempt to bolster the results of their firm-level analyses by adopting an alternative empirical strategy, which they describe as "panel regressions." Using a sample that follows the panel of all 587 firms in their database over the 1984-1991 period, they regress a measure of *D&O ownership* against the *AfterLaw* dummy (which equals one for a given firm in a given year if the firm's state of incorporation had adopted its first ATS by the end of the previous year and zero otherwise), industry and year fixed effects, and firm-level controls. Cheng et al. find that the estimate of the coefficient for the *AfterLaw* dummy is negative and statistically significant. *Id.* at 650-51. However, the estimated specification does not control for either state or firm fixed

footnote continued on next page

To analyze the robustness of Cheng et al.'s results, we obtained ownership data from the same database of director and managerial ownership used by Cheng et al.¹⁰³ We were able to match 710 firms with a state of incorporation.¹⁰⁴

Table 1 below shows our replication of the analysis performed by Cheng et al.¹⁰⁵ We start by employing the same methodology as Cheng et al. As shown in the second row of Table 1, this 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 postadoption years minus the average of the ownership percentages in the preadoption years. The mean of that variable (*actual % change in ownership (untransformed)*) is -0.012 percentage points. This

effects. As a result, Cheng et al.'s finding may be driven by the fact that average *D&O ownership* in firms incorporated in ATS states was always lower than the corresponding average for firms incorporated in states that never adopted an ATS. To explore this possibility, we used the data described in note 103 below to replicate Cheng et al.'s basic specification in their panel regressions (and also found a negative and statistically significant estimate for the coefficient of *AfterLaw*). However, once we included controls for state fixed effects, the estimate of the coefficient of *AfterLaw* switched signs, becoming statistically insignificant. To obtain further evidence for our conjecture, for each year in the 1984-1991 period, we compared the average *D&O ownership* in firms incorporated in ATS states with the corresponding average in firms incorporated in states that never adopted an ATS. Consistent with our regression results, we found that, in each year, the average *D&O ownership* among the former set of firms was lower than among the latter set of firms.

103. 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 different volumes published by the Investor Responsibility Research Center during the late 1980s and 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 for 764 of the 792 firms. After discarding 54 firms that reincorporated during the sample period or were not incorporated in one of the states or the District of Columbia, we finished with a sample of 710 firms.
104. 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 officers/directors and by CEOs, and in other descriptive statistics.
105. Our sample for the firm-level tests, like Cheng et al.'s, is smaller than the number of unique firms in the sample for the panel regressions since it includes only firms incorporated in states that adopted a statute during the period of analysis.

means that, on average, ownership declined by about 1/100 of 1 percentage point, a drop that is economically trivial and statistically insignificant.¹⁰⁶

Table 1
Change in Ownership After ATS Adoption

	N	Mean	Mean (<i>p</i> -value)	Percentile								
				10th	20th	30th	40th	50th	60th	70th	80th	90th
Cheng et al. Table 9 Transformed Change in Ownership	467	-0.157	0.006	-1.74	-1.09	-0.626	-0.281	-0.033	0.072	0.288	0.629	1.316
Our Replication: Same Methodology and Variable	610	-0.095	0.044	-1.57	-0.977	-0.531	-0.203	-0.017	0.071	0.240	0.580	1.295
Actual Change in % Ownership (Untransformed)	610	-0.012	0.964	-3.82	-1.66	-0.700	-0.225	-0.017	0.073	0.271	0.787	2.650

The analysis in Table 1, of course, does not control for alternative reasons why *D&O ownership* in a firm may have changed. In particular, it does not control for secular changes in ownership over time. In theory, once additional controls are added, the relationship between *D&O ownership* and being subject to an ATS could become significant. We therefore ran a series of regressions including controls for firm and year fixed effects. This methodology, like the one employed by Cheng et al., is designed to tease out the factors that are related to changes in *D&O ownership* for a particular firm. Compared to the

106. In unreported results, we performed a similar analysis as that of the second row of Table 1, but using $\ln(1 + D\&O \text{ stockholdings})$ —instead of *D&O stockholdings*—to construct the measures of average pretreatment and average posttreatment *D&O ownership*. This transformed measure of ownership is the same that Cheng et al. used as a dependent variable in their panel regressions. The results we obtained were qualitatively similar to those of the third row of Table 1.

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 pretreatment *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 pretreatment baseline for each firm. In that case, the mean of the *transformed change in ownership* became much smaller in magnitude (-0.008 instead of -0.095, as in Table 1) and statistically insignificant.

method used by Cheng et al., however, year fixed effects are a more effective and conventional way to control for ownership changes over time that are unrelated to ATSs.¹⁰⁷

Table 2 summarizes the results. In Columns (1) and (2), the dependent variable is the fraction of shares owned by directors and officers; in Columns (3) and (4), it is a transformed ownership variable Cheng et al. use in some of their other regressions. Columns (2) and (4) include, in addition to firm and year fixed effects, firm-level controls like the ones included in Cheng et al.'s panel regressions.¹⁰⁸ The estimate of interest is that of the coefficient of the *AfterLaw* dummy.¹⁰⁹ Notably, in each specification, the coefficient is statistically indistinguishable from zero, providing no evidence that the statutes are associated with a change in ownership. (For example, the point

107. Cheng et al. use a different methodology in the regressions they report in their tables 10 and 11. In those regressions, the dependent variable is the one described in note 100 above, 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. See Cheng et al., *supra* note 3, at 661-62. To control for secular time trends in ownership, they add as a control the variable *ownership trend* that proxies for the average change in *D&O ownership* experienced by the firms incorporated in the states that never adopted an ATS (the "control states") during the relevant period. (The relevant period depends on the state of incorporation of the firm in the observation of interest. For example, if the observation corresponds to a Delaware firm, Cheng et al.'s *ownership trend* variable is a measure of the change in average *D&O ownership* for firms in the control states between 1989-1991 and 1986-1988. *Id.* at 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 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 thirty-five firms did not become subject to any such statute during the sample period (and data for all these thirty-five firms may not even be available for their regressions). See *id.* at 646 tbl.1. 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 trend* 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. One can try to control for secular trends in ownership 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).

108. The estimates of the coefficients for those controls are unreported to preserve space.

109. Since the *AfterLaw* dummy is constructed as we described in note 102 above, estimating its coefficient in a linear specification that controls for year and firm fixed effects yields a difference-in-differences estimate of the impact of the adoption of the first ATS on *D&O ownership*.

estimate of 0.112 for the *AfterLaw* coefficient in Column (1) indicates that, after a firm becomes subject to an ATS, *D&O ownership* tends to increase by approximately 0.1 percentage points, an increase that is statistically insignificant.)

Table 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 Fixed Effects	Y	Y	Y	Y
Year Fixed Effects	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 their table 4.

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 ATS. 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 ATSs and, respectively, leverage and bond yields disintegrates. The findings in these empirical papers, therefore, do not present persuasive evidence that ATSs matter, and the papers should not be taken to show how managers and firms responded to a change in the threat of a takeover. Moreover, the fact that the three papers from top journals that we reviewed all arrived at results that did not withstand closer scrutiny suggests that some skepticism may be warranted in dealing with empirical studies, especially if their results make little theoretical sense.

III. Categorization Problems

Part II above and the Appendix provide examples of three finance studies wherein the supposed link between ATSs and managerial ownership, leverage, and bond prices, respectively, can be explained by the failure to control for key variables or by mere misspecifications. There are, of course, many other studies of ATSs that we do not review in such a detailed way. Parts III through V discuss three problems that, to our knowledge, affect all studies of ATSs. These problems, together with the knowledge that ATSs do not materially increase a

target's ability to defend itself (as explained in Part I), make us very doubtful that the results derived in these studies are causally attributable to ATSs.

As mentioned, 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 particular statute, to errors regarding the state in which a firm was incorporated, and to errors regarding which firms a state's ATS actually covered. These errors, together with a very conservative assessment of the effects of poison pills, tend to generate massive mismeasurement of whether and when firms became subject to takeover protection.¹¹⁰

The errors we identify cannot be dismissed as noise that merely results in less accurate regression estimates. Rather, they have systematic (nonrandom) effects on the categorization of firms that render any result unreliable. This Part discusses some of these systematic effects.¹¹¹ Parts IV and V will address biases generated by the failure of studies to take account of managerial ownership and by the endogeneity of the state of incorporation, respectively.

The various categorization errors identified have systematic effects because the firms affected by these errors are not randomly selected. Assigning the wrong year to when an ATS 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 of incorporation and which firms were covered by a state's ATS affect 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, and 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 have their principal place of business in that same state.¹¹² 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:

- *Firms erroneously categorized as never being treated are systematically smaller:* Under Bertrand and Mullainathan's categorization, 331 of the 2391 firms in our comparison sample were incorporated in

110. See *supra* Part I.D.

111. 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., DEL. CODE ANN. tit. 8, § 102(b)(7) (2015).

112. See Robert Daines, *The Incorporation Choices of IPO Firms*, 77 N.Y.U. L. REV. 1559, 1574 (2002).

states that did not adopt an ATS.¹¹³ However, almost half of these firms (152) were in fact incorporated in states that had adopted either a business combination or a pill validation statute. These miscoded 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).¹¹⁴

- *Firms erroneously categorized as being treated between 1989 and 1990 are concentrated in Ohio:* 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 1990.¹¹⁵ Since Ohio had adopted a pill validation statute in 1986, we regard these firms as having become subject to takeover protection four years earlier.¹¹⁶ 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.
- *Firms erroneously categorized as being treated between 1985 and 1986 are systematically larger and have lower managerial ownership:* 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).¹¹⁷ 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 *and* headquartered in New York (median assets \$33 million; median D&O ownership 18.9%).¹¹⁸

Because economic shocks may have differential effects on, for example, large firms, firms located in a specific area, or firms with low board and

113. Bertrand & Mullainathan, *Quiet Life*, *supra* note 4, at 1048 tbl.1.

114. In a Mann-Whitney test, the difference in median values between those 152 firms and the other firms in our comparison sample is significant at the 1% level.

115. Bertrand & Mullainathan, *Quiet Life*, *supra* note 4, at 1048 tbl.1.

116. An Act of November 22, 1986, sec. 8, § 1701.16(B)(1)(f), 1986 Ohio Laws 6107, 6152-53 (codified as amended at OHIO REV. CODE ANN. § 1701.16 (B)(1)(f) (West 2015)).

117. Bertrand & Mullainathan, *Quiet Life*, *supra* note 4, at 1048 tbl.1.

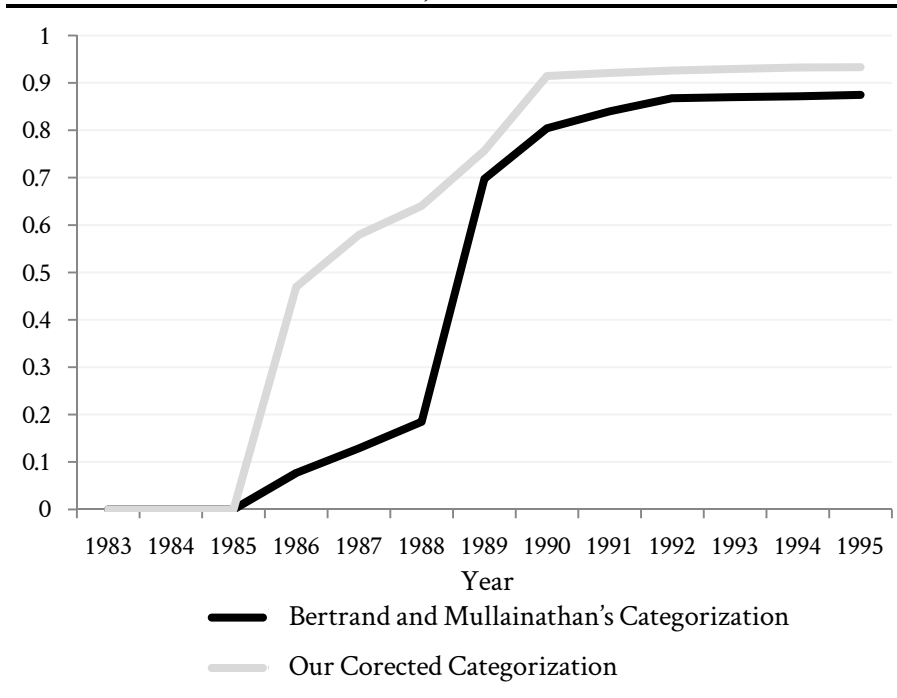
118. The managerial ownership data correspond to the year 1989. *See infra* note 123 and accompanying text. In a Mann-Whitney test, the difference in the median value of assets between the New York-incorporated firms headquartered in New York and the New York-incorporated firms headquartered elsewhere is significant at the 5% level. The Mann-Whitney test comparing the median *D&O ownership* between these two groups yields a *p*-value of 0.11. (This is unsurprising, since the ownership data are available only for 39 out of the 52 firms headquartered out of New York.)

managerial ownership, these systematic errors affect the regression estimates more severely than measurement errors that are simply random. For example, if Ohio firms suffered an economic shock in around 1990, the impact of the shock would be reflected in the estimate for the *ATS* variable.¹¹⁹

Moreover, the categorization errors result in firms being systematically regarded as becoming subject to takeover protection at a later date than was actually the case. Figure 1 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 became subject to takeover protection during 1990 or 1991 and only 7.5% of firms did not become subject to takeover protection at all during the standard study period (1976 to 1995). Under Bertrand and Mullainathan's methodology, the respective percentages are 5.2% and 13.9%. Importantly, under our categorization, the timeframe over which the bulk of firms became subject to takeover protection is much more compressed: within five years, over 90% of firms were 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.

119. Although some of these potential sources of bias could be mitigated by adding multiple layers of highly granular fixed effects (e.g., location-by-year fixed effects, industry-by-year fixed effects, size-quintile-by-year fixed effects, and insider-ownership-quintile-by-year fixed effects), we know of no published paper that comes close to doing so.

Figure 1
Fraction of Firms Subject to Takeover Protection



IV. Failure to Take into Account 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 takeover protection. First, large inside ownership provides a defense against a hostile takeover. If management, say, owns thirty percent of the target stock, it becomes hard for a hostile raider to acquire a majority, especially since management can raise its stake further once a hostile bid is announced. 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)¹²⁰ or that an increase in the takeover threat induces management to pursue short-termism at the expense of long-term value (as argued by others),¹²¹ large inside ownership should produce significant counterincentives. Reasonable minds may differ as

120. See, e.g., Bertrand & Mullainathan, *Quiet Life*, *supra* note 4, at 1072.

121. See *infra* note 137

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. 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 antitakeover devices is not material. And once the board and upper management own at least 20% of the company's stock, we would regard the incentives provided by that ownership as so significant that the additional incentives generated by the presence or absence of other antitakeover devices are not material.¹²²

We were able to recover information on inside ownership for 1807 firms in our comparison sample. In 30% of these firms, directors and executive officers owned at least 30% of the firm's shares. In 44% of the firms for which we could recover ownership data, inside ownership exceeded 20% of the firm's shares.¹²³

The effect of director and officer ownership does not simply attenuate the impact of ATSs 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 *D&O ownership* was 28.2%, and the fraction of firms in which directors and officers held more than 30% of the shares was 47.6%. By comparison, among firms incorporated in states that eventually adopted a business combination statute, the median *D&O ownership* was only 15.2%, and the fraction of firms whose directors and officers held more than 30% of the shares was 27.5%.¹²⁴ 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 firms that never became subject to a business combination statute was \$190.6 million (\$20.7 million),¹²⁵ while the respective values for firms that

122. To be sure, even for a firm with large board ownership, antitakeover 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 above. However, Cheng et al. do not find an economically significant effect of ATSs on board ownership, *see* Cheng et al., *supra* note 3, at 661-62, and in our replication, the effect is neither economically nor statistically significant. Moreover, if an ATS were to induce a board to reduce its ownership stake, this would have substantial bearing on the interpretation of the results in finance studies.

123. 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 the 1984-1991 period that *D&O ownership* is very stable within firms over the sample period.

124. In a Mann-Whitney test, the difference in the median *D&O ownership* between the two groups was significant at the 1% level.

eventually became subject to a business combination statute were \$903 million (\$65 million).¹²⁶

These differences (and possibly 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 1990-1991 recession.¹²⁷ Should one presume that firms where insiders owned 28% of the shares responded similarly to the downturn—in terms of reducing expenses, maintaining long-term investments, changing leverage, selling assets, and so on—as firms where insiders owned a much lower stake? We think not. But finance studies, by failing to control for ownership stake, implicitly make this assumption. These studies use 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 group of firms. However, those groups consist of very different types of firms. Thus, 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 ATSs 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). These finance studies assume an environment in which firms first 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. In such an environment, it would be correct, as Bertrand and Mullainathan assert, that “[ATSs] avoid the endogeneity problem to the extent that they are passed by states and are not endogenously driven by firm-specific conditions. Unlike firm-specific takeover defenses, laws are not passed on a firm-by-firm basis.”¹²⁸

125. 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 \$137 million (\$21.9 million). The average (median) value of assets was significantly lower than the respective value among the remaining firms in the comparison sample at the 1% (5%) level.

126. The differences in median and mean values 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.

127. See *US Business Cycle Expansions and Contractions*, NAT'L BUREAU ECON. RES., <http://www.nber.org/cycles.html> (last visited Mar. 3, 2016) (indicating that the U.S. economy suffered a recession between July 1990 and March 1991).

128. Bertrand & Mullainathan, *Quiet Life*, *supra* note 4, at 1045.

That the protection afforded by ATSs is not endogenous is central for the design of these studies. Consider, as an analogy, an experimental drug that has the potential to alleviate a serious illness but also entails severe side effects. A study where patients are randomly assigned to the drug or a placebo resembles a finance study where the protection afforded by ATSs *is not* endogenous. A study where patients can choose whether to receive the drug resembles a finance study where the protection afforded by ATSs *is* endogenous. 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 differences among patients rather than the effect of the drug.

As we show in this Part, the premise that the protection afforded by ATSs is exogenous is wrong for three reasons: First, firms can reincorporate. Second, even prior to the adoption of the first ATS, one state—California—had legal rules that differed from other states in the level of takeover protection it afforded. And third, finance studies include in their samples firms that went public *after* the first ATSs were adopted and thus chose between going public in a state that had adopted a statute or one that had not.¹²⁹

A. Reincorporations

Empirical scholars almost always obtain the information on each firm's state of incorporation from Compustat.¹³⁰ 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 ATSs, sometime (depending on the study) between 1995 and today. But where a firm was incorporated in 1995, or 2016, is endogenous: determined by choices made by firms—whether or not to reincorporate—rather than by “random” decisions by states to adopt statutes in the late 1980s.¹³¹

129. See, e.g., Giroud & Mueller, *supra* note 1, at 314. Other scholars have noted that several states adopted their ATSs at the behest of particular firms (or groups of firms). See, e.g., Karpoff & Malatesta, *supra* note 63, at 305; Roberta Romano, *The Political Economy of Takeover Statutes*, 73 VA. L. REV. 111, 136-37 (1987). 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.

130. See, e.g., Bertrand & Mullainathan, *Quiet Life*, *supra* note 4, at 1052-53; Giroud & Mueller, *supra* note 1, at 314.

131. 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 three companies in a sample of 200 reincorporated during a twenty-year period. See Bertrand & Mullainathan, *Quiet Life*, *supra* note 4, at 1053; see also Giroud & Mueller, *supra* note 1, at 314. As we discuss, reincorporations are substantially more frequent than Bertrand and Mullainathan found.

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.¹³² We supplemented this 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 that were available in Thomson One Banker. Out of the 2391 firms in our comparison sample, we identified approximately 12.2% that changed their states of incorporation between 1985—the year the first business combination statute was adopted—and 1995.

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

But firms that did not reincorporate also made a choice: to remain in their state of incorporation. For any given firm that is happy with the 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 takeover protection. In our view, if ATSs 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. Reincorporation costs were low—\$40,000 to \$80,000 for a company with 100,000 shareholders, according to a contemporary estimate by Bernard Black.¹³³ And shareholders, who regularly approved all kinds of antitakeover devices, such as staggered boards and antitakeover “fair price” charter amendments, during that period, would have been unlikely to balk at a proposed reincorporation.¹³⁴

132. See *WRDS SEC Analytics Suite*, WHARTON SCH., UNIV. PA., <http://www.whartonwrds.com/our-datasets/wrds-sec-analytics-suite> (last visited Mar. 3, 2016). Compact Disclosure is a dataset put together by a private vendor that processed and systematized the information contained in SEC filings. 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.

133. Bernard S. Black, *Is Corporate Law Trivial?: A Political and Economic Analysis*, 84 Nw. U. L. REV. 542, 558 (1990). If an ATS generated economically meaningful effects that can be picked up in finance studies, then the 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 decisionmakers care most about these statutes.

134. According to data from the Investor Responsibility Research Center, by 1987, 158 of 424 Fortune 500 companies had fair price provisions, most of which were adopted in the 1980s. VIRGINIA K. ROSENBAUM, TAKEOVER DEFENSES: PROFILES OF THE FORTUNE 500, at 1-2 (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 K.J. Martijn
footnote continued on next page

Given these low barriers, one may wonder why only twelve percent 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, in our comparison sample, firms that reincorporated out of California account for the bulk of the reincorporation activity in the 1985 to 1995 period. But for finance scholars who find that ATSs had substantial effects on multiple aspects of managerial and firm behavior, the fact that only 12.2% of firms reincorporated, and that only a small minority of reincorporations involved a move from a state without an ATS to one with (or vice versa), remains a puzzle that they fail to address.¹³⁵

B. California

As discussed in Part I above, California has longstanding laws that, until 1989, prohibited staggered boards and continue to require that companies permit shareholders to remove directors without cause and to call special meetings. 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—fifteen percent 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 antitakeover. 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 on the supposed exogeneity of ATSs, the samples they use in their regressions include firms that became public after the first ATSs 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 had already adopted the statute at issue or opted to go public in a state that had not adopted the statute (but might in the future).

Cremers & Simone M. Sepe, *The Shareholder Value of Empowered Boards*, 68 STAN. L. REV. 67, 96 & fig.1 (2016).

135. Like Sherlock Holmes, one can sometimes obtain the best evidence for a hypothesis by looking at the things that did *not* happen. For us, the fact that firms did not reincorporate in response to the adoption of ATSs is the dog that did not bark. See ARTHUR CONAN DOYLE, *Silver Blaze*, in A TREASURY OF SHERLOCK HOLMES 380, 398 (1955).

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 ATSs 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 wanted to be subject to an ATS and that the factors affecting this firm choice are responsible for the change in firm or managerial behavior.

Conclusion

In this Article, we presented our legal argument for why most antitakeover statutes have 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, consistent with the legal argument, found that these studies' 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 antitakeover statutes. Legal scholars tend to dismiss them as barely relevant, while empirical finance scholars find that they have significant effects. One contribution of this Article is thus to show that the empirical results generated by finance scholars may be due to factors other than the causal effect of antitakeover statutes.

But this Article has important implications that go beyond antitakeover 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.¹³⁶ Another set of scholars argued that the threat of a takeover

136. Easterbrook & Fischel, *supra* note 16, at 1173-74 (arguing that hostile tender offers are an important device to reduce agency costs); Gilson, *supra* note 16, at 841 (“[I]t 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
footnote continued on next page”)

induces an excessive short-term focus by management and thereby lowers long-term shareholder value.¹³⁷ Yet others have suggested that the takeover threat may lead management to take actions that benefit shareholders, but harm other constituents, and may therefore not enhance overall social value.¹³⁸

Takeovers and takeover defenses continue to generate significant controversy. In 2015, for example, a blue-ribbon commission co-chaired by

shareholders may be constrained.”); Dale Arthur Oesterle, *Delaware’s Takeover Statute: Of Chills, Pills, Standstills, and Who Gets Iced*, 13 DEL. J. CORP. L. 879, 897 & n.79 (1988) (“[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) (“[T]he market for corporate control in general, and tender offers in particular, [is] the most important disciplinary factor[] in the corporate governance system”); Bebchuk, *supra* note 16, at 1047 (noting that the threat of takeovers induces managers to do more to maximize profit); *see also* Edgar v. Mite Corp., 457 U.S. 624, 643 (1982) (“The 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 that takeover threats increase management efficiency date to Henry G. Manne, *Mergers and the Market for Corporate Control*, 73 J. POL. ECON. 110, 113 (1965).

137. *See, e.g.*, Lucian Arye Bebchuk & Lars A. Stole, *Do Short-Term Objectives Lead to Under- or Overinvestment in Long-Term Projects?*, 48 J. FIN. 719 (1993) (constructing a model in which takeover threat can induce inefficiencies); Peter F. Drucker, *Corporate Takeovers—What Is to Be Done?*, 82 PUB. INT. 3, 12-16 (1986) (arguing that a wave of hostile takeovers caused erosion of American competitive and technological leadership); Thomas Lee 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, 205-06 (1991) (concluding that short-term planning has been overly emphasized by corporate investors and managers); Lipton, *supra* note 17, at 23 (arguing that takeovers cause management to focus on short-term profits at the expense of long-term planning); Shleifer & Vishny, *supra* note 17 (arguing that short horizons of arbitrageurs can lead to short horizons of corporate managers); 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 the presence of asymmetric information, managers may behave myopically even when faced with a rational stock market); Stein, *supra* note 17 (analyzing how myopic behavior might arise when takeover threats lead managers to seek high stock prices in the short term); Lynn A. Stout, *Do Antitakeover Defenses Decrease Shareholder Wealth?: The Ex Post/Ex Ante Valuation Problem*, 55 STAN. L. REV. 845 (2002) (arguing that antitakeover provisions encourage nonshareholder groups to make extracontractual investments in corporate team production).
138. *See, e.g.*, Mark L. Mitchell & J. Harold Mulherin, *The Stock Price Response to Pension Terminations and the Relation of Terminations with Corporate Takeovers*, FIN. MGMT., Autumn 1989, at 41; Andrei Shleifer & Lawrence H. Summers, *Breach of Trust in Hostile Takeovers*, in CORPORATE TAKEOVERS: CAUSES AND CONSEQUENCES 33 (Alan J. Auerbach ed., 1988) (arguing that takeovers lead to expropriation from stakeholders such as employees and suppliers to shareholders).

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, and thus help combat excessive short-termism.¹³⁹ At the same time, under pressure from shareholder rights advocates and institutional investors, most large companies that used to have staggered boards decided to move to annual elections of the entire board, thereby facilitating hostile takeovers.¹⁴⁰ Moreover, the longstanding debate over the effects of hostile takeovers has a curious parallel to a more recent debate with many of the same partisans who are rehashing many of the same arguments about the effects of activism by hedge funds.¹⁴¹

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 that faces a sudden increase (or decrease) in the takeover threat and another set for which the takeover threat is stable—and compare how they perform. This is the rationale behind many of the studies on antitakeover statutes, including the study 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. Using this rationale, both studies conclude that firms that become subject to an antitakeover statute (posited to reflect a reduction in the takeover threat) experience an increase in managerial slack, a conclusion that is consistent with the hypothesis that takeover threats keep managers on their toes. Similarly, a recent article by Julian Atanassov concludes that firms that become subject to an antitakeover statute experience a decline in innovation, a finding at odds with the hypothesis that takeover threats induce short-termism.¹⁴²

But if these statutes do not impact the takeover threat, or if (re-)incorporation decisions render a firm's exposure to them endogenous, or if the firms incorporated in states that never adopted an ATS are not really comparable to those incorporated in states that did adopt ATSs, 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 effects of takeover threats have yielded little knowledge. Rather than pouring even more

139. See *supra* note 18.

140. See Andrew Ross Sorkin, *An Unusual Boardroom Battle*, in *Academia*, N.Y. TIMES: DEALBOOK (Jan. 5, 2015, 9:42 PM), <http://nyti.ms/1yuj7Wg>.

141. See, e.g., Lucian A. Bebchuk, *The Myth that Insulating Boards Serves Long-Term Value*, 113 COLUM. L. REV. 1637, 1676 (2013) (arguing that hedge funds do not induce short-termism); Memorandum from Martin Lipton, 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) (on file with authors); see also Marcel Kahan & Edward B. Rock, *Hedge Funds in Corporate Governance and Corporate Control*, 155 U. PA. L. REV. 1021, 1083-91 (2007) (reviewing the debate).

142. Atanassov, *supra* note 5, at 1099.

energy into empirical studies of antitakeover statutes, scholars should develop a different approach.¹⁴³

Our findings also have some farther-reaching implications. The use by empirical scholars of antitakeover 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. A number of law-related variables that lack coherent theoretical grounding are frequently used by empiricists. At the 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 6 factors that attract significant opposition by institutional investors.¹⁴⁴ Similarly, Michael Klausner has recently argued that the GIM index contains elements that are irrelevant for all companies and elements that are irrelevant for a subset of companies.¹⁴⁵ Even to the extent that the index captures useful variables, he explains, empiricists have not understood the underlying governance mechanisms and have therefore misinterpreted their empirical results.¹⁴⁶ Misinterpretations of this sort have a long pedigree—fifteen years ago, John Coates argued that economists widely misinterpret the import of a company adopting a poison pill.¹⁴⁷

143. Another popular approach involves event studies related to the enactment of antitakeover statutes and to major legal opinions. *See, e.g.*, Karpoff & Malatesta, *supra* note 63 (using event studies to examine multiple antitakeover statutes). Event studies on antitakeover 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 AM. L. & ECON. REV. 380, 386 tbl.2 (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 at most measure the market's expectation of the effect of a legal event. *See, e.g.*, Alma Cohen & Charles C.Y. Wang, *How Do Staggered Boards Affect Shareholder Value?: Evidence from a Natural Experiment*, 110 J. FIN. ECON. 627, 630 (2013) (analyzing *Airgas* rulings, which had disparate effects on companies with staggered boards depending on the timing of a company's annual meeting, and finding evidence consistent with the view that staggered boards reduce stock price).

144. *See* Lucian Bebchuk et al., *What Matters in Corporate Governance?*, 22 REV. FIN. STUD. 783, 784-86 (2009).

145. In a related piece, Atanasov and Black survey a large sample of papers that attempt to estimate the causal effect of corporate governance on firm value and related outcomes by exploiting external shocks. They conclude that only a small minority of the papers actually employ convincing causal research designs. Vladimir Atanasov & Bernard Black, *Shock-Based Causal Inference in Corporate Finance and Accounting Research*, 6 CRITICAL FIN. REV. (forthcoming 2016) (manuscript at 1).

146. Michael Klausner, *Fact and Fiction in Corporate Law and Governance*, 65 STAN. L. REV. 1325, 1368 (2013).

147. *See* Coates, *supra* note 27, at 276.

To our mind, all of these instances reflect the generation of a variable—antitakeover statutes, GIM index, or 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, to test whether they have a statistical relationship with multiple potential outcomes—leverage, wages, patents, dividends, and so on—by employing various methodologies and adding differing sets of controls. When the analysis lacks any proper theoretical foundation, this exercise amounts to data-mining.

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 reputations. Put differently, just like managers suffer from agency costs that distort behavior, academics (in finance, but also in law—ourselves 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 to pay little heed to arguments that the variable has no theoretical validity.

During the past couple of decades, scholarship at 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 but only if the scholarship employs proper methodologies and builds upon 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.”

Appendix

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

*Capital Structure and Corporate Control: The Effect of Antitakeover Statutes on Firm Leverage*¹⁴⁸ begins from the assumption that leverage can keep managers on their toes. Managers, in turn, would prefer to issue less debt than shareholders desire. Since ATSs are thought to reduce the hostile takeover threat, the argument then goes, managers of firms subject to ATSs are likely to reduce the amount of leverage in their firms' capital structures.¹⁴⁹

Garvey and Hanka's data consist of annual observations for 1200 publicly traded firms over the 1982-1993 period.¹⁵⁰ They construct their main explanatory variable, the *protected* dummy, as a dummy that, for each firm, switches from zero to one in the year after the firm's state of incorporation adopted an ATS (and is zero for every period in the case of firms incorporated in states that did not adopt any ATS).¹⁵¹ 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. Using this methodology, Garvey and Hanka find 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 firms not subject to an ATS.¹⁵²

The main flaw in Garvey and Hanka's analysis is the way in which they try to control for leverage trends over time unrelated to antitakeover statutes.¹⁵³

148. Garvey & Hanka, *supra* note 2.

149. *Id.* at 519-20.

150. Other scholars have found that Garvey and Hanka's results are not robust to alternative sample constructions. *See, e.g.,* John & Litov, *supra* note 2, at 735.

151. Although the paper is not entirely clear about which kinds of statutes count, it seems to include control share acquisition, business combination, and constituency statutes, and may or may not include fair price statutes. Garvey & Hanka, *supra* note 2, at 522.

152. *Id.* at 528.

153. 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 id.* at 522. In fact, Delaware and Pennsylvania's statutes took effect in December 1987 and March 1988, respectively. *See* Karpoff & Wittry, *supra* note 15, at 39 tbl.2. Second, the authors have a peculiar way of dealing with states that had adopted antitakeover 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, *footnote continued on next page*

Although doing so would have been standard, Garvey and Hanka's specifications do not control for shocks that may have affected the entire economy in a given year or period by including year fixed effects. Instead, their regressions include the control variable *time*. For firms in states that never adopted an ATS (control states), the *time* variable takes the value of 1 in 1988 and thereafter (and is zero otherwise), but for firms in states that did adopt an ATS, *time* takes the value of 1 only in the years after the statute was adopted (and is zero beforehand). 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.¹⁵⁴

To illustrate the effect of the peculiar construction of the *time* variable, assume that Maryland adopted its ATS on December 31, 1989, while California never adopted one. In addition, assume that one has a sample consisting of two 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. In each year, the value of *Y* is identical for Firms *A* and *B*. This, therefore, represents a scenario in which the business combination statute had no impact on *Y* and firms in both states experienced identical annual shocks to *Y*. If one used this data to estimate the impact of Maryland's statute on *Y* using a standard difference-in-differences approach, the estimate one would recover would equal zero. This is exactly what one would expect to recover from a difference-in-differences analysis. But if one instead used these 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*

at 522, 523 tbl.1. 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 tbl.2. The rationale, we presume, is that while the pre-*CTS* statute was invalid, the post-*CTS* statute was valid. This 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 antitakeover 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 result in a miscoding of the *protected* dummy in, respectively, 70%, 16%, and 3% of the firms in their sample.

154. 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 obtained 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, 628-30 (2014). By the same token, there is no reason to expect that variable to control for time trends.

would have a value 1.¹⁵⁵ In other words, even though the adoption of the business combination statute was completely irrelevant, the estimate of the coefficient from *protected* would seem to suggest otherwise. The intuition behind this result is straightforward: by including *time* (instead of a *period 1990-91* dummy or year fixed effects for each year 1987 to 1991) 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.¹⁵⁶ 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.¹⁵⁷ The estimate for the coefficient of *protected* dropped to -0.0036 and stopped being statistically significant.¹⁵⁸ When, in addition, we corrected the miscodings described in footnote 153, our results remained essentially unchanged.

155. Following Garvey and Hanka's coding, the *state* dummy would equal 1 for the Maryland firm on every year and would equal zero for the California firm on every year; the *time* dummy would equal one for the Maryland firm for years 1990 and 1991, would equal 1 for the California firm for years 1988 to 1991, and would equal zero for all other observations. The coefficient for *state* would have an estimate of -0.5, the coefficient for *time* would have an estimate of -0.5, and the estimate for the constant would be 1.

156. 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.

157. 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.

158. 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 tbl.7.

Table A2
Garvey and Hanka Replication: Leverage and ATs

	Protected	Time
Garvey and Hanka, Table 3, Column 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. *, **, *** denote statistical significance at the 0.1, 0.05, and 0.01 level, respectively.

[†] 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.¹⁵⁹ Hence, the estimates for the coefficient of the *protected* dummy in Garvey and Hanka's regressions do *not* capture the impact of the ATs they study. When corrected for coding errors and properly specified, there is no evidence for an association between these ATs and leverage changes.

Jiaping Qiu and Fan Yu's article *The Market for Corporate Control and the Cost of Debt*¹⁶⁰ examines the relationship between business combination statutes and bond yields and concludes that these statutes are associated with a significant increase in yields.¹⁶¹ Qiu and Yu construct a yearly panel that spans

159. 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 tbl.3. 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 that the leverage of firms incorporated in different states should have, but for the adoption of an ATs (and other controls), followed the same trend over time. But Garvey and Hanka's results for the 1983-1986 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 an ATs. Thus, their own results contradict the assumption that, but for ATs, leverage trends across states would have been equivalent and instead show that firms incorporated in control states do not constitute a proper control group.

160. Qiu & Yu, *supra* note 7.

161. *Id.* at 507. 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
footnote continued on next page

the 1976-1995 period and includes yield data for bonds issued by approximately 700 individual firms.¹⁶² 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);¹⁶³ 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.¹⁶⁴

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.¹⁶⁵ 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 134 basis points for speculative-grade (a.k.a. junk) bonds.¹⁶⁶ For an average junk bond with five (ten) years to maturity that 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% (8%).¹⁶⁷

decline in yields as debt becomes less risky. We are grateful to Zohar Goshen for alerting us to this contradiction.

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

163. 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.*

164. 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 506. 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 Daines, *Does Delaware Law Improve Firm Value?*, 62 J. FIN. ECON. 525, 530 (2001).

165. 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. Qiu & Yu, *supra* note 7, at 513. Their analysis of the relation between business combination laws and competition raises issues that we do not address in this Article.

166. *Id.* at 507.

167. 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 and Yu's coding) was approximately 5.5%.

Qiu and Yu attribute their results to the “coinsurance effect”: the possibility that an acquirer’s strong financial position can make the repayment of the target’s debt safer.¹⁶⁸ Business combination statutes, by making acquisitions less likely, would then reduce bond prices by reducing the likelihood of acquisitions that generate a coinsurance 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 the bonds’ issuer was acquired.¹⁶⁹ But Billett et al. explicitly exclude leveraged buyouts, which are associated with a decline in bond values,¹⁷⁰ from their sample. Their results thus overstate the average effect of all acquisitions on bond values.

Most crucially, however, the Billett et al. 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 likelihoods 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;¹⁷¹ 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 a (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 meltdown in the junk bond market after 1988. As relayed by Robert Comment and William Schwert, “The junk bond market crashed in September 1989 when Campeau, which had become a major issuer of (non-Drexel) junk bonds, revealed the extent of its liquidity crisis and when UAL failed to secure buyout financing.”¹⁷² Other contributing factors, according to Comment and Schwert, were the

168. Qiu & Yu, *supra* note 7, at 508.

169. Matthew T. Billett et al., *Bondholder Wealth Effects in Mergers and Acquisitions: New Evidence from the 1980s and 1990s*, 59 J. FIN. 107, 109 (2004). When looking only at hostile acquisitions, the average effect drops to 3.2%. *Id.* at 119 tbl.3.

170. See, e.g., Paul Asquith & Thierry A. Wizman, *Event Risk, Covenants, and Bondholder Returns in Leveraged Buyouts*, 27 J. FIN. ECON. 195, 212 (1990) (finding losses of 2.8% from leveraged buyouts); Arthur Warga & Ivo Welch, *Bondholder Losses in Leveraged Buyouts*, 6 REV. FIN. STUD. 959, 962 (1993) (finding losses of 6% from leveraged buyouts).

171. 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 Martijn Cremers & Allen Ferrell, *Thirty Years of Shareholder Rights and Stock Returns* 8 fig.2 (Dec. 2012) (unpublished manuscript), <http://ssrn.com/abstract=2020471>. By the same token, Cain et al. report that, in 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 62, at 29 fig.1.

172. Comment & Schwert, *supra* note 62, at 9.

demise of Drexel Burnham Lambert in 1990 and the passage of federal legislation penalizing savings and loan associations for holding junk bonds in August 1989.¹⁷³ Finally, the United States experienced a recession between July 1990 and March 1991.¹⁷⁴ Junk bond issuers are particularly likely to be negatively affected by recessions, as the cash flows on which they rely 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.¹⁷⁵

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 1995.¹⁷⁶ As Figure A1 below 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 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.¹⁷⁷

173. *Id.* 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), <http://nyti.ms/1OSxjht> (describing a trading day in which the average price of junk bonds dropped approximately two 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 to be 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 ATSs was perceived as a cause of the turmoil in that market.

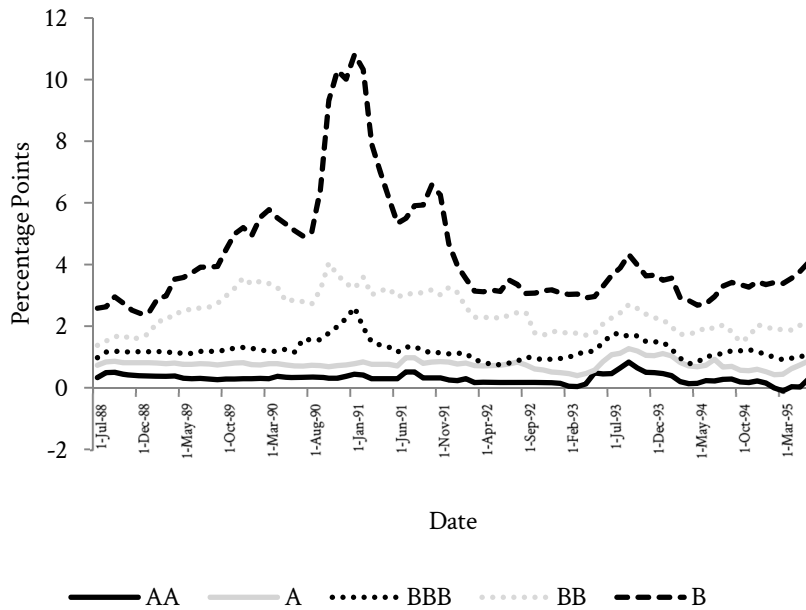
174. *US Business Cycle Expansions and Contractions*, *supra* note 127.

175. Jean Helwege & Paul Kleiman, *Understanding Aggregate Default Rates of High Yield Bonds*, CURRENT ISSUES ECON. & FIN., May 1996, at 1, 2, 4.

176. Figure A1 was constructed using data from the Standard & Poor's Corporation Bond Guides.

177. 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 7, at 508 n.6.

Figure A1
Average Spread of Bonds of Different Ratings Relative to AAA-Rated Bonds



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% were incorporated 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 ATs may simply be due to the fact that the adoption of those statutes coincided with the shocks to the bond market, for which Qiu and Yu's regressions do not adequately control.

With this potential explanation in mind, we next took a closer look at the regressions in the Qiu and Yu paper. In the regressions that use all the observations in their full sample, 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, and the variable *BC*speculative*, which takes the value of 1 if the bond is rated junk and the issuer is incorporated in a state that has passed a business combination law by the relevant year (and zero otherwise).¹⁷⁸ Qiu and Yu obtain their high estimate from the *BC*speculative* variable.

178. See *id.* at 515 tbl.6, col. 3.

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 twelve 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 assumed specification, 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 around the time firms became subject to business combination statutes.¹⁷⁹ 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 $BC^*speculative$.

To test our hypothesis that Qiu and Yu's estimate reflects the collapse in the junk bond market that occurred at around 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.¹⁸⁰ However, when we removed the

179. 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 eighty-five percent 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 BC (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 had 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.

180. In our replication of the estimation run by Qiu & Yu in table 6, column (3) of their paper, the estimate for BC was 0.033 (insignificantly different from zero), while the point estimate for $BC^*speculative$ was 1.13 (statistically significantly different from zero at the 1% level). The way in which Qiu and Yu's specification attempts to control for location-year and industry-year fixed effects is inadequate. Estimating an analogous specification that adequately controls for (3-digit SIC code) industry-year and location-year fixed effects yielded qualitatively similar results.

constraint that the relationship between credit rating and yield be linear and stable over time, the result disappeared.¹⁸¹

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, even if one assumes that business combination statutes significantly affected management’s ability to defend against a hostile takeover. Instead, we suggest that the association between yield spread and business combination statutes that Qiu and Yu describe is driven by omitted-variable bias: a massive contemporaneous shock to the credit market, for which Qiu and Yu’s regressions do not adequately control, that increased the yield spreads for junk bonds. When we replicate Qiu

181. In these regressions, we used year-rating fixed effects instead of the year and rating dummies used by Qiu and Yu. By using this more granular specification, we are effectively comparing a bond issued by a firm that became subject to a BC statute with another bond of the same credit rating and in the same year issued by a firm that did not become subject to a BC statute. We used these granular fixed effects as controls in several specifications. In every specification, we regressed the yield spread against *BC* and *BC*speculative*, with different types of controls. In our baseline estimation, we only controlled for firm and rating-year fixed effects. In that case the point estimates for *BC* and *BC*speculative* were 0.19 and -0.31, respectively (significant at the 5% level and insignificant, respectively). When we augmented the baseline specification by controlling for location-year and industry-year fixed effects, the point estimates became 0.03 and 0.07, respectively (in both cases, insignificantly different from zero). When we instead augmented the baseline specification by including the set of controls that Qiu and Yu use in table 6, column (3) of their paper, the point estimates we recovered were 0.22 (significant at the 1% level) and -0.09 (insignificantly different from zero), respectively. When we augmented the baseline specification by controlling for location-year and industry-year fixed effects and including the set of controls Qiu and Yu use in table 6, column (3), the point estimates were 0.10 and 0.54 (in both cases, insignificantly different from zero). The sum of the estimates of the coefficients for *BC* and *BC*speculative* were insignificantly different from zero in each specification. Although 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 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 coinsurance 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 7, at 133-42 (offering evidence that suggests that, in the cross-section, yield spreads are higher for bonds issued by firms incorporated in “takeover-friendly” states than for bonds issued by firms incorporated in states that have adopted more antitakeover statutes); see also Asquith & Wizman, *supra* note 170, at 201-03 (finding that bond values declined after LBOs); Warga & Welch, *supra* note 170, at 962 (finding similar results). The meltdown in the junk bond market, however, does not by itself explain why Qiu and Yu find a significant increase in the yield spread even in a separate regression that includes only junk bonds. Qiu & Yu, *supra* note 7, at 515 tbl.6, col. 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.

and Yu's regression in a manner that controls for this shock, the association between the statutes and junk bond yields disappears.