

Poison or placebo? Evidence on the deterrence and wealth effects of modern antitakeover measures

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Abstract

This paper provides large-sample evidence that poison pill rights issues, control share laws, and business combination laws have not systematically deterred takeovers and are unlikely to have caused the demise of the 1980s market for corporate control, even though 87% of all exchange-listed firms are now covered by one of these antitakeover measures. We show that poison pills and control share laws are reliably associated with higher takeover premiums for selling shareholders, both unconditionally and conditional on a successful takeover, and we provide updated event study evidence for the three-quarters of all poison pills not yet analyzed. Antitakeover measures increase the bargaining position of target firms, but they do not prevent many transactions.

Key words: Poison pill; Antitakeover law; Merger; Tender offer; Takeover premium

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1. Introduction

At the end of the 1980s, the level of takeover activity fell dramatically. Many commentators have speculated that the demise of the market for corporate control was caused by the evolution of sophisticated antitakeover measures. This paper analyzes data on all exchange-listed firms from 1975–91 and casts doubt on the hypothesis that antitakeover measures shut down the market for corporate control.

The 1980s saw many improvements in corporate antitakeover methods. More than 1,500 firms adopted shareholder rights plans (poison pills) during the second half of the decade. These securities, issued as dividends, serve to create impossibly burdensome obligations for anyone who buys a controlling block of shares without management approval. (Bruner, 1991, provides a detailed review of the mechanics of poison pills.) Separately, states developed several forms of antitakeover statute able to pass constitutional muster: *control share laws* that restrict the voting rights of a controlling shareholder and *business combination* (merger-moratorium or freeze-out) *laws* that delay any business combination. In either variant, the prohibitions last for three to five years and, like poison pills, apply to the buyer of a controlling block of shares who buys without management approval. Ohio adopted the first business combination law in 1982 and New York adopted the first control share law in 1985. Many states have both types of law. The U.S. Supreme Court upheld Indiana's control share law in April 1987, and an appellate court upheld Wisconsin's business combination law in June 1989.¹ The appellate court ruled that investors have no constitutional right to receive tender offers and that state law need not leave bidders a meaningful opportunity for success.

The demise of the 1980s market for corporate control is closely associated with the spread of these new antitakeover methods. Fig. 1 plots the percentage of all firms listed on the New York (NYSE) or American Stock Exchange (Amex) that received initial takeover offers in each month from January 1975 to December 1991, along with the proportion covered by the three kinds of modern antitakeover measures. Coverage by business combination statutes jumped from 15% to 63% when Delaware adopted its law in January 1988 (half the exchange-listed firms are incorporated in Delaware) and reached 80% by 1991. Coverage by control share laws increased gradually to reach 24% by 1991. Coverage by poison pills, trivial before 1986, increased gradually to reach 35%. By 1991, 87% exchange-listed firms were covered by a poison pill, a business combination law, or a control share law. In comparison, takeover rates started at levels below 0.5% per month in 1975 and increased to a peak of about 1.5%

¹ See *CTS Corp. v. Dynamics Corp. of Am.*, 481 U.S. 69, 86–87 (1987); *Amanda Acquisition Corp. v. Universal Foods Corp.*, 877 F. 2d 496, 504–05 (7th Cir.), cert. denied, 110 S. Ct. 367 (1989).

per month during 1987 and 1988. A long, gradual rise in merger and acquisition activity culminated in a sharp reversal during 1989. Takeover rates were again below 0.5% per month in 1990 and 1991.

The association seen in Fig. 1 suggests that incumbent managers used their enhanced decision rights to deter takeovers systematically. This interpretation is consistent with (1) a substantial literature that portrays managers as more interested in entrenchment than in maximizing shareholder wealth, (2) anecdotal evidence from well-publicized cases of managers relying on antitakeover measures to defeat premium-priced takeover offers, and (3) indirect evidence from many event studies that find average reductions in shareholder wealth from adoption of antitakeover measures. The aim of this study is to use large-sample evidence to test whether modern takeover defenses have been used systematically to deter takeovers.

The evidence from our direct test of deterrence provides little support for the proposition that modern antitakeover measures have been used to deter takeovers systematically. We infer from this evidence that firm-specific antitakeover

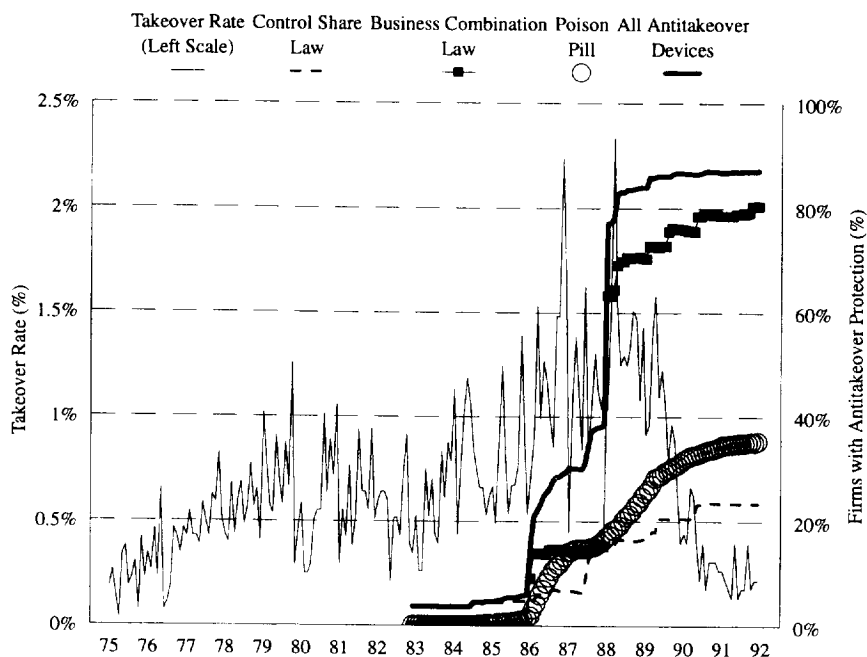


Fig. 1. Monthly time series plot of proportion of all exchange-listed firms that are covered by (1) control share laws, (2) business combination laws, (3) poison pill rights issues, (4) any of the above, and (5) the proportion of all exchange-listed firms that received initial merger proposals, merger agreements, or inter-firm tender offers each month during 1975–91 (left-hand scale).

measures played a minor role in ending the 1980s merger wave. Our evidence points instead to broad-scale political or economic forces that manifest themselves as secular variation. In addition, our review of the major takeover-related court decisions suggests that this secular variation is not due to an evolution in takeover jurisprudence.

While systematic evidence is scarce, practitioners and researchers have long acknowledged that antitakeover measures could enhance the bargaining power of incumbent management versus bidders, thereby yielding higher takeover premiums for selling shareholders. We find that selling shareholders do receive premium prices for their shares when antitakeover measures are in place. Since higher prices surely deter buyers, this result is consistent with deterrence, and yet we fail to detect such deterrence. Moreover, we find that expected premiums are higher with an antitakeover measure in place even with expected premiums calculated to account for the possibility of added deterrence. Finally, we update previous studies of the stock price effects of announcements of adoptions of poison pills and closely replicate their results. Overall, our evidence casts doubt on the view that antitakeover measures are used primarily to entrench incumbent management.

Section 2 discusses some *a priori* grounds for skepticism regarding the deterrent effect of antitakeover measures, and offers several alternate explanations, notably the credit crunch that began with the dismantling of Drexel Burnham Lambert, for the sharp reduction in takeover activity that occurred in the late 1980s. We describe our data sources in Section 3. Section 4 updates the existing evidence on the wealth effect of poison pills. In Section 5 we exploit the firm- and time-specific nature of coverage by poison pills and state antitakeover laws to decide if deterrence is observable in the takeover experience of individual firms. In Section 6 we estimate the relation between antitakeover measures and conditional and unconditional takeover premiums. Section 7 discusses various specification checks that assure the insensitivity of our results to reasonable changes in the definition of the variables or tests. In particular, evidence for deterrence is not materially enhanced when we analyze hostile takeovers rather than all transactions. Section 8 presents our conclusions. The Appendix contains a brief history of case law in Delaware and Federal courts as it pertains to takeover defenses.

2. Takeover defenses as placebos

There is much *a priori* ground for skepticism about the deterrent effect of modern antitakeover measures. To begin with, incumbent managers wielded considerable influence over takeover outcomes even before modern antitakeover measures, as shown by the predominance of negotiated transactions during the first half of the 1980s. Considering all takeover attempts involving tender

offers for exchange-listed firms between 1981 and 84, Comment and Jarrell (1987) report that half of all bidders obtained a merger agreement before starting an offer, 22% of all transactions started as hostile but were superseded by negotiated bids, while another 12% started as hostile but ended with no shares purchased by any bidder. Few bids (the remaining 16%) were executed without management's approval. This evidence shows that deterrence is not management's main interest (eventually two-thirds of all offers were approved), and that management wielded considerable bargaining power versus bidders throughout the 1980's and not just after the spread of modern antitakeover measures.

Moreover, the burden imposed on buyers by a control share law (i.e., restricted voting rights) may be avoidable at low cost. As noted by Gilson and Black (1992), this type of law provides for a shareholder vote to decide if the law's restrictions on voting rights are to apply in a given instance. These elections are paid for by the buyer, triggered by a disclosure of intent to seek control, held within 55 days, and require a simple majority to grant the buyer full voting rights. Thus, the minimal burden imposed by a control share law is the cost of a special election and a 55-day delay (where the minimum tender-offer period is 20 days in any event). As a practical matter, bidders can make their offers conditional on the outcome of this vote, just as they might on the outcome of an antitrust review. Bidders might prefer such a vote since it would be viewed as an expression of shareholder preference that would undermine any claim of coercion as a legal rationale for antitakeover measures. For example, as an adjunct to its unnegotiated tender offer for Kansas Gas & Electric in 1990, Kansas City Power & Light sought a court order to force the target firm to comply with the Kansas Control Share Acquisitions Act by calling a special election to approve voting rights for any shares it might buy in its offer.

A corresponding provision for a shareholder vote occurs in business combination laws, but with the crucial difference that the vote is triggered by an actual purchase of shares, rather than by a stated intent to purchase. Thus, a buyer must win a vote among nontendering shareholders, rather than among all holders, to avoid the burden imposed by a business combination law (a five-year delay before clean-up mergers).

Event studies of the stock price effects of adoptions of antitakeover measures provide considerable indirect evidence regarding deterrence, and although stock prices fall, implying deterrence, the size of the decline is inconsistent with strong deterrence. The typical decline is less than 1% for most types of antitakeover measures.² Ryngaert (1988) examines 283 poison pills adopted through 1986

²Exceptional results are reported by Szewczyk and Tsetsekos (1992). They find an average return of – 9.09% for 56 Pennsylvania firms during the legislative process (from October 10, 1989 through April 27, 1990) for the adoption of that state's cashout law. When cumulated over key dates, a methodology comparable to that used by Karpoff and Malatesta, the average return is – 3.33%.

and finds an average two-day announcement return of -0.34% . Malatesta and Walkling (1988) examine 132 poison pills adopted through March 1986 and find an average two-day return of -0.92% . Romano (1993) reviews 13 studies of the wealth effects of adoptions of antitakeover laws by individual states. Pooling these events, Karpoff and Malatesta (1989) examine the average stock price effect for a sample of 1,505 firms affected by 40 different instances of an introduction of a law to a state legislature in 26 different states, and find a two-day announcement return of -0.29% . In addition, Jarrell and Poulsen (1987) report an average announcement return of -1.25% for 649 fair-price, classified-board, and supermajority-voting charter amendments from 1980–85, although other studies find little evidence of an adverse reaction to antitakeover charter amendments (for example, DeAngelo and Rice, 1983; Linn and McConnell, 1983).

In juxtaposition to these studies, Nathan and O'Keefe (1989) show that shareholder gains of 50% are typical in takeovers and thus typical of what is forgone with deterrence. For instance, if the probability of a takeover decreases by 10% with the adoption of an antitakeover measure, and if a 50% gain is lost, then the wealth decline on adoption should be about 5%. The empirical estimates of wealth loss generally do not approach this level. Thus, the size of the average wealth decline on the adoption of an antitakeover measure is consistent with only a modest change in takeover probability.

A substantial literature relating merger waves and business cycles provides further grounds for skepticism (see Weston, Chung, and Hoag, 1990, Ch. 11, for a review of this evidence). It is possible that the 1980s restructuring boom, like earlier booms, simply succumbed to market forces. The National Bureau of Economic Research marks the start of a recession in July 1990, which lags the decline in takeover activity in Fig. 3, but there is evidence of an earlier credit crunch. As reported by Putnam (1991), U.S. flow of funds data show a collapse in net new lending to the nonfinancial business sector by commercial banks from \$33 billion in 1989 to \$2 billion in 1990. Commercial banks were the dominant providers of bridge or transaction financing for large, cash acquisitions. The Securities and Exchange Commission (1986) shows that commercial banks provided over three-quarters of the immediate financing for 272 successful tender offers from 1981–85.

The availability of long-term and subordinated financing had shriveled before 1990 due to declining prices, illiquidity, and government intervention in the market for junk bonds in 1989 (see Yago, 1991, for a detailed discussion). In

This law requires a buyer of more than 20% of the shares to purchase at fair value any remaining shares that holders wish to put to them. This type of antitakeover law appears late in our sample period and only Maine and South Dakota have followed Pennsylvania in adopting a cashout law, so we lack sufficient experience with this type of law to estimate its deterrence effects.

September 1988, the S.E.C. sued Drexel Burnham Lambert, the investment bank most active in financing takeovers, alleging a secret alliance with Ivan Boesky. The impact on Drexel's ability to serve as a financial intermediary was immediate. By October, the \$570 million, Drexel-financed leveraged buyout of Wickes was canceled and Drexel had to cover out of its own capital almost half its 'highly confident' commitment to raise \$1.4 billion for an offer for Interco. In a December 1988 attempt at damage control, Drexel dismissed Michael Milken, pleaded guilty to six felony counts, and paid a \$650 million fine. In August 1989, Congress passed the Financial Institutions Reform, Recovery and Enforcement Act (FIRREA), which penalized savings and loans for holding junk bonds and mandated their sale, while regulators issued guidelines barring commercial bank participation in highly leveraged transactions (including all acquisition loans that raised liabilities to 75% of assets, or doubled the debt ratio while raising it to 50% of assets). 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. The decline in the value of Drexel's portfolio of junk bonds triggered its declaration of bankruptcy and dissolution in February 1990. Finally, the National Association of Insurance Commissioners adopted guidelines in June 1990 requiring insurance companies to increase reserves against junk bonds not in default.

Although a formal test of a credit crunch model is beyond the scope of this paper, we do provide two illustrative charts. Fig. 2 plots the number of cash-only transactions and the number of securities-only transactions (common and preferred stock, debt, and combinations), year-by-year, from 1975 to 1991. The number of securities-only transactions remains steady at 15–25 transactions per year, so both boom and bust are attributable to cash transactions. Fig. 3 plots monthly the yield spread between long-term low-quality corporate bonds (rated below Baa) and high-quality bonds (rated Aaa), along with the takeover rates from Fig. 1.³ The most noticeable change in the yield spread is a jump after 1988. Combined, this evidence suggests that the demise of the takeover market coincided with tighter credit.⁴ In the formal tests below, we represent changing economic conditions (from 1986 on) with yearly dummy variables.

³Long-term monthly bond yields are from Salomon Brothers and Ibbotson Associates. The gradual increase in the junk bond yield spread from 1975–88 probably reflects a shift in the composition of the below-Baa group of bonds due to the gradual addition of a large number of original issue bonds issued in the early and mid-1980s to fund takeovers and restructurings.

⁴Tight credit conditions for takeovers could outlast any economy-wide problem. Neither Viacom nor QVC were able to borrow enough to make an all-cash offer for Paramount Communications in 1994.

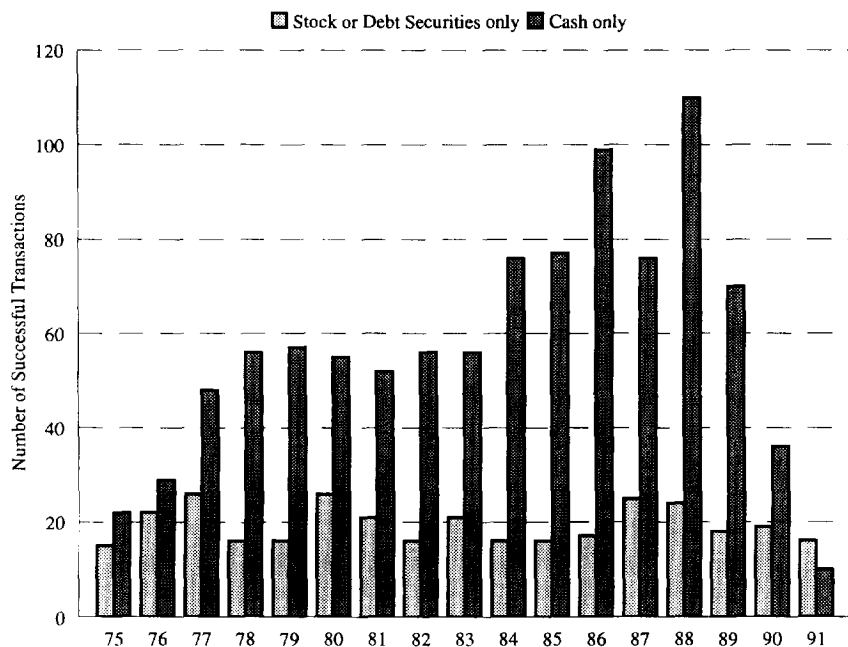


Fig. 2. Number of cash-only and securities-only successful takeovers of exchange-listed firms in the period 1975–91, by year of announcement.

3. Data sources and descriptive statistics

The information on merger and acquisition announcements comes from our proprietary mergers & acquisitions (M&A) database, which covers all exchange-listed target firms in the period 1975–91. These announcements were obtained through various keyword searches of the *Dow Jones News Retrieval (DJNR)* database, inspection of the *Wall Street Journal Index*, and from Commerce Clearing House's *Capital Changes Reporter* [the original source for Center for Research in Security Prices (CRSP) delisting codes]. In this study, we use the subset of records covering merger proposals, merger agreements, and interfirm tender offers. Merger proposals are distinguished from merger talks by a public disclosure of terms of purchase. We use these data to identify (1) successful takeovers, (2) announcement dates, (3) method of payment (cash versus stock), and (4) whether a target is bought in an auction (multiple, publicly revealed bidders). An announcement qualifies as the initial one (that puts the firm in play) if there has been no other qualifying announcement in the prior year.

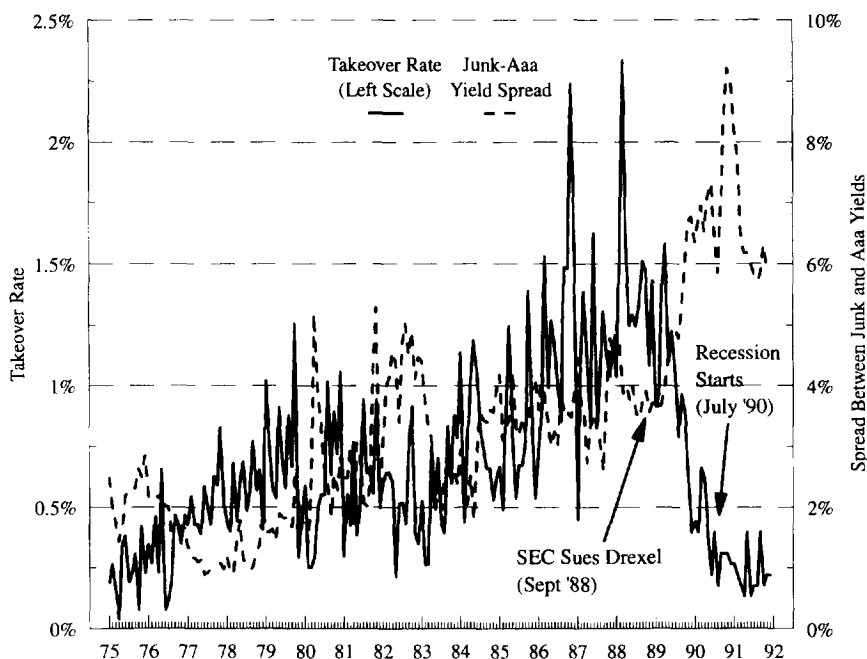


Fig. 3. Monthly time series plot of the spread between the junk ($< \text{Baa}$) and Aaa corporate bond yields and the proportion of all exchange-listed firms that received initial merger proposals, merger agreements, or inter-firm tender offers each month, 1975-91 (left-hand scale).

We use data from CRSP to calculate takeover premiums as the compound difference between the returns to target firms' stock and the value-weighted NYSE and Amex market portfolio during announcement periods that vary in length from target to target. We know from event-study evidence that on average the wealth effect of takeovers occurs in a period from about 10 trading days before to 10 trading days after the initial announcement of a takeover. To ensure that we capture any runup, we begin our periods 20 trading days before the initial takeover announcements. To ensure that we capture the wealth effects of multiple bids or auctions, we end our periods five days after the announcement of the successful offer.

Our information on adoptions of state antitakeover laws comes from the Investor Responsibility Research Center, and our information on states of incorporation (and reincorporations) comes from various issues of *Moody's Manuals*. Our information on adoptions of poison pills is from annual compilations contained in *Corporate Control Alert*, from various *DJNR* keyword searches, from *Capital Changes Reporter*, and from *Moody's Manuals*. We identify a total of 1,584 original poison pill rights issues made through December 1991. Of these, 60% were issued by firms listed on the NYSE or Amex at the

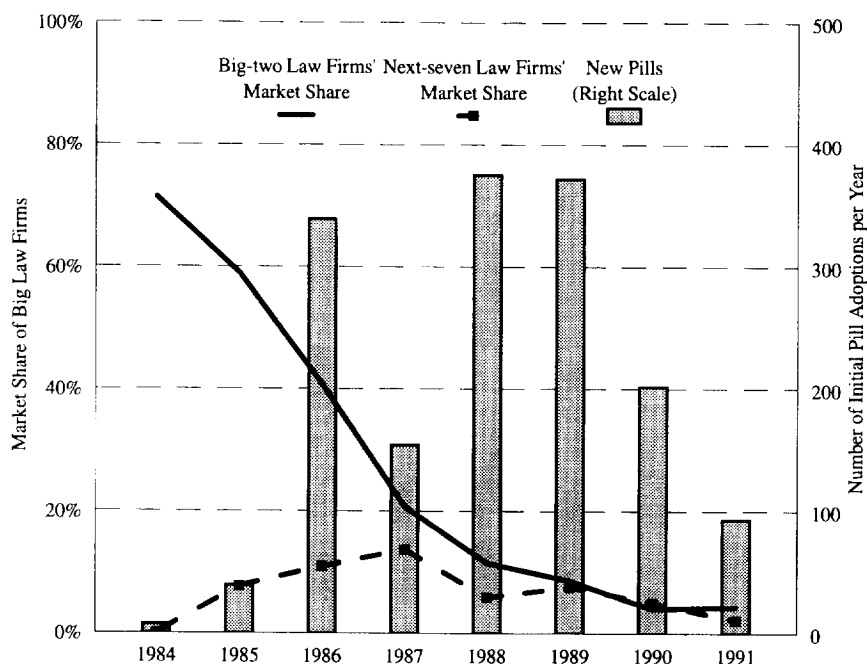


Fig. 4. Number of initial poison pill adoptions by year for all exchange-listed firms in the period 1984-91. Also, market share of large law firms used in pill adoptions. The 'big-two' law firms are Wachtell Lipton and Skadden Arps. The 'next-seven' law firms are Fried Frank Harris, Sullivan Cromwell, Cravath Swaine, Paul Weiss, Wilkie Farr, Shearman Sterling, and Jones Day.

time of issue. For purposes of determining antitakeover coverage when predicting takeovers, we account for pills adopted before exchange listing and for the handful of pills redeemed without being replaced. It is likely that we fail to account for most opt outs from state laws due to the difficulty of identifying these events. Because they have not been enforced by federal courts, we ignore provisions in some state laws that attempt to extend coverage to firms incorporated in other states.⁵

3.1. Evolution of the legal market for poison pill securities

Fig. 4 shows the number of new pills adopted each year from 1984 to 1991. Pill adoptions became common after 1985, with 1988 and 1989 being the peak years. The number of adoptions declines after this, and 1991 had the fewest

⁵The U.S. Supreme Court found such extraterritorial effects unconstitutional in *Edgar v. Mite Corp.*, 457 U.S. 624 (1982).

adoptions since 1985. It is unlikely that the falloff in pill adoptions is due to saturation, as two-thirds of all exchange-listed firms remain without pills, consistent with our analysis of the legal treatment of antitakeover devices (in the Appendix). Because the legality of antitakeover devices depends only on the carefulness of the deliberations of the board of directors at the time that a pill is used, pills are no less valid when adopted on short notice, and the adoption itself is not a complex legal matter.

We can get a sense of the extent of specialization in the legal market for poison pills from information in *Corporate Control Alert* on the identity of the law firm that advised on the adoption. Fig. 4 shows the market shares of several large law firms that frequently advise on pill adoptions. The ‘big-two’ law firms are Wachtell Lipton and Skadden Arps, the market leaders. Wachtell Lipton invented the poison pill and Skadden Arps developed a popular variation. The ‘next-seven’ group includes the next seven firms most commonly retained to advise on pill adoptions: Fried Frank, Sullivan Cromwell, Cravath Swaine, Paul Weiss, Wilkie Farr, Shearman Sterling, and Jones Day. The big-two firms advised on about two-thirds of the pills adopted in 1984 and 1985, when pills were rare and experimental. The market shares of the two leaders fell continuously as pills became common place, ending at 7% in 1991. The market share of the next-seven law firms fell from 10% through 1986 to 7% afterwards, so that the shift over time was toward the use of in-house counsel and local law firms, suggesting that poison pills did not present complex legal issues for long.

3.2. Accounting and stock market predictors of takeover activity

Palepu (1986) uses several accounting and stock market performance measures to predict takeovers. Following his approach, we create a sample of all firm-years with data available on Compustat for exchange-listed firms with fiscal years that end December 1976 through December 1990, yielding 21,887 fiscal-year forecast periods that begin between January 1977 and January 1991. This sample approximates the population. We exclude firm-years without four years’ worth of previous accounting data on Compustat, and we exclude a handful of firm-years that begin after the year of an initial takeover announcement. We use accounting and stock return variables in our prediction models to account for variation, across time and firms, in business conditions that affect the likelihood of takeovers. The variables are all calculated over the four years before each fiscal-year forecast period, and include:

abnormal stock return for firm i and day t , $u_{it} = R_{it} - \alpha_i - \beta_i R_{mt}$, averaged over four years before the forecast period (where the market model parameters α_i and β_i for the CRSP value-weighted portfolio are estimated in the fifth year before the forecast period),

sales growth measured as the proportional change in sales over four years [Compustat item 12],

liquidity measured as a four-year average of the ratio of net liquid assets to total assets [Compustat items (4 – 5)/6],

debt/equity measured as a four-year average of the ratio of debt to equity [Compustat items 9/60],

market/book (sometimes called the ‘*q*-ratio’) measured as a four-year average of the ratio of the year-end market value of common stock to the book value of equity [Compustat items 24·25/60],

price/earnings measured as a four-year average of the ratio of the year-end stock price to earnings per share [Compustat items 24/58], and

size measured as the log of total assets at the start of the forecast period in millions of dollars [Compustat item 6].

Extreme outliers, such as market/book, price/earnings, or debt/equity ratios greater than 100, were omitted from the sample.

Table 1 shows means and standard deviations for these variables, along with *t*-tests for whether mean values differ for the 669 takeover firm-years compared with the full sample of 21,887 firm-years. Consistent with Palepu (1986), there are some modest differences. The average abnormal stock return is slightly higher for takeover targets. This could be due to anticipatory, pre-bid runups in the target’s stock price, although by construction this prior return excludes any runup within the fiscal year of the announcement. In addition, target firms have below-average sales growth, higher cash positions, and lower debt/equity ratios. These results imply that target firms are poor performers that make inefficient use of their financial capital, consistent with Jensen’s (1986) discussion of free cash flow. While their price/earnings ratios are not unusual, targets do have lower-than-average market/book ratios, consistent with several interpretations. One interpretation is that targets have more assets in place and fewer growth options (Myers, 1977). Another is that firms become targets because they are undervalued in the stock market, and market/book measures this undervaluation. Third, takeover targets could be run less efficiently (Lang, Stulz, and Walkling, 1989). In any event, the largest *t*-statistic by far is for (the log of) size, confirming the well-known fact that small firms are more likely to be acquired. Overall, these comparisons provide some reason to think that these accounting and stock market measures of performance can help predict takeover activity.

3.3. Coverage by antitakeover laws and poison pills

Since state antitakeover laws represent a legal innovation, there is ambiguity about when coverage took effect. It can be argued that control share laws took

effect only after the U.S. Supreme Court ruled on the Indiana law in April 1987, and that business combination laws took effect only after the appellate court ruled on the Wisconsin law in June 1989. The fact that the S.E.C. filed a friend-of-the-court brief challenging the Wisconsin law as unconstitutional implies that some uncertainty remained even after the Supreme Court ruling. We report descriptive statistics for alternate specifications of a pill coverage dummy variable in Table 1. In our subsequent analysis we define the effective dates of these laws using the dates of these two important judicial decisions, although this choice does not much affect the results. We ignore the issue of important court decisions for pills since the relevant decision, *Moran*, occurred in 1985 before all but a handful of pills were adopted.

Table 1 reports means and standard deviations for dummy variables that equal one when a firm is covered by a law or a poison pill at the beginning of the forecast fiscal year, so that mean values represent proportions of the sample covered (i.e., exchange-listed firms). Using adoption dates yields a total of 1,263 firm-years of experience with control share laws (5.8% of the sample) and 3,417 firm-years of experience with business combination laws (15.6%). When coverage is defined to begin after the judicial decisions, for laws adopted before these decisions, coverage drops to 884 firm-years for control share laws (4.0%) and to 1,625 firm-years for business combination laws (7.4%). There are 1,779 firm-years of experience with poison pills in the sample (8.1%). We treat firm-years as covered only when an antitakeover measure is in place at the start of the year, ignoring part-year coverage from midyear adoptions.

Foreshadowing the estimates from our prediction model reported in Table 3, which show almost no deterrence, the univariate results in Table 1 show that coverage by control share laws is as frequent in the takeover sample as in the full sample (5.5% versus 5.8%, t -statistic of -0.28), while coverage by business combination laws is slightly lower in the takeover sample than in the full sample (13.5% versus 15.6%, t -statistic of -1.66). Coverage by poison pills is more frequent in the takeover sample than in the full sample (9.6% versus 8.1%, t -statistic of 1.30). The only possible evidence of deterrence in Table 1 is in business combination laws for which coverage is assumed to begin only after the June 1989 appeals court decision that validated these laws. Here, coverage is less frequent in the takeover sample than in the full sample (3.1% versus 7.4%, t -statistic of -6.33). However, this difference could also reflect a secular decline in the takeover market, rather than deterrence. Takeovers were unusually infrequent for fiscal years beginning in 1990 (t -statistic of -2.43) and 1991 (t -statistic of -11.00), confirming the evidence in Fig. 1.

3.4. *Characteristics of successful takeovers*

Table 1 describes some variables that are only defined for takeovers. Exchange-listed firms were acquired at a rate of 3.1% per year. Auctions account

Table 1
Means and standard deviations of accounting and stock market performance variables, antitakeover protection, and takeover characteristics for 21,887 firm-years beginning January 1977 through January 1991 for NYSE- and Amex-listed stocks with necessary data available in Compustat

Variable	Full sample, 21,887 firm-years		Takeover sample, 669 firm-years		T-test for equal means
	Mean	Std. dev.	Mean	Std. dev.	
Abnormal return (in % over 4 years)	0.0117	0.1777	0.0256	0.1776	2.06
Sales growth (4-year average)	0.0867	0.1272	0.0768	0.1127	-2.30
Liquidity (4-year average)	0.2470	0.2061	0.2767	0.2190	3.57
Debt/equity (4-year average)	0.7410	1.8455	0.6063	0.9939	-3.43
Market/book (4-year average)	1.4814	1.7316	1.3718	1.0951	-2.57
Price/earnings (4-year average)	11.1357	11.0488	10.6688	10.4521	-1.17
Size	5.6291	1.8596	5.1638	1.4927	-8.12
<i>Proportion of firm-years covered by:</i>					
Control share law:					
Date law became effective	0.0577	0.2332	0.0553	0.2287	-0.28
Supreme Court upheld IN law (April 1987)	0.0404	0.1969	0.0389	0.1934	-0.21
Business combination law:					
Date law became effective	0.1561	0.3629	0.1345	0.3415	-1.66
Circuit Court upheld WI law (June 1989)	0.0742	0.2622	0.0314	0.1745	-6.33

Poison pill					
1986	0.0811	0.2731	0.0957	0.2944	1.30
1987	0.0632	0.2433	0.0912	0.2881	2.57
1988	0.0611	0.2396	0.0658	0.2481	0.49
1989	0.0589	0.2354	0.0822	0.2749	2.24
1990	0.0562	0.2304	0.0628	0.2427	0.71
1991	0.0495	0.2169	0.0329	0.1785	- 2.43
	0.0462	0.2100	0.0075	0.0862	- 11.00
Successful takeover (= 1)	0.0306	0.1721			
Auction (= 1 if multiple bidders)			0.2407	0.4278	
Cash (= 1)			0.6741	0.4690	
Tender offer (= 1)			0.4499	0.4979	
Market-adjusted premiums in successful takeovers	0.0107	0.0828	0.3489	0.3263	

Abnormal return is the average daily prediction error in percent from a market model regression using the CRSP value-weighted portfolio, where the parameters are estimated in the year before the measurement period (year - 5 relative to the forecast fiscal year). Sales growth is the average growth rate in sales over the four years before the fiscal year. Liquidity is the four-year average of net working capital divided by total assets. Debt/equity is the four-year average ratio of debt to equity. Market/book is the four-year average ratio of the year-end market value of stock to the book value of equity. Price/earnings is the four-year average ratio of the year-end stock price to earnings per share. Size is the log of total assets at the start of the forecast period. The control share law and business combination law dummy variables are both defined in two ways: (a) they equal one if the forecast fiscal year occurs after the effective date of the law for the state in which the company is incorporated, or (b) they equal one if that company's state has a law which is effective for the forecast period and there has already been a major judicial precedent indicating that such a law is likely to be upheld if challenged. Poison pill equals one if the firm has a shareholder rights plan in effect before the forecast period. The dummy variables 1986 through 1991 equal one in these years to show variation in the last part of the sample period. Successful takeover equals one when an initial tender offer or merger proposal that leads to a successful takeover occurs during the forecast firm-year. Auction equals one when more than one bidder tries to takeover the target firm. Cash equals one when the payment to target shareholders is in the form of cash. Tender offer equals one when the winning bid occurs in the form of a tender offer. The market-adjusted premium in successful takeovers is the compounded difference in the returns to target firm's stock and the CRSP value-weighted NYSE/Amex market portfolio during the period from 20 trading days before the first merger proposal or tender offer through five trading days after the successful offer; it equals zero for firm-years in which no takeover occurs.

for 24.1% of the 669 takeovers, cash is the only consideration paid in 67.4%, and tender offers are used 45.0% of the time. Net-of-market premiums received by target shareholders average 34.9%. We refer to this below as the conditional mean premium, conditioning on the occurrence of a successful takeover. We also calculate an unconditional mean premium using all 21,887 firm-years, assigning a takeover premium of zero to all of the firm-years with no initial announcement leading to a successful takeover. There are 669 firm-years with a mean premium of 34.5%, and 21,218 firm-years with a mean premium of zero, aggregating to a population or unconditional mean premium of 1.1% per firm-year.

The unconditional mean premium suggests the economic gain that could be forgone if the takeover market remains dormant, although future losses would be mitigated through alternate ways of regulating managerial slack, such as proxy fights or incentive pay schemes. The demise of the takeover market is often cited as a justification for greater involvement by institutional investors in corporate decisions and governance. Partly in response to the demands of institutional investors, the S.E.C. revised its proxy statement rules in October 1992 to expand disclosure of management compensation, to ease disclosure rules for communications among investors, and to facilitate partial slates of nominees to corporate boards.

4. New evidence on the wealth effect of poison pills

4.1. Poison pill announcement effects, 1983–91

In contrast to event studies of the wealth effects of adoptions of state antitakeover laws, which have been conducted for most of the firms covered by such laws, existing event studies of pill adoptions have been done for only the earliest one-quarter of all the pills adopted to date. We update the event studies of Ryngaert (1988) and Malatesta and Walkling (1988), drawing on the entire population of 1,577 original poison pills adopted through December 1991. Of these, we exclude 118 with inadequate stock return data available from CRSP.

The wealth effect of a pill adoption is a combination of a stock price decline amounting to the expected present value of future takeover premiums forgone due to deterrence, offset by the expected present value of any increase in premiums due to a gain in bargaining power versus bidders. In addition, prices can change due to a revelation of management's private information. Some managers probably wait to adopt pills until they know that an offer is imminent. They could be engaged in merger talks, for which public disclosure is not usually required, they could be aware that an investment bank is shopping the company, or they could anticipate a takeover based on undisclosed corporate performance. A good indication that investors suspect managers of basing their

decisions to adopt pills on private information is that 56% of our *Dow Jones News Retrieval* stories on pill adoptions include a denial by management of knowledge of any pending takeover.

Information effects make it difficult to measure deterrence using stock prices. To address this problem, previous researchers have analyzed the subset of pill adoptions made after the market is already aware of a merger interest. Presumably, management's informational advantage regarding any outstanding merger interest is diminished when a merger interest has already been revealed. We refer to these cases below as the 'control premium' subset of pill adoptions. Even in this subset, however, it can be difficult to separate deterrence and information effects. During merger negotiations, the announcement of a pill is tantamount to a disclosure of the bad news that a deal has yet to be struck, so a decline in stock price does not necessarily imply deterrence. Subject to this caveat, our evidence on the wealth effects of pill adoptions confirms earlier studies (i.e., it is consistent with deterrence, but not strong deterrence).

Table 2 shows estimates of the effect of poison pill adoptions on shareholder wealth, measured by the three-day return on the common stock of the issuing firm, centered on the day of the announcement. To adjust for market movements, we subtract the prediction from a market model regression using the CRSP value-weighted portfolio, as estimated over the previous 253 trading days. If the *DJNR* time-stamp for a story is later than 4:00 p.m., we treat the following day as the announcement date. When we cannot find a *DJNR* mention, which happens in 8.5% of the cases, the announcement date is from the *Wall Street Journal Index* or from *Corporate Control Alert*. The *DJNR* text often provides information about coincident confounding news, pending takeover proposals, and rumors. We also check the *Wall Street Journal Index* for such items. Our sample includes 242 control premium cases (16.6% of the sample) in which the adoption was preceded within 12 months by a 13D filing, a published story about merger rumors, or an explicit takeover bid, and where no M&A event was announced simultaneously with the pill. There are 100 cases (6.9% of the sample) with a simultaneous M&A announcement.

Table 2 contains two estimates of average abnormal returns associated with announcements of poison pill adoptions. The fourth column shows the average abnormal return for each subset of the sample, and the second column shows the coefficients from a multiple regression that accounts for interaction effects among the variables. Most of the results are similar, but we focus on the multiple-regression estimates because they account for interactions among the subsamples.

The regression in Table 2 includes dummy variables for control premium cases, for simultaneous M&A news, for NASDAQ listing, and for the year of adoption. We include adoption-year dummy variables in case there is a different market reaction to more recent pills. Principally, we find that a prior control premium decreases the pill announcement effect by -2.09% (t -statistic of

Table 2

Least-squares regression showing the average wealth effect of initial poison-pill rights issues announced in the period 1983–91 for NYSE, Amex, and NASDAQ-listed stocks, by whether a 13D filing, rumors of a bid, or an explicit takeover bid make it likely that a control premium is built into the issuer's stock price at the time of announcement. Another variable measures whether merger and acquisition (M&A) news is announced at the same time as the pill. A different dummy variable indicates NASDAQ-listed firms. Dummy variables for the year of adoption represent the intercepts for each year (to distinguish the early pills from the later ones). The dependent variable is measured as the cumulative abnormal return over days -1 to $+1$ using a market model estimated with the CRSP value-weighted market portfolio during the year (trading days -255 to -2) prior to the pill announcement. The third column shows the proportion of the sample for which the respective dummy variables are nonzero. The right column shows the average abnormal return for each subset of the sample. White's (1980) heteroskedasticity-consistent t -statistics are in parentheses.

Variable	Regression coefficients (t -statistic)	Proportion of sample	Average return (t -statistic)
Prior control premium ($= 1$)	-0.0209 (-6.60)	0.1659	-0.0155 (-5.20)
Contemporaneous M&A news ($= 1$)	0.0326 (3.16)	0.0685	0.0404 (4.81)
NASDAQ-listed firm ($= 1$)	-0.0076 (-2.40)	0.3633	-0.0022 (-0.92)
<i>Year-by-year intercepts</i>			
1984	-0.0232 (-1.71)	0.0062	-0.0285 (-1.56)
1985	0.0068 (0.81)	0.0254	0.0017 (0.23)
1986	0.0032 (1.31)	0.2255	0.0005 (0.20)
1987	0.0038 (0.81)	0.0987	0.0022 (0.45)
1988	0.0098 (3.29)	0.2358	0.0072 (2.98)
1989	0.0023 (0.66)	0.2282	-0.0014 (-0.50)
1990	0.0036 (0.74)	0.1206	0.0032 (0.71)
1991	0.0087 (1.18)	0.0596	0.0060 (1.09)
\bar{R}^2	0.048		
Standard error of regression	0.0537		
Degrees of freedom	1,448		

– 6.60). This finding confirms Ryngaert (1988), who reports an average return of – 1.51% (t -statistic of – 2.77) for his control premium group, implying either deterrence or bad news about the progress of takeover negotiations.

NASDAQ companies have lower announcement effects, all else equal, by – 0.8%, with a t -statistic of – 2.40. This result could reflect larger fractional insider ownership. McWilliams (1990) shows that the stock market reaction to adoptions of antitakeover charter amendments is negatively related to the fraction of insider ownership, based on 763 announcements by exchange-listed firms in the period 1980–84. Brickley, Coles, and Terry (1994) show that the stock market reaction to 247 pill adoptions depends on the composition of the board of directors: firms with insider-dominated boards had more negative announcement effects. They find no evidence that the takeover rate is lower for firms with insider-dominated boards. We include only NYSE- and Amex-listed companies in our analyses of deterrence and premiums (Tables 3 and 4), and this differential announcement effect for NASDAQ companies implies that our conclusions may not apply to smaller companies.

Among the yearly dummy variables, the only negative intercept estimate (– 2.3% t -statistic of – 1.71) is for 1984, which covers the first nine pills (including two from 1983). The earliest adopters may have been more likely to use poison pills to the detriment of target stockholders, or the market may have initially overestimated the deterrence effect of pills. Finally, average abnormal returns are higher by 3.3% (t -statistic of 3.16) when pills are announced jointly with M&A news. This effect is small compared to initial takeover announcements because these are usually cases in which a pill is announced after an initial takeover announcement, often as part of an announcement of management's response to a takeover attempt.

4.2. Can deterrence be measured for poison pills?

Fig. 5 shows the endogenous nature of decisions to adopt poison pills (i.e., managers often adopt pills when a takeover attempt is unusually likely). It plots (in daily event time) the cumulative proportion of pill adopters that received an initial takeover attempt within one year on either side of the adoption date. The sample consists of all 960 adoptions of original poison pills by exchange-listed firms in the period 1983–90. We exclude pills adopted after 1990 to ensure a year's worth of M&A data after every adoption. Takeover attempts include the first occurrence of a merger proposal, merger agreement, or interfirm tender offer. The plotted value for the n th event day is a count of the total number of takeover announcements in a period starting 250 trading days before an adoption and ending with the n th day relative to the adoption. This count is divided by the sample size, so the plotted values represent the cumulative proportion of pill adopters put into play within one year of adoption.

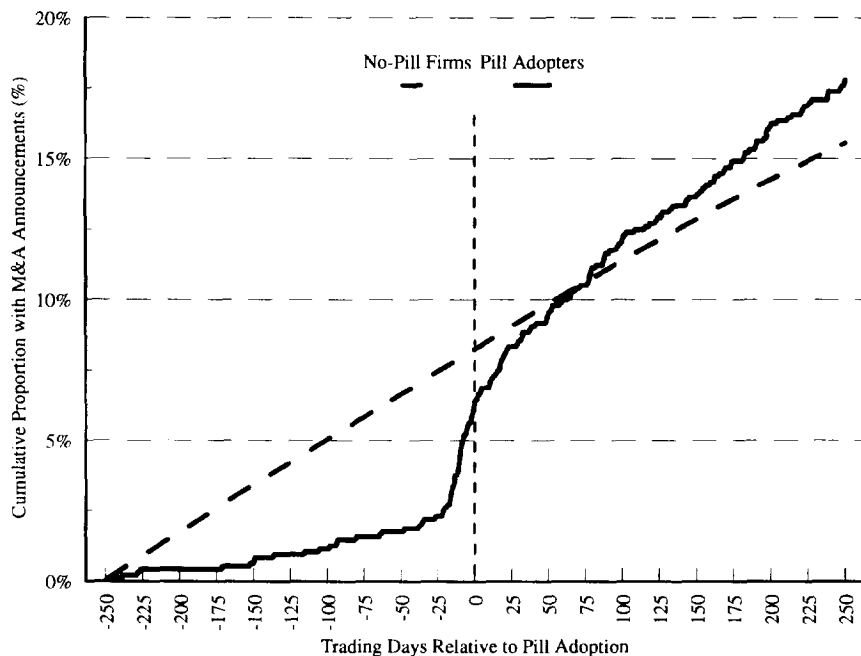


Fig. 5. Event study showing the cumulative proportion of firms receiving merger and acquisition (M&A) announcements within a year of the date of adoption of a initial poison pill, for 960 exchange-listed firms that adopted pills in the period 1983–90. The dotted line shows the cumulative fraction, over corresponding two-year periods, for exchange-listed firms that never adopted a poison pill. M&A announcements include merger proposals, merger agreements, and inter-firm tender offers, regardless of success.

For comparison, we also plot the average proportion of no-pill firms that are put into play over coincident intervals. No-pill firms are all those exchange-listed firms that did not adopt a pill. For each of the 960 adoptions, we calculate the cumulative proportion of no-pill firms in play as of the n th day relative to that adoption. We average these values across events to produce a series of n (plotted) values that can be interpreted as the proportion of firms expected to be in play as of each event day, allowing for secular variation in takeover frequency and an uneven distribution of pill adoptions over time. Since two-thirds of the exchange-listed firms are in the no-pill group, the cumulative proportion of no-pill firms that are put into play should be a smooth and continuous curve over any given two-year period. Because we average these over 960 pill-adoption periods to get the cumulative proportion of no-pill firms that are put into play in the typical period, our expected values plot as a straight line.

The tendency for managers to adopt pills when the likelihood of a takeover is unusually high shows up in Fig. 5 as a steeper slope for pill firms (solid line) than for no-pill firms (dashed line) near pill adoptions. The proportion of pill adopters that are in play increases from 2.4% one month before (day -25) to 19.4% one year after (day 250). Since the corresponding proportions for no-pill firms are 7.8% and 16.2%, the change among pill firms is about double that of no-pill firms. Most of the difference arises near the pill adoption, with 6.4% of pill adopters put in play between days -25 and 25 compared with 1.7% of no-pill firms. Fig. 5 also shows a curious deficit in merger and acquisition activity before poison pills are adopted, compared with the coincident experience among no-pill firms (2.7% versus 7.8% as of day -25). This deficit is largely eliminated during the month before adoption and eliminated about three months after adoption. The proximity of the endpoints of the two curves (at the far right of Fig. 5) is consistent with a long-run rate of receiving takeover bids that is largely independent of pill adoption, and consistent with the results of our prediction analysis in Section 5. Ken French has pointed out that the low level of M&A activity before pill adoptions may be an artifact of the decision to adopt pills when management perceives that M&A activity is likely, given that the long-run rate of receiving takeover bids is not strongly related to pill adoption.

This tendency for managers to adopt poison pills when takeovers are unusually likely is less of an issue with adoptions of state antitakeover laws because these laws are less under the control of a target firm's management, although they can switch their state of incorporation, lobby for new legislation, or opt out of existing legislation. Romano (1987) notes that antitakeover laws are sometimes adopted to protect a particular firm that is already in play. Since all of the firms domiciled in that state become covered, though few are in play at the time, law adoptions are endogenous for a small fraction of firms. Thus, in Section 5, we implement a two-stage statistical procedure to address endogeneity in pill coverage, but use a standard procedure for law coverage.

5. Direct evidence of deterrence

5.1. Previous studies

Pound (1987) provides direct evidence that takeover frequency is lower for firms with antitakeover charter provisions. He compares the post-adoption takeover experience of a sample of 100 NYSE firms that adopted supermajority and classified board charter amendments (as a package) in the period 1973–79 and the takeover experience of a time-matched sample of 100 NYSE firms that did not adopt either type of amendment. Over periods that run from adoption through 1984 (averaging eight years in length), the takeover frequency is 28% for adopters and 38% for nonadopters. Ambrose and Megginson (1992)

estimate a logit model using cross-sectional data on a sample of 117 exchange-listed target firms in the period 1981–86 and a time-matched sample of 214 exchange-listed firms. Their model includes (among other things) dummy variables for whether each firm has a classified board, fair price charter provision, dual class capitalization, blank check preferred stock authorization, or poison pill. Preferred stock authorizations provide the only evidence of deterrence. Bhagat and Jefferis (1993) study classified board charter provisions, poison pills, and fair price amendments adopted in 1984–85 and test whether they deterred takeovers over the following two years for 344 firms, and find little evidence that takeover defenses reduce takeover activity.

Other prediction studies do not include antitakeover measures as predictors. Hasbrouck (1985) uses a logit model to predict takeovers for a sample of 86 targets and a sample of 344 time-, size-, and industry-matched nontargets, and finds that larger market/book ratios and larger size reduce the likelihood of a takeover, but that liquidity and leverage are unimportant. Palepu (1986) provides logit estimates based on a sample of 163 target and 256 nontarget firms in the period 1971–79 using the accounting and stock-based predictors described in Section 3. He obtains negative coefficient estimates for abnormal return, sales growth, leverage, and size, so these reduce the likelihood of a takeover. He obtains a positive coefficient estimate for a growth-resource mismatch dummy variable, so mismatched firms face a higher likelihood of a takeover. Market/book, price/earnings, and liquidity do not affect the likelihood of a takeover. Mørck, Shleifer, and Vishny (1988) estimate a probit model using 1980 data for 454 Fortune-500 firms, of which 82 were takeover targets between 1981 and 1985, and find that larger size and q -ratio deter hostile takeovers, but not friendly ones. Mikkelsen and Partch (1989) use a logit model and pool data as of 1973, 1978, and 1983 for 240 exchange-listed firms. They find that larger size and affiliate-firm cross-holdings deter acquisitions, but that leverage and managerial stockholdings do not affect the likelihood of a successful takeover. Shivdasani (1993) estimates a logit model using data on 193 hostile targets and 194 nontarget firms from 1980–88 and finds that size, managerial stockholdings, and affiliate-firm cross-holdings deter hostile takeovers, but that earnings growth and board composition do not matter. Size is the only predictor that is consistently successful in these studies, while mixed success has been achieved with abnormal return, sales growth, leverage, q -ratio, or market/book, and certain ownership variables.

5.2. Predicting poison pills

If managers adopt poison pills when takeovers are unusually likely, as suggested by the evidence in Fig. 5 and Table 2, then this tendency will mask the estimated deterrence effect of pills. If the tendency is severe, pills could appear to cause takeovers. Using a medical analogy, the consumption of aspirin might be

a good predictor of the visible symptoms of the common cold, not because aspirin causes colds, but because people often take aspirin when they feel a cold coming on. Similarly, managers may adopt pills when they feel that a takeover is unusually likely. We mitigate this possibility by following a suggestion of Maddala (1983, p. 246). We first estimate a model to predict pill coverage, using variables that are likely to predict management's choice of whether and when to adopt a pill. This yields 'predictable' and 'surprise' components of pill coverage. The logic is that pills that are a surprise (to researchers) are the ones most likely to have been adopted when management has information about a pending takeover attempt. We hope to isolate the deterrence effect of pill coverage, in a second-stage analysis discussed below, after stripping away the surprise component.

Candidates for inclusion in the first-stage (pill) prediction model are the accounting and stock return variables listed in Table 1, along with law coverage. Since poison pills represent an innovation, we include yearly dummy variables (for 1986–91) to reflect the spread of pill coverage over time. The right panel of Table 3 shows estimates of a probit model that predicts pill coverage for all 21,887 firm-years. Since probit coefficients are difficult to interpret, Table 3 reports the marginal effect of a change in each predictor variable calculated at its sample mean (i.e., similar to the least-squares coefficient in a linear probability model).⁶

Being incorporated in a state with an antitakeover law is a good predictor of whether a firm has a poison pill. A control share law increases the likelihood of having a pill by 2.33% (*t*-statistic of 5.49) and a business combination law increases it by 4.81% (*t*-statistic of 7.88). These variables will be negatively related if one form of protection substitutes for another, and positively related if managers who prefer protection choose to both incorporate in a state with an antitakeover law and also to adopt a pill. The positive sign on these estimates implies that state laws and poison pills are more complements than substitutes.

Among the accounting and stock market predictors, only firm size is both statistically and economically significant. Larger firms are less likely to be acquired, as detailed in the next section, and more likely to have pills. An increase of just \$1.7 million (*t*-statistic of 20.79) in total assets, which average \$280 million, increases the probability of having a pill by 1%. For the average firm-year in our sample, this means an increase from 8.1% to 9.1% in the probability of having a pill. This positive relation between size and the probability of pill adoption is understandable if there are economies of scale in buying pill protection due to fixed costs in adopting pills, but the rapid trend toward

⁶See, for example, Greene (1993, p. 639) for a description of this calculation. The predictions from a probit model estimated using data after 1983 (when poison pills were invented) are virtually identical to those from the full sample of firm-years.

Table 3

Probit estimates of the dependence of takeover probability on accounting and stock price data, state antitakeover laws, and poison pills (left panel), and the dependence of poison pill adoptions on accounting and stock price data and state antitakeover laws (right panel), using pooled time-series cross-sectional data for exchange-listed firms and for fiscal-year forecast periods beginning January 1977 through January 1991 (21,887 firm-years). The dependent variable is either a dummy that equals one if a successful tender offer, merger proposal, or merger agreement is announced during the forecast period, or a dummy that equals one if a firm adopts an original poison pill during the forecast period. The predictor variables are all defined as of the start of the forecast period.

	Takeover dummy		Pill adoption dummy	
	Probit coefficient	Adjusted <i>t</i> -statistic	Probit coefficient	Marginal effect <i>t</i> -statistic
<i>Poison pill</i>				
Predicted pills (from probit)	0.345	4.60		0.0234
Surprise pills (error from probit)	-0.735	-2.01		-0.0498
	0.418	5.23		0.0283
Control share law	0.184	1.55	0.277	0.0124
Business combination law	0.267	1.74	0.573	0.0181
Abnormal return (in % over 4 years)	0.177	1.42	-0.054	0.0120
Sales growth (4-year average)	-0.086	-0.51	-0.450	-0.0059
Liquidity (4-year average)	0.073	0.64	0.453	0.0048
				5.49
				7.88
				-0.46
				-3.28
				4.59
				0.0233
				0.0481
				-0.0045
				-0.0378
				0.0380

Debt/equity (4-year average)	-0.024	-1.30	-0.0017	-0.011	-0.99	-0.0009
Market/book (4-year average)	-0.012	-0.70	-0.0008	0.011	0.96	0.0009
Price/earnings (4-year average)	-0.001	-0.62	-0.0001	0.003	2.33	0.0003
Size	-0.048	-3.24	-0.0032	0.231	20.79	0.0194
Constant	-1.602	11.81	-0.1086	-4.997	-31.39	-0.4199
1986	0.216	1.61	0.0147	1.188	7.52	0.0998
1987	0.155	1.17	0.0105	2.374	17.29	0.1995
1988	0.300	2.20	0.0204	2.577	18.85	0.2165
1989	0.234	1.41	0.0159	2.866	21.04	0.2408
1990	-0.114	-0.58	-0.0077	2.689	18.22	0.2260
1991	-0.681	2.58	-0.0462	2.746	18.19	0.2307
Log-likelihood		-2922.89			-3291.12	
Degrees of freedom		21,869			21,871	

Coefficient and *t*-statistic for the *poison pill* dummy variable are from a regression including all the other explanatory variables, except predicted pills and surprise pills. By definition, the sum of the predicted and surprise components of pills equals '*poison pill*'. For other variable definitions, see the note to Table 1. The marginal effect column transforms the probit coefficient into the marginal effect of the variable on the estimated probability, evaluated at the sample means of the explanatory variables. *T*-statistics in the takeover prediction model have been adjusted to account for the fact that explanatory variables are estimates from another statistical model (i.e., the pill prediction model in the right panel); see Maddala (1983, p. 247).

in-house and local counsel to advise on pill adoptions (seen in Fig. 4) suggests that the costs of adopting a pill, fixed or otherwise, are small. A positive relation could also occur if small firms have a natural or incidental immunity, perhaps due to ownership structures that are more concentrated, so that their demand for pill protection is low. By this reasoning, however, the demand for pill protection would be lower for larger firms because (as detailed below) size itself seems to be a natural or incidental deterrent.

The next-best predictors after size are sales growth and liquidity. A 1% increase in the probability of having a pill requires a decrease in sales growth of 26% per year (t -statistic of -3.28), where typical sales growth is 8.7% per year. The same effect requires an increase of 27% (t -statistic of 4.59) in the ratio of net liquid assets to total assets, which would involve doubling the typical ratio of 24.7%. We do not judge either of these effects to be economically important.

5.3. *Predicting takeovers*

We test for the deterrence effect of antitakeover measures by including dummy variables defined on antitakeover coverage in a takeover prediction model. The power of this research design depends on dispersion in antitakeover coverage. At each time, we want some firms to be covered, while others are not. Because half of all exchange-listed firms became covered *en masse* when Delaware adopted its business combination law in January 1988, the deterrence effect of business combination laws could show up as a post-1987 drop in takeover frequency, rather than as a firm-specific phenomenon. Conversely, a late-1980s shift in takeover frequency, perhaps due to recession or regulatory intervention in credit markets, could show up as deterrence due to business combination laws. Our review of the events of the late 1980s in Section 2 favors the latter interpretation and persuades us to control for year-by-year variation in takeover frequency. Thus, our dummy variables for pills and laws tell us whether antitakeover measures had an effect *beyond* a probable secular decline in takeover activity. With one exception noted below and discussed in Section 7.2, our conclusions do not depend on the inclusion of yearly dummy variables.

The left-hand panel of Table 3 shows estimates of a model that predicts takeovers among exchange-listed firms with fiscal years beginning 1977–91 (the sample described in Section 3). We predict a dummy variable equaling one if a successful merger proposal, merger agreement, or interfirm tender offer is announced during a fiscal year. The predictors include dummy variables for coverage by antitakeover measures, seven accounting and stock market predictors, and six yearly dummy variables (1986 through 1991).

The first row of Table 3 shows that the marginal deterrence effect of a pill is 2.34% (t -statistic of 4.60). A literal interpretation of the positive sign on this estimate is that a pill increases the likelihood of a takeover, but a more plausible interpretation is that pills tend to be adopted in anticipation of a takeover

attempt, and that this (private) information is not captured by the (public) accounting and stock market factors that we include. To deal with any tendency for managers to adopt pills when a takeover is unusually likely, we break the dummy variable for pills into surprise and predictable components, based on estimates of the first-stage pill prediction model reported in the right panel of Table 3. We use these components as predictors in a second-stage takeover prediction model, reported in the left panel of Table 3. Thus, the predictable component of pill coverage is an estimate of the probability of pill coverage, and the surprise component is the difference between the pill dummy variable and this prediction.

The coefficient estimate for the surprise component of pills is positive, as expected, and implies that the annual probability of takeover is higher by 2.83% (t -statistic of 5.23) when a pill is a total surprise (i.e., the predicted probability of having a pill is zero, based on publicly available accounting and stock price data). The corresponding estimate for the predictable component of pill coverage is negative and implies that the annual probability of takeover is lower by 4.98% (t -statistic of -2.01) when a pill is completely predictable (i.e., the prediction of pill coverage = 1.0). None of the pills in our sample are completely predictable, however. Even in 1991, the year with the highest average predicted pill coverage (44.2%), the pill-induced decrease in annual takeover probability is only 2.15% per year (4.87% times 44.2%) for the typical firm.⁷ Moreover, when we omit the yearly dummy variables that control for secular variation, the t -statistic for the predictable component of pill coverage falls to -0.84 (see Section 7.2).

The estimated coefficients for control share laws and business combination laws are positive (t -statistics of 1.55 and 1.74), so antitakeover laws appear to increase takeover probabilities slightly, holding other factors constant (including secular variation). Since the law adoptions are motivated by takeover expectations for only a few firms, we conclude from these estimates that managers have not used these laws to deter takeovers systematically, and infer that antitakeover laws played a minor role in depressing the 1980s market for corporate control.

Few of the accounting and stock market performance variables are reliable predictors of takeovers. Size is the one exception. Larger firms are less likely to be acquired (t -statistic of -3.24) and the economic significance of the estimated

⁷The standard errors have been corrected to account for the fact that this explanatory variable is an estimate from another statistical model. See Maddala (1983, p. 247). In earlier drafts of our paper we reported uncorrected logit estimates that differed little from the corrected probit estimates reported here. Since the same variables are used to predict pills and then takeovers, there is substantial collinearity between the pill predictions and the other variables in the takeover prediction model (except for surprise pills). In fact, identification is achieved in these models only because of the nonlinearity of the prediction models.

coefficient is not trivial. A decrease of \$23 million from the average level of total assets (of \$280 million) increases the annual likelihood of takeover by 1%. The probability of takeover falls reliably below the sample period norm only for fiscal years that start in 1991 (t -statistic of -2.58). As discussed in Section 2, these forecast periods follow the bankruptcy of Drexel Burnham Lambert and coincide roughly with the onset of recession and the credit crunch.

6. Predicting takeover premiums

Table 4 shows estimates of the dependence of takeover premiums on antitakeover measures, the accounting and stock market performance variables (described in Section 3), and certain takeover characteristics. Huang and Walkling (1987) estimate a regression model in a sample of 204 exchange-listed target firms acquired by merger or tender offer in the period 1977–82, and find that premiums are reliably higher in all-cash transactions, but not in tender offers. Bradley, Desai, and Kim (1988) estimate a similar model for 236 tender offers for exchange-listed firms in the period 1963–84, and find that premiums are higher in auctions. Thus, we include dummy variables defined by whether the transaction is (1) an auction with multiple, publicly revealed bidders, (2) an all-cash transaction, and (3) a tender offer.

The right panel of Table 4 reports estimates based on the sample of 669 successful takeover targets (conditional premiums) and the left panel of Table 4 reports estimates of the same model using all 21,887 firm-years with the takeover premium set to zero in nontakeover firm-years (unconditional premiums). The estimated effect of antitakeover measures on the unconditional premium (left column) is of interest because it is a net effect of a decrease in the premium if antitakeover devices deter offers and an increase if they increase premiums in successful offers. The first line of Table 4 shows (*in italics*) estimates for the coefficient on a dummy variable that combines the predictable and surprise components of pill coverage (described in the previous section). These estimates imply that both conditional and unconditional premiums are higher with a pill in place. Since we control for the (positive) effect of auctions on premiums, these estimates understate the effect of pills on premiums to the extent that pills cause auctions, as their proponents claim.⁸

⁸Using probit models, similar to Table 3, to predict which successful takeovers involve auctions shows an insignificant effect on the predictable component of pills (t -statistic of -1.77) and the surprise component (t -statistic of 0.14), but reliable positive effects of control share laws (t -statistic of 3.50) and business combination laws (t -statistic of 2.10).

Table 4

Least-squares estimates of the relation of takeover premiums with state antitakeover laws, poison pills, accounting and stock price data, and characteristics of the takeover. Takeover premiums are measured as the compounded difference in the returns to target firm's stock and the CRSP value-weighted NYSE/Amex market portfolio during the period from 20 trading days before the first merger proposal or tender offer through five trading days after the successful offer. In the left panel, the sample consists of pooled time-series cross-sectional data for exchange-listed firms with fiscal-year forecast periods beginning January 1977 through January 1991 (21,887 firm-years). The right panel uses the 669 firm-years when a subsequently successful takeover begins. The predictor variables are all defined as of the start of the forecast period. The dates when important legal precedents established the effectiveness of control share laws (April 1987) and of business combination laws (June 1989) are used to define coverage by these laws (i.e., the dummy variable equals one for firm-years in which the firm was covered by a law that had already been certified by a major judicial decision). The *t*-statistics use White's (1980) heteroskedasticity-consistent standard errors.

Predictors	Full sample		Successful takeover sample	
	Coefficient	<i>t</i> -statistic	Coefficient	<i>t</i> -statistic
<i>Poison pill</i>	0.0144	4.38	0.1627	2.95
Predicted pills (from probit)	0.0024	0.30	0.4588	1.60
Surprise pills (error from probit)	0.0151	4.33	0.1453	2.31
Control share law	0.0080	1.96	0.1094	1.61
Business combination law	0.0054	2.22	0.0269	0.29
Abnormal return (in % over 4 years)	0.0059	1.71	0.0439	0.61
Sales growth (4-year average)	0.0048	1.27	0.3964	3.48
Liquidity (4-year average)	0.0096	3.18	0.0813	1.33
Debt/equity (4-year average)	− 0.0003	− 1.77	− 0.0167	− 1.37
Market/book (4-year average)	− 0.0005	− 2.52	− 0.0212	− 2.00
Price/earnings (4-year average)	− 0.0001	− 1.46	− 0.0012	− 0.97
Size	− 0.0008	− 2.38	0.0020	0.20
Constant	0.0137	5.31	0.2048	3.02
1986	0.0020	0.92	− 0.1214	− 3.74
1987	− 0.0012	− 0.48	− 0.1665	− 2.78
1988	0.0119	2.66	0.0198	0.29
1989	− 0.0007	− 0.18	− 0.2400	− 2.30
1990	− 0.0103	− 3.97	− 0.3008	− 4.42
1991	− 0.0144	− 4.21	− 0.3300	− 2.08
Auction (= 1 if multiple bidders)			0.1137	3.61
Cash (= 1)			0.0857	3.53
Tender offer (= 1)			0.1296	5.27
\bar{R}^2	0.005		0.192	
Standard error of regression	0.0826		0.2933	
Degrees of freedom	21,869		648	

Coefficient and *t*-statistic for the *poison pill* dummy variable are from a regression including all the other explanatory variables, except predicted pills and surprise pills. The other coefficients from that regression are not shown. The pill predictions come from the model shown in Table 3. By definition, the sum of predicted and surprise pills equals 'poison pill'. For other variable definitions, see the note to Table 1.

The surprise component of pill coverage is reliably related to higher premiums. For pills that are a total surprise (i.e., the predicted probability of pill coverage = 0.0), conditional premiums are higher by 14.5% (*t*-statistic of 2.31) and unconditional premiums by 1.5% (*t*-statistic of 4.33). Not only are takeovers more frequent the more surprising is pill coverage (from Table 3), average premiums are higher as well. With the predictable component of pills, the most we can conclude is that shareholders are not harmed, but this conclusion is important in view of the weak evidence in Table 3 that predictable pills deter takeovers. Specifically, the positive effect on conditional premiums is large, at 45.9%, but quite imprecise (*t*-statistic of 1.60). The effect on unconditional premiums is small but positive, 0.24% (*t*-statistic of 0.30), reflecting a large increase in conditional premiums and a smaller decrease in the probability of a takeover.

A control share law raises conditional premiums by 10.9% (*t*-statistic of 16.1) and a business combination law has almost no effect (*t*-statistic of 0.29). Control share and business combination laws have small positive effects on unconditional premiums, 0.8% and 0.5% respectively (*t*-statistic of 1.96 and 2.22). In tests that are not reported here, we find that neither law has an effect on premiums reliably different from zero when coverage is defined using the dates these laws took effect instead of judicial decision dates.

The accounting and stock market performance variables predict premiums better than they do takeovers. Recent sales growth has a modest positive effect on conditional premiums. A deviation in sales growth of 2.5% per year (from a sample mean of 7.7%) increases conditional premiums by 1% (*t*-statistic of 3.48), but unconditional premiums are not reliably related to sales growth (*t*-statistic of 1.27). Although liquidity has no reliable effect on conditional premiums, it is associated with larger unconditional premiums (*t*-statistic of 3.18). A higher market/book ratio reduces conditional premiums (*t*-statistic of -2.00) and unconditional premiums (*t*-statistic of -2.52). Although size has only a small positive effect on conditional premiums, size does predict lower unconditional premiums (*t*-statistic of -2.38). This finding is no surprise since size is the one variable with reliable predictive power for takeovers. The fact that the estimates for yearly dummy variables are generally reliably negative for the years 1989–91 means that takeover premiums became much lower for unprotected targets after antitakeover protection became the norm, suggesting either a shift in the composition of the group of unprotected firms or a growing association between antitakeover measures and management bargaining power versus bidders.

We confirm a result reported by Bradley, Desai, and Kim (1988) with our finding that an auction is associated with an added takeover premium of 11.4% (*t*-statistic of 3.61), compared with their estimate of 13% (*t*-statistic of 4.23). We also confirm a result reported by Huang and Walkling (1987) with our finding that all-cash transactions are associated with an added premium of 8.6%

(*t*-statistic of 3.53), compared with their estimate of 12.9% (*t*-statistic of 3.68). Our evidence contradicts another of their results, however, since we find that tender offers are associated with an added takeover premium of 13.0% of (*t*-statistic of 5.27), after controlling for method of payment, compared with their estimate of -0.2% .

7. Sensitivity analysis

As a check, we estimate alternate specifications for the tests in Tables 2, 3, and 4. In the interests of parsimony, we summarize these tests below but do not report detailed results.

7.1. Hostile takeovers

There are arguments in the literature that hostile takeovers are different from friendly takeovers in the sense that the sources of gain from the two kinds of transactions are likely to be different (for example, Mørck, Shleifer, and Vishny, 1988, 1989). The takeover prediction estimates in Table 3 do not distinguish, however, between hostile and friendly transactions. By definition, antitakeover measures add nothing to management's ability to reject friendly transactions, i.e., those conditioned implicitly on management approval, so including such transactions cannot help us measure the deterrence effect of antitakeover measures. Depending on how well we can measure friendliness, filtering out friendly transactions could reduce noise. Considering how large our sample is, however, our concern over bias outweighs our concern over efficiency. Filtering would introduce a bias if stronger takeover defenses cause a substitution of negotiated for unnegotiated transactions, rather than a reduction in the frequency of transactions. Since some substitution is likely, a test of whether takeover defenses deter hostile transactions alone overstates the costs of antitakeover measures.

Despite these reservations, we estimate the effect of antitakeover measures on hostile takeovers alone using specifications of hostility that are, at least, replicable. Hostility is hard to measure in a replicable way because most initially hostile takeovers end as friendly (negotiated) transactions through a process of negotiation and competition. Thus, the stage at which hostility is measured, within this process, determines whether transactions are classified as hostile. Moreover, these negotiations typically are confidential. When they are revealed before settlement, it is often because one of the negotiating parties sees its bargaining position enhanced by presettlement publicity, and this presettlement publicity is the raw material for any measurement of hostility. We know of no way to define hostility that does not suffer from these practical difficulties.

Nonetheless, we treat takeovers as hostile if:

- (1) during the 12 months before an initial merger or tender offer bid, a 13D statement is filed in which the buyer discloses an intent to seek control, or there are significant merger rumors about the target firm (suggesting an effort to put the firm in play), or there is a targeted share repurchase for more than 5% of the target company's stock;
- (2) the initial bid is either an unnegotiated tender offer for control of the target firm, or a merger proposal specifying a price (a 'bear hug').

Our full sample includes 438 takeovers that fit the first description, with hostility based on pre-bid events, and 172 that fit the second description, with hostility based on the character of the opening bid.

When we replicate our takeover prediction analysis for hostile takeovers, neither of these definitions yields results (not shown here) that differ substantively from those in Table 3, although the smaller number of successful takeovers resulting from these two definitions of hostility results in less precise estimates of all of the coefficients in the model.

7.2. *Controlling for a secular trend*

Our takeover prediction model in Table 3 includes yearly dummy variables for 1986–91 because there is reason to expect a decline due to general business conditions, particularly a cash crunch, in the last few years of our sample period. When we replicate this analysis without yearly dummy variables, the estimated effects of the state antitakeover law variables are stronger. *T*-statistics for the control share and business combination law variables in the pill prediction equation are 19.16 and 37.78 (compared with 5.49 and 7.88 in Table 3). The marginal effect of coverage by control share and business combination laws increases the probability of pill coverage by 10.14% and 15.14%, respectively (compared with 2.33% and 4.81% in Table 3). The *t*-statistic for the predictable component of pill coverage in the takeover prediction model is smaller (–1.17 compared with –2.01 without the yearly dummy variables). Thus, there is no reason to believe that the yearly dummy variables substitute for the effects of the antitakeover laws or for pills to reduce the evidence for deterrence in the takeover prediction models.

Since poison pills were not introduced until 1983, and the significant legal precedents supporting state antitakeover laws also occurred after 1983, we also estimate our tests using only firm-years after 1983. The negative coefficient of the predicted component of pills and the positive coefficients for the surprise components of pills and for coverage by control share or business combination laws in the takeover prediction models are all larger and the *t*-statistics are

larger, but the general conclusions described in Sections 5 and 6 are unchanged.

Also, Table 3 reports yearly dummy variables for only the last six years in the sample period in Table 3 to economize on the results we report. When we estimate a version of the takeover prediction model that includes dummy variables for all years (and no constant), the estimates of the yearly intercepts from 1977–85 are between -1.379 and -1.628 with no discernible pattern. Constraining these coefficients to be equal in Table 3 does not affect the estimates of the other coefficients in the model.

Finally, the failure of the accounting and stock market performance measures, other than size, to help explain takeovers is due neither to the inclusion of the poison pill nor state law variables nor to the inclusion of the yearly dummy variables. We estimate models using only the accounting and stock market variables separately for each year from 1977–91, and find only size helps predict takeovers in most years.

7.3. Wealth effect of pills

Szewczyk and Tsetsekos (1992, Table 4) provide evidence that the wealth effect of the adoption of the Pennsylvania antitakeover law was less negative for firms that already had a poison pill, the presumption being that the added deterrence from a state antitakeover law is lower in these cases. Reversing this, we test whether the wealth effect of the adoption of a pill is less negative for firms that are already covered by a law. Adding a dummy variable to the regression in Table 2, we find that the stock price reaction to pill announcements does not depend on whether a firm is already covered by a law (coefficients of 0.00003 and -0.0009 , with t -statistics of 0.01 and -0.19 , for business combination and control share laws, respectively). Also, since hiring expensive outside counsel to implement a pill could signal litigiousness, we included dummy variables equaling one if the adopting company retained a big-two or next-seven law firm, but these coefficients are not different from zero (coefficients of -0.0020 and 0.0026 , with t -statistics of -0.57 and 0.41 , respectively).

7.4. Effective dates for state antitakeover laws

We estimate versions of the models in Table 3 using the dates that the individual state antitakeover laws took effect (when these precede the relevant judicial decision dates). These results show even less evidence of deterrence for state laws, probably because they include more firm-years in the mid-1980s when takeover activity is high. Using the judicial decision dates, which are closer to the beginning of the crash in the market for corporate control, as we have done in Tables 3 and 4, gives the state laws a better chance to explain the time

series and cross-sectional behavior of takeover rates, but that advantage still yields no reliable evidence of deterrence.

7.5. Morning-after pills and success rates

The endogeneity problem, or the tendency for managers to adopt poison pills shortly before impending takeover attempts (seen in Fig. 5 and discussed in Sections 4 and 5), would be worse if our measurement interval were narrower than one year. Fig. 5 shows that many poison pills are adopted close to the time of tender offers or merger bids. We call these ‘morning-after pills’. When we include these pills in our takeover prediction model, after subtracting the predictable component of these pills, they have positive effects similar to those of the surprise component of pill coverage, but about half as large. As for takeover premiums, in a regression similar to Table 4 the coefficient for the surprise component of morning-after pills is 0.0366 (*t*-statistic of 0.61) for conditional premiums and 0.0277 (*t*-statistic of 4.01) for unconditional premiums. Thus, morning-after pills affect premiums much as does the surprise component of pill coverage.

We do not report on takeover success rates elsewhere in this study, but we find that takeover success rates are lower for morning-after pills than for the predictable and surprise components of pill coverage, which are in place at the start of forecast fiscal years. In Table 3 we predict successful bids within the whole sample. We can predict takeover success rates by replicating our analysis on the subset of firm-years containing initial bids, regardless of success. Doing so, the marginal effect of the surprise component of pills is 4.8% (*t*-statistic of 0.80) and –17.0% (*t*-statistic of –2.95) for the surprise component morning-after pills. Of the 50 firms with morning-after pills, half are acquired in the following 12 months. This compares with a 76% success rate for the 875 firm-years in which a target firm receives a tender offer or merger proposal (resulting in the 669 firm-years with successful takeovers). In summary, although takeover bids are less likely to succeed when target firm management adopts a pill after the initial bid, expected takeover premiums remain positive even for these pills, so that from the selling shareholders’ point of view, it is never too late to adopt a poison pill, nor is there a clear gain from preemptive adoption.

7.6. Clinical experience

Clinical experience leaves little doubt that some takeovers were deterred by antitakeover measures. For example, there were 27 instances from 1985 through 1990 in which a court upheld a poison pill challenged by a bidder. Since this sample excludes negotiations that settle quickly (before lawsuits are filed or before courts rule), these tend to be deals with difficult negotiations, where pills matter. Of these target firms, 11 remained independent one year after the

decision and 16 were acquired by the plaintiff or some third party.⁹ Clearly, total deterrence is the less common outcome even among these obviously hostile takeovers.

8. Conclusions

If the 1980s merger wave was ended by modern antitakeover measures, evidence of deterrence should be seen in the experience of individual firms following adoption of these measures, and this evidence should survive attempts to control for secular trends in takeover activity. Otherwise, it is more likely that broad-scale political or economic forces are responsible for the demise of the market for corporate control. We find no evidence of deterrence from control share or business combination laws. There is weak evidence of deterrence from poison pills that are predictable based on publicly available information about the firm's performance. Poison pills adopted by firms that do not fit the pattern in the market (which we call surprise pills) are associated with reliably higher takeover rates. This finding probably reflects the choice by management to adopt a poison pill when it has private information that a bid is imminent. The net effect of these two motivations for adopting poison pills is a positive association with takeover rates.

Anecdotal evidence and clinical experience strongly suggest that some potential takeovers were deterred by modern antitakeover measures, but this conclusion is not supported by our systematic evidence. In its decision upholding the Indiana control share law, the U.S. Supreme Court noted that the case against the law ultimately rested on an unsubstantiated empirical claim of deterrence, and our results validate the Court's skepticism.

Our new evidence on how stock prices change with poison pill adoptions does not suggest an economically meaningful degree of deterrence. Controlling for other measures of information that reflect the likelihood of a subsequent takeover bid, only the earliest pills (before 1985) are associated with large declines in shareholder wealth on the adoption of these measures. There is

⁹Firms whose pills were upheld in court, and who stayed independent for a year following the decision (date), include CTS (4/23/86), DeSoto (2/5/90), Household International (1/19/85), Johnson Controls (3/25/85), Phillips Petroleum (6/3/86), Property Trust of America (10/2/89), Revlon (10/9/85), Sea Containers Ltd (11/27/89), Universal Foods (3/18/89), Santa Fe-Southern Pacific (3/11/88), and USG (4/28/88). Firms that were acquired within a year include Allied Stores (10/24/86), Damon (9/16/88), Facet Enterprises (4/15/88), Federated Department Stores (3/18/88), Gelco (11/10/86), Great Northern Nekoosa (12/12/89), Hayes Albion (11/25/86), Holly Farms (12/30/88), Koppers (4/1/88), Milton Roy (11/9/89), Prime Computer (12/20/88), Ryan Homes (10/24/86), Southwest Forest (3/20/85), Staley Continental (5/9/88), T W Services (3/2/89), and Vermont American (10/13/89).

a negative reaction to pill announcements when the firm is already in play through some prior public announcement that is likely to foreshadow M&A activity. This effect could represent either deterrence or negative information about the process of ongoing negotiations.

Our new evidence that takeover premiums are higher when target firms are protected by state laws or by pills suggests that the relative bargaining positions of bidders and targets are altered by these antitakeover devices, raising the costs to the bidder and the gains to the target. This implies deterrence, but we find that target shareholders gain even after accounting for deals that are never completed.

The combination of the event study evidence in Table 2, the previous event studies on antitakeover devices in the literature, and the takeover premium evidence in Table 4 raises an interesting puzzle. Why is there ever a negative reaction to poison pills or to antitakeover laws when, based on the evidence through 1991, the net effect of takeover protection from laws and pills is positive? One possible answer is that the market misestimated the eventual effect of pills and laws, overestimating the costs of deterrence and underestimating the benefits of added bargaining power. Even in an efficient market, after all, we do not presume that investors have perfect foresight about the future evolution of takeover strategies. From this perspective, the evidence we provide about the actual takeover rate and premiums paid as a function of antitakeover devices outweighs the event study evidence in judging deterrence.

The most plausible explanations for the collapse of the market for corporate control at the end of the 1980s are that (1) the spread of modern antitakeover measures made absolute deterrence feasible and that (2) the 1990 recession and credit crunch greatly affected this market. The first explanation is buttressed by our evidence that 87% of exchange-listed firms are now covered by one or another form of modern antitakeover measure (35% by poison pills, 80% by business combination laws, and 24% by control share laws), but refuted by our failure to find direct evidence of deterrence using all available data through 1991. We therefore conclude that the demise of the market for corporate control was caused by secular trends and not the introduction, spread, and legal acceptance of poison pill rights plans, control share laws, or business combination laws. An interesting test to discriminate between these alternate hypotheses will be to see whether the market for corporate control comes back from its recent doldrums. If state laws and pills are effective deterrents, the market will not recover unless firms rescind poison pills and opt out of state law protection (or switch to states like Texas and California that do not have antitakeover laws).¹⁰

¹⁰Since the first version of this paper in January 1993, the mergers and acquisitions market has become much more active. A front page story in the October 14, 1993 *Wall Street Journal* discussed the recent merger boom, and led to a brief summary of this paper on the front page of the October 21, 1993 *Wall Street Journal*.

Appendix: Case law about takeover defenses

The Delaware Supreme Court established the legality of defensive measures in May 1985 when it decided that Unocal's board of directors could exclude Mesa Petroleum from a defensive self-tender offer. This decision set a standard by which courts should focus solely on the nature of the deliberative process followed by the board of directors and ignore the optimality or 'fairness' of the antitakeover measure that they happen to use. According to this (Unocal) standard, there are no legally defective antitakeover devices, only defective deliberations. This ruling explains why the takeover litigation of the 1980s never yielded a list of proscribed defensive measures, and why adjudications of takeover defenses have been commonplace and seemingly repetitive.

Specifically, the Unocal standard took the business judgement rule, the well-established principle that ordinary business decisions ought not be subject to judicial review, and extended it to court decisions regarding antitakeover measures. In takeover situations, however, the standard would allow for 'special scrutiny', where boards of directors would need to show (1) reasonable grounds for believing that a 'danger to corporate policy and effectiveness' existed and (2) that their actions are based on 'good faith and reasonable investigation'. The active involvement of independent, outside directors would show such a good faith deliberation.

The Delaware Supreme Court found an immediate application for the Unocal standard in its 1985 Moran decision, in which it upheld the adoption of a poison pill but noted that '[t]he ultimate response to an actual takeover bid must be judged by the Directors' actions at that time, and nothing we say here relieves them of their basic fundamental duties to the corporation and its stockholders'.¹¹ The Unocal standard was subsequently adopted by most other courts. For example, in 1986, a U.S. District Court blocked a pill adopted by CTS because the board did not adequately consider the bidder's offer and acted without sufficient independent investigation. CTS cured its defect by forming a special committee of outside directors and by obtaining advice from outside counsel and investment bankers, and then adopted another pill, which the Court upheld. In the Buckhorn case in 1987, a U.S. District Court blocked a poison pill and an ESOP-based defense because had the board made reasonable inquiries, it would have discovered that the investment bankers 'relied too much' on management's assumptions and optimistic profit projections in computing fair value.¹²

¹¹Unocal Corp. v. Mesa Petroleum Co., DelSupCt 5 17/85; Moran v Household International Inc., DelSupCt 11/19/85.

¹²Dynamics Corp. v. CTS Corp., DC NIll 6/20/86; Buckhorn, Inc. v. Ropak Corp., DC SOhio, 2/11/87.

The Delaware Supreme Court added a nuance to the Unocal standard in 1986 to address the proper conduct of an auction. In its *Revlon* decision, the Court overturned *Revlon's* lockup agreement with an LBO group led by Forstmann Little. In exchange for the lockup, Forstmann agreed in part to assume certain notes that *Revlon* had issued in a defensive exchange offer several months earlier, and that had fallen in value. 'In reality', the Court said, 'the *Revlon* board ended the auction in return for very little actual improvement in the final bid. The principal benefit went to the directors, who avoided personal liability to a class of creditors [note holders] to whom the board owed no further duty under the circumstances'. The added nuance was that, once the board decided that the firm would not survive in its current form, '[t]he directors' role changed from defenders of the corporate bastion to auctioneers charged with getting the best price for the stockholders at a sale of the company'. The Delaware Supreme Court applied similar reasoning in its *MacMillan* decision in 1989, in which it denounced the 'illicit manipulation of a board's deliberative processes' and overturned a lockup agreement granted to a management buyout group, adding that the board's hostility toward the competing bidder was unjustified once it decided to abandon its restructuring attempts and sell the entire company.¹³

Takeovers were completed so quickly in the 1980s that most court cases were either settled or rendered moot before they were appealed. This left the Delaware Supreme Court with only a few opportunities to reinforce the notion of a corporate bastion. In its *Newmont* decision, in 1987, it allowed a special dividend and rapid open-market purchases (a street sweep) to block a takeover bid. The Court found it significant that these actions did not constitute an abandonment of the corporation's continued existence, that most of the directors were outsiders, and that their investment banker estimated the foreclosed bid to be 8.7% below 'fair' value.¹⁴ This seemed to leave premium-priced offers open to question, but in the *Time-Warner* decision in 1990, the Delaware Supreme Court ruled that *Time's* board of directors could reject a premium offer, saying that 'directors are not obliged to abandon a deliberately conceived corporate plan for a short-term shareholder profit unless there is clearly no basis to sustain the corporate strategy'.¹⁵ The Court also identified several previous lower court decisions that it thought were in error, but which had passed without review, saying that 'the Court of Chancery has suggested that an all-cash, all-shares offer, falling within a range of values that a shareholder might reasonably prefer, cannot constitute a legally recognized 'threat' to shareholder

¹³*Revlon Inc. v. MacAndrews & Forbes Holdings Inc.*, DelSupCt, 3/13/86; *MacMillan*, DelSupCt, 5/3/89.

¹⁴*Ivanhoe Partners v. Newmont Mining Corp.*, DelSupCt, 11/18/87.

¹⁵*Paramount Communications Inc. v. Time Inc.*, DelSupCt, 2/26/90 (oral ruling 7/24/89).

interests sufficient to withstand a Unocal analysis'. This view 'represents a fundamental misconception of our standard of review under Unocal principally because it would involve the court in substituting its judgment for what is a 'better' deal for that of a corporation's board of directors'. Again, the Court was impressed by the deliberative approach taken by Time's directors to a decision 'made only after what could be fairly characterized as an exhaustive appraisal of Time's future as a corporation'.¹⁶

In another noteworthy case, the Delaware Supreme Court acquiesced when the Chancery Court allowed Polaroid to deter an unnegotiated tender offer by Shamrock in 1989. The main issue was Polaroid's creation of an ESOP to hold 14% of its stock, which the lower court did not see as 'unfair'. Rather, it found that Polaroid's directors acted in good faith after a reasonable investigation, noting that all but two of Polaroid's 13 board members were independent outsiders, and that the board met at least six times in a four-month period to review Shamrock's various offers and to discuss appropriate actions with financial and legal advisers.¹⁷ Finally, the Unocal standard applies in the latest decisions. In 1993, the Delaware Chancery Court blocked a lockup provision of the merger agreement between Paramount Communications and Viacom, saying: 'In connection with [Paramount CEO] Davis's efforts to entrench himself through the Viacom merger, Paramount's directors failed adequately to inform themselves of the relevant facts and circumstances. As a result, Davis was able to secure the approval of Paramount's directors for the Lockup Agreements in breach of the directors' fiduciary duties . . . [T]he question finally becomes whether the directors have demonstrated that they were sufficiently informed to have a reasoned basis for favoring Viacom's offer. (Interestingly, regarding the lockup agreement, the Court asked: 'Why such redundancy? They already had pill protection'. The implication is that the Court believed the pill was sufficient to defeat the offer.)¹⁸

As an aside, we should note that courts overturned some pills because their 'flip-in' features were found to conflict with general provisions in state corporation laws that require equal treatment of shareholders.¹⁹ The affected states subsequently modified these provisions to exclude the discriminatory effect of pills, and we ignore these 'pro-pill' antitakeover laws in our analysis.

This review of the important legal decisions regulating the use of defensive measures shows that directors have not been significantly constrained in their

¹⁶The lower court decisions that the Delaware Supreme Court identified as erroneous were *Grand Metropolitan, PLC, v. Pillsbury Co.*, Del. Ch., 12/16/88, and *City Capital Associates v. Interco, Inc.*, Del. Ch., 1988.

¹⁷See *Shamrock Holdings, Inc. v. Polaroid Corp.*, Del. Ch., 1/6/89, and Del SupCt, 3/23/89.

¹⁸*QVC Network v. Paramount Communications*, Del. Ch., 10/21/93.

¹⁹See, for example, *Amalgamated Sugar Co. v. NL Industries, Inc.*, DC SNY, 8/5/86.

use of poison pills (or other measures) to deter takeovers; it also shows that the treatment of antitakeover measures was stable after 1985 (see also Greene, 1991). Consequently, we take the dates that poison pills were adopted to be the dates that they became effective, and estimate the deterrence effect of poison pills as a constant over the sample period. Overall, it leaves us disinclined to attribute the secular variation that we observe in takeover rates to any discontinuity in the legal environment.

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