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Capital Structure and Corporate Control: The Effect of Antitakeover Statutes on Firm Leverage

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ABSTRACT

We find that firms protected by "second generation" state antitakeover laws substantially reduce their use of debt, and that unprotected firms do the reverse. This result supports recent models in which the threat of hostile takeover motivates managers to take on debt they would otherwise avoid. An implication is that legal barriers to takeovers may increase corporate slack.

CORPORATE MANAGERS HAVE DISCRETION over capital structure choices, as the firm's founding shareholders cannot write a comprehensive ex ante contract specifying all future financing decisions. Most capital structure models make the simplifying assumption that managers choose capital structure in the interests of shareholders. Examples of this approach range from the classic static trade-off between tax benefits and expected costs of financial distress to Leland and Toft's (1996) dynamic analysis that allows for agency problems between debtholders and shareholders. Increasingly, however, research into capital structure has explicitly recognized that managers' self-interest can lead to financial policies that do not maximize shareholder wealth. An early example is Donaldson's (1969) field study of financing choices, which emphasizes goals such as organizational survival and growth. Recently, Jung, Kim, and Stulz (1996) identify security issue decisions that seem inconsistent with shareholder wealth maximization, and Jensen (1993) summarizes evidence of investment decisions that reduce shareholder wealth. We focus on the approach of Grossman and Hart (1982), Stulz (1990), and Hart and Moore (1995) in which debt constrains managers who, in turn, would prefer to issue less debt than shareholders desire. Zwiebel (1996) and Novaes and Zingales (1995) construct models in which managers use debt not because it

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benefits shareholders but because it reduces the threat of a hostile takeover. Chang (1992) derives similar predictions from the assumption that managers wish to retain control of restructuring decisions. We test the straightforward implication that managers will reduce leverage when they are shielded from hostile takeovers.

The adoption of the "second generation" of state-level antitakeover laws in the period from 1987 to 1990 provides an example of a relatively exogenous increase in the cost of hostile takeover, along with a natural control sample of states that did not pass such laws. The effectiveness of these laws is questioned by Comment and Schwert (1995) who find little evidence that they reduced the frequency of takeovers. If these laws are indeed completely ineffective then they would not produce any significant capital structure changes. However, Comment and Schwert also find that antitakeover laws are associated with a significant increase in takeover premiums, Karpoff and Malatesta (1989, 1995) find that they significantly reduce stock values, and Wahal, Miles, and Zenner (1995) find that large institutional shareholders pressured some firms to opt out of the Pennsylvania law. A natural interpretation of this evidence, which we formalize in the Appendix, is that antitakeover laws raise both the costs and benefits of mounting a hostile takeover. By raising the cost of takeover they allow managers to pursue goals other than maximizing shareholder wealth, and the resulting "slack" increases the payoff from a successful takeover. The observed frequency of takeovers changes little even as stock value declines and takeover premiums increase.

We contrast firms' financing policies before and after they are covered by antitakeover laws. To control for other contemporaneous changes in the relative costs and benefits of debt, we use data from firms in states that did not adopt antitakeover laws, along with a set of firm-specific controls. We find that protected firms issued less debt and, over time, substantially reduced their leverage ratios. Unprotected firms did the reverse. This result survives a number of robustness checks, and is not due to variation in size, industry, or profitability. Flow-of-funds models indicate that the post-law reduction in debt funding was approximately offset by post-law reductions in funds paid out through net stock repurchases and dividends.

Our evidence complements that of Berger, Ofek, and Yermack (1997) who find that corporate executives tend to use debt more aggressively when faced with overt control threats, or when they have incentives to increase share value because of stock-based compensation or outside directors. A limitation of Berger et al. (1997), as its authors recognize, is that their measures of managerial incentives are endogenous and are determined simultaneously with leverage. Compensation and board structure are partly controlled by managers (Yermack (1997)) and overt control threats are more likely to arise if the manager has not maintained adequate leverage (Billett (1996)). Relative to such measures, passage of an antitakeover law provides a change in managerial incentives that is less forceful but more exogenous, except for the specific firm whose takeover skirmishes seemed to prompt the law's passage. (Our sample includes nine such firms and they do not drive the

results.) Following protection by antitakeover laws, we observe four-year cumulative abnormal leverage reductions of about 30 percent, the same order of magnitude as the immediate leverage changes that Berger et al. (1997) observe after firm-specific control events.

Control models of ownership structure, such as that by Stulz (1988), show that managers can use debt to concentrate their percentage voting control, thereby increasing their ability to extract a high takeover premium. Like our agency hypothesis, a control model predicts that antitakeover laws will be followed by debt reductions, as antitakeover laws substitute for debt as a means to extract premiums from bidders. We find evidence of the importance of managerial voting power in that antitakeover legislation ceases to have a significant effect on capital structure when insider ownership exceeds 25 percent. An obvious interpretation is that tightly held firms were effectively immune from hostile takeover before the passage of legislation. The postlegislation debt reduction is significant for firms in which insiders control 5 to 25 percent of shares, and is even larger for those firms in which insiders hold less than 5 percent. Insiders' percentage vote control is a minor issue for such firms, suggesting that the relation between debt and antitakeover legislation is not entirely driven by the use of debt to concentrate managers' percentage voting control. We also show in the Appendix that the documented effects of antitakeover laws on prices, takeover premia, and takeover activity collectively imply the existence of agency effects.

We find no marked post-law change in firm size or profitability, which is inconsistent with the notion that antitakeover laws brought on gross overinvestment or free cash flow abuses. We do find some evidence that the protected firms exhibit more "inertia" in the sense of undertaking fewer major new investments or disinvestments. Because protected firms also tend to reduce leverage, the reduction in disinvestments is consistent with Kovenock and Phillips's (1997) finding that high-leverage firms are more likely to close plants than their less-levered rivals.

Overall, our results support the view that capital structure choices reflect managerial discretion, and that impediments to takeovers induce a shift from debt to equity financing. Future research might examine in more detail the relation between takeover impediments and policies such as employment and investment.

The paper is organized as follows. Section I briefly summarizes key features of second-generation antitakeover legislation. Section II describes the data set, Section III presents leverage results, Section IV examines other changes in investment and financing policies, Section V assesses some alternative interpretations of the results, and Section VI concludes.

I. A Brief History of Recent U.S. Antitakeover Legislation

In 1982, a majority of states had in place "first generation" antitakeover laws, and federal intervention was based largely on the various antitrust laws and on the 1968 Williams Act prohibition of "fraudulent, deceptive or

manipulative acts or practices in connection with any tender offer." For example, in 1981 a federal circuit court ruled that the Williams Act prohibited Marathon's attempted "Crown Jewel" defense (*Mobil Corp. v. Marathon Oil*). The following year, in *Edgar v. MITE* the Supreme Court ruled that the Williams Act preempted state antitakeover laws, thus striking down the first-generation laws in 37 states. In 1985, in *Schreiber v. Northern Burlington*, the Supreme Court further curtailed government regulation of takeovers, effectively precluding the sort of federal intervention seen in *Mobil v. Marathon*, by ruling that the Williams Act prohibited only "egregious" acts.

However, the Supreme Court changed course in 1987, ruling in *Dynamics v. CTS* that state antitakeover laws were enforceable so long as they did not actually prevent compliance with the Williams Act. Following this ruling, a majority of states passed new antitakeover laws. Most of the new laws took effect between 1987 and 1989, but notable laws in Pennsylvania and Delaware did not take effect until 1990. These "second generation" laws (some authors subdivide them further, into third and fourth generations) generally took the form of "control share" and "business combination" restrictions that constrained block shareholders in various ways, or constituency expansions that allowed managers to consider how a takeover would affect employees, communities, or other nonowner stakeholders. In 1989 the court upheld the Wisconsin business combination law, further indicating that the new laws could be enforced. See Roe (1993) for further description and assessment of these laws.

II. The Data

The sample is the set of all firms in both COMPUSTAT and CRSP in 1990, excluding financial services firms (SIC 60–69), utilities (SIC 40,48,49), and firms lacking eight complete years of back data. A total of 1,359 firms meet these criteria; 113 firms are deleted because their states passed laws before 1987[1] and 43 firms are deleted because they are incorporated outside the United States, leaving a final sample of 1,203 firms. Table I presents some summary statistics.

A. Differences between the Test and Control Samples

The summary statistics in Table I indicate that the firms in the control sample tend to be smaller, less profitable, and less levered than those in the antitakeover sample. The two samples also differ somewhat in their industry composition. Several of the two-digit SIC codes in the antitakeover sample are absent from control sample, though the reverse is never true. Our regression models control for these effects, but as a further check we repli-

[1] As Romano (1993) stresses, there was essentially no effective antitakeover legislation at the federal or state level between 1980 and the Supreme Court's upholding of the Indiana Law in 1987.

Table I

Descriptive Statistics

1983 mean and (median) for all firms with complete 1982–1990 COMPUSTAT and CRSP data, excluding utilities, financials, and firms in states that passed a second-generation antitakeover law before 1987. Antitakeover indicates incorporation in a state that passed a second-generation antitakeover laws in the period 1987 to 1990. Control includes all firms not in antitakeover states. The restricted sample is designed to reverse the differences between the pro- and antitakeover samples and excludes pro-takeover firms with less than $10M assets, and antitakeover firms in the top quartile of ROA, size, or long-term debt. The total sample size is 12,777 firm years. All variables are Winsorized at the 1 percent tails.

	Full Sample		Restricted Sample	
	Antitakeover States	Pro-Takeover Control	Antitakeover States	Pro-Takeover Control
Long-term debt/assets	0.17	0.12	0.09	0.15
	(0.15)	(0.05)	(0.07)	(0.11)
Log total assets ($M)	4.2	2.8	3.1	3.9
	(4.1)	(2.7)	(3.2)	(3.7)
Operating earnings/assets	0.12	0.08	0.06	0.15
	(0.13)	(0.11)	(0.09)	(0.15)
Number of firms	1084	119	438	73
Fraction with complete 1991–1993 data	0.71	0.56	0.67	0.67

cate our tests with a size- and industry-matched sample, and with a restricted sample that reverses some of the cross-sample differences by deleting from the antitakeover sample all firms in the upper quartiles of size, profitability, or leverage in 1983, and deleting from the control sample all firms with 1983 assets less than $10M. The summary statistics in Table I indicate that these exclusions are sufficient to reverse the preexisting differences between the samples. Table II summarizes the geographic distribution of the samples. Over half of the antitakeover firms are in Delaware. As a robustness check the main results are replicated with all Delaware firms excluded from the antitakeover sample, and again with all *non*-Delaware firms excluded from the antitakeover sample.

B. Model Specification

The empirical tests examine various measures of leverage changes in the pooled cross section and time series of the sample firms over the period 1983 to 1993, from four years before to four years after the period when most second-generation laws were passed. The results are not sensitive to the model specification, and we present a variety of specifications. We follow the sample firms over several years to capture a partial adjustment process in which managers adjust leverage at opportune times. In most tests we treat each year as a separate observation in order to allow for the possibility that

Table II

Geographic Breakdown of Sample Composition

Sample comprises all U.S.-incorporated firms with complete 1982–1990 COMPUSTAT and CRSP data, excluding utilities, financials, and firms in states that passed a second-generation antitakeover law before 1987. Antitakeover states are those that passed second-generation antitakeover laws in the period 1987 to 1990. Control sample includes all firms not in antitakeover states.

Location of Incorporation	Number of Firms
Antitakeover states	
Delaware	612 (56%)
Pennsylvania	49 (5%)
Minnesota	44 (4%)
Ohio	43 (4%)
New Jersey	33 (3%)
Florida	31 (3%)
Virginia	30 (3%)
Other states	242 (24%)
Total	1084 (100%)
Control sample	
California	51 (42%)
Texas	28 (24%)
Colorado	27 (23%)
Other states	13 (11%)
Total	119 (100%)

leverage determinants like size and performance may change over time, but we also report similar results for cumulative multiyear changes. Because the dependent variable is a change, regression standard errors should not be strongly biased by use of a pooled sample in which each firm appears multiple times. We also report similar results from tests that are not affected by serial dependence of successive observations.

As suggested by Novaes and Zingales (1995), managerial models of capital structure are consistent with the observed debt increases during the takeover wave of the 1980s, followed by debt reductions as the takeover market chilled after 1990. As there are many alternative explanations for this observation, our focus is on the difference between firms that were covered by antitakeover laws and those that were not. The effect of antitakeover laws is gauged by the coefficient on a dummy variable *protected*, which indicates whether the sample point is in an antitakeover state after the passage of the relevant law. To control for time-invariant cross-sectional differences between the pro- and antitakeover states, and for sample-wide time-series changes in leverage policies, the models also include the cross-sectional and time-series control dummies, *state* and *time*, which indicate separately whether the firm was in an antitakeover state or in the post-law period.

Our results hold under several definitions of net leverage changes. However, net leverage changes reflect the combined effects of debt issues and retirements, equity issues and repurchases, dividend and investment policy, acquisitions, profitability, and the exercise of any outstanding conversion options. Because many of these factors are not directly under managers' control, we follow Berger et al. (1997) by focusing on the effects of security issues and retirements. Berger et al. measure deliberate leverage increases as the dollar value of net debt issues less net equity repurchases as a fraction of lagged assets. This measure combines two important aspects of financial policy: changes in leverage ratios and changes in the sources and uses of funds. Our approach is to separate these two effects. In most of the paper, we focus on leverage ratios. In Section IV.B we look at changes in the sources and uses of funds.

Berger et al.'s (1997) measure does not perfectly characterize leverage changes because debt issues increase the firm's fixed obligations as well as its assets (provided the proceeds are not paid out as a dividend[2]) but equity repurchases increase leverage only by reducing the firm's asset base. We focus on a dependent variable that directly captures the net leverage effect of debt and equity issues:

$$\textit{Leverage change} = \frac{D + d}{A + d + e} - \frac{D}{A}, \tag{1}$$

where D is lagged total debt, A is lagged assets, d is debt issues net of retirements, and e is equity issues net of repurchases. In the robustness section we try different measures of leverage changes and get the same results.

To control for reversion to the average capital structure, the models include lagged leverage levels. To capture linear and nonlinear effects of scale and earnings capacity, the models control for logged and nonlogged total assets, change in log assets, operating earnings as a fraction of assets, and a dummy for negative reported earnings. If large firms prefer higher leverage, then both size changes and size levels will predict leverage changes, as growing and recently grown firms adjust toward their new target leverage. Assets and lagged leverage are components of our measures of leverage changes, so in addition to controlling for size and mean reversion these variables may help to capture the effects of specification error in the dependent variable. These controls could also introduce multicollinearity bias, so we verify that our key results are unchanged if these controls are deleted or if size is measured by sales revenue. If managers target market-value measures of leverage, then changes in stock value will require adjustments to book leverage, so we include the prior-year stock return from CRSP. The results are not changed if extra controls are added for same-year stock re-

[2] We find no statistically significant change in dividend policy after the passage of antitakeover laws.

turns, market/book ratio, the fraction of fixed assets depreciated, the ratio of fixed to total assets, capital spending, acquisitions, sale of fixed assets, and presence or absence of discontinued operations.

Our primary control for industry effects is the contemporaneous industry mean of the dependent variable, where "industry mean" is defined as the same-year mean among other firms (i.e., excluding the firm in question) in the narrowest SIC code that includes at least four other firms. This method has the advantage that it yields a single coefficient for the strength of the industry effect. We also present similar results with industry dummies and in an industry-matched sample.

The model is not particularly parsimonious, and contains some controls that are arguably unnecessary. For example, the cross-sectional control variable *state* is not in the information set in the early years of the sample, and some of our size controls may be redundant. Our approach is to demonstrate that a surplus of control variables does not markedly change the univariate results. Finally, to ensure that the results reflect a pervasive phenomenon rather than scattered outliers, all variables are Winsorized[3] at both the upper and lower 1 percent tails, and the main results are replicated with all values replaced by their sample rank.

III. Leverage Results

A. *Primary Results*

Figure 1 shows the time series of mean leverage levels for firms with complete data, and Figure 2 shows the fraction of firms that issued more debt than they retired. Both plots indicate that, as predicted, firms in the antitakeover sample markedly reduced their use of debt in the 1987 to 1990 period. Figure 1 suggests a post-law leverage reduction on the order of 20 to 30 percent, or more if one were to extrapolate the pre-law trend. Firms in the control sample show no such pattern.

These plots show that firms adjust their leverage in the predicted manner after the passage of antitakeover laws. Unexpectedly, Figure 1 also suggests that the pro- and antitakeover samples behaved differently in the years immediately *before* the passage of antitakeover laws. This divergence is not fully explained by variation in growth or profitability. A potential explanation for this pre-law trend is that the states which passed takeover legislation tend to be those with the highest incidence of control threats. Such threats would have caused managers to maintain higher leverage to fend off takeovers, and also to lobby more insistently for antitakeover legislation. This hypothesis is consistent with the observation that some of the state antitakeover laws were motivated in part by takeover attempts against ma-

[3] In other words, values outside the 1st or 99th percentile are set equal to the 1st or 99th percentile.

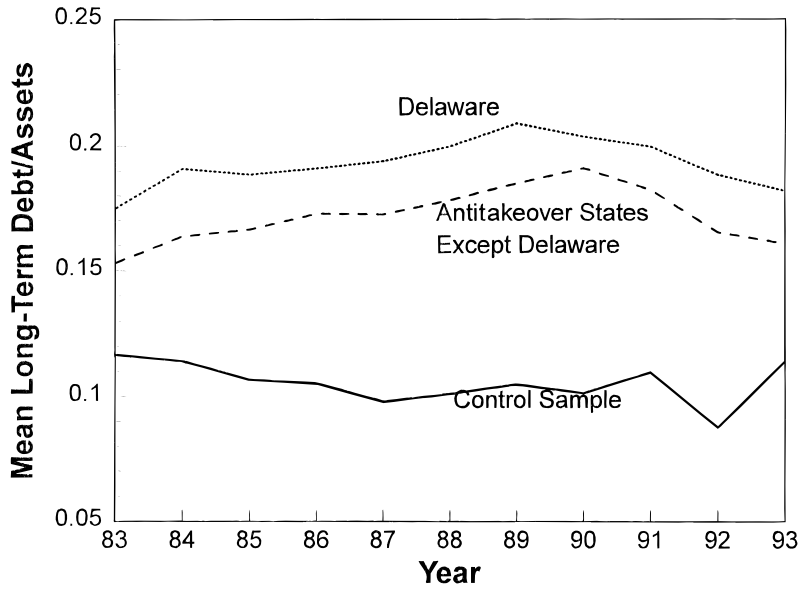

Figure 1. Mean leverage for 835 COMPUSTAT firms with complete data. Sample is all firms with complete 1982–1993 COMPUSTAT and CRSP data, excluding utilities, financials, and firms in states that passed a second-generation antitakeover law before 1987. Antitakeover states are those that passed second-generation antitakeover laws in the period 1987 to 1990. "Control Sample" includes all firms not in antitakeover states.

jor local employers.[4] A systematic test of this hypothesis is beyond the scope of the paper, but a scan of all U.S.-incorporated CRSP firms reveals no significant difference in the frequency of pre-law merger delistings between the pro- and antitakeover states. Comment and Schwert (1995) perform similar tests on a longer time-series, and find no significant pre- to post-law divergence in net incidence of takeovers. Although the pre-law divergence in leverage policy is not inconsistent with our hypothesis, it is econometrically inconvenient because it obscures the inferences from a simple post-law comparison of leverage levels, and requires some extra tests to determine whether the observed leverage changes should be interpreted as an unusual post-law event or as a return to normal practice following the dissipation of an unusual pre-law circumstance.

Table III presents a multivariate OLS model of the net leverage consequences of security issues and retirements, as defined by equation (1). The dummy variable *state* has a significantly negative coefficient in the post-law period, indicating that protected managers were reducing their leverage. The separate regressions for the pre- and post-law periods show that the post-

[4] Prominent examples are North Carolina's "Control Share Acquisition" law passed when Burlington Industries was a target and Washington's "Fair-Price" law passed when Boeing Aerospace was under threat.

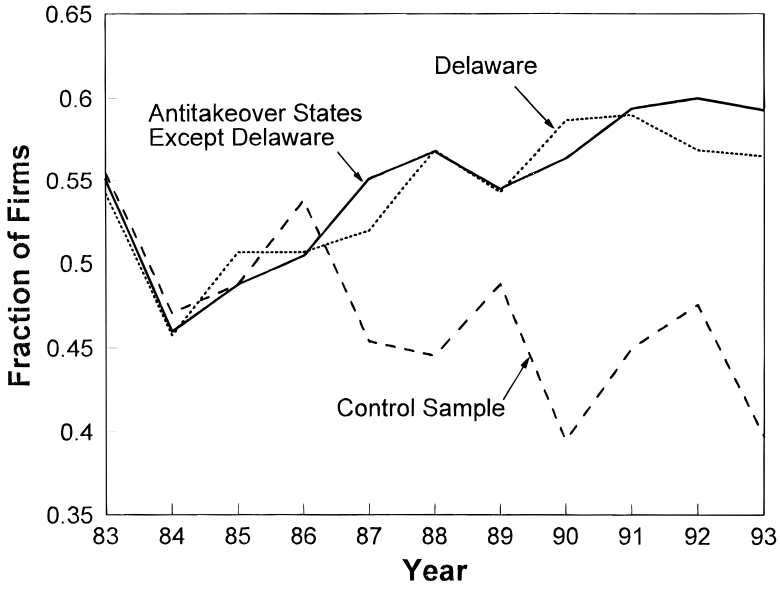

Figure 2. Fraction of COMPUSTAT firms that retire more debt than they issue. Sample is all firms with complete 1982–1990 COMPUSTAT and CRSP data, excluding utilities, financials, and firms in states that passed a second-generation antitakeover law before 1987. Sample size is 1,203 firms. Antitakeover states are those that passed second-generation antitakeover laws in the period 1987 to 1990. Control Sample includes all firms not in antitakeover states. Debt issues and retirements are as reported in Compustat.

law debt reductions of protected firms represent a reversal of their abnormal pre-law leverage increases, though the pre-law effect is smaller and is significant at only the 10 percent level. The *protected* coefficient is expressed in terms of the annual implied change in debt/assets. For a firm with the sample-average debt/asset ratio of 0.17, the *protected* coefficient of -0.013 implies an abnormal post-law leverage reduction of about 8 percent per year, which agrees well with the net leverage changes observed in Figure 1. The coefficient on *protected* is more than twice as large as that of the oppositely signed coefficient on *state* (an F test rejects equal magnitude at $p < 0.01$) so the effects of antitakeover laws are more than large enough to reverse the pre-law differences between the antitakeover and control samples. Note also that while the changes are expressed in annual terms, they represent averages maintained over four years.

The coefficients on the control variables indicate that the use of debt decreases with prior leverage and profitability, and increases with size. The negative coefficient on prior debt/assets ratio is consistent with the view that firms tend to adjust toward the sample mean leverage, but is probably overstated because of the mechanical relation between lagged leverage and our dependent variable. The negative relation between leverage and profitability is well known. The coefficients on size and size changes are positive,

Table III

OLS Model of Financing Changes

Dependent variable is annual net leverage effect of debt and equity issues = $[(D + d)/(A + d + e)] - (D/A)$ where D = lagged total debt, d = debt issues net of retirements, e = equity issues net of repurchases, and A = lagged assets. Sample size is 1,084 antitakeover firms and 119 control firms, for a total of 12,777 firm years. The sample consists of all U.S.-incorporated firms with complete 1982–1990 COMPUSTAT and CRSP data, excluding utilities, financials, and firms in states that passed a second-generation antitakeover law before 1987. All variables are Winsorized at the upper and lower 1 percent tails. Intercepts are not reported. Standard errors are in parentheses. Industry mean is the same-year mean among other firms in the narrowest SIC that includes at least four other firms.

Explanatory Variable	1983–1986	1990–1993	1983–1993	1983–1993 Standardized Coefficients
Protected = 1 if antitakeover state AND after law takes effect			−0.013***	−0.085***
			(0.004)	(0.026)
State dummy = 1 in states that passed an antitakeover law	0.0066*	−0.0093***	0.0046	0.019*
	(0.0039)	(0.0032)	(0.0028)	(0.011)
Time dummy = 1 if post-law, or post-1988 in no-law states			0.0093**	0.060**
			(0.0040)	(0.027)
Industry mean of dependent variable	0.15***	0.058	0.13***	0.047***
	(0.037)	(0.041)	(0.02)	(0.009)
Prior year's debt/assets	−0.12***	−0.037***	−0.069***	−0.16***
	(0.01)	(0.005)	(0.0004)	(0.01)
ROA = Prior year's operating cash flow/assets	−0.005	−0.035***	−0.021***	−0.032***
	(0.012)	(0.010)	(0.008)	(0.010)
Change in ROA from prior year	−0.050***	−0.11***	−0.083***	−0.10***
	(0.014)	(0.01)	(0.008)	(0.01)
Loss dummy = 1 if prior year's net income was negative, 0 otherwise	−0.0013	−0.0025	−0.0009	
	(0.0035)	(0.0024)	(0.0018)	
Prior year's stock return (from CRSP)	0.0001	−0.0022	−0.0015	
	(0.0021)	(0.0017)	(0.0012)	
Log of prior year's total assets, $M	0.0059***	0.0034***	0.0045***	0.11***
	(0.0008)	(0.0006)	(0.0004)	(0.01)
Prior year's total assets, nonlogged $ $\times 10^{12}$	−1.2	−0.33	−0.91**	
	(0.8)	(0.43)	(0.34)	
Change in log assets from prior year	0.061***	0.058***	0.068***	0.23***
	(0.005)	(0.004)	(0.003)	(0.01)
Adjusted R^2	0.12	0.08	0.10	0.10
Sample size	4,798	4,379	12,777	12,777

*, **, *** Superscripts indicate statistical significance at the 10, 5, and 1 percent levels, respectively (two-tailed t-test).

as found by Berger et al. (1997), suggesting that growing and recently grown firms were adjusting toward higher optimal leverage levels during our sample period. The two size-level controls collectively increase our model's R^2 by

less than 1 percent and do not affect our key result, but their high statistical significance in a model of leverage changes is intriguing. We investigate further by expanding our sample to include all states and all years since 1971, and estimate the model in cross section for each year for 1971 through 1993, without the time and state controls. The size coefficient is positive in every year and trends steadily upward in the years after 1977 (results not reported.) This unexplained trend shows no obvious changes around the 1987 to 1990 period when antitakeover laws were being passed. In the next section we present a number of robustness checks to ensure that size effects do not drive our results.

The univariate plots indicate that antitakeover laws can largely explain the gross time-series trend in sample-mean leverage. However, if all regression controls are deleted, the antitakeover variables explain only about 1 percent of the firm-level variation in annual security issues, and 2.5 percent of the firm-level variation in four-year cumulative leverage changes. Consequently the bulk of the firm-level variation in debt policy is explained by factors other than antitakeover laws. To indicate the relative economic significance of the various regressors, Table III presents standardized regression coefficients from a model with the redundant control variables omitted. Standardized regression coefficients equal the raw coefficient multiplied by the ratio of the standard deviations of the regressor and the dependent variable. By this measure, antitakeover laws influence the mix of security issues about as strongly as does profitability or size, half as strongly as prior leverage, one-third as strongly as size changes, and half again as strongly as industry effects.

B. Robustness Checks

This section briefly discusses a number of robustness checks, which are summarized in Tables IV and V. The tables show the coefficient and standard error for *protected* under various alternative specifications and sample selection criteria. Unless stated otherwise, the results are those from the full 1983 to 1993 model in Table III with all controls.

The results are not sensitive to the choice or specification of regression controls. The results have similar magnitude and significance when all model variables are replaced by their sample rank, when all control variables except the time and state dummies are excluded, and when extra controls are added for same-year stock returns, market/book ratio, the fraction of fixed assets depreciated, the ratio of fixed to total assets, capital spending, acquisitions, sale of fixed assets, and the presence or absence of discontinued operations.

A panel model offers the advantage of controlling for contemporaneous determinants of optimal leverage. However, it is debatable whether leverage changes in successive years should be treated as independent observations, and our panel model may not fully capture the cumulative effects of multiyear historical trends in profitability or other determinants of leverage. To

Table IV

Summary of Alternative Specifications of Financing Model in Table III

Reported values are the regression coefficients on *protected* (a dummy variable that equals 1 if the firm is shielded by an antitakeover law, 0 otherwise) and can be interpreted as the annual abnormal debt change associated with protection by state antitakeover laws, expressed as a fraction of assets. Except as noted, each coefficient is obtained from an OLS model similar to that described in Table III. In the Rank regression, all nondummy variables are replaced by their sample rank.

Specification	*Protected* Coefficient (std err)
Base model (n = 12,777)	−0.013*** (0.004)
Industry mean replaced by dummy variables for two-digit SIC (Results are the same for 3- and 4-digit SIC) (n = 12,777)	−0.013*** (0.004)
Dependent variable is first difference of long-term debt/assets (n = 12,777)	−0.012** (0.006)
Dependent variable is first difference of total debt/assets (n = 12,771)	−0.012** (0.006)
Dependent variable is first difference of long-term debt/(book debt + market value of equity), added control for same-year stock return (n = 11,676)	−0.0093* (0.0050)
Dependent variable is 4-year cumulative change in long-term debt/assets, years 1983–86 and 1990–93 only. Control variables measured as the 4-year total or as the level at the beginning of the 4-year period (n = 1,670)	−0.055*** (0.021)
All controls except time and antitakeover dummies excluded (n = 12,777)	−0.017*** (0.004)
Added controls for proportion of assets that are fixed, fraction of fixed assets depreciated, market/book ratio, capital expenditures/assets, asset sales/assets, presence of discontinued operations, and acquisitions/assets (n = 12,662)	−0.011*** (0.004)
Heteroskedasticity-consistent estimate from GMM (n = 12,777)	−0.013*** (0.004)
Rank regression (n = 12,777)	−465** (206)

*, **, *** Superscripts indicate statistical significance at the 10, 5, and 1 percent levels, respectively (two-tailed t-test).

address these concerns, we estimate the model on the cumulative changes in debt/asset ratio over the four-year periods 1983 to 1986 and 1990 to 1993, before and after the laws were passed. The sample is restricted to firms with complete data, and the control variables are measured either at the beginning of the four-year period or are cumulated over the four-year period, as appropriate. The coefficients on the control variables retain their signs, and the coefficient on *protected* remains negative and significant at the 0.01 level. The antitakeover variables alone explain about 2.5 percent of the vari-

Table V

Summary of Alternative Sample Selection Criteria of Model in Table III

Reported values are the OLS regression coefficients on *protected* (a dummy variable that equals 1 if the firm is shielded by an antitakeover law, 0 otherwise) and can be interpreted as the annual abnormal debt change associated with protection by state antitakeover laws, expressed as a fraction of assets. Except as noted, each coefficient is obtained from a model similar to that described in Table III.

Subsample	*Protected* Coefficient (std err)
Whole sample (n = 12,777)	−0.013*** (0.004)
Size and industry matched sample (n = 1,474). Mean log 1983 assets = 3.3 for antitakeover sample, 3.2 for control.	−0.020*** (0.007)
Restricted sample: Excludes all antitakeover firms in upper quartile of 1983 assets, long-term debt/assets, or ROA, and exclude pro-takeover firms with 1983 assets < \$10M. See Table I for descriptive statistics (n = 5,283)	−0.013*** (0.005)
Consider only years 1990–93 and exclude antitakeover firms that increased leverage over 1983–89. Mean 1983–89 change in long-term debt/assets is −0.10 for antitakeover sample, 0.004 for control (n = 2,117)	−0.0068** (0.0033)
Exclude California and Texas (n = 11,952)	−0.016** (0.007)
Exclude Delaware (n = 6,291)	−0.011*** (0.004)
Exclude all antitakeover states *except* Delaware (n = 7,736)	−0.015*** (0.004)
Only firms with at least \$100M in 1983 assets (n = 5,033)	−0.017** (0.008)
Only firms with less than \$100M in 1983 assets (n = 7,744)	−0.014*** (0.005)
Exclude firms identified by Karpoff and Malatesta as motivating the passage of laws in their state (n = 12,701)	−0.013*** (0.004)
Exclude firms missing any data in sample period (n = 9,182)	−0.016*** (0.005)

*, **, *** Superscripts indicate statistical significance at the 10, 5, and 1 percent levels, respectively (two-tailed t-test).

ation in cumulative leverage changes. The coefficient of −0.055 implies that, for the average firm, protection by antitakeover laws is followed by a four-year cumulative abnormal leverage reduction of about one-third, nearly the same as that implied by the 8 percent annual reduction observed in the panel model. For comparison, if we apply the transformation in equation (1) to Berger et al.'s (1997) estimates and use their sample average asset and leverage values, we obtain a one-year leverage increase of about one-third

following unsuccessful tender offers and between one-third and one-fourth following the involuntary replacement of the CEO or the addition of a major shareholder to the board. Thus, the cumulative four-year leverage changes we observe after antitakeover laws are of the same scale as the one-year leverage changes that Berger et al. (1997) observe after firm-specific control events.

We find no reason to suspect bias from heteroskedasticity, as the dependent variable is size-normalized and its standard deviation varies by only 0.01 (about 10 percent) between the pro- and antitakeover samples. As a precaution we compute both White and GMM standard errors and verify that in both cases the main result remains significant at the 0.01 level.

The industry compositions of the two samples do not match perfectly, and the firms in the antitakeover sample tend to be larger, more profitable, and more highly levered than the firms in the control sample. The regression controls for industry, size, profitability, and prior leverage have little effect on the result. As a further check we construct a size- and industry-matched sample in which each of the 67 control firms with complete data is paired with the antitakeover firm that provides the nearest size match (based on 1983 assets) in the same two-digit SIC. This produces two samples with the same number of data points and nearly identical mean log assets. The results in the matched sample are unchanged. As our data do not permit a simultaneous match on size, profitability, and leverage, we also replicate the results in a restricted sample that strongly reverses the initial differences between the samples by excluding the upper quartile of 1983 size, profitability, or leverage from the antitakeover sample, and excluding firms with less than \$10M in 1983 assets from the control sample (see Table I for summary statistics). Again the results are unchanged. We conclude that the results are not driven by variation in size, profitability, or prior leverage.

The firms in the antitakeover states tended to increase their leverage in the pre-law years, suggesting that their post-law leverage reduction can be interpreted as a reversal of their abnormally aggressive pre-law debt policies. This interpretation is not inconsistent with our hypothesis, if the reversal was driven by a reduced threat of takeover. However, the post-1990 debt issues of protected firms are abnormally low even when the antitakeover sample excludes all firms that increased their leverage in the pre-law years. We conclude that the results cannot be attributed entirely to reversal of anomalous debt policies in the pre-law years.

If leverage changes are measured by the first difference in debt ratios rather than by security issues, the sign and magnitude of the main result are changed little but statistical significance is generally lower. This may reflect the fact that overall leverage changes are driven partly by factors beyond managers' control. The results are still significant at $p = 0.06$ or better (two-tailed test) with leverage changes measured by first difference in long-term debt/asset ratio, long-term debt/market value, or total debt/asset ratio. A few firms report zero debt, creating a potential censorship

bias, but OLS and Tobit models give virtually the same results. Antitakeover laws are followed by reductions in long-term debt but little change in short-term debt. This result is consistent with an agency hypothesis, as short-term debt is composed largely of liabilities such as commercial paper, trade credit, and short-term revolving bank credit, which seem more closely related to the firm's daily operations than to managerial discipline and control.

During the sample period, shocks to the oil and defense industries drastically affected the fortunes of some firms in Texas and California (both pro-takeover states) but the results are the same if Texas and California are excluded. The results are also unchanged if Delaware is excluded, if all antitakeover states *except* Delaware are excluded, or if the sample is restricted to either small firms or large firms, defined by whether the firm had more than $100M in assets at the beginning of the sample period.

C. The Effects of Poison Pills and Opting In or Out of the Ohio and Pennsylvania Laws

Managers' responses to antitakeover legislation may be conditioned by their other actions. For example, some managers had already installed other takeover defenses, and in some states, Ohio and Pennsylvania for example, managers could "opt out" and choose not to be covered by the antitakeover law (see Wahal et al. (1995) for more details). Such managerial choices are not exogenous, so it is difficult to predict their relation to post-law leverage changes. The firms that opt in to a law gain greater protection and hence may be more likely to reduce their leverage, but they also reveal that they feel threatened and hence may be more likely to increase leverage as an additional defense. Similarly, the presence of a poison pill suggests that managers feel threatened, which may mean that they will benefit more from an antitakeover law, but may also mean that they are likely to maintain unusually high leverage anyway. If poison pills and antitakeover laws are substitutes, then we should observe smaller post-law leverage reductions in firms that already have poison pills, but the reverse could be true if passage of an antitakeover law is interpreted as a signal that a firm's poison pill will likely be upheld if challenged in court.

The IRRC (Investor Responsibility Research Center) reports the opt-in/opt-out decisions for 19 of the Ohio and Pennsylvania firms in our sample. If the antitakeover sample is restricted to these 19 firms and the variable *protected* is crossed with dummies for opting in and opting out, the opt-in and opt-out firms have *protected* coefficients of -0.18 and -0.15, respectively, but the standard errors of the coefficients (0.20 and 0.11, respectively) are too large to permit any meaningful conclusions. The IRRC also reports data on the presence or absence of poison pills for 304 (28 percent) of the firms in our antitakeover sample. If the antitakeover sample is restricted to these firms and the variable *protected* is crossed with dummies for the presence or absence of a pre-law poison pill, the abnormal post-law leverage

reductions are statistically significant at the 0.01 level regardless of whether the firm has a poison pill. The post-law leverage reduction is slightly smaller for firms with poison pills, with a *protected* coefficient of −0.12 versus −0.15, but this difference is not significant ($p = 0.4$ from F test for equal coefficients). Overall, our examination of opt-in/opt-out decisions and poison pills yields no clear conclusion.

IV. Other Investment and Financing Variables

A. The Leverage Effects of Debt versus Equity Issues

Debt issues and equity repurchases do not have symmetric effects on leverage ratios, because equity issues change only the denominator of the leverage ratio. The leverage consequence of security issues can be decomposed into the separate effects of debt and equity issues as follows:

$$\textit{Leverage change} = \frac{D+d}{A+d+e} - \frac{D}{A} = \frac{d}{A}\left(\frac{A-D}{A+d+e}\right) - \frac{e}{A}\left(\frac{D}{A+d+e}\right),$$

$$= \textit{debt effect} - \textit{equity effect}, \qquad (2)$$

where, as before, D is lagged total debt, d denotes debt issues net of retirements, e denotes equity issues net of repurchases, and A denotes lagged assets. Since the debt ratio is usually less than one-half, the leverage effect of a debt issue is usually larger than that of an equal-size equity issue. Empirically, this disparity is substantial. In our sample, the mean absolute value of the debt effect is 0.04, about six times larger than the mean absolute value of the equity effect, 0.007. The 90th percentiles of the debt effect are −0.05 and +0.07; the 90th percentiles of the equity effect are −0.01 and +0.003. For comparison, the 90th percentiles of observed changes in long-term debt ratio are −0.07 and +0.09. Consequently, the leverage changes in our sample are dominated by the effects of debt issues. If the main model in Table III is estimated separately on the debt and equity effects, as defined in equation (2), the *protected* coefficient in the debt effects model is −0.11, significant at the 0.01 level. The *protected* coefficient in the equity effects model, −0.02, has the predicted sign but is only about 1.3 standard errors from zero.

B. Sources and Uses of Funds

We have shown that protection by antitakeover laws is associated with reduced use of debt financing, leading ultimately to lower leverage ratios. As an accounting identity, if protected firms reduce their debt funding they must increase their equity funding to compensate, or else make up the difference with increased profits or reduced growth. Here we take a closer look at firms' total flow of funds to determine where they obtain the funding to reduce their leverage ratios.

Table VI

Summary of OLS Models of Changes in Flow of Funds

Reported values are the OLS regression coefficients *protected* (a dummy variable that equals 1 if the firm is shielded by an antitakeover law, 0 otherwise) and can be interpreted as the abnormal change associated with protection by state antitakeover laws. Except as noted, each coefficient is obtained from a model similar to that described in Table III, deleting the controls (debt, performance, or size change) that appear in the dependent variable. Standard errors are in parentheses.

	Specification	
Dependent Variable	Year-by–Year, All Years 1983–93 (n = 12,537)	4-year Cumulative over 1983–86 or 1990–93 (n = 1,625)
Funds from debt (Issues less retirements less interest)/Assets	−0.010** (0.005)	−0.017*** (0.008)
Funds from Equity (Issues less repurchases less dividends)/Assets	0.0092** (0.0050)	0.013 (0.008)
Funds from Earnings (Net income before interest)/Assets	−0.0038 (0.0073)	0.004 (0.013)
Change in size (Change in assets)/assets	−0.019 (0.018)	−0.002 (0.074)

*, **, *** Superscripts indicate statistical significance at the 10, 5, and 1 percent levels, respectively (two-tailed t-test).

Table VI presents the coefficient and standard error on *protected* in OLS models of the four sources of funding: earnings; asset changes; net debt issues minus interest expense; and net equity issues minus dividends, all normalized by total assets. The models are similar to those presented earlier except that the debt controls are excluded from the debt model, the performance controls are excluded from the earnings model, and the size change controls are excluded from the size change model. Total sources and uses of funds will not exactly balance because size and earnings are affected by mergers, divestitures, and write-downs. Interest and dividends cause the dependent variables to be autocorrelated, so the standard errors of the coefficients may be understated in a panel model. Consequently the model is also estimated with all variables cumulated over the four-year pre-law and post-law periods. The cumulative model is less affected by autocorrelation, but cannot adjust for year-to-year changes in the control variables.

The results indicate that the observed post-law reductions in net debt funding were approximately offset by increases in net equity funding. If the effects of dividends are estimated separately from those of equity issues, then neither is statistically significant. The other models reveal no statistically significant changes in net funding from earnings or from asset changes, but the coefficients have high standard errors.

Table VII

Summary of OLS Models of Changes in Investment Strategy

Reported values are the regression coefficients on *protected*, a dummy that equals 1 if the firm is shielded by a state antitakeover law, 0 otherwise. The sample period is 1983 to 1993. Except as noted, each coefficient is obtained from an OLS model similar to that described in Table III, excluding the control for size change. 11 percent of observations report discontinued operations. Standard errors are in parentheses.

	Sample	
Dependent Variable	All Firms (n = 12,702)	Firms with at Least \$10M in 1983 Assets (n = 10,312)
Capital investment/Assets	−0.0083**	−0.0051
	(0.0047)	(0.0054)
Fixed asset sales/Assets	−0.0023	0.0007
	(0.0016)	(0.0018)
Cash acquisitions/Assets	−0.0067***	−0.0089***
	(0.0029)	(0.0036)
All acquisitions	−0.0047	−0.0081
(Sales contribution of all acquisitions)/Sales	(0.0055)	(0.0067)
Probability of reporting discontinued operations	−0.29	−0.65***
(Logit Model)	(0.25)	(0.30)

*, **, *** Superscripts indicate statistical significance at the 10, 5, and 1 percent levels, respectively (two-tailed t-test).

Overall, the changes in financial policy in this sample suggest that antitakeover laws were followed by a shift from debt finance to equity finance. The post-law increase in equity funding was approximately large enough to compensate for the loss of debt funding, but had only small effects on leverage. We find no evidence of gross post-law changes in size or profitability.

C. Other Investment Decisions

Agency theory suggests that managers are more prone to overinvest when they are protected from takeovers. We might therefore expect protected firms to grow larger but become less profitable. Although we find no evidence of such effects, our data provide some evidence that protected managers altered their investment strategies. In this section we briefly depart from our focus on financial policy in order to take note of a modest change in investment and disinvestment in protected firms. Table VII summarizes the results of OLS models of five measures of investment and disinvestment: cash acquisitions, all acquisitions, capital expenditures, funds obtained from sale of fixed assets, and whether or not the firm reported income effects of discontinued operations. The results suggest that protected firms are somewhat less likely to undertake major investments and disinvestments. Protected firms spend significantly less on cash acquisitions, and are significantly less

likely to report discontinued operations. The results for capital investment, asset sales, and total acquisitions are not statistically significant, but are consistent with reduced activity. Some of these results are moderately sensitive to the inclusion of the smallest firms, so results are reported both with and without small firms. Including these investment variables as controls in the leverage models presented earlier does not change their results.

These results suggest that protected managers display a degree of inertia. The reduction in discontinued operations is consistent with Kovenock and Phillips' (1997) finding that firms that undergo a major leverage increase are more likely to close plants, especially since major leverage increases are often motivated by the threat of takeover (Denis (1990)). Our protected firms are less likely to exhibit major leverage increases, and appear less likely to discontinue operations. The reduction in acquisition expenditures is more difficult to explain. One possibility is that some acquisitions in the pre-law period served as a takeover defense by increasing the importance of the incumbent management team as suggested by Shleifer and Vishny (1989). Whether this force is sufficiently powerful and widespread to cause the post-law drop in acquisition activity is an open question.

V. Alternative Interpretations

A. Other Events That May Drive the Results

Figures 1 and 2 show that, although debt issues in the two samples clearly diverged around 1987, actual debt ratios did not visibly change course until 1989. This suggests that antitakeover laws did not have real effects until the Supreme Court upheld the Wisconsin antitakeover law in 1989. An alternative interpretation is that the results reflect a leveling out of geographic variation in junk bond financing, following the downturn in the junk bond market and culminating in the collapse of Drexel Burnham Lambert in February 1990. Given the role of junk bonds in control contests, it seems reasonable to expect that the effects of antitakeover laws would be related to events in the junk bond market.

We test these hypotheses in two ways: first by examining the timing of leverage changes in more detail, and second by testing whether the results are driven by termination of the large debt issues likely to be associated with junk bond issues.

A.1. Timing

The sample includes antitakeover laws passed in the years 1987 to 1990. We know when each law formally took effect, but it is difficult to say precisely when any given law, or the set of all antitakeover laws, began to influence firm behavior. For example, the Pennsylvania law formally took effect in 1990, but was already well on its way through the legislature by late 1989. Also, though the Supreme Court decision in 1987 cleared the way for state antitakeover laws, the effectiveness of the new laws may not have

Table VIII

Summary of Logit Models of Sharp Net Debt Issues and Retirements

Reported values are the logit coefficients on *protected* (a dummy variable that equals 1 if the firm is shielded by an antitakeover law, 0 otherwise) and reflect the effect of antitakeover laws on the probability of sharp leverage changes. Debt effect represents the change in leverage caused by net debt issues, assuming that net debt issues are added to assets but assets do not otherwise change. Except as noted, each coefficient is obtained from a model similar to that described in Table III. Standard errors are in parentheses.

	Logit Coefficient on Dummy for Antitakeover States	
Event	1983–1986	1990–1993
Top-decile debt effect	0.45**	−0.44**
(Sharp debt increase)	(0.17)	(0.17)
Bottom-decile debt effect	−0.18	0.66***
(Sharp debt reduction)	(0.18)	(0.22)

*, **, *** Superscripts indicate statistical significance at the 10, 5, and 1 percent levels, respectively (two-tailed chi-square-test).

become apparent for some time, perhaps not until the court upheld the Wisconsin law in 1989. We test for timing effects by respecifying the model so that the time dummy indicates a fixed year (either 1989 or 1990) rather than the year each state passed its laws. If antitakeover laws did not affect firm behavior until some event occurred in 1989 or 1990, then these respecifications should increase the model's explanatory power. In fact these respecifications reduce the model's explanatory power, but by a trivial amount, less than 0.001. Consequently our data do not clearly reveal when antitakeover laws began to affect firm behavior.

A.2. The Distribution of Leverage Changes

To test for the influence of junk bond financing, we estimate pre-law and post-law logit models of the probability of sharp (extreme decile) net debt issues, controlling for all the variables in Table III. If the results are driven primarily by the collapse of the junk bond market, then we should observe sharp pre-law debt increases in the antitakeover states, but more or less normal debt changes in the post-law (and post-Drexel) years, after controlling for any mean reversion from the pre-law years. The results, summarized in Table VIII, do not quite follow this pattern. In the pre-law period, sharp debt increases were more frequent in the soon-to-be antitakeover states, but in the post-law period the differences between the samples reversed sign and widened, rather than simply fading as one would expect if the results were driven by firms losing their ability to issue junk bonds. The model controls for previous leverage levels, so these effects are measured net of normal mean reversion.

B. Agency versus Concentration of Managerial Ownership

There are at least two distinct reasons why antitakeover legislation might be followed by reduced use of debt. In models such as Zwiebel (1996) and Novaes and Zingales (1995), hereafter termed agency models, debt deters takeovers by increasing the stand-alone value of the firm. In a pure agency model, shareholder wealth is reduced when antitakeover laws allow managers to reduce debt. In a control model such as that of Stulz (1988),[5] debt deters takeovers by concentrating managers' voting control. One version of such models would feature managers who maximize shareholder wealth by trading off the prospect of an increased premium against the reduced probability of the bid succeeding, so that share value increases when antitakeover laws provide a cheap substitute for debt as a means to maximize expected takeover premiums. An alternative approach closer to the agency model in Zwiebel (1996) has managers choosing capital structure in their own interests, balancing the private costs of debt against the private benefits of concentrated voting control, with ambiguous effects on share value.

Previously published empirical results imply that agency issues are important, but do not rule out the possibility of control effects. Karpoff and Malatesta (1989) find that antitakeover legislation reduces stock price, and Comment and Schwert (1995) find that antitakeover laws increased expected takeover premiums, as they were associated with little change in the frequency of takeovers but an increase in the premiums paid in successful takeovers. These results imply the presence of agency costs, as a pure control model provides no other mechanism for share value to decline concurrent with a rise in expected takeover premiums. We formalize this argument in the Appendix.

The apparent presence of agency costs does not rule out the hypothesis that debt deters takeovers primarily by concentrating managerial ownership, rather than by increasing the stand-alone value of the firm. If so, then the effects of antitakeover laws should depend on whether managers own a sizable percentage of outstanding shares. If managers own an insignificant fraction of outstanding shares even before antitakeover laws are passed, then their percentage voting control is not an issue. For example, if the firm is very large, the sheer dollar value of a 2 percent managerial stake may represent a meaningful hurdle for some potential bidders, but this hurdle is not materially raised (i.e., the cost of buying enough shares to outvote the managers is not materially increased) by using leverage to boost the voting

[5] Israel's (1991) model is similar but is unlikely to apply to the case of antitakeover legislation because it relies on the assumption that target debtholders strictly gain from takeover. This assumption is reasonable for friendly mergers with the potential to coinsure target debt, but is less persuasive for the hostile takeovers that were the target of the legislation. Overall, there is little evidence that bondholders systematically gain from takeovers, and in some cases they lose, especially when takeovers involve leverage increases such as those envisioned in the disciplinary approach (see, e.g., Crabbe (1991)). That target debtholders do not welcome hostile takeovers is most graphically illustrated by the use of poison put and event-risk bond covenants.

Table IX

Results in Subsamples Based on Managerial Stock Ownership

Reported values are the OLS regression coefficients on *protected* (a dummy variable that equals 1 if the firm is shielded by an antitakeover law, 0 otherwise) and can be interpreted as the annual abnormal leverage change associated with protection by state antitakeover laws. The sample period is 1983 to 1993. Except as noted, each coefficient is obtained from a model similar to that described in Table III. Inside stock ownership (from Compact Disclosure) is as reported in the 1987 proxy statement. Standard errors are in parentheses.

	Dependent Variable	
Sample	Leverage Effect of Security Issues (as per Table III)	Change in Long-Term Debt/Assets
Insiders own less than 5 percent of stock (221 firms)	−0.025** (0.010)	−0.028** (0.014)
Insiders own 5–25 percent of stock (366 firms)	−0.019*** (0.007)	−0.014 (0.009)
Insiders own more than 25 percent of stock (381 firms)	−0.003 (0.007)	−0.006 (0.009)

*, **, *** Superscripts indicate statistical significance at the 10, 5, and 1 percent levels, respectively (two-tailed *t*-test).

power of managers' shares from, say, 2 to 5 percent. Consequently, the control hypothesis predicts that antitakeover laws should affect leverage most strongly when managers own a meaningful fraction of outstanding shares.

To test this prediction we obtain data on managerial stock ownership, as reported in 1987 proxy statements. From Compact Disclosure we were able to obtain ownership data for 968 (81 percent) of the sample firms. We divide our sample into subsamples based on the three ranges of management ownership stressed by Morck, Shleifer, and Vishny (1988): less than 5 percent, 5 to 25 percent, and greater than 25 percent. On each of these subsamples we estimate our OLS models for two measures of debt changes, the net effect of security issues as in most of our tests, and the net change in long-term debt/assets. The results are reported in Table IX. When managers own less than 5 percent of shares, the post-law debt reduction (as measured by the *protected* coefficient) is large (−0.025) and statistically significant. When managers own 5 to 25 percent of stock, the post-law debt reduction is still significant but slightly smaller, −0.019. When managers own more than 25 percent of stock the post-law debt reduction is small, −0.003, and is less than one-third of a standard error from zero, despite a sample size larger than that of the other two ownership ranges. The results are similar if leverage changes are measured by net change in the debt ratio.

Thus, the data suggest that post-law debt reductions are strongest among firms with low percentage management ownership, slightly weaker in firms with moderate management ownership, and insignificant in firms with

dominant management ownership. These findings are not consistent with the hypothesis that post-law debt reductions are due to post-law relaxation of managers' use of debt to concentrate their voting control. Rather, these results suggest that managers with large voting blocks are effectively entrenched and hence are affected little by the passage of antitakeover laws. We conclude that, although managers in our sample may use debt to concentrate their percentage voting control, this effect does not entirely explain the observed leverage reductions following protection by antitakeover laws.

VI. Concluding Remarks

Entrenchment models of financial policy such as Novaes and Zingales (1995) and Zwiebel (1996) envision managers who take on debt to reduce the threat of hostile takeover. A direct implication is that impediments to takeovers will allow managers to reduce leverage, forgoing the tax and agency benefits of debt in order to reduce the risk of financial distress and avoid constraints on their allocation of cash flows.

The passage of second-generation state antitakeover laws, in the years 1987 to 1990, provides a relatively exogenous increase in the cost of a hostile takeover, along with a natural control sample of states that did not pass such laws. We predict that protected firms will reduce their use of debt relative to unprotected firms. We find that protected firms substantially reduced their debt ratios relative to unprotected firms over a period of four years. These results survive a number of robustness checks and are not due to variation in size, industry, or profitability.

Leverage is affected by changes in either debt or equity, but changes in debt have larger effects on the debt ratio and largely drive the leverage changes in our sample. Debt changes are also more strongly related to the passage of antitakeover laws. While post-law changes in equity funding have little effect on leverage ratios, they are of approximately the right magnitude to compensate for the post-law loss of debt funding, though our estimates are not precise in this regard. We find no significant post-law changes in size or profitability, so we find no evidence of gross overinvestment or free cash flow abuses among protected managers. We find weak evidence that protected managers undertake fewer major restructuring and investment programs, suggesting some degree of inertia.

An unexpected finding is that firms that were eventually covered by antitakeover legislation used leverage more aggressively in the years *preceding* the adoption of such laws. This suggests that the results might be interpreted as a return to normality following the dissipation of some unusual pre-law circumstance. However, the post-law geographic variation in debt changes is larger than the pre-law variation, especially for the sharpest debt changes, and we find unusual post-law debt reductions in protected firms that had not increased their leverage in the pre-law years. Consequently the results suggest a post-law effect rather than the dissipation of a pre-law effect.

Dividing the sample by insider ownership levels, we observe the largest post-law leverage reductions when insiders control less than 5 percent of equity, and no significant post-law reductions when insiders control more than 25 percent of equity. This result is consistent with a straightforward entrenchment hypothesis, and suggests that our results are not entirely due to reversal of managers' pre-law use of debt to concentrate their percentage voting control.

These results provide a concrete instance of the disciplinary role of takeover threats, and they support the view that capital structure is affected by managerial discretion. Future research might fruitfully investigate nonfinancial variables such as investment and employment decisions.

Appendix: A Formal Model of Control and Agency Effects

Consider the following stylized model, which captures key features of the agency and control approaches. Assume that managers make a once-off choice of the firm's debt/assets ratio, denoted L. The stand-alone value of the firm denoted $V(L)$ is a single-peaked function of L with $V'(0) > 0$, $V'' < 0$, and is maximized at $V_{\max} \equiv V(L_{\max})$, $V'(L_{\max}) = 0$. There is a single potential bidder who is willing to pay $V_{\max} + s$ for the firm, where s is a private synergy gain and $V_{\max}$ reflects the assumption that the bidder intends to optimally leverage the firm. The bidder must pay a premium over the firm's stand-alone value, denoted $P(X,L)$ where X denotes the effectiveness of state antitakeover laws with $P_X > 0$. Stulz's (1988) control argument implies that leverage also directly increases the premium that a successful bidder must pay, $P_L > 0$. To simplify the exposition we assume there is no direct cross-effect, $P_{LX} = 0$.

The bidder's synergy value is his private information, and the manager and the rest of the market know only that it is drawn from the cumulative distribution $H(s)$. Thus, the firm is taken over with probability $1 - H(s^*)$ where s^* is the synergy value at which the raider is just willing to pay the premium P. For any given leverage choice by the manager, the critical synergy value satisfies $s^* + V_{\max} = P + V(L)$, so $s^* = P(X,L) - [V_{\max} - V(L)]$. To ensure an interior solution, we assume that the hazard rate $h(y)/H(y)$ strictly decreases in y. The manager can also lose her private benefits if the firm goes into financial distress, which occurs with probability $F(L)$ with $F_L > 0$. The manager chooses leverage to maximize the probability that she averts both financial distress and takeover, $M \equiv H(s^*(L))[1 - F(L)]$. The first-order condition characterizing her optimal choice L^* is:

$$M_L(L^*) = h(s^*)[1 - F(L^*)][V_L(L^*) + P_L(L^*)] - H(s^*)F_L(L^*) = 0. \qquad \text{(A1)}$$

The last term of equation (A1) indicates that managers avoid leverage to reduce the prospect of distress. The first term captures the two ways in which higher leverage could reduce the threat of takeover. The term P_L re-

flects Stulz's (1988) control effect; higher leverage increases the premium paid by a successful bidder and thus reduces the prospect of takeover. The term V_L captures the agency cost effect; higher leverage can increase the stand-alone value of the firm, thus reducing the gain to a potential bidder. Note that if the control effect is small (so that P_L is near-zero), equation (A1) requires V_L to be positive, meaning that the firm is underleveraged from the shareholders' perspective.

The result that antitakeover legislation reduces leverage is a straightforward comparative statics implication of equation (A1). The effect of takeover legislation on the manager's leverage choice has the same sign as

$$M_{LX} = P_X[h'(s^*)(1 - F(L))[V_L + P_L] - h(s^*)F_L]. \tag{A2}$$

Using equation (A1) to substitute out for the term $(1 - F(L))[V_L - P_L]$, we can express equation (A2) as

$$M_{LX} = P_X F_L \left(\frac{h'(s^*)H(s^*)}{h(s^*)} - h(s^*) \right). \tag{A3}$$

Equation (A3) is strictly negative, so that leverage is decreased by antitakeover legislation, by the decreasing hazard rate property of H. This conclusion holds so long as there is an interior solution to equation (A3). Such a solution exists so long as there is an agency cost effect (meaning that V_L is positive at L^*), a control effect (so that P_L is positive), or both. Thus, our finding that antitakeover legislation reduced the use of leverage does not directly establish whether one or both of the models are important.

The two approaches also share the property that antitakeover legislation need not have a chilling effect on takeover activity. The probability of a takeover in this model is simply $1 - H(s^*)$. The effect of antitakeover legislation on takeover activity is thus

$$\frac{\partial(1 - H(s^*))}{\partial X} = -h(s^*)\frac{\partial s^*}{\partial X} = -h(s^*)\left[P_X - \frac{\partial L^*}{\partial X}(P_L + V_L) \right]. \tag{A4}$$

The term $-h(s^*)P_X$ reflects the direct effect of legislation on the probability of takeover and is unambiguously negative. The term $h(s^*)[(\partial L^*/\partial X)(P_L + V_L)]$ is, however, unambiguously positive because the reduced leverage following antitakeover legislation increases the attractiveness of the firm as a target.

The control and agency cost approaches do, however, differ in their implications for the stock-price reaction to the announcement of the legislation. Stockholder wealth at the time of the legislation, $W = V(L^*) + P(1 - H(s^*))$, reflects the stand-alone value of the firm plus the expected takeover premium. The effect of legislation on the stock price can thus be expressed:

$$W_X = \frac{\partial L^*}{\partial X} V_L + \left(P_X - \frac{\partial L^*}{\partial X} P_L \right)(1 - H(s^*)) - P\frac{\partial(1 - H(s^*))}{\partial X}. \tag{A5}$$

The first term of equation (A5) is negative, reflecting the insight of the agency cost model that takeover barriers allow managers to retain more slack. The second term reflects the increase in takeover premium conditional on a successful bid, accounting for the fact that leverage is reduced, which may reduce the premium. Comment and Schwert (1995) find that legislation significantly increased premiums, so the direct effect P_X seems to dominate. Comment and Schwert (1995) also find that the last term, reflecting the reduced probability of a takeover, is near zero. Recall, however, that Karpoff and Malatesta (1989) find a significant negative stock-price reaction to the announcement of antitakeover legislation, implying that equation (A5) is in fact negative. This finding cannot be reconciled with Comment and Schwert's (1995) evidence unless there is a significant agency cost effect. Thus, although the evidence does not reject the control theory, it does suggest that agency costs are present.

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