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State takeover legislation and share values: The wealth effects of Pennsylvania's Act 36

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Abstract

Proponents of state antitakeover legislation argue that previous empirical tests by financial economists of the wealth effects of Pennsylvania's 1990 antitakeover law are biased. We show that the proponents are correct. In particular, firm size, event-time clustering, and non-synchronous trading effects account for the wealth decreases reported in earlier studies. We also show, however, that both proponents and critics of the Pennsylvania legislation have ignored the earliest press release about it. The wealth effect associated with this announcement is negative, large, and statistically significant. These results therefore are consistent with the hypothesis that the Pennsylvania law decreased company values *and* with the hypothesis that the initial market reaction is an unbiased estimate of the law's effect on firm values.

1. Introduction

Pennsylvania's Act 36 is notable if for no other reason than the large amount of ink spilled over it. The act, signed into law on April 27, 1990, erected new barriers to hostile acquisition of Pennsylvania companies. It also

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attracted scrutiny from large investors, the media, and researchers at a level unprecedented in the history of state takeover legislation. [1]

The Act attracted much attention because many investors and researchers believe that laws impeding takeovers entrench poorly performing managers and decrease shareholder wealth. Several studies of the Pennsylvania law yield results that are consistent with this belief (see Karpoff and Malatesta [1990], Nesbitt [1990], Szewczyk and Tsetsekos [1992], and Marr [1992] for a survey). Proponents of restrictive antitakeover measures, however, remain unconvinced, and point to estimation biases that may affect the results. Wallman [1991] argues that size effects and estimation error account for the measured stock return effects in these studies, and Margotta [1991] argues that the evidence does not support the conclusion that Act 36 resulted in market value losses.

This paper seeks to resolve the debate by explicitly controlling for firm size, non-synchronous trading, and stock return cross-sectional covariances in testing hypotheses about Act 36's effects. We find that these controls do substantially weaken the evidence of negative stock price reactions reported in the earlier studies. For example, Szewczyk and Tsetsekos [1992] report a cumulated abnormal return of -3.33% with a t-statistic of -4.80 for key dates in the Act's legislative history. Introducing the firm size and other controls in an otherwise similar experiment, however, we find a cumulated abnormal return of only -0.52% with a t-statistic of -0.36. Controls for size, non-synchronous trading, and cross-sectional covariance therefore undermine the earlier evidence that Act 36 reduced Pennsylvania company share values. Nevertheless, it would be incorrect to conclude that the Act left share values unaffected. This is because earlier studies do not consider the period when information about Act 36 first became publicly available. Including all controls, the two-day cumulated abnormal return for this first news release is -1.43% with a t-statistic of -2.89. Thus, we find evidence of a negative stock price effect when the announcement period is defined to include the first news release. Our findings therefore are consistent with the hypothesis that Act 36 decreased company values *and* with the hypothesis that the initial market reaction is an unbiased estimate of the Act's effect on firm values.

The remainder of the paper is organized as follows. Section 2 outlines the main provisions of Act 36. Section 3 reviews previous research on the Act's effects and the criticisms of this research. In section 4, we describe the data

[1] Examples include unfavorable articles in *The Wall Street Journal* (Pound [1989]), The *New York Times* (Wayne [1990]), and *Forbes* (Zweig [1990]). The director of the Pennsylvania public employee pension fund claimed that the bill would force him to divest the fund of Pennsylvania companies, and the California Public Employees Retirement Fund asked 22 large companies incorporated in Pennsylvania to opt out of coverage by the law (Jaffe [1990]).

and empirical method we employ to avoid these criticisms. Section 5 reports our results, and section 6 concludes the paper.

2. Act 36's main provisions [2]

Act 36 contains five provisions that offer covered Pennsylvania companies barriers to hostile takeover bids. The first provision is a variation of Indiana's control share acquisition statute previously upheld by the U.S. Supreme Court in 1987. [3] Unlike previous control share acquisition laws, however, the Pennsylvania law limits the voting rights of a greater number of shares. In some cases voting rights are restricted on shares acquired by any investors within 12 months of a control contest, stripping voting rights not only from the bidder, but also from any passive or active new shareholders such as risk arbitrageurs. The provision therefore increases management's chances to defeat a takeover bid even if a majority of shares are owned by non-bidder investors who favor the bid.

Act 36's second provision explicitly allows corporate directors to weigh the effects of their decisions on *all* corporate stakeholders, including customers, employees, suppliers, and local community groups. This provision provides legal cover to managers who wish to thwart an unwanted takeover bid even if the bid would benefit shareholders. The provision protects managers from shareholder lawsuits, removing a mechanism shareholders could use to hold managers accountable for poor decisions.

Act 36's third provision requires bidders who sell stock in the corporation during the 18 month period following a takeover attempt to relinquish to the corporation any profits from the sale. This disgorgement provision increases the risk that a bidder will suffer substantial losses, and decreases the likelihood that a takeover bid will be mounted. [4]

The fourth provision mandates a minimum severance payment to certain employees who are terminated within two years of a corporate control change, and the fifth provision makes preexisting labor contracts binding on the new management team in the event of a control change in a Pennsylvania company. At the urging of the managers of Westinghouse Electric, Inc., the

[2] Szewczyk and Tsetsekos [1992], and Marr [1992] provide more detailed descriptions of the legislative history and the main provisions of Act 36.

[3] *CTS v. Dynamics Corp. of America*, 481 U.S. 69 (1987).

[4] The bidder may be able to hedge his exposure by selling short against the box, or entering into an appropriate swap contract. Nevertheless, such transactions are imperfect substitutes for an outright sale of shares, and the disgorgement provision seems likely to raise takeover barriers to some degree.

Act was written to allow Pennsylvania companies to opt out of coverage by any of the first three provisions. The fourth and fifth provisions apply only to companies that did not opt out of coverage by the control share acquisition portion of Act 36.

3. The debate over Act 36's effects

In addition to the attention it attracted from the press and financial institutions, Act 36 has produced a flurry of research. Karpoff and Malatesta [1990] report that Pennsylvania companies' stock prices declined by an average of 5.8% relative to the S&P 500 Index from the time the bill was first publicized until the date it was introduced in the Pennsylvania House. Using a market model, Szewczyk and Tsetsekos [1992] find that Pennsylvania firms' stock suffered abnormal losses averaging 9.09% from the date the bill was introduced in the Pennsylvania Senate through the date it became law. Nesbitt [1990] uses a multifactor model of stock returns and concludes that the Pennsylvania law reduced share prices of affected firms an average of 4%. Marr [1992] summarizes the evidence as indicating that the average Pennsylvania firm lost 5.9% of its market value because of Act 36.

These results have been criticized by supporters of the law. The most damaging criticism of previous empirical studies is that they do not adequately control for firm size effects on stock returns (e.g., see Wallman [1991]). In particular, small firms underperformed large firms in late 1989 through early 1990. For example, the seven-month return for the S&P 500 from October 1989 through April 1990 was −5.3%, while it was −11.2% for the NASDAQ Composite Index. Firm size effects may be particularly important in this case, because the typical Pennsylvania company is smaller than the companies used to construct benchmarks to measure abnormal performance. Fifty-seven Pennsylvania companies were listed on the NYSE and AMEX from October 1, 1989 through April 30, 1990. The mean market value of common stock for these firms was $1.3 billion as of September 30, 1989. In contrast, the equity capitalization of firms listed on the NYSE averaged $1.8 billion as of December 31, 1989.

The relatively poor performance of small stocks clearly accounts for at least some of the measured effect of Act 36 on Pennsylvania firms' stocks. For example, duplicating Karpoff and Malatesta's [1990] experiment, but using the Value Line equally-weighted index instead of the S&P 500 Index as a benchmark, Pennsylvania firms' underperformed the benchmark from early October 1989 through January 2, 1990 by only slightly more than 1.0%, not 5.8%.

The smallness of Pennsylvania firms may also contribute to estimation problems due to non-synchronous trading. Since Pennsylvania firms are small compared to the average NYSE firm, standard least squares estimates of

their market model betas tend to be biased downward. During a period of falling stock prices, this bias in betas leads to a downward bias in estimates of Pennsylvania companies' abnormal stock returns.

An additional problem with previous examinations of Act 36's effects is that they do not control for the effects of real-time clustering of sample firms' returns on test-statistics. In constructing hypothesis tests, Karpoff and Malatesta, Nesbitt, and Szewczyk and Tsetsekos all assume that estimated abnormal stock returns for given event dates are uncorrelated across different Pennsylvania companies. This assumption is defensible in most event studies, in which abnormal stock returns of different firms are examined at different times. In the case of Act 36, however, the zero correlation assumption is surely false because the event periods are the same for all Pennsylvania companies. Ignoring the contemporaneous correlations of Pennsylvania firms' estimated abnormal stock returns biases test statistics upward. The empirical tests reported below demonstrate that firm size effects, non-synchronous trading, and event time clustering substantially affect inferences about the impact of Act 36 on stock prices.

4. Data and empirical method

4.1. Data

Using Moody's Stock Guide, we identified 61 firms that were incorporated in Pennsylvania and listed on the New York or American Stock Exchanges from October 1989 through April 1990. The sample size was increased to 62 to include PNC Financial Corp., which is incorporated in Pennsylvania but not identified in Moody's as such. Stock returns were taken from the 1992 CRSP daily returns file. Five firms were excluded due to missing returns data, leaving a final sample of 57 firms.

Act 36 was first introduced in the Pennsylvania Senate as Senate Bill 1310 on October 20, 1989. The first print media report on the Senate bill appeared in *The Wall Street Journal* on December 1, 1989. During the subsequent two weeks the bill attracted significant press coverage in the national and regional press, including all major Pennsylvania newspapers. The bill passed the Pennsylvania Senate on December 13, 1989, toward the end of the two-week period of newspaper coverage. It then was introduced in the Pennsylvania House on January 2, 1990, where it passed on April 3, 1990. The bill was signed into law as Act 36 on April 27, 1990.

To measure the impact of Act 36, we focus (initially) on stock returns around each of the following five events:

(i) October 20, 1989, when the Senate Bill 1310 was introduced in the Pennsylvania Senate;

(ii) November 30–December 15, 1989, during which the first press article about the then-proposed legislation was published (December 1), the legislation received significant press attention, and the legislation passed the Senate (December 13, 1989).
(iii) January 2, 1990, when the Act was introduced in the Pennsylvania House.
(iv) April 3, 1990, when the Act passed the House; and
(v) April 27, 1990, when the Act was signed by the Pennsylvania Governor.

In each case, we report stock return forecast errors for the two-day period that includes the day of the legislative event and the day after, to allow for reporting lags. The exception is event interval (ii), which begins on the day immediately before the first press article about the legislation and continues through the last day of a string of press articles about the legislation.

4.2. Empirical method

Our empirical methods are based on the event-parameter approach discussed by Binder [1985a, 1985b, 1988], Schipper and Thompson [1983], Malatesta [1986], and Salinger [1992]. In this approach dummy variables are appended to the time series market model as additional regressors. Each dummy variable corresponds to a specific event day of interest and is set equal to one for that day and equal to zero for all other days. In particular, we estimate the multi-factor model given by equation (1) using returns from 310 through 60 trading days before the first wire service report on the legislation (October 13, 1989), plus the event intervals:

$$r_{pt} = b_0 + b_1 r_{m,t-1} + b_2 r_{m,t} + b_3 r_{m,t+1} + b_4 SIZE_t + \sum_{i=1}^{D} \gamma_i d_i + e_{pt}. \qquad (1)$$

where r_{pt} = the date t return on the equally-weighted portfolio of Pennsylvania stocks
$r_{m,t-1}$ = the lagged return on the CRSP value-weighted index
$r_{m,t}$ = the contemporaneous return on the CRSP value-weighted index
$r_{m,t+1}$ = the leading return on the CRSP value-weighted index
$SIZE_t$ = the date t return on the NASDAQ composite index minus the date t return on the Standard and Poor's stock index, and
d_i = a dummy variable set equal to one for event day i, for each of the D event days under consideration.

b_0, b_1, b_2, b_3, b_4 and the γ_i are parameters to be estimated, and the error terms, e_{pt}, are assumed to be identically and independently normally dis-

tributed. The event days in D depend on the specific test, but typically include the five event periods identified in section 4.1.

The event parameter approach allows us easily to incorporate the effects of event-time clustering, firm size, and non-synchronous trading in estimates of Act 36's effects. Our solution to the problem of cross-sectional abnormal return correlation (due to event-time clustering) is straightforward. Except when attempting to replicate earlier work, we do not estimate event period abnormal returns and their standard errors for each firm separately and then aggregate the results. Instead, for most of our experiments we form an equally weighted portfolio of Pennsylvania firms and analyze the single time series of portfolio returns.

The size factor in equation (1) is included to reflect the differences in stock returns between large and small firms during the event period. Including the size factor in our market model provides an improved benchmark against which to measure abnormal returns attributable to Pennsylvania's Act 36. To improve beta estimates for less frequently traded stocks, equation (1) also includes lagged and leading market factor terms. Dimson [1979] finds that including lagged and leading market factor terms in the market model improves beta estimates substantially for infrequently traded securities. Following Dimson, we include one lagged and one leading market factor term, as well as the contemporaneous market index return, in our benchmark model.

Ordinary least squares estimates of the dummy variable coefficients γ_i are identically equal to stock return conditional forecast errors calculated using the market model. Sums of dummy variable coefficient estimates correspond to cumulated conditional market model forecast errors. Equation (1) therefore permits us easily to construct correct standard error estimates and relevant t-statistics for testing hypotheses about cumulated forecast errors. Thus, unlike earlier studies of Pennsylvania's Act 36, we do not ignore the serial correlation of estimated abnormal stock returns in constructing test statistics. As Salinger [1992] demonstrates, ignoring abnormal return serial correlation biases test statistics and may lead to erroneous inferences about cumulated abnormal returns.

In some cases we want also to compare stock price reactions for one group of Pennsylvania firms to reactions for a different group. To do this, we form two equally weighted portfolios, one for each group of firms. Market model (plus event dummy) equations for each portfolio form a seemingly unrelated regressions system. We estimate and perform hypothesis tests on the coefficients of the system using the standard EGLS methods described in Judge, et al [1985]. Because the regressors in the system are the same for both equations, the estimated event dummy variable coefficients are identically equal to stock return conditional market model forecast errors for the two portfolios of firms. However, the event parameter approach allows us easily

Table 1
Cumulated abnormal returns around key dates in the adoption of Pennsylvania's Act 36, 1989–1990 for 57 Pennsylvania companies listed on the New York and American Stock Exchanges. These results are computed using Dimson beta estimates for the Pennsylvania portfolio. Column (A) reports results that mimic previous reported effects of Act 36, in which neither size effects nor the first informational event are considered. Column (B) reports results that include a control for the effect of firm size on returns. Column (C) results incorporate both firm size effects and the first informational event. Abnormal returns for the informational event intervals are in percent. For all event intervals except no. 3, the interval consists of the day cited and the following trading day. t-statistics are in parentheses

	(A)	(B)	(C)
Control variables			
Lagged market return	0.084	0.043	0.043
	(2.75)	(1.44)	(1.44)
Contemporaneous market return	0.596	0.771	0.771
	(19.65)	(16.78)	(16.78)
Leading market return	0.009	0.010	0.010
	(0.29)	(0.36)	(0.36)
Size factor		0.339	0.339
		(4.91)	(4.91)
Informational event intervals			
1. Oct. 13–16			**−1.43**
(First newswire story)			**(−2.89)**
2. Oct. 20	−0.95	−0.73	−0.73
(Introduced in PA Senate)	(−2.15)	(−1.73)	(−1.73)
3. Nov. 30–Dec. 15	−1.59	−0.00	−0.00
(First press story;	(−1.39)	(−0.00)	(−0.00)
Senate debates and passes bill)			
4. Jan. 2	0.27	0.17	0.17
(Introduced in PA Assembly)	(0.60)	(0.40)	(0.40)
5. Apr. 3	0.02	0.05	0.05
(Assembly passes bill)	(0.04)	(0.13)	(0.13)
6. Apr. 27	0.04	−0.01	−0.01
(Governor signs bill)	(0.10)	(−0.03)	(−0.03)
Sum over all event intervals	−2.21	−0.52	−1.96
	(−1.50)	(−0.36)	(−1.25)

to compute standard errors and statistics for testing hypotheses on linear functions of the coefficients across the equations.

5. Empirical results

5.1. Wealth consequences of Pennsylvania's Act 36

The major results from our experiment are summarized in Table 1, which contains the estimation results for the model given in equation (1) and two

other model specifications. In all three cases dummy variables are included to measure the abnormal stock returns for the five event periods identified in section 4.1: (i) The introduction of Senate Bill 1310, (ii) the period of the first press announcement and during which the Senate passed Bill 1310, (iii) the introduction of the bill in the Pennsylvania House, (iv) the passage of the bill by the House, and (v) the Pennsylvania Governor's signing of the bill into law.

The results reported in column A of Table 1 are for a market model that excludes the size factor. The cumulated abnormal return for the portfolio of Pennsylvania firms for the day Senate Bill 1310 was introduced and the following trading day is -0.95%, with a t-statistic of -2.15. This result is consistent with previous researchers' findings, and with the interpretation that the Pennsylvania law is associated with significant stock price declines for the affected companies. Cumulating over all five event periods, the abnormal stock return for Pennsylvania companies also is negative, -2.21%, although the t-statistic is small, -1.50.

As reported in column B, however, these results are sensitive to the inclusion of the firm size factor. When the firm size factor is included, its coefficient is significantly positive (0.339, $t = 4.91$), indicating that Pennsylvania stocks are significantly exposed to small firm risk. Furthermore, inclusion of the size factor changes the estimates of the Pennsylvania law's effects on Pennsylvania companies. The abnormal return for the Senate introduction period is -0.73% and statistically insignificant at normal levels ($t = -1.73$). The abnormal return cumulated over all five event intervals is -0.52% with a t-statistic of -0.36. These results support the claims made by defenders of Act 36, that earlier reports of large and statistically significant stock price effects are biased.

However, none of the results reported above or in prior research capture the effect of the first news about the Act. The first *printed* press article about the legislation appeared in *The Wall Street Journal* on December 1, 1989. Szewczyk and Tsetsekos and Nesbitt identify October 20, 1989 – the date the legislation was introduced in the Pennsylvania Senate – as the first time news was released about the legislation. Both events, however, were preceded by an October 13, 1989 wire service story that was titled "Rorer: Don't Stop Corporate Takeovers – Stop Abuses". The article reports on a speech made to the Pennsylvania Senate Republican Caucus by Robert E. Cawthorn, chairman and CEO of Rorer Group, Inc., a Pennsylvania company. Cawthorn is quoted as praising Pennsylvania State Senator Wenger for sponsoring the bill that was soon to be introduced as Senate Bill 1310.

We have found no evidence that this story was ever published by a newspaper or magazine. Its depiction of the upcoming legislation, however, was carried by a major wire service (PR Newswire). It was included in the news services used by many stock analysts, and therefore was in the public domain.

Column C of Table 1 reports the results of a test designed to measure the effects of both firm size and the October 13, 1989 wire service story. For the day of the wire service story and the day after, the abnormal return for the portfolio of Pennsylvania stocks is −1.43% with a t-statistic of −2.89. This stock price effect is larger in absolute value than that of any of the subsequent event periods. As indicated above, the Pennsylvania portfolio cumulated abnormal return is −0.52% over all five event periods following the initial newswire story, with a t-statistic of −0.36. [5]

The results in Table 1 indicate that neglecting the firm size factor has a material impact on estimates of the wealth effect of Act 36. But so does neglecting the first public release of information about the Act. In this case, these two errors of omission almost exactly offset each other. Our expanded treatment of the key events, however, also is consistent with market efficiency because it demonstrates that the largest stock price reaction occurred at the time of the first news release about the legislation. The cumulated abnormal stock return for all key events following the first news release is not significantly different from zero. We interpret these findings as consistent with the hypothesis that the initial stock price reaction was an unbiased estimate of the valuation effects of the legislation.

5.2. *Effect of event time clustering*

The results in Table 1 indicate that previous research findings about the effects of the Pennsylvania law are influenced by their failure to consider firm size effects and the initial release of information about the law. The results in Table 2 indicate that previous findings also are biased by failure to consider the effects of event time clustering on abnormal stock return cross-sectional covariances. To illustrate, column (A) of Table 2 reports the main findings of Szewczyk and Tsetsekos [1992, Table 4]. Szewczyk and Tsetsekos estimate the mean abnormal returns for a sample of 56 Pennsylvania companies for each of four important event periods. [6] Abnormal returns

[5] Including leading and lagged size factor terms does not change these results materially. For example, the cumulated abnormal return for the two-day wire service story interval is −1.49% with a t-statistic of −2.86. The effects of firm size and the initial wire service story are even more pronounced when the leading and lagged market returns are omitted from the model. Including contemporaneous firm size effects but omitting the October 13, 1989 wire service story, the cumulated abnormal return for all five event intervals is −0.34% with a t-statistic of −0.23. The abnormal return for the two-day wire service story interval in this case is −1.61% with a t-statistic of −3.38.

[6] These events correspond to four of the five intervals presented in section 4.1., but exclude the introduction of the bill in the Pennsylvania Assembly.

Table 2
Estimates of abnormal returns, in percent, around four key dates in the legislative history of Pennsylvania's Act 36. Column (A) reports the findings reported in Szewczyk and Tsetsekos [1992] for 56 PA firms. Column (B) reports the results from replicating the Szewczyk-Tsetsekos procedure using data from 57 PA firms. Column (C) reports the results from the event dummy procedure reported in section 4 and used for subsequent empiricial tests, which controls for contemporaneous covariances between the stock returns of the sample firms. In none of the three sets of results are there controls for size effects. Each event interval consists of the three-day window centered on the date identified. t-statistics are in parentheses

	(A) Szewczyk-Tsetsekos results	(B) Karpoff-Malatesta replication	(C) Event-parameter approach
Informational event intervals:			
Oct. 20	−1.19	−1.27	−1.27
(Introduced in PA Senate)	(−2.84)	(−3.40)	(−2.29)
Dec. 13	−1.26	−0.80	−0.80
(Passed PA Senate)	(−2.64)	(−1.59)	(−1.45)
Apr. 3	−0.12	−0.01	−0.01
(Passed PA Assembly)	(−1.64)	(−1.21)	(−0.03)
Apr. 27	−0.76	−0.72	−0.72
(Governor signs bill)	(−2.47)	(−2.56)	(−1.30)
Sum over all event intervals	−3.33	−2.80	−2.80
	(−4.80)	(−4.38)	(−2.46)

are computed relative to the single factor market model. The S&P 500 Index is used as the market factor, and model parameters are estimated from the 200 trading day period ending immediately before October 20, 1989. Each event period is defined as the three-day window centered on the date cited in Table 2. As indicated in column (A), Szewczyk and Tsetsekos report a mean cumulated effect over the four event periods of −3.33%, with a t-statistic of −4.80.

Column B reports results from replicating Szewczyk and Tsetsekos's experiment using our sample of 57 Pennsylvania companies. Our results are very similar to those reported by Szewczyk and Tsetsekos. We also find an apparently statistically significant cumulated abnormal return for the four event intervals.

Column C reports the abnormal returns for these four event periods using the event-parameter procedure described in section 4 but with the same estimation interval and market benchmark as Szewczyk and Tsetsekos used. This procedure correctly incorporates the cross-sectional covariances of estimated abnormal stock returns in estimating standard errors and computing test statistics. The point estimates for the abnormal stock returns in columns B and C are (necessarily) identical, but the test-statistics are lower in column C. For example, the cumulated abnormal return for the three-day interval centered on the day Senate Bill 1310 was introduced in the Pennsylvania

Senate is −1.27%. Using the standard event-study procedure (as did Szewczyk and Tsetsekos), the associated t-statistic is −3.40. Using the event-parameter procedure, however, yields a t-statistic of −2.29. The cumulated abnormal return for all events is −2.81%, with a t-statistic of −4.38 using the traditional procedure but only −2.46 using the event-parameter procedure. The test statistic is 44% lower when cross-sectional covariances are treated properly, indicating that previous inferences about the Pennsylvania law's effects are affected in part by failure to consider such covariances.

5.3. Wealth effects conditional on prior takeover defenses

State antitakeover laws like Pennsylvania Act 36 are just one device that may protect managers from unsolicited takeover bids. Firms also can adopt antitakeover protections such as poison pills and supermajority vote requirements. Karpoff and Malatesta [1989] propose that the effect of a state antitakeover law on an affected firm's value depends on the firm's other takeover protections. In particular, the substitution hypothesis implies that the protection from unsolicited bids provided by a state law is largely redundant with that provided by firm-level defenses. The incremental effect of a state law therefore is small for firms that have other takeover defenses, and the stock price effect similarly is small.

The complements hypothesis, in contrast, predicts that coverage by a state antitakeover law will have relatively large stock price effects for firms that have preexisting takeover defenses. Most state laws give managers the authority to avoid triggering the laws' antitakeover provisions. Such is the case for Pennsylvania Act 36, which allowed managers to opt out of coverage by some or all of the law's provisions by July 26, 1990, and which allows some discretion even for managers of firms that did not opt out of coverage. Thus, state laws are similar to firm-level takeover defenses in the sense that the provisions of both are triggered at managers' discretion. The complements hypothesis holds that managers who seek protection from unsolicited takeover bids will adopt firm-level takeover defenses and be willing to trigger the takeover protection afforded by the state law. Under this hypothesis, state takeover laws provide one additional line of defense for managers who are most likely to use such defenses. The stock price effect of the state antitakeover law therefore should be largest for firms whose managers have signalled willingness to use takeover defenses, i.e., for firms that already have such defenses.

To test the substitution and complements hypotheses, we examine the abnormal stock returns for each of two groups of firms obtained by partitioning the sample into firms that had takeover defenses before Senate Bill 1310 was introduced and firms that did not. According to the Investor Responsibility Research Center, Inc. publication, Corporate Takeover Defenses (Rosen-

Table 3
Cumulated abnormal returns around key dates in the adoption of Pennsylvania's Act 36, 1989–1990 for two groups of Pennsylvania companies listed on the New York and American Stock Exchanges. Column (A) contains results for 29 firms that each had at least one of the following firm-level takeover defenses in place by October 1, 1989; poison pill, classified board, fair price provision, and supermajority voting provision. Column (B) contains results for 28 firms that had none of these takeover defenses in place by October 1, 1989. Abnormal returns for the informational event intervals are in percent. For all event intervals except no. 3, the interval consists of the day cited and the following trading day. t-statistics are in parentheses

	(A) 29 firms with takeover defenses	(B) 28 firms without takeover defenses
Control variables:		
Lagged market return	0.076	0.010
	(2.68)	(0.10)
Contemporaneous market return	0.827	0.713
	(19.16)	(9.16)
Leading market return	0.014	0.007
	(0.51)	(0.14)
Size factor	0.307	0.371
	(4.75)	(3.18)
Informational event intervals:		
1. Oct. 13	**−0.56**	**−2.33**
(First newswire)	**(−1.21)**	**(−2.78)**
2. Oct. 20	−0.92	−0.54
(Introduced in PA Senate)	(−2.31)	(−0.75)
3. Nov. 30–Dec. 15	0.38	−0.39
(First press story;	(0.36)	(−0.21)
Senate debates and passes bill)		
4. Jan. 2	−0.29	0.64
(Introduced in PA Assembly)	(−0.73)	(0.89)
5. Apr. 3	0.14	−0.04
(Assembly passes bill)	(0.36)	(−0.06)
6. Apr. 27	−0.69	0.69
(Governor signs bill)	(−1.77)	(0.97)
Sum over all event intervals	−1.94	−1.97
	(−1.32)	(−0.74)

baum [1989]) 29 Pennsylvania firms had at least one of the following takeover defenses before 1989: poison pill, classified board, fair price provision, and supermajority vote requirement. Twenty-eight firms had none of these defenses.

Column A of Table 3 reports the results for the group of 29 companies with prior firm-level defenses. Column B reports the results for the group of 28 firms without prior defenses. The results indicate that, once again, one's inference turns on the correct identification of the first public release of

information about the law. The abnormal return associated with the initial wire service story about Act 36 is larger for the group of undefended firms, −2.33% (t-statistic = −2.78), than for the defended firms, −0.56% (t-statistic = −1.21). Using EGLS methods that take into account the cross-sectional covariance in the abnormal returns of the two groups, the t-statistic for the difference in abnormal returns is 2.04. This is consistent with the substitution hypothesis. Ignoring the initial publicity, though, leads to a different inference. Summed over the five event intervals after the initial newswire story, the abnormal return for the portfolio of defended firms is −1.38%, compared to 0.36% for the undefended firms. (The t-statistic for the difference is −0.68) This result could lead to the mistaken inference that the data provide no support for the substitution hypothesis.

Focusing on the first press announcement for each of a broad sample of 40 different stateantitakeover laws, Karpoff and Malatesta [1989] report evidence favoring the substitution hypothesis. The results reported here, when we focus on the first wire service report about the Pennsylvania law, are consistent with those reported in our earlier work. It appears that stock price reactions to the Pennsylvania law were similar to those for other antitakeover laws in that undefended firms experienced larger stock price declines than defended firms. This result supports the substitution hypothesis and reinforces the conclusion drawn in earlier work.[7]

6. Conclusions

Previous research on state takeover laws indicates that, on average, such laws have adverse but small effects on the values of affected companies (e.g., Karpoff and Malatesta [1989]). Pennsylvania's Act 36 appears to be an anomaly, as researchers have claimed that it decreased the equity values of affected companies by a much larger amount, on average −5.9% (Marr [1992]). Defenders of Act 36, however, claim that researchers were mistakenly blaming Act 36 for the generally poor performance of small firm stocks during the 1989–90 period.

In this paper we show that the defenders are at least partially correct. Controlling for firm size substantially reduces estimates of abnormal losses suffered by Pennsylvania firm stockholders. When the event intervals used by

[7] Even though they do not consider the initial wire service story, Szewczyk and Tsetsekos [1992] interpret their findings as consistent with the substitution hypothesis. Such an interpretation is inconsistent with our results when we exclude the initial wire service story. The difference may be due to the different ways we have partitioned the firms. Szewczyk and Tsetsekos identify 12 of their 56 sample firms as having prior takeover defenses, whereas we have identified 29 of our 57 sample firms as having such prior defenses.

most previous researchers are examined and a size factor is included in the market model, the stock price impact of the Pennsylvania law is statistically insignificant. We also show, however, that previous research has overlooked the first public release of information about Act 36, a wire service story that to our knowledge was never published in a newspaper. The abnormal return to Pennsylvania companies during the period the wire service story was released is negative and statistically significant. The two-day announcement interval abnormal return is −1.43%. This is still large compared to that found for other state takeover laws, on average, but smaller than reported in previous research on the Pennsylvania law.

Focusing on the first public release of information about the Pennsylvania law also is critical when investigating how the law interacts with firm-level takeover defenses. Consistent with prior research, our data indicate that the Pennsylvania law worked to substitute for, and was largely redundant with, firm-level defenses. If we were to ignore the first publicity about the law, however, we would tend to conclude just the opposite. Overall, these results indicate that the valuation effects of the Pennsylvania antitakeover law were similar to, although perhaps larger than, those of other state antitakeover laws, and occurred primarily when information about the law was first released through the wire service.

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