IS THERE POWER BEHIND THE DEAD HAND? AN EMPIRICAL INVESTIGATION OF DEAD HAND POISON PILLS

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Abstract

Dead hand poison pills prevent potential hostile acquirers from circumventing a poison pill with a proxy contest whereby newly elected directors could redeem the pill. Dead hand provisions only permit continuing directors to redeem. Shareholder rights advocates and legal scholars have criticized dead hand poison pills as an assault on shareholder governance, but economic theory suggests potential shareholder benefits. We provide the first empirical study of dead hand poison pills. We find that adoption of dead hand poison pills leads to gains for shareholders and losses for bondholders. This supports Schwert's (2000) conjecture that poison pills provide shareholders with better premiums rather than entrench ineffective managers.

Keywords: corporate governance, hostile acquisition, takeover defense, poison pill

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1. Introduction

Merger waves, hostile acquisitions, and defensive tactics have been a subject of intense policy debate and empirical analysis for decades. One of the more controversial defenses is the poison pill. Poison pills have been the subject of much empirical analysis. Jarrell and Poulson (1987) describe the classic poison pill as the issuance of rights to all shareholders that can be converted into valuable securities upon the occurrence of a triggering event, such as a successful tender offer not recommended by the directors, or the acquisition of a threshold percentage of shares by a related group. This deters potential acquirers from attempting a takeover without the support of the incumbent directors.

Some commentators and researchers see poison pills as an assault on shareholder governance that allows managers to entrench themselves, exacerbating agency conflicts. Gordon (1997) and Gompers, Ishii, and Metrick (2003) provide examples of this perspective. But others (notably Romano, 1993) have argued that firms that adopt high cost governance structures will be driven out of the competitive market. This perspective suggests that poison pills could be an efficient solution to contracting problems. Empirical studies yield ambiguous results. A relatively recent development in poison pill technology provides an opportunity to construct a more powerful statistical test.

One method of circumventing the poison pill defense has been to wage a proxy fight to replace the directors with a new board, which can then redeem the poison pill. To counter this strategy, a new type of poison pill was developed that has been labeled the dead hand poison pill. Dead hand provisions in poison pills place restrictions on redemption. These poison pill securities are only redeemable by continuing directors. This enables ousted directors to continue to control the firm from their figurative grave. Dead hand poison pills are a relatively recent development, and have received much scrutiny in the legal literature. Anecdotes involving dead hand poison pills have appeared in leading corporate finance texts, *e.g.* Brealey and Myers (2003, p. 951).

Dead hand adoptions have declined since 2001 commensurate with the general decline in acquisitions. Nevertheless shareholder activist groups such as Institutional Shareholder Services continue to target firms with dead hand provisions and work for removal. It is important for both government and private organizations seeking to influence corporate governance to have the empirical facts about the effects of dead hand provisions on shareholder wealth.

Due to their relatively recent development and limited sample, dead hand poison pills have yet to be empirically studied although they are a natural topic for empirical analysis. However, dead hand provisions have accumulated in use as a defensive tactic, and there are now enough readily available observations for an event study. Using Thomson Financial's SDC

Platinum database, we identified about three hundred instances of dead hand pills, and ended up with a useable sample of about two hundred after excluding repeat observations (firms with more than one dead hand adoption). We contribute to the literature by analyzing this special type of controversial pill within an event study context. The event study methodology and its application to corporate law and regulation is summarized well in two survey articles by Bhagat and Romano (2002a, 2002b).

Our analysis contributes to the public debate about whether or not it is good policy to legally ban such defensive measures. Since dead hand pills are a more extreme form of protection than conventional poison pills, any effect on returns should be easier to discern statistically. Thus they are interesting both as a special type of protection device, but also because event study methodology will yield a more powerful test on this extreme sample and therefore the results are less likely to be ambiguous. We obtain the interesting result that there are positive gains to stockholders together with losses for bondholders associated with the adoption of dead hand poison pills. We interpret this as a benefit related to strengthened collective bargaining power for the shareholders.

Prior research has demonstrated that although a corporate governance measure might be positively associated with equity performance, it is not necessarily associated with unambiguous positive performance. Events which increase shareholder wealth could either be creating value or transferring value (or both). In order to further investigate the nature of the abnormal returns to equity, we also investigate the returns to debt using the methodology of Maxwell and Stephens (2003). We find that the adoption of dead hand poison pills results in losses to bondholders. Given the general findings of Gompers, Ishii, and Metrick (2003) and Klock, Mansi, and Maxwell (2005) that corporate governance provisions have opposing effects on debt and equity, this further supports the argument that dead hand provisions often work in the interests of shareholders rather than against them. These provisions appear to transfer some wealth from debt to equity. This suggests that shareholder rights advocates who argue against these corporate governance devices are arguing against their interests and need more information about both the empirical facts and the theory of general equilibrium in competitive markets.

The remainder of this paper is organized as follows. Section 2 reviews the literature covering the theory and empirical research on poison pills generally, and then reviews current policy discussions over dead hand provisions. Section 3 describes the data and methodology. We present and discuss the results in Section 4. Section 5 summarizes and concludes.

2. Literature Review

2.1 Plain Vanilla Poison Pills

There are two competing theories about the role of the poison pill in corporate governance literature. One theory is based on a partial equilibrium model of a poison pill examining short-run effects in isolation. This approach is frequently called the management entrenchment hypothesis and models the pill as a tool of managers that is used to protect their jobs when they are either underperforming or overcompensating themselves. This view was popularized by Easterbrook and Fischel (1981) who not only took the position that takeovers serve as a disciplining device on managers, but argued for policies to prohibit managers from actively resisting takeovers. This view of poison pills as a tool of self-serving managers and a device that weakens corporate governance has had much popular appeal, but it suffers from the weakness that it does not consider the long-run effects in the entire market. If poison pills are solely barricades between management interests and shareholder governance, then firms that do not employ them will enjoy a lower cost of capital and have a natural cost advantage, and firms that do employ them will disappear in a competitive equilibrium (Romano, 1993).

The competing hypothesis offers an explanation as to why shareholders benefit from the poison pill. Under the shareholder benefit hypothesis, poison pills are not a management entrenchment tool, but are instead a collective bargaining device that assists managers in negotiating a larger premium for the shareholders of a target. Examples of this theory are given in Ryngaert (1988) and Comment and Schwert (1995). Anecdotally, this tale is consistent with the case of AlliedSignal versus AMP in which AMP deflected an attempted takeover with a dead hand poison pill that was upheld by a federal district court applying Pennsylvania law. AMP was eventually taken over by a white knight at a substantially greater premium.

A variation of the collective bargaining with bidders hypothesis suggests that shareholders benefit even outside of the acquisition context because the poison pill provides job security to managers that encourages sunk investments in managers' human capital for the benefit of shareholders. A theoretical exposition of this model is given in Knoeber (1986, p. 156) who wrote, "The object of this paper is to examine the contractual relation between shareholders and managers ... and to suggest that it may well be in shareholders' as well as managers' interest to agree to restrict the possibility for outsiders to disrupt their relation with a hostile tender offer." This theory was extended by Harris (1990) who developed a model that offers insights about why shareholders would benefit from the simultaneous use of different forms of managerial protection, e.g., golden parachutes combined with poison pills.

There is yet another similar story suggesting why such devices could be beneficial to shareholders. The availability of takeover defenses not only improves the credibility of contractual commitments between shareholders and managers. It can also improve the relationships between the firm and suppliers, laborers, creditors and so forth all of which will be more likely to make cost- reducing, sunk investments into relationships if they perceive a long-term stable management is at the helm. This explanation was advanced by Shleifer and Summers (1988).

Early empirical evidence was mixed. Jarrell and Poulson (1987) found significant negative returns to the adoption of management protecting devices. DeAngelo and Rice (1983) were unable to reject the null hypothesis of no effect for management protecting devices. Linn and McConnell (1983) found significant positive returns from adoption of management protection devices. Ryngaert (1988, p. 411) wrote, "In its entirety, the evidence suggests that poison pill defenses have, in practice, only marginally altered the market for corporate control."

However, more recent evidence has been somewhat supportive of the shareholder benefit hypothesis. Bizak and Marquette (1998) found that stock prices react negatively when shareholders pass resolutions to rescind poison pills. In a highly comprehensive study of poison pills, Comment and Schwert (1995) find no statistical evidence that poison pills deter takeovers, and find instead that shareholders receive larger premiums when poison pills are in place. In a more recent study of takeover contests, Schwert (2000) concludes:

> Taking all of the evidence together, there is support for both target management entrenchment and for bargaining strategy as explanations for the perception of hostility in takeover contests. This is not surprising because these hypotheses are not mutually exclusive. Nevertheless, on balance, hostility in takeover negotiations seems to be most strongly related to strategic bargaining.

The broad empirical question still lacks a definitive answer. Although Schwert is more supportive of a shareholder benefit, a recent study by Gompers et al. (2003) takes the opposite view. They characterize takeover defenses as weak corporate governance and find that more takeover defenses are associated with lower stock performance. Their work suggests that investors can profit by shorting portfolios of firms with many takeover defenses and buying portfolios of firms without many takeover defenses.

Since dead hand provisions are a more potent form of takeover protection, it is an interesting empirical question as to whether they are more closely associated with stronger strategic bargaining for the benefit of shareholders or stronger management entrenchment. Additionally, because they are potent, it is possible that the effects will be easier to discern statistically.

2.2 Dead Hand Poison Pills

To date, the academic literature on dead hand poison pills resides entirely within legal commentary. This literature is focused on legal arguments about the validity of dead hand provisions and policy arguments about the desirability. Gordon (1997, p. 551) wrote, "A decision sustaining the deadhand pill ... would also have a devastating impact on the control market and, ultimately, would have large scale economic effects." Lese (1998) also argued that these provisions have no legitimate purpose. On the other hand, there have been commentators such as Rodman (1999) who have argued that dead hand provisions are not detrimental to the public interest and can benefit investors. Essentially, the policy arguments over dead hand provisions are an extension of the debate over takeover defenses generally. If one views takeover defenses as efforts by managers to protect their own position at the expense of the owners, then dead hand provisions are just a more egregious affront to sensibility. A typical argument is that it is wrong to permit the directors to constrain the future choices of future directors.

On the other hand, decisions made now always alter the feasible set in the future. Issuance of fixed income securities will constrain future cash flows, as Jensen (1986) argues. It is difficult to distinguish permissible and impermissible director actions based on a determination as to whether the action constrains future directors.

The legal arguments over dead hand poison pills are not clearer than the policy arguments. The federal government takes a prominent role in regulating the marketplace for securities, but the contractual relationships between directors and shareholders are primarily a matter of state law. This means that even if we had clear laws, we would likely have many different ones depending on the state of incorporation. However, states' legal codes are generally silent on dead hand provisions, the sole exception being Virginia that explicitly authorizes dead hand poison pills by statute. In every other state, the legality is a matter of judicial interpretation. No case involving a dead hand provision has reached an appellate court. One case (Quickturn v. Shapiro) involving a stronger variant of the dead hand poison pill did reach Delaware's Supreme Court. But that involved something called a "no hands" provision that could not be redeemed by anyone, and the court's invalidation of the no hands poison pill potentially leaves open the legal validity of the dead hand provision in Delaware. Published opinions from trial court cases involving dead hand pills exist in just four states: Delaware, New York, Pennsylvania, and Georgia. These states are evenly split on the decisions.

The legal arguments over dead hand provisions are therefore unsettled, and likely to remain in that state for a long time. Empirical facts regarding the

effects of dead hand poison pills are therefore likely to be especially useful in shaping the future. According to a report by Dewey (1998), a study of 1,600 poison pills revealed that 280 contained dead hand provisions. Thus this is a significant development worthy of independent empirical analysis.

3. Data and Methodology

A. Data Sources

An initial sample of 317 dead hand poison pills adoptions between 1986 and 2001 is obtained from Thomson Financial's SDC Platinum database. Some firms list multiple dates of announcement of adoption. Only the earliest date of announcement is investigated, eliminating 99 observations.

Cusips and daily returns are collected for the 218 remaining firms in the sample from the Center for Research in Securities Prices (CRSP) database. Four firms are eliminated from the sample due to missing CRSP data, leaving a final sample of 214 firms. Annual report data on sample firm characteristics is obtained from Research Insight's Compustat database. Differences in mean and median values listed in Table 1 show that dead hand poison pill adoptions are clustered among smaller firms with lower market-to-book equity ratios. At the same time, firms adopting dead hand poison pills generally had strong sales growth during the previous fiscal year as well as positive returns to equity.

[Insert Table 1 Here]

An analysis of sample firms' first-digit SIC codes also shows some clustering by firm type. The majority of firms adopting dead hand poison pills can be characterized as manufacturing firms or financial firms. The relationship between dead hand poison pill adoptions and industry is most pronounced for SIC first-digit code 3, which includes manufacturing firms in the highly competitive and innovative electronics and computer industries.

[Insert Figure 1 Here]

A common concern with poison pill adoptions is that they are often associated with companies that have been put into play. Figure 2 shows a strong relationship between proxy fight activity and dead hand poison pill adoption beginning in 1997, when dead hand poison pill adoptions became popular antitakeover devices. There was a 0.74 correlation between proxy fights and dead hand poison pill adoptions between 1997 and 2001.

[Insert Figure 2 Here]

To mitigate this, we utilize the methodology of Schwert (2000). The methodology divides the premium into the markup (the market model adjusted return occurring immediately after the adoption) and the runup (the market model adjusted return preceeding the adoption). Market model alpha (intercept term) and beta coefficients are calculated for each firm by regressing the firm's continuously compounded daily returns on the returns to CRSP's value-weighted NYSE/AMEX/Nasdaq index for the time period between 316 and 64 trading days before the deadhand announcement date. Cumulative daily runup returns are then calculated for each firm for the 63 trading days before the dead hand announcement date. Each daily runup return is calculated as the difference between the firm's actual return and it's return as predicted by the market model. Daily markup returns are similarly calculated and cumulated for the dead hand announcement date and the following 10 trading days. A relatively short markup period is used since longer timeframes result in the loss of a significant number of observations due to events such as mergers and acquisitions. For example, the use of a six-month timeframe for markup resulted in the loss of 50 observations due to missing returns data. Total return premiums are calculated as the sum of the runup and markup returns.

Data on publicly traded debt is collected from the Lehman Brothers Bond Database (LBBD). The database has been used extensively in research, and Elton et al. (2001) report an accuracy rate in the data as reliable as CRSP. Because the LBBD data ends in February of 1998, we collected supplemental data by hand from *Mergent's Bond Record.* Not all firms in our original sample have publicly traded debt, so the number of observations for the bond analysis is based on 97 announcements.

Reliable bond data only exists for monthly observations. As noted by others, the use of monthly data biases the results towards a finding of insignificance (Brown and Warner, 1980). To analyze the effect of dead hand poison pill adoptions on bondholders we follow the methodology of Maxwell and Stephens (2003). To account for changes in term structure, bonds are matched against the Treasury security with the closest duration and the bond's monthly return in excess of the matched Treasury is calculated. The mean expected excess return is calculated over the estimation period, and then the abnormal excess return is obtained by subtracting the mean expected excess return. Where firms have multiple issues, the results are averaged on a value-weighted basis. The abnormal excess return for each bond is then standardized by dividing by the estimated standard deviation of the excess returns over the matched Treasury during the estimation period. Again following Maxwell and Stephens, a three-month estimation period is used.

To complete the bond analysis, an equally weighted portfolio for the 97 firms is formed for the event month by combining the standardized excess returns. From this we calculate the mean portfolio standardized bond return and are able to construct a test statistic having a t-distribution assuming independence in the observations.

B. Empirical Method

We use standard event study methodology to estimate abnormal stock returns and abnormal yield spreads around the deadhand poison pill announcement.

Because this the first study that examined the impact of deadhand announcements on pricing, we have no expectation on what the relation should be from the perspective of both stockholders and bondholders. Below, we highlight the methodology used to obtain abnormal bond yield spreads and abnormal stock returns. Later we compute the impact of the announcements on stock and bond values.

B1. Abnormal Yield Spreads

To analyze the effects of deadhand announcements on bondholder wealth we employ mean adjusted event study methodology as in Brown and Warner (1980). Mean adjusted event study methodology is used in several bond event studies including those by Handjinicolaou and Kalay (1984), Jayaraman and Shastri (1988), and more recently Maxwell and Stephens (2003). The LBFI database provides month end bond price data and covers the majority of the sample period from 1986 to 1998. The remaining bond pricing data for the sample period from 1998 through 2001 are obtained from Moody's corporate bond manuals (currently Mergent). Brown and Warner (1985) point out that daily event studies are more precise. However, the use of daily bond data may be problematic since it is not available for the entire sample and many bonds do not trade on a daily basis. The use of monthly yield spreads decreases the power of the tests and biases our results against finding significant changes in yield spreads.

The bond yield spread ($YS_{i,t}$) is defined as the difference between the yield to maturity on a corporate bond for month *t* of bond *i* and its duration matched Treasury security (MT). That is

$$YS_{i,t} = YTM_{i,t} - YTM_{MT} \qquad (1)$$

The mean expected yield spread ($EYS_{i,t}$) for bond *i* at the announcement month *t* is estimated as the average yield spread prior to the announcement over the estimation period *T* (in this case three months prior to the event). That is

$$EYS_{i,t} = \frac{1}{T}\sum_{t=-1}^{-T} YS_{i,t} \qquad (2)$$

The abnormal yield spread during the announcement month *t* is calculated as the difference between the observed yield spread for bond *i*, $YS_{i,t}$, and the expected yield spread, $EYS_{i,t,}$. That is

$$AYS_{i,t} = YS_{i,t} - EYS_{i,t} \qquad (3)$$

Since many firms in the sample have multiple bonds outstanding, we compute abnormal yield spread for a given firm using the summation of the weighted average of the abnormal yield to maturity of all bonds, with the weight being the amount outstanding for each bond divided by the total amount outstanding for all traded bonds. This methodology removes any bias introduced by treating each bond for a firm separately, which in turn could inflate the *t* statistics due to high correlation between bonds of the same firm. The weighted average approach over-estimates the standard error and biases the t-statistics downward since a firm's bonds are not perfectly correlated.

Under the assumption that monthly abnormal yield spreads are normally distributed and cross sectionally independent, it is possible to determine whether the abnormal bond yield spreads are significantly different from zero. First, we compute the standard deviation of firm *i*'s yield spread over the monthly estimation period. That is

$$S_i = \sqrt{\frac{1}{T-1}\left(\sum_{t=-1}^{-T}\left(YS_{i,t} - EYS_i\right)^2\right)} \qquad (4)$$

The abnormal yield spreads, $AYS_{i,t}$, are then standardized by the period estimate of standard deviation. That is

$$SAYS_i = \frac{AYS_i}{S_i} \qquad (5)$$

The variable $SAYS_i$ is distributed student-*t* with a mean of zero and two (T=3 minus 1) degrees of freedom. The standardized abnormal yield spreads for each firm are combined into equally weighted portfolios. The average standardized yield spread of the portfolio of *N* firms is

$$SAAYS = \frac{\sum_{i=1}^{N} SAYS_i}{N} \qquad (6)$$

Applying the central limit theorem, portfolio abnormal yield spreads are approximately normally distributed with mean zero and variance 1/N.[15] Testing for abnormal yield spreads in the announcement month consists of determining whether the portfolio abnormal yield spreads are significantly different from zero. The t-statistic is[16]

$$t = SAAYS\sqrt{N} \qquad (7)$$

[15] Alternatively, portfolio abnormal yield spreads may be tested directly for the sum of abnormal yield spreads in which case the equivalent test statistic becomes $t = \sum_{i=1}^{N} SAYS / \sqrt{N}$.

[16] The t-statistic computed here is similar to the one provided in Handjinicolaou and Kalay (1984) and Jayaraman and Shastri (1988), but slightly different from the one provided by Maxwell and Stephens (2003). In their paper, equation 8, they divided the mean portfolio standardized bond return by the square root of N. Although they do not explicitly state the order of the computation, their calculation is based on computing the standard error. In this case, we follow the methodology of Handjinicolaou and Kalay (1982), p45, equation 12 and multiply the SAAYS by the square root of N.

B2. Abnormal Stock Returns

Because of the availability and reliability of daily stock data, we report changes in abnormal stock returns on a daily basis. For robustness, we perform monthly estimations of abnormal stock returns and find similar results. We use the market model as in Brown and Warner (1985) and apply the CRSP equally weighted index as the market portfolio. The estimation period for the daily market coefficients is comprised of 255 trading days, ending 31 days before the announcement date. We report cumulative abnormal stock returns (CARs) over a three-day window of (-1,1) and report the results in the empirical section below.

4. Empirical Results

We analyzed the stock returns on a full sample and several subsamples. Panel A of Table 2 presents the results on the full sample using 214 observations. We then divide the full sample into two subsamples, one covering 1986- 1996 and one covering 1997-2001. This was done to separate out the possible effects of a learning curve between earlier and later dead hand poison pill adoptions. The number of dead hand provisions in the earlier period is much smaller. About ten percent of the observations occurred in the earlier eleven year period and ninety percent in the later five year period.

[Insert Table 2 Here]

Panel B of Table 2 presents the results for a sample that excludes firms with some missing observations during the estimation period. The "full" sample in this case contains 202 observations, and as in Panel A, we divided the entire time period into two subperiods. The differences between the samples including and excluding the observations with missing returns data are small.

We report parametric and nonparametric test statistics, respectively, for comparison of mean and median runups, markups, and premiums for each time period to null hypotheses of zero. We also report parametric and nonparametric test statistics for differences in subsample mean and median runups, markups, and premiums. Visual inspections of the full samples and subsamples show that the abnormal returns are reasonably approximated by a normal distribution. This is confirmed for the smaller subsamples by the Shapiro-Wilk statistic, which failed to reject the null hypothesis of normality. For the full sample and larger subsamples, we can appeal to the central limit theorem. However, it may not be reasonable to assume that the normal distributions of the subsamples have equal variances. To account for this, we report the statistics for the Wilcoxon signed-rank test comparisons of median runups, markups, and premiums.

The estimated mean (median) premium over the 1986-2001 time period exceeds 9% (4%) for both the 214 and 202 observation samples. These premiums are statistically significant at a 95% confidence level using both Student's t and Wilcoxon signed-rank tests. There is no discernable pattern in the split of the premium between markup and runup. Runup is higher than markup for the 214 observation sample and its subsamples, but lower than markup for the 202 observation sample and its subsamples. For the 214 observation sample, runup is significant at a 95% and 90% confidence level, respectively, using Student's t and Wilcoxon signed-rank tests. For the 202 observation sample, runup is significant at a 90% and 95% confidence level, respectively, using Student's t and Wilcoxon signed-rank tests. Markup is statistically significant at a 10% confidence level (using both tests) for the 202 observation sample, but is insignificant for the 214 observation sample. For the 202 observation sample, the markup makes up a little more than half of the premium, which is comparable to Schwert's (2000) findings on a larger sample of hostile tender offers. The difference in the results between the 1986-1996 and 1997-2001 subsamples provides some evidence of a learning curve effect. Premiums and their components are larger for dead hand poison pill adoptions in the 1997-2001 time period. The estimated mean premiums are over 9% for the 1997-2001 subsamples and are statistically significant at a 90% or 95% confidence level, while the estimated mean premiums are just over 5% for the 1986-1996 subsamples and are statistically insignificant. It appears that the insignificant results for the 1986-1996 time period are caused by the small number of observations since there is very little difference in the estimates obtained from the full sample and the 1997-2001 time period. Student's t-test and Wilcoxon signed-rank tests of differences in mean runups, markups, and premiums across the 1986-1996 and 1997-2001 subsamples are insignificant.

We interpret the finding of gains to shareholders around the adoption of dead hand provisions as evidence against the management entrenchment hypothesis, and evidence supporting the shareholder benefit hypothesis. Resistance to a takeover and the adoption of stronger protection would appear to typically be associated with expectation of a higher takeover premium, or expectation of an additional infusion of human capital by management. The evidence obtained from analysis of bond returns is also supportive of the shareholder benefit hypothesis.

When the bond returns are analyzed in event time, we derive just two numbers to report. An estimated mean portfolio standardized increase in the cost of debt of 2.47 basis points and a highly significant test statistic of 22.48. Although bondholders are legally entitled to the same fixed income stream, hostile takeovers signify change and change is risky. From an option perspective, bondholders have written a call and lose value when the risk of the underlying assets increases. Our finding of losses to bondholders suggests that dead hand adoptions empower shareholders to extract a better takeover premium for themselves and transfer wealth in the process. Finding positive value to equity and negative value to debt

suggests that dead hand poison pill adoptions are not harmful per se to shareholders. This is important information in the current public discussions regarding shareholder governance.

We next sought to replicate Table 2 for the subsample of observations used in the bond return analysis. Table 3 reports these results. The smaller sample size makes it difficult to discern statistical significance for the runup and markup components. But when focusing on the total premium for the largest available bond sample, we see that the estimated premium is 9% as in the full sample and is statistically significant at the ten percent level.

In order to compare the magnitude of the losses to bondholders against the gains to shareholders we made a simple calculation of the average change in market value of equity per event. That amount is a positive $72.5 million. We made a similar simple calculation for debt (utilizing the average change in yield times the average duration to get the average percentage change in the outstanding debt). The average change in the market value of debt is negative $221,000 per event. The small magnitude of the change in the value of the debt is not unreasonable. It suggests that most of the benefit to shareholders comes from expectations of a higher premium, not from a large wealth transfer.

5. Summary and Conclusions

We obtain stock return data to investigate more than two hundred dead hand poison pill adoptions after excluding subsequent dead hand amendments by the same firms. We also obtain data on publicly traded debt for 97 of these firms. We argue that because dead hand provisions result in a more potent poison pill, whether the effects are positive or negative, they should be larger in absolute value and easier to discern. A negative effect would be consistent with the management entrenchment hypothesis that suggests protection exacerbates agency costs. A positive effect would be consistent with improved shareholder bargaining strength during acquisitions.

We report positive gains from dead hand poison pills for shareholders that are at least partially reinforced by losses to bondholders. Although it is possible for management to abuse this tactic in any given case, it appears that the market generally perceives a net benefit from this strong form of takeover protection. Helping shareholders by reducing managerial power to resist is likely not the type of help most shareholders want.

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Table 1. Summary statistics of sample firm characteristics, including total assets ($ million), shareholders equity ($ million), long-term debt to equity ratio, most recent percentage change in annual sales, liquidity index (defined as average collection period multiplied by receivables plus days inventory multiplied by inventory, divided by total current assets), return on equity, market-to-book equity ratio, and price-to-earnings ratio reported at the end of the fiscal year just prior to the announcement date of dead hand poison pill adoption. Q1 and Q3 refer to the twenty-fifth and seventy-fifth percentiles, respectively.

Firm Characteristic	N	Mean	Min	Q1	Median	Q3	Max
Total Assets	210	1206.53	5.62	71.09	217.61	850.71	33457.80
Shareholders Equity	205	348.22	-158.57	39.08	108.83	306.38	4522.00
Debt-to-Equity Ratio	205	31.49	-3481.94	1.16	22.40	78.10	444.92
Change in Sales	196	69.97	-60.19	3.33	14.90	35.14	4243.75
Liquidity Index	170	45.32	-3481.94	1.16	22.40	78.10	444.92
Return on Equity	209	-10.65	-1273.61	-8.87	7.48	13.82	219.56
M/B Equity Ratio	208	2.70	-65.57	1.36	2.00	3.36	62.46
P/E Ratio	196	9.21	-715.63	-3.94	13.12	22.27	745.83

Table 2. Results for full sample of 214 observations are presented in Panel A. Results for subsample of 202 observations with no missing daily returns to compute the market model parameters are presented in Panel B. Runup = $\sum R_{it} - \alpha_i - \beta_i R_{mt}$ over t = – 63 to t = –1, the 63 trading days preceeding the dead hand poison pill adoption on day t = 0. R_{it} is the continuously compounded daily return to firm i's stock. R_{mt} is the continuously compounded daily return to the CRSP NYSE/AMEX/Nasdaq value-weighted portfolio. The market model regression parameters α_i and β_i are estimated for the trading period t = – 316 to t = – 64. Markup = $\sum R_{it} - \alpha_i - \beta_i R_{mt}$ over t = 0 to t = 10. Premium = runup + markup. N = number of observations for each sample or subsample. The ratio of positive to negative returns is reported in parentheses below each N. Results of two-tailed t-test comparisons of mean runups, markups, and premiums to zero null hypotheses are reported in parentheses below means. Results of Wilcoxon signed-rank tests of median runups, markups, and premiums to zero null hypotheses are reported in parentheses below medians. Results of t-test comparisons of subsample means and Wilcoxon signed-rank tests for differences in subsample distributions are reported below each subsample pair.

Parameter	Time Period	Panel A			Panel B		
		N	Mean	Median	N	Mean	Median
Runup	1986-2001	214	0.04738	0.03114	202	0.04239	0.03016
		(117:97)	(2.26)[a]	(2217)[a]	(109:93)	(1.96)[b]	(1789)[a]
	1986-1996	22	0.03305	0.02216	21	0.02105	0.01295
		(13:9)	(0.93)	(31)	(12:9)	(0.60)	(21)
	1997-2001	192	0.04902	0.03401	181	0.04487	0.03093
		(104:88)	(2.13)[a]	(1737)[a]	(97:84)	(1.88)[b]	(1428)[a]
Student's t/Signed-rank tests			0.23	2330		0.34	2064
Markup	1986-2001	214	0.04337	0.04359	202	0.05241	0.06301
		(111:103)	(1.45)	(1247)	(108:94)	(1.70)[b]	(1479)[b]
	1986-1996	22	0.02072	0.03639	21	0.03840	0.06311
		(12:10)	(0.22)	(5)	(12:9)	(0.40)	(13)
	1997-2001	192	0.04596	0.04359	181	0.05403	0.06291
		(99:94)	(1.45)	(1042)	(96:85)	(1.65)[b]	(1186)[b]
Student's t/Signed-rank tests			0.26	2307		0.15	2106
Premium	1986-2001	214	0.09075	0.03783	202	0.09480	0.04286
		(116:98)	(2.33)[a]	(1800)[a]	(111:91)	(2.33)[a]	(1705)[a]
	1986-1996	22	0.05378	0.03324	21	0.05946	0.03528
		(12:10)	(0.62)	(17)	(12:9)	(0.65)	(19)
	1997-2001	192	0.09499	0.04286	181	0.09890	0.04682
		(104:88)	(2.24)[a]	(1489)[b]	(99:82)	(2.23)[a]	(1394)[a]
Student's t/Signed-rank tests			0.32	2297		0.30	2076

Significance at a 5% level and 10% level are denoted by a and b, respectively.

Table 3. Results for subsample of 97 observations with bond returns are presented in Panel A. Results for subsample of 87 observations with bond returns and no observations with missing daily returns to compute the market model parameters are presented in Panel B. Runup = $\sum R_{it} - \alpha_i - \beta_i R_{mt}$ over t = – 63 to t = –1, the 63 trading days preceeding the dead hand poison pill adoption on day t = 0. R_{it} is the continuously compounded daily return to firm i's stock. R_{mt} is the continuously compounded daily return to the CRSP NYSE/AMEX/Nasdaq value-weighted portfolio. The market model regression parameters α_i and β_i are estimated for the trading period t = – 316 to t = – 64. Markup = $\sum R_{it} - \alpha_i - \beta_i R_{mt}$ over t = 0 to t = 10. Premium = runup + markup. N = number of observations for each sample or subsample. The ratio of positive to negative returns is reported in parentheses below each N. Results of two-tailed t-test comparisons of mean runups, markups, and premiums to zero null hypotheses are reported in parentheses below means. Results of Wilcoxon signed-rank tests of median runups, markups, and premiums to zero null hypotheses are reported in parentheses below medians. Results of t-test comparisons of subsample means and Wilcoxon signed-rank tests for differences in subsample distributions are reported below each subsample pair.

Parameter	Time Period	Panel A			Panel B		
		N	Mean	Median	N	Mean	Median
Runup	1986-2001	97	0.01901	0.01296	87	0.01255	0.00848
		(47:50)	(0.71)	(293)	(44:43)	(0.44)	(192)
	1986-1996	18	0.04765	0.01072	17	0.03368	0.00848
		(8:10)	(1.37)	(22)	(9:8)	(1.00)	(13)
	1997-2001	79	0.01248	0.02940	70	0.00742	0.01291
		(39:40)	(0.39)	(142)	(35:35)	(0.21)	(96)
Student's t/Signed-rank tests			0.51	928		0.36	771
Markup	1986-2001	97	0.06832	0.06390	87	0.00813	-0.00249
		(45:52)	(1.58)	(425)	(42:45)	(0.61)	89
	1986-1996	18	0.01709	0.01078	17	0.00038	-0.00407
		(9:9)	(0.15)	(2)	(7:10)	(0.05)	(4)
	1997-2001	79	0.08000	0.06390	70	0.01001	0.00005
		(36:43)	(1.70)[b]	(337)[b]	(35:35)	(0.61)	(77)
Student's t/Signed-rank tests			0.56	862		0.28	721
Premium	1986-2001	97	0.08733	0.03528	87	0.02068	0.01990
		(44:53)	(1.79)[b]	(427)	(46:41)	(0.64)	(231)
	1986-1996	18	0.06474	0.03324	17	0.03406	-0.01704
		(8:10)	(0.62)	(14)	(7:10)	(0.91)	(9)
	1997-2001	79	0.09248	0.03677	70	0.01743	0.02952
		(36:43)	(1.68)[b]	(297)	(39:31)	(0.44)	(143)
Student's t/Signed-rank tests			0.22	892		0.20	734

Significance at a 5% level and 10% level are denoted by a and b, respectively.

Figure 1. Number of Announcements of Dead Hand Poison Pill Adoptions by Industry, 1986-2001 (214 observations)

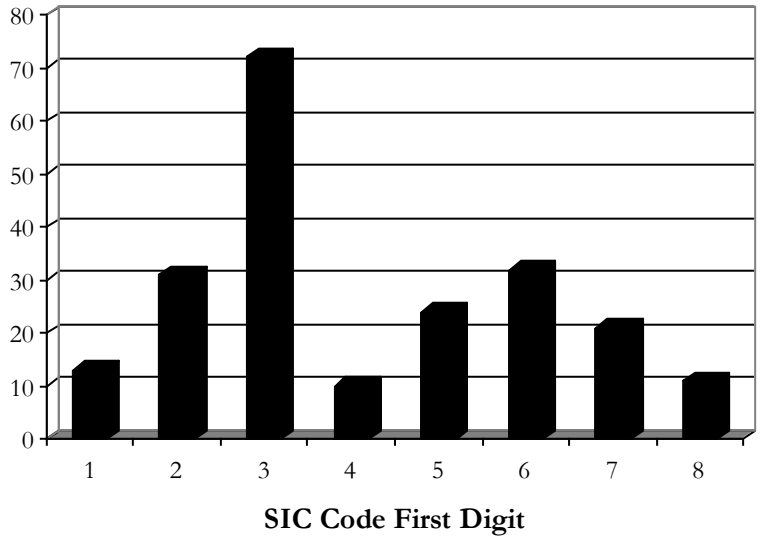

Figure 2. Number of Announcements of Dead Hand Poison Pill Adoptions and Proxy Fights by Year