Internet Appendix
for
"The Impact of Stronger Shareholder Control on Bondholders"

Abstract

This online appendix provides detailed discussions and analyses related to (1) diminishing marginal effect of bondholder wealth expropriation of stronger shareholder control, (2) regression discontinuity design and its validity tests, such as continuity in vote distribution and preexisting differences, (3) superiority of credit default swap relative to bond yield spreads, (4) a graphical analysis of the cumulative adjusted CDS spreads, (5) constructing the covenant index, and (6) a battery of robustness checks that includes (i) controlling for equity returns, (ii) using unadjusted CDS spreads, (iii) vote manipulation, (iv) CDS sample selection bias. This internet appendix contains 12 tables and 6 figures.

IX. Internet Appendix

Tables

Table A.1
Shareholder Governance Proposals

This table summarizes the shareholder proposals for observations with nonmissing company name, voting date, and vote result from ISS and CDS from Markit from 2001 to 2011.

Panel A: Shareholder Proposal Summary Statistics

Year	Shareholder Proposals	Approved Proposals	Percentage Approved Proposals	Average Vote Outcome	Std. Dev. Vote Outcome
2001	105	21	27.3%	20.0%	21.01
2002	177	61	36.4%	34.5%	22.64
2003	331	110	35.1%	33.2%	23.48
2004	289	75	32.0%	26.0%	24.67
2005	274	75	34.5%	27.4%	23.37
2006	318	104	40.8%	32.7%	22.80
2007	327	83	37.0%	25.4%	21.64
2008	214	50	38.0%	23.4%	22.62
2009	282	93	43.2%	33.0%	20.39
2010	242	66	40.5%	27.3%	19.67
2011	159	37	40.0%	23.3%	20.53
Total	2,718	775	37.2%	28.5%	22.58

Panel B: Type of Governance Proposals

Proposal Type	Shareholder Proposals	Percentage Approved Proposals	Average Vote Outcome
Auditors	35	2.86%	16.65%
Board	381	3.41%	25.20%
Compensation	918	10.35%	27.20%
G-Index	947	58.18%	53.76%
Voting	228	40.79%	47.82%
Other	209	10.53%	20.07%
Total	2,718	28.51%	37.22%

Table A.2

Description of All Shareholder Proposals

Type	Description Proposal	Observations	Average Vote Outcome	Discontinuity #-2,+2	Discontinuity #-5,+5	Discontinuity #-10,+10
Audit		35	2.9%	0	1	1
	Rotate auditors	1	0.0%	0	0	0
	limit consulting by auditors	31	0.0%	0	0	0
	Shareholder approval of auditor	3	33.3%	0	1	1
Board		381	3.4%	13	26	63
	Commit to/report on board diversity	10	0.0%	0	0	0
	Increase audit committee Independence	1	0.0%	0	0	0
	Increase key committee Independence	13	0.0%	0	0	0
	Lead director	4	0.0%	0	0	1
	Limit director tenure	25	0.0%	0	0	0
	Miscellanea	68	2.9%	4	8	12
	Separate chairman/CEO	222	4.1%	8	14	44
	Allow union/employee reps on the board	2	0.0%	0	0	0
	Create nominating committee	1	0.0%	0	0	0
	Increase compensation committee independence	5	20.0%	1	2	3
	Independent nominating committee	4	0.0%	0	0	0
	Majority of independent directors	26	3.8%	0	2	3
Compensation		918	10.4%	54	132	257
	Add performance criteria to equity-based awards	27	14.8%	2	5	10
	Advisory vote on compensation	185	18.4%	30	71	129
	Approve/disclose/limit SERPs	28	14.3%	3	5	11
	Award performance-based stock options	88	2.27%	2	5	18
	Expense stock options	69	60.9%	13	26	43
	Disclose executive compensation	37	0.0%	1	1	3
	Hire independent compensation consultant	2	0.0%	0	2	2
	Link executive pay to social criteria	63	0.0%	0	0	0
	Misc compensation	38	5.3%	1	3	8
	Pension fund surplus reporting	15	6.7%	0	1	6
	Require equity awards to be held	41	0.0%	0	0	0
	Restrict director compensation	14	0.0%	0	0	0
	Approve executive compensation	1	50.0%	0	0	1
	Cap executive pay	308	2.1%	2	12	25
	No repricing underwater stock options	1	0.0%	0	1	1
	Pay directors in stock	1	0.0%	0	0	0
Other		209	10.9%	11	23	39
	Double board nominees	26	0.0%	0	0	0
	Miscellanea	133	15.8%	11	23	36
	Opt out of state takeover statute	1	100.0%	0	0	1
	Reincorporate to U.S. state	27	3.7%	0	0	2
	Restore preemptive rights	1	0.0%	0	0	0
	Change annual meeting date	1	0.0%	0	0	0
	Change annual meeting location	6	0.0%	0	0	0
	Study sale of company	14	0.0%	0	0	0
Voting		228	40.8%	32	72	130
	Equal access to proxy	4	0.0%	0	1	2
	Majority vote to elect directors	223	41.7%	32	71	128
	No discretionary voting	1	0.0%	0	0	0
Total G-Index		947	58.2%	60	134	288
G-Delay	Shareholders may call special meeting	129	38.8%	16	36	73
G-Delay	Repeal classified board	239	89.1%	9	24	62
G-Other	Remove antitakeover provisions & other	23	13.0%	0	0	1
G-Other	Adopt antigreenmail provision	3	33.3%	0	0	1
G-Other	Redeem or vote on poison pill	155	73.5%	15	28	45
G-Protection	Maximum director liability	2	0.0%	0	0	0
G-Protection	Vote on future golden parachutes	115	50.4%	8	21	47
G-Voting	Adopt cumulative voting	159	3.8%	6	12	32

Type	Description Proposal	Observations	Average Vote Outcome	Discontinuity #-2,+2	#-5,+5	#-10,+10
G-Voting	Confidential voting	2	100.0%	0	0	0
G-Voting	Eliminate supermajority provision	114	86.0%	6	13	27
G-Voting	Require only majority vote	6	87.5%	0	0	0

Table A.3
Frequency of Proposals per Firm-Meeting

This table presents the frequency of numbers of proposals that are put to vote on a single meeting day in our sample. Column 1 displays the total number of proposals on a meeting day; Column 2 shows the number of proposals that received at least 50% of shareholder votes; Column 3 shows number of proposals that received less than 50% of votes; and Column 4 is the ratio of Column 2 divided by Column 1.

	(1)	(2)	(3)	(4)
No. of Proposals per Meeting	Total	Passed	Not Passed	% Passed
1	850	326	524	38.35
2	700	197	503	28.14
3	408	112	296	27.45
4	320	81	239	25.31
5	200	27	173	13.50
6	90	13	77	14.44
7	49	6	43	12.24
8	56	5	51	8.93
9	45	8	37	17.78
	2,718	775	1,943	28.50

Table A.4

Heterogeneity in the Effect of Votes on Adjusted CDS Spreads with Respect to G-Index

This table compares the regression of the changes in adjusted CDS spreads on the passage of proposals for companies with high versus low G-Index. The dependent variable is the adjusted CDS spreads that are calculated using a rating-adjusted. The model specification is given in Equation (A6). The results for companies with G-Indexes above the median are presented in Columns (1) and (3), and G-Indexes below the median companies are in Columns 2 and 4. All columns use seven separate polynomials of order, six to control for the effect of any determinant of changes in adjusted CDS spreads that is continuous in vote share. The cumulative changes in CDS spreads on days *t*, *t* + 1, and *t* + 2 in Column 1 is significant at 0.10 and in Column 3 at 0.065. Standard errors are clustered by firm. *p*-values are reported in parentheses and significance at the 10%, 5%, and 1% levels are indicated by *, **, and ***, respectively.

	Cumulative Adjusted CDS Spread			
	(1)	(2)	(3)	(4)
	G-Index ≥ 10	G-Index ≤ 9	G-Index ≥ 10	G-Index ≤ 9
Day of Vote, *t*	-0.623	1.450*	-3.663	1.836*
	(0.720)	(0.060)	(0.240)	(0.060)
One Day Later, *t* + 1	-4.752	0.521	-5.878	0.779
	(0.290)	(0.650)	(0.250)	(0.640)
Two days later, *t* + 2	-0.884	-5.608	-1.975	-5.408
	(0.460)	(0.460)	(0.690)	(0.490)
Days *t* + 3 to *t* + 7	8.234	-0.261	-8.206	2.787
	(0.340)	(0.970)	(0.300)	(0.660)
Year Fixed Effect	Yes	Yes	Yes	Yes
Firm Fixed Effect			Yes	Yes
R^2	0.170	0.040	0.300	0.070
Observations	1,304	1,293	1,304	1,293

Table A.5

G-Index and Change in Adjusted CDS Spread

This table displays the result of the ordinary least squares (OLS) regression of changes in annual adjusted CDS spread on G-Index for 2002, 2004, and 2006. Control variables include accounting variables from COMPUSTAT that are used as control: Size, Leverage, ROA, Interest Coverage Ratio and an integer index. See Table A.5 for the definition of the control variables.

	CDS Spread
G-Index	-5.177**
	(0.040)
Industry Fixed Effect	Yes
Year Fixed Effect	Yes
R^2	0.5481
Observation	960

IX.A Diminishing Marginal Effect of Bondholder Wealth Expropriation of Stronger Shareholder Control

In Section II.B, our framework implicitly assumes that the marginal bondholder wealth expropriation effects of stronger shareholder control fall as the level of shareholder-bondholder conflict rises. To justify this assumption, we first need a proxy for the level of shareholder-bondholder conflict and then we ought to show that the value of the risky bond falls at a decreasing pace as the proxy for shareholder-bondholder conflict increases.

According to Jensen and Meckling (1976), leverage gives rise to shareholder–bondholder conflict, where shareholders have an incentive to take on more risk to increase their value. Moreover, it is well-known that a raise in asset volatility can aggravate shareholder-bondholder conflict (Merton, 1974). Specifically, in the context of Merton-type models, equity is a call option on the corporate assets in a levered firm. The value of a risky bond is equal to the value of a portfolio of a risk-free but otherwise identical bond plus a short position in a put option written on the firm's assets. In this context, risk shifting raises asset volatility and thereby increases the value to shareholders (i.e., the value of the call option increases) but reduces the value to bondholders (i.e., the value of the short put increases). That is, risk shifting exacerbates asset substitution concerns for bondholders, which in turn aggravates shareholder-bondholder conflict. Therefore, asset volatility (volatility henceforth) is a reasonable proxy for the level of shareholder-bondholder conflict.

To justify our assumption, we need a theoretical bond valuation model and then we have to show that the first derivative of the bond value with respect to volatility is negative, while the second derivative is positive. We use Merton (1974) model as the underpinning theory. As mentioned above, in a Merton framework the value of a risky debt is essentially equal to cash (risk-free debt) minus a put option. Therefore, given that cash is insensitive to volatility, the first derivative of the bond value with respect to volatility is equal to the negative of the put option's *vega*. *Vega* of any option is always positive. Option values increase in volatility. Therefore, negative *vega* is always negative, indicating that the value of the risky bond falls as shareholder-bondholder conflict increases.

The second derivative of the bond value with respect to volatility is essentially equal to the second derivative of the put option value with respect to volatility. This "Greek" is called v*omma* (sometimes referred to as v*olga* or volatility-Gamma). *Vomma* is the rate at which v*ega* changes. However, unlike *vega, vomma* can be either positive or negative. Using Black-Scholes notations, *vomma* is negative when d_1 and d_2 have different signs.

Given that *vomma* could be either positive or negative, we resort to simulation to show that the marginal bondholder wealth expropriation effects of stronger shareholder control fall as the conflict rises. Specifically, using Merton model (1974), we simulate bond values by changing volatility while holding other parameters unchanged. These parameters are held at values that are consistent with our sample. Figures below depict the results of our simulations.

Our sample leverage ratio is 29%, therefore, in all simulation we use $29 and $100 for the face value of debt (strike of the put) and the asset value, respectively. Results are insensitive to other values for the face value of debt. According to TRACE database, the average maturity of traded plain vanilla corporate bonds during our sample period is around 9.2 years; therefore, time to maturity parameter is kept at 9.2 years.[1] We separately use the average of 3-month, 1-year, 2-year, and 10-year constant maturity Treasury rates (from FRED database) during our sample period to proxy for the risk free rate parameter.

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[1] Results are robust to changing the maturity to 5 years to alleviate the concern that our sample is comprised of 5-year CDS contracts.

As depicted in Figures A.1 through A.4, our simulation result confirms that for high levels of shareholder-bondholder conflict as proxied by volatility, the marginal bondholder wealth expropriation effects of stronger shareholder control fall as the level of shareholder-bondholder conflict rises; and this simulation result is insensitive to different values of time to maturity, interest rates, and face value of debt.

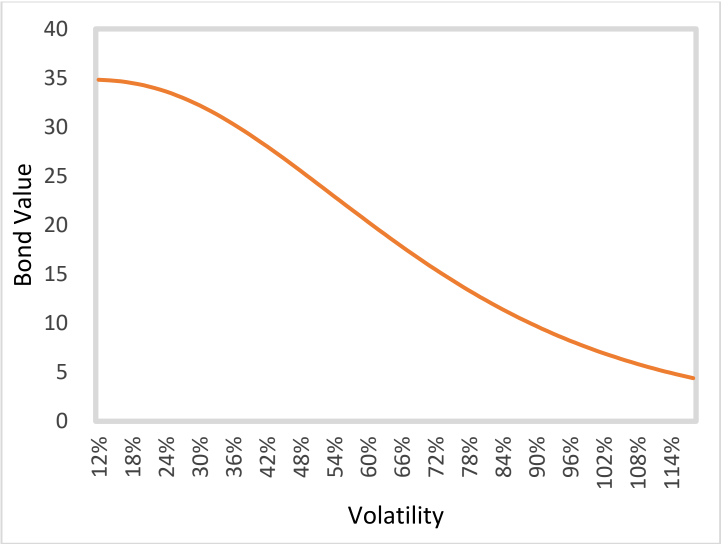


Figure A.1- Face value of is $29. Asset value is $100. Maturity is 9.2 years. Risk-free rate is 1.98%, the mean of the 3-month

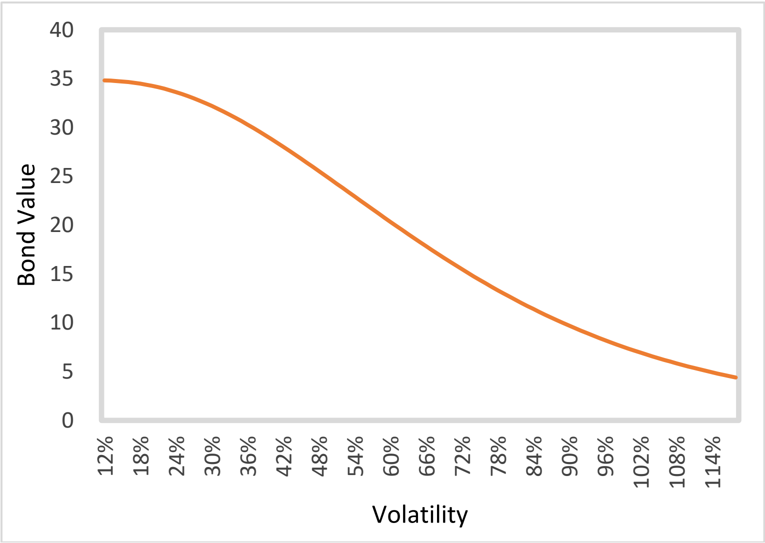


Figure A.2- Face value of is $29. Asset value is $100. Maturity is 9.2 years. Risk-free rate is 2.2%, the mean of the 1-year rate.

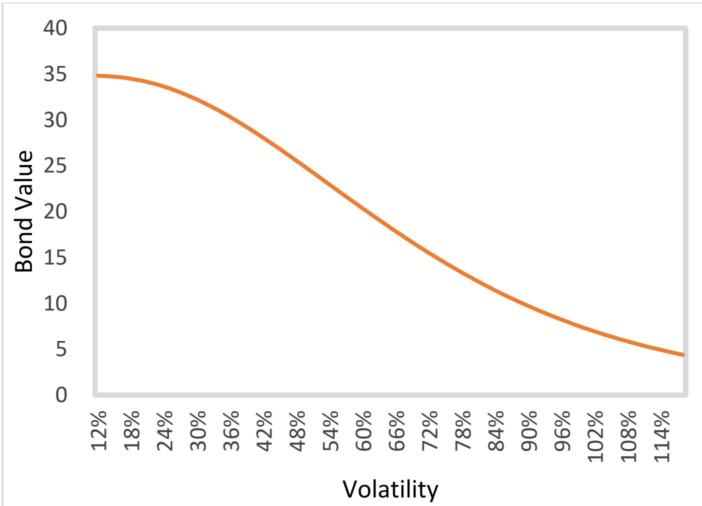


Figure A.3- Face value of is $29. Asset value is $100. Maturity is 9.2 years. Risk-free rate is 2.5%, the mean of the 2-year rate.

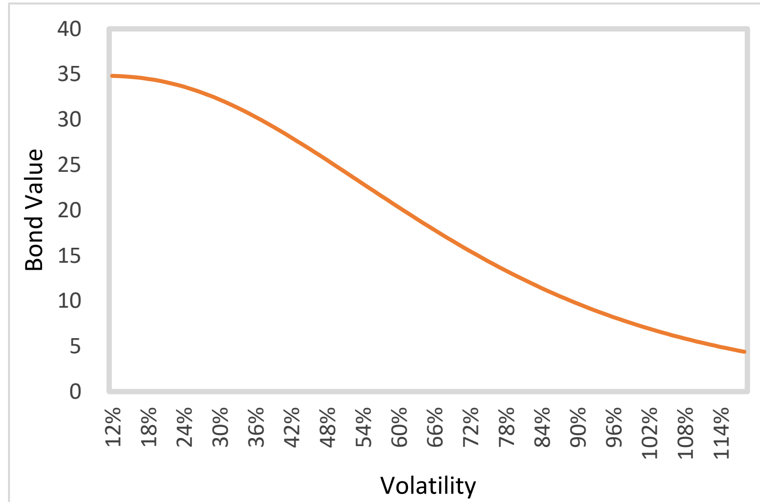


Figure A.4- Face value of is $29. Asset value is $100. Maturity is 9.2 years. Risk-free rate is 4.05%, the mean of the 10-year rate.

IX.B Regression Discontinuity Design

Consider v_{ft} as the percentage of votes for passing a governance proposal for firm *f* on a meeting date at time *t*. If $v_{ft} \geq v^*$, where v^* is the majority threshold, the proposal passes. Using D_{ft} as an indicator for whether the proposal is passed, we call an observation "treated," or D_{ft} =1 if $v_{ft} \geq v^*$, and otherwise "untreated" or $D_{ft} = 0$.

To estimate the effect of treatment D_{ft} on an outcome variable y_{ft}, (e.g., the effect of passing a governance proposal on the adjusted CDS spread), we can write:

$$y_{ft} = k + \theta D_{ft} + u_{ft} \tag{A1}$$

where θ is the effect of passing a proposal on outcome y_{ft} ; and u_{ft} , the error term, is the omitted firm characteristics at time *t* that also affect the outcome variable, y_f. The endogeneity between the treatment, D_{ft}, and the error term, u_{ft}, (i.e., the voting outcome may be a function of unobservable firm characteristics) makes it quite difficult to estimate θ from Equation (A1).

To overcome the endogeneity problem, regression discontinuity design uses the exogenous shift in voting outcome for a narrow window of votes around the majority threshold. As formally shown by Lee (2008), as long as there is random noise components to the vote, the assignment of observations to the treatment group (pass a proposal and therefore $D_{ft} = 1$) and the control group (failing to pass a proposal, or $D_{ft} = 0$) can be considered random. The random assignment of observations to treatment and control enables us to get a consistent estimate that is not affected by the omitted variables.

Following the example of Lee and Lemieux (2010) to use all the observations to improve the efficiency of our estimates, we use a polynomial in votes to capture the effect of any variable that is a continuous function of the vote and affects the outcome. Using separate polynomials for observations on the right side, $P_r(v_{ft}, \gamma^r)$, and $P_l(v_{ft}, \gamma^l)$ on the left side of the majority threshold, we can write:

$$y_{ft} = \theta D_{ft} + P_r(\nu_{ft}, \gamma^r) + P_l(\nu_{ft}, \gamma^l) + u_{ft}. \quad \text{(A2)}$$

Two distinct features of the data distinguish it from a standard regression discontinuity design: (a) the dynamic nature of the treatment, i.e., treatments occur at different points of time, and the possibility of a continuation of the impact of treatment occurs over time in periods after the treatment, and (b) the intensity of treatment, i.e., on some voting days more than one proposal is passed as illustrated in Table 5. To address the dynamic features of treatment we follow the Cellini et al. (2010) dynamic version of Equation (A2) given by:

$$y_{f,t+\tau} = \theta^\tau D_{ft} + P_r(\nu_{ft}, \gamma_\tau^r) + P_l(\nu_{ft}, \gamma_\tau^l) + \alpha_\tau + \eta_c + \lambda_{ft} + e_{ft\tau} \quad \text{(A3)}$$

where $y_{f,t+\tau}$ is the outcome variable in τ periods after the vote date, α_τ is a fixed effect for the time distance to election date, η_c is the calendar year fixed effect, and λ_{ft} is the firm-election fixed effect for firm f in period t.

Alternatively, instead of estimating a separate equation for each τ, we can add distributed lags in treatment to the model as follows. Note that in this case the coefficient θ is interpreted as the causal effect per proposal passed.

$$y_{ft} = \sum_{\tau=0}^{T} \theta^\tau D_{f,t-\tau} + \sum_{\tau=0}^{T}\left[P_r(\nu_{f,t-\tau}, \gamma_\tau^r) + P_l(\nu_{f,t-\tau}, \gamma_\tau^l)\right] + \alpha_\tau + \eta_c + \lambda_{ft} + e_{ft} \quad \text{(A4)}$$

As for the second feature, multiple proposals, we follow the work of Cuñat et al. (2012) and capture the intensity of treatment by aggregating the number of proposals, D_{ft}^K, passed on a meeting day and adding up vote shares, $\sum_{K=1}^{N} \nu_{ft}^K$, for $K = 1, \ldots, N$ as follows:

$$y_{ft} = \theta \sum_{K=1}^{N} D_{ft}^K + \left[P_r\left(\sum_{K=1}^{N} \nu_{ft}^K, \gamma^r\right) + P_l\left(\sum_{K=1}^{N} \nu_{ft}^K, \gamma^l\right)\right] + u_{ft}. \quad \text{(A5)}$$

Finally, to combine the dynamic features with vote aggregation, we combine Equations (A4) and (A5) and write:

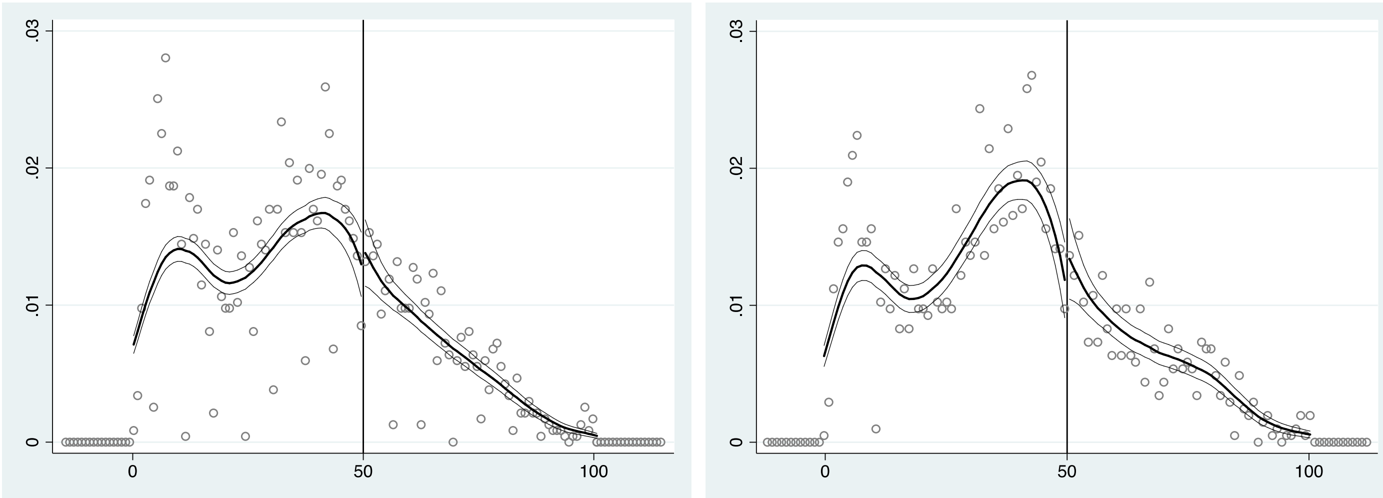
$$y_{ft} = \sum_{\tau=0}^{T} \theta^\tau \sum_{K=1}^{N} D_{f,t-\tau}^K + \sum_{\tau=0}^{T}\left[P_r\left(\sum_{K=1}^{N} \nu_{f,t-\tau}^K, {\gamma_\tau}^r\right) + P_l\left(\sum_{K=1}^{N} \nu_{f,t-\tau}^K, \gamma_\tau^l\right)\right] + \alpha_\tau + \eta_c + \lambda_{ft} + e_{ft} \quad \text{(A6)}$$

IX.C Tests for Quasi-Experiment

In this section, we evaluate the extent to which our design offers a "quasi-experiment" via random assignment of observations to treatment (pass) and control (fail) groups. We test for the possibility of vote manipulation around the passing threshold and the imbalance of the resulting control and treatment groups.

IX.C.1 Continuity in Vote Distribution

To verify whether the assignment to control and treatment groups is random around the threshold, we perform the standard McCrary (2008) test for continuity of vote distribution around the passing threshold. The test statistics of 0.1187 and standard deviation of 0.1385 (or a *p*-value of 0.38) indicates no discontinuity in vote distribution around the passing threshold, suggesting no manipulation of votes around the threshold. We repeat this test for proposals put to vote after 2003, given that Bach and Metzger (2019) show that there is evidence of vote manipulation around the threshold of the corporate charter for the period after 2003. Again, the test statistics of 0.2419 and standard error of 0.1838 (or *p*-value of 0.19) is suggestive of no manipulation. The density plots for both tests are provided below (Figure A.5). This is consistent with the findings of other studies (i.e., CGG and Flammer, 2015), which also find smooth distribution for shareholder-sponsored proposals around the passing threshold.



Panel A: All Proposals

Panel B: Proposals after 2003

Figure A.5 McCrary Test Results

This figure presents a density plot for the McCrary (2008) test to test for the continuity of the distribution of vote shares for shareholder proposals. Data in Panel A include all proposals in our sample between 2001 and 2011, whereas the sample is restricted in Panel B to those proposals after 2003. The horizontal axis represents the share of votes, and vertical axis is the logarithm of the estimated density.

IX.C.2 Preexisting Differences

If the assignment to treatment and control groups is random, we expect the resulting groups to be similar. In Table A.6, we examine whether there are preexisting differences in firm characteristics between the two groups. Also, since one would expect the heterogeneity to reside in managerial and shareholder ownership distribution characteristics, we include variables related to both characteristics, namely CEO ownership, CEO tenure, CEO duality, E-index, and percentage of institutional ownership (collected from Factset). We test for differences in means before the election for the entire sample in Columns 1, 2, 4, and 5 as well as for observations close to the threshold in Columns 3 and 6.

In Columns 1 to 3, we test for similarity of characteristics of the treatment and control groups in the year (or day in the case of CDS spreads) before the election. Results in Column 1 show that except for size and credit rating, no other characteristics differ significantly between the two groups. When we add a polynomial in percentage of votes in Column 2, we find no significant differences between the two groups, including for size and credit rating. We find similar results for the subsample of observations close to the threshold in Column 3. Similarly, in Columns 4 to 6 we find no significant differences in changes in firm characteristics from $(t-2)$ to $(t-1)$. For managerial and shareholder ownership, we observe the same pattern. For close call proposals, there is some significant difference around discontinuity associated with CEO ownership and duality, but the difference vanishes when we employ the polynomials.

In sum, we find no evidence of vote manipulation or any preexisting differences in the treatment and control groups that contradict the random assignment assumption. Therefore, we conclude that there is no systematic difference between the treatment and control groups before the election, thus confirming the validity of our identification strategy.

In the end, we acknowledge that RDD is subject to the standard criticism that it only identifies the local average treatment effect. Bach and Metzger (2019) raise new concerns about using RDD to identify causal effects of governance provisions. However, RDD is a widely used methodology in corporate finance research to circumvent the endogeneity issues and to establish causality. As such, we reported earlier that we find no evidence of manipulation of votes around the threshold.

Table A.6

Pre-Existing Differences as a Function of Vote Outcome

In this table we examine whether there are preexisting differences in firm characteristics between the treatment (pass) and control (fail) groups. We test for differences in means in the year before the election for the entire sample in Columns 1, 2, 4, and 5 as well as for observations close to the threshold in Columns 3 and 6. The only exception is the CDS change in the first two rows, where *t* refers to days instead of years. Columns 1 to 3 consider the levels of firm characteristics, whereas Columns 4 to 6 consider the change in characteristics. Variable definitions are provided in the Appendix in the paper. Each entry is estimated using a separate regression. *p*-values are reported in parentheses, and significance at the 10%, 5%, and 1% levels are indicated by*, **, and ***, respectively.

	Before Meeting (*t* - 1)			Change from (*t* – 2) to (*t* – 1)		
	(1)	(2)	(3)	(4)	(5)	(6)
CDS Spread Changes (Unadjusted)	0.142	0.2	-0.376			
	(0.61)	(0.78)	(0.17)			
CDS Spread Changes (Adjusted)	-1.241	3.451	1.052			
	(0.29)	(0.34)	(0.57)			
Size	-1.084***	-0.194	-0.45	0.011	-0.009	-0.026
	(0.00)	(0.63)	(0.11)	(0.34)	(0.84)	(0.46)
Leverage Ratio	-0.024	-0.012	-0.02	-0.003	0.006	0.011
	(0.17)	(0.74)	(0.42)	(0.29)	(0.64)	(0.32)
Cash/Assets	0.01	0	0.03	-0.00001	-0.002	-0.001
	(0.21)	(0.99)	(0.20)	(0.97)	(0.86)	(0.87)
ROA	0.008	0.034	0.002	-0.001	0.001	-0.003
	(0.33)	(0.39)	(0.91)	(0.85)	(0.96)	(0.61)
Sales Growth	0.01	0.054	0.02	-0.003	0.122	0.026
	(0.28)	(0.24)	(0.55)	(0.85)	(0.20)	(0.64)
Cash Flow/Assets	0.001	0.01	0.003	0.001	-0.008	0.001
	(0.81)	(0.71)	(0.82)	(0.67)	(0.53)	(0.90)
Cash Flow Growth Volatility	-0.001	0.00	0.006	-0.001	0.001	0.00
	(0.52)	(0.96)	(0.21)	(0.37)	(0.65)	(0.99)
Credit Rating (1 to 22)	-1.341***	-0.135	-0.518	0.073	0.078	0.148
	0.00	(0.89)	(0.46)	(0.12)	(0.71)	(0.30)
Interest Coverage Ratio	0.129	-0.046	0.382	-0.038	0.011	0.003
	(0.28)	(0.88)	(0.37)	(0.86)	(0.98)	(1.00)
CEO Ownership	0.002	0.004	0.006*	-0.001	-0.002	-0.002
	(0.32)	(0.39)	(0.08)	(0.36)	(0.30)	(0.30)
CEO Tenure	0.119	0.851	0.647	0.275	-0.944	-0.739
	(0.74)	(0.48)	(0.43)	(0.19)	(0.24)	(0.20)
CEO Duality	-0.056**	0.126	0.125**	-0.013	-0.067	-0.011
	(0.02)	(0.17)	(0.01)	(0.36)	(0.45)	(0.88)
E-Index	0.717***	0.431	0.326	-0.057	-0.003	0.11
	(0.00)	(0.27)	(0.23)	(0.38)	(0.99)	(0.59)
%Institutional Ownership	6.568***	3.801	4.177	0.926**	-0.695	-1.344
	(0.00)	(0.34)	(0.15)	(0.02)	(0.68)	(0.37)
Sample	All Votes	All Votes	Close Calls	All Votes	All Votes	Close Calls
Polynomial in vote share	No	Yes	No	No	Yes	No

IX.D Superiority of Credit Default Swap (CDS) relative to bond yield spread

Firms have a variety of bonds outstanding with different maturities, seniority, and liquidity. How to aggregate these different bonds to measure the total effect of a corporate event is not obvious (Bessembinder et al., 2008). In contrast, while there are CDS contracts with different maturities referencing the same entity, five-year single-name CDSs are the most common and most liquid format (Hull et al., 2004); thus, only one CDS per firm needs to be valued. By using CDS spread data, we also avoid the introduction of any additional noise arising from choosing a particular risk-free specification. The choice of a risk-free benchmark introduces noise into yield spread specifications (Houweling and Vorst, 2005), and the choice of a method to mitigate the coupon effect could exacerbate the problem. On the contrary, the notional amount of CDS contracts grew from $0.6 trillion in June 2001 to a peak of $62.2 trillion by the second half of 2007[2] and has rapidly become the most prominent and liquid credit derivative. In general, the CDS market is known to be far more liquid and efficient than the corporate bond market, with CDS spreads reflecting changes in the credit quality of a reference entity in a more timely manner than the spreads of the corresponding bond issues (Blanco et al., 2005; Ericsson et al., 2009). Studies by Daniels and Jensen (2005) and Zhu (2006) show that price discovery occurs first in the CDS market and subsequently in the bond market. Furthermore, since new CDS contracts can be written at any time, the CDS market is less susceptible to liquidity risk (Longstaff et al., 2005). The Ericsson et al. (2009) results further confirm that CDS spreads are less noisy in reflecting riskiness of debt than yield spreads. Contrary to Collin-Dufresne, Goldstein, and Martin (2001), they find limited evidence for the existence of a common factor.

[2]See ISDA Market Survey Summaries, 2010-1995 (http://www2.isda.org/functional-areas/research/surveys/market-surveys/).

IX.E Graphical Analysis of the Cumulative Adjusted CDS Spread

Figure A.6 shows the difference in the average cumulative adjusted CDS spreads for corporate governance proposals that pass or fail within 2% of the election threshold in a time window around the election date. The time window varies from two days prior to seven days after the election date. This is the same measure as that in the fourth column of Table 3, except that in Table 3 the adjusted CDS spread is computed over a (0, +1) time window, whereas here it is calculated from (-2, 0) to (0, +7) where t = 0 is the election date. (For example, for t = 1 or the equivalent (0, +1) time window, the value is -4.060, which is identical to the value in Column 4 of Table 3.) Also presented are 90% confidence intervals as indicated by dashed lines.

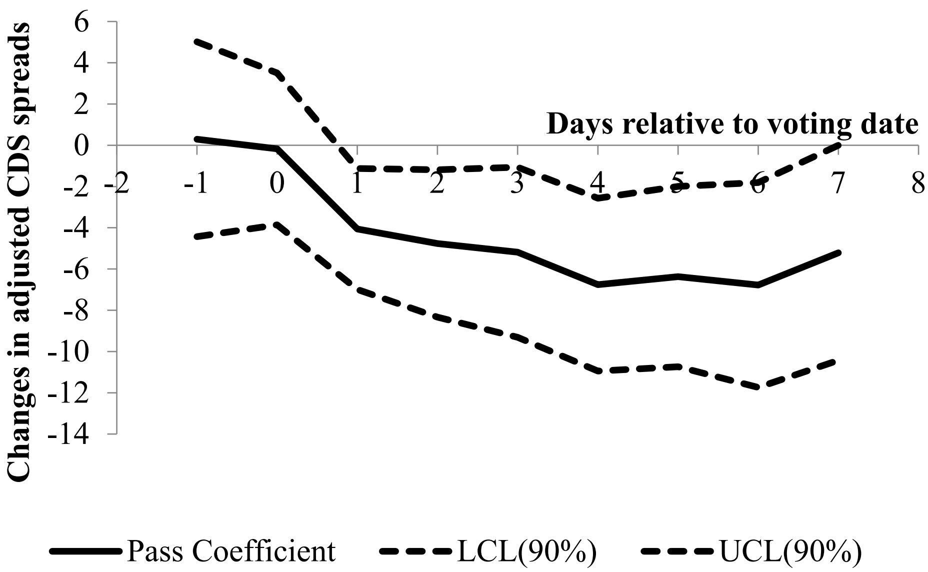


Figure A.6
Cumulative CDS Adjusted Return Around the Election Date
The solid line in this figure represents the dynamics of the average effect of passing a corporate governance proposal on the adjusted change in CDS spread over different time windows around the meeting date, t = 0. The dashed lines represent the 90% confidence interval for this effect. The effect is measured using a regression of the adjusted change in CDS spreads on whether the proposal passed for observations within two points of the majority threshold for a rolling window of time. This window starts from two days before meeting to the meeting date, [-2, 0], and next moves to [-1, 0], [0, 1], [0, 2], [0, 3], [0, 4], [0, 5], [0, 6], and [0, 7].

We can see from the graph that prior to the election date the cumulative adjusted CDS spreads for the proposals that pass are insignificantly different from those that do not pass. After the election date, the cumulative adjusted CDS spread for passing proposals is 4.06 bps lower than the rejected proposals in one day (time window (0, +1)), which is statistically significant at 3%. This difference widens to an average of -6.78 bps over the following days, $t = +2$ to $t = +7$. We can also observe that the largest drop in difference in cumulative spreads occurs on the first day following the election with no reversal pattern on the following days. Thus, to the extent that CDS spreads are a reliable proxy for bondholder risk, our results indicate a reduction in the riskiness of debt, and bondholders view improvement in corporate governance (defined as stronger shareholder rights) to have a net positive effect.

IX.F Constructing the Covenant Index

Helwege, Huang, and Wang (2016) classify covenants into 30 types and then, following Chava et al. (2010), they aggregate them into four categories, namely restrictions on dividend (S1), subsequent financing (S2), investment (S3), and firm behavior during specific events (S4). For the purpose of this study, we focus on covenants related to dividend payout (S1) and takeover (type29).[3] We follow Helwege et al. (2016) for the calculation of S1 and type29. At the issuance of any debt instrument, we define two separate indicator variables for dividend and takeover covenants and set their value to 1 if at least one of the related covenants for each of these categories is included in that issue. As discussed in the footnote, dividend and takeover covenant categories are comprised of multiple covenants restricting the same activity. This strategy avoids inflating

[3] Restriction on dividend payout (S1) is comprised of the following covenants: Dividend-related payments and dividend restrictions. After reviewing all the 30 covenant types and 4 categories, we concluded that covenant type 29, merger restrictions, is more closely related to takeover-related restriction. Type 29 is comprised of the following covenants: consolidation_merger, after_acquired_property_clause, voting_power_ percentage,ESOP_ voting_power_percentage, where ESOP is employee stock ownership plan. According to Helwege et al. (2016), type 29 "typically specify that the surviving entity must assume the debt and abide by all of the covenants in the debt." See Table 2 in Helwege et al. (2016) for more detail.

the effect of a covenant category. Next, we aggregate our issue-level covenant data to firm-month level. Specifically, we construct two new indicator variables corresponding to the same issue-level indicators. For each firm in each month, we set the value of the newly constructed indicator equal to 1 if its corresponding issue-level indicator is 1 for at least one of the issues outstanding for that firm in that month. DT for a firm in a given month is then the sum of the two new indicators for that firm in that month. According to the results in Chava et al. (2010), higher values of the index are associated with more exposure to shareholder opportunism. Finally, we use a similar aggregation algorithm to move from monthly-level index to annual level for each firm by finding the maximum of the index for each firm across the 12 months in each year.

IX.G Robustness Checks

IX.G.1 Controlling for Equity Return

A potential concern with our main result is that the drop in CDS spreads may not be a direct result of governance improvement, but rather a mechanical effect of positive equity return as documented by CGG. To address this concern, in Table A.7 we re-estimate an augmented version of our models (Equations (A1) and (A2)) where equity return is added as a control variable. In Panel A, equity returns are calculated using the market model, whereas in Panel B the Fama-French Model is used to calculate equity returns.[4] Although in some cases the magnitude is smaller, overall we find that our results are robust with respect to the inclusion of equity returns. This suggests that the impact of governance improvement on the CDS spread is not solely the result of the increase in equity value but rather reflects the increase in the entire firm value, i.e., both equity and debt values.

[4] Given the finding by CGG that it takes only one day for the stock market to react to shareholder proposals, we narrow our time window to one day here instead of two days in Table 3.

Table A.7

Adjusted CDS Spread Response to Governance Proposals Conditional on Equity Return

This table presents regression results of the cumulative adjusted change in CDS spreads from the day of the meeting $t = 0$ to the next day $t = 1$ in response to passage of a governance proposal. Adjusted CDS spreads are calculated using a rating-adjusted method. The model specifications for Columns 1-5 are given in Equation (A1) and for Column 6 in Equation (A2) with the only difference that here equity abnormal return is added as a control variable to Equations (A1) and (A2). Equity returns are calculated using Market Model in Panel A and Fama-French Model in Panel B. Column 1 estimates are based on the whole sample. Column 2 restricts the sample to observations with a vote share within 10 points of threshold; Columns 3 to 5 restrict the sample to 5, 2, and 1 points of the threshold, respectively. Column 6 uses the full sample (winsorized at 1% and 99%) by introducing a polynomial in the vote share of order 6, one on each side of the threshold. All columns control for year fixed effects. Standard errors are clustered by firm. *p*-values are reported in parentheses, and significance at the 10%, 5%, and 1% levels are indicated by *, **, and ***, respectively.

	Panel A: Market Model					
	(1) All Votes	(2) −10; +10	(3) −5; +5	(4) −2; +2	(5) −1; +1	(6) Full Model
Pass	0.505	-1.279	-4.263***	-2.682**	-2.934*	-6.486**
	(0.600)	(0.120)	(0.010)	(0.020)	(0.100)	(0.020)
Equity Abnormal Return (Market Model)	-0.409	0.201	-0.083	-0.824*	-1.012**	-0.386
	(0.330)	(0.780)	(0.920)	(0.090)	(0.050)	(0.370)
R^2	0.010	0.010	0.034	0.065	0.082	0.017
Observations	2,718	776	387	170	105	2,724

	Panel B: Fama-French					
	(1) All Votes	(2) −10; +10	(3) −5; +5	(4) −2; +2	(5) −1; +1	(6) Full Model
Pass	0.515	-1.260	-4.287***	-2.713**	-2.945	-6.426**
	(0.590)	(0.130)	(0.010)	(0.020)	(0.110)	(0.020)
Equity Abnormal Return (Fama French Model)	-0.464	0.116	0.039	-0.864	-1.042	-0.444
	(0.290)	(0.880)	(0.970)	(0.160)	(0.120)	(0.320)
R^2	0.010	0.010	0.034	0.065	0.082	0.017
Observations	2,718	776	387	170	105	2,724

IX.G.2 Unadjusted CDS Spreads

Another potential concern is the way the abnormal CDS spreads are calculated by using a rating-adjustment method. As illustrated in Equation (1), rating-adjusted CDS spreads are calculated by subtracting the average CDS spreads for issues with the same rating. Gormley and Matsa (2014) show that demeaning the dependent variable with respect to the group can produce inconsistent estimates; they recommend using a fixed effect model instead. To do this, we re-estimate the baseline specification in Tables 3 and 4 by using changes in unadjusted CDS spreads as a dependent variable and add fixed effects for credit rating categories. The results are presented in Tables A.8 and A.9.

Table A.8
Unadjusted CDS Spread Response to Governance Proposals
This table presents regression results of the cumulative unadjusted change in CDS spreads from the day of the meeting $t = 0$ to the next day $t = 1$ in response to passage of a governance proposal. The model specification for Columns 1-5 is given in Equation (A1) and for Column 6 in Equation (A2). All columns control for year and rating fixed effects. Column 1 estimates are based on the whole sample. Column 2 restricts the sample to observations with a vote share within 10 points of the threshold; Columns 3 to 5 restrict the sample to 5, 2, and 1 points of the threshold, respectively. Column 6 uses the full sample (winsorized at 1% and 99%) by introducing a polynomial in the vote share of order 6, one on each side of the threshold. Standard errors are clustered by firm. *p*-values are reported in parentheses, and significance at the 10%, 5%, and 1% levels are indicated by *, **, and ***, respectively.

	Cumulative Changes in Unadjusted CDS Spread					
	(1) All Votes	(2) −10; +10	(3) −5; +5	(4) −2; +2	(5) −1; +1	(6) Full Model
Pass	0.210	-1.473	-2.926**	-3.061**	-3.461	-4.811**
	(0.880)	(0.340)	(0.040)	(0.020)	(0.150)	(0.020)
R^2	0.033	0.022	0.047	0.137	0.198	0.037
Observations	2,718	776	387	170	105	2,718

Table A.9
Dynamics of Impact of Aggregate Votes on Unadjusted CDS Spreads
This table presents the effect of passing a proposal on changes in the unadjusted CDS spread on the meeting date (t), one day after ($t + 1$), and the cumulative effect from $t + 2$ to $t + 7$. The dependent variable is the unadjusted CDS spread. The model specification is given in Equation (A6). All columns use seven separate polynomials of order six to control for the effect of any determinant of change on adjusted CDS spreads that are continuous in the vote share. Standard errors are clustered by firm. *p*-values are reported in parentheses, and significance at the 10%, 5%, and 1% levels are indicated by *, **, and ***, respectively.

	Changes in Unadjusted CDS Spread Using All Proposals		
	(1)	(2)	(3)
Day of Vote, t	-1.353**	-1.308**	-1.942*
	(0.030)	(0.030)	(0.060)
One Day Later, $t + 1$	-1.336	-1.302	-1.853
	(0.130)	(0.150)	(0.200)
Days $t + 2$ to $t + 7$	-2.305	-2.205	-6.434
	(0.410)	(0.480)	(0.310)
Year Fixed Effect	Yes	Yes	Yes
Firm Fixed Effect		Yes	
Firm-Meeting Fixed Effect			Yes
Distance to Election Fixed Effect	Yes	Yes	Yes
Rating Fixed Effect	Yes	Yes	Yes
R^2	0.016	0.034	0.102
Observations	11,376	11,376	11,376

Table A.8 shows that our main results are robust with respect to how we adjust for rating. When using fixed effects instead of demeaning the CDS spreads for rating categories, the size of the Pass coefficient drops by up to 1.7 bps, but by and large the results are still statistically significant with the caveat that *p*-values are marginally larger than those in Table 2. Table A.9 also provides similar evidence in support of robustness of the results for the dynamics of the impact. Similar to Table 3, CDS spreads drop proportionately on day *t*, *t* + 1, and the days between *t* + 2 and *t* + 7. However, unlike the results in Table 3, the drop in CDS spreads is statistically significant only on day *t*.

IX.G.3 Vote Manipulation

Another legitimate concern with our analysis is that RDD is invalid if agents can alter or manipulate the outcome. In the context of our study, the concern is that managers may have incentives to acquire power, formal and informal, to intervene in close votes and manipulate the outcome towards the passage (failure) of manager-friendly (shareholder power-enhancing) proposals (see Bach and Metzger (2019)). Therefore, it is imperative to empirically establish the validity of the random-assignment assumption.

Given that shareholder-sponsored proposals are not binding, intuition suggests that managers will only intervene in proposals that are more likely to be implemented. Thus, we disaggregate shareholder proposals in terms of likelihood of implementation and then examine the impact on CDS spreads through the RDD. In doing so, we borrow from the results in Ertimur et al. (2010). They document that the likelihood of implantation is higher for defense and voting proposals (labeled as shareholder right) as well as for the proposals that are sponsored by the unions. To show that our results are not driven by these proposals, we drop them from our sample and employ the RDD on a new sample that contains proposals that are less likely to be manipulated. Results are reported in Table A.10. In the first column, we drop 1,175 defense and shareholder right proposals from the 2,718 total proposals that were originally analyzed and reported in Table 2. Results for the remaining proposals shows a 9 basis points drop in adjusted CDS that is statistically significant at 5%. In the second column, we drop 899 proposals sponsored by the unions. CDS spreads for the remaining proposal drop by 6 basis points (significant at 1%). In the third column, we drop defense and voting proposals as well as proposals sponsored by the unions. The result shows a statistically significant drop in the CDS spreads of about 11 basis points. These results are consistent with our earlier finding in Table 2 and add to its credibility.

Table A.10
Vote Manipulation

Cumulative Changes in Adjusted CDS Spread			
	(1)	(2)	(3)
Proposals Dropped:	Defense and Voting	Sponsored by Unions	Defense and Voting & Sponsored by Unions
Pass	-9.0710**	-6.1920***	-11.1250**
	(0.0400)	(0.0100)	(0.0500)
R^2	0.0264	0.0235	0.0338
Observations	1,543	2,117	1,110

This table presents regression results of the cumulative adjusted change in CDS spreads from the day of the meeting $t = 0$ to the next day $t = 1$ in response to passage of a governance proposal. Adjusted CDS spreads are calculated using a rating-adjusted method. The model specification is given in Equation (A1). Column 1 estimates are based on the whole sample (winsorized at 1% and 99%) after dropping defense and voting proposals. In Column 2 proposals sponsored by unions are dropped (winsorized at 1% and 99%). Column 3 further restricts the sample by dropping all defense and voting proposals as well as those sponsored by unions. All columns control for year fixed effects and the information contained in distance to majority threshold using a polynomial in the vote share of order 6, one on each side of the threshold. Standard errors are clustered by firm. *p*-values are reported in parentheses, and significance at the 10%, 5%, and 1% levels are indicated by *, **, and ***, respectively.

IX.G.4 CDS Sample Selection Bias

Another major concern with our analysis is the sample bias that may arise from considering only firms with traded CDS contracts. Subrahmanyam et al. (2014a) show that the inception of CDS trading is not random. *Ex-ante*, CDS trading may be more likely to be initiated for certain types of firms. *Ex-post*, the existence of CDS trading may distort real incentives of managers and/or impact the severity of the shareholder-bondholder conflict. The presence of CDS trading as an insurance contract ameliorates bondholder agency risk from shareholder opportunism, and therefore lowers the sensitivity of CDS spreads to changes in shareholder control, implying that our estimates likely understate the effect in the overall population. Subrahmanyam, Tang, and Wang (2014a; 2014b) show that CDS firms tend to be larger, safer, more profitable, with more working capital, and hold more cash. In this case also, our estimates likely understate the effect in the overall population. Nevertheless, we address this concern in two ways. First, we compare the distribution of our sample financial- and governance-related characteristics with those of public firms during the sample period. Second, and more formally, we address the selection bias by employing a Heckman (1979) type self-selection model (similar to Subramanyam et al. (2014a)) in conjunction with RDD design.

Table A.11 reports the comparison of our sample characteristics and those of the COMPUSTAT universe during the sample period. Consistent with Subramanyam et al. (2014a), we also find that the firms with traded CDS contracts in our sample are larger and have stronger balance sheets with relatively high credit quality. But the evidence is mixed with respect to their managerial entrenchment levels. Their managers have shorter tenures but are more likely to also be the chairman of the board. Furthermore, these firms have greater number of major antitakeover defenses in place, as measured by E-Index. Overall, it does not appear that the benefit of stronger shareholder control to bondholders are overstated in our results but may be understated.

Table A.11

Comparing the Distribution of the Sample Characteristics with Those of COMPUSTAT Universe

	Our Sample			All COMPUSTAT			
	Mean	Median	Obs.	Mean	Median	Obs.	Diff
Ln(Assets)	10.056	9.966	1,750	5.24	5.418	127,581	4.816***
Cash/Assets	0.101	0.065	1,750	0.195	0.086	127,558	-0.094***
EBIT/Assets	0.085	0.077	1,749	-1.472	0.029	125,860	1.557***
Sales/Assets	0.824	0.661	1,750	1.061	0.583	126,796	-0.237**
PPENT/Assets	0.308	0.256	1,686	0.256	0.146	124,599	0.052***
Leverage Ratio	0.234	0.171	1,745	0.209	0.11	113,630	0.025***
Rated	0.986	1.000	1,750	0.214	0.000	128,165	0.772***
RE/Assets	0.209	0.192	1,747	-29.083	-0.004	124,061	29.292***
CAPX/Assets	0.043	0.035	1,720	0.064	0.028	120,922	-0.021***
Tenure	5.464	4.000	1,709	7.527	5.000	37,725	-2.063***
Duality	0.838	1.000	1,479	0.674	1.000	19,709	0.164***
E-Index	3.250	3.000	1,141	3.089	3.000	28,304	0.161***

To formally address the effects of selection bias caused by CDS trading, we use Heckman's selection model. Heckman's selection model improves our estimation results for the effect of being selected for CDS trading. Subrahmanyam et al. (2014a) find that CDS contracts are more likely to be traded for firms with high credit quality and visibility (size). Therefore, our revised empirical model consists of two equations: the main RDD equation, which is the same as equation (A2),

$$y_{ft} = \theta D_{ft} + P_r(\nu_{ft}, \gamma^r) + P_l(\nu_{ft}, \gamma^l) + u_{ft}. \tag{A2}$$

and a selection equation that describes the characteristics of the firms for which CDS spread, y_{ft}, is observable, i.e., firms that have active CDS trading:

$$Active\ CDS_{ft} = \gamma Z_{ft} + v_{ft}. \tag{A7}$$

with $$corr(u_{ft}, v_{ft}) = \rho$$

where Z_{ft} is the firm characteristics for credit quality (i.e., Cash/Assets, EBIT/Assets, Sales/Assets, PPENT/Assets, Leverage, RE/Assets standing for retained earnings to total assets ratio, CAPX/Assets, and Rated standing for having a bond rating or not) and visibility (Ln(Assets)) that Subrahmanyam et al. (2014a) found to be significant determinants of having a CDS contract or not. If $\rho \neq 0$ the standard OLS estimations for the single RDD equation will be biased. Using Heckman's maximum likelihood estimation procedure to combine the RDD and the selection equation provides consistent and efficient estimates. Finally, we test for $\rho = 0$ to assess the extent to which our original RDD estimates (single equation) are biased because of the endogeneity between the error terms for the two equations.

Table A.12 shows the estimation results. The estimation results for the RDD model and the selection model are reported in the upper and lower panels, respectively. Columns 1 and 3 report the original OLS model in which selection is ignored, whereas Columns 2 and 4 use Heckman's model and take the selection issue into consideration. Comparing Columns 1 to 2 or 3 to 4 shows that estimation results for the RDD model and for the combination of the RDD and Heckman selection model are very similar and consistent with our earlier main findings. This is reassuring that our original RDD estimates are not biased. Moreover, the results of the endogeneity test show that the correlation coefficient, ρ, does not reject the null hypothesis that $\rho = 0$ with *p*-values of .315 and .137 for models in Columns 2 and 4, respectively. Overall, this exercise confirms that sample selection does not seem to generate any bias in our estimates.

Table A.12

Robustness Check: Sample Selection bias

	Cumulative Changes in Adjusted CDS Spread			
RDD Equation	(1)	(2)	(3)	(4)
Pass	-6.087***	-5.970***	-5.559**	-5.413**
	(0.006)	(0.005)	(0.011)	(0.012)
Selection Equation				
Ln(Assets)		0.416***		0.443***
		(0.000)		(0.000)
Cash/Assets		0.539**		0.476*
		(0.047)		(0.090)
EBIT/Assets		0.021***		0.490***
		(0.000)		(0.010)
Sales/Assets		0.006***		0.151***
		(0.000)		(0.000)
PPENT/Assets		0.546***		1.028***
		(0.000)		(0.000)
Leverage		-1.074***		-0.933***
		(0.000)		(0.000)
Rated		1.369***		1.344***
		(0.000)		(0.000)
RE/Assets				0.054***
				(0.000)
CAPX/Assets				-4.171***
				(0.000)
Censored obs.		106,773		100,688
Uncensored obs.	2,611	2,611	2,557	2,557
Total obs.	2,611	109,384	2,557	103,245
Endogeneity test				
Rho		0.068		0.092
p-value (Rho = 0)		0.315		0.137

This table presents regression results of the cumulative adjusted change in CDS spreads from the day of the meeting $t = 0$ to the next day $t = 1$ in response to passage of a governance proposal. Adjusted CDS spreads are calculated using a rating-adjusted method. The model specification for Columns 1 and 3 are given in Equation (A1) and for Column 2 and 4 in Equation (A7) in which sample selection is taken into account using Heckman procedure. The upper panel presents the estimation results for the main equation, whereas the lower panel present the estimation result for the selection model. All columns use the full sample (winsorized at 1% and 99%). All columns control for year fixed effects and the information contained in distance to threshold by using a polynomial in the vote share of order 6, one on each side of the threshold. Standard errors are clustered by firm. *p*-values are reported in parentheses, and significance at the 10%, 5%, and 1% levels are indicated by *, **, and ***, respectively.

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