Price and Probability: Decomposing the Takeover Effects of Anti-Takeover Provisions

Vicente Cuñat

Mireia Giné

Maria Guadalupe *

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Abstract

We study the effects of anti-takeover provisions (ATP) on the takeover probability, the takeover premium and target selection. Voting to remove an ATP increases both the takeover probability and the takeover premium, i.e. there is no evidence of a trade-off between premiums and takeover probabilities. We provide causal estimates based on shareholder proposals to remove ATPs and deal with the endogenous selection of targets through bounding techniques. The positive premium effect in less protected firms is driven by better bidder-target matching and merger synergies.

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Vicente Cuñat: LSE, Houghton St, London WC2 2AE, UK (v.cunat@lse.ac.uk); Mireia Gine: WRDS, University of Pennsylvania and IESE Business School, Av. Pedralbes 21, Barcelona, Spain (gine@wharton.upenn.edu); Maria Guadalupe: INSEAD, Boulevard de Constance, 77300 Fontainebleau, France (maria.guadalupe@insead.edu).

I. Introduction

Anti-takeover provisions (ATP) such as staggered boards, dual-class shares, poison pills and similar governance mechanisms have been found to affect firm value (e.g. Gompers, Ishii, Metrick, 2003; Cuñat, Gine, Guadalupe, 2012).[1] In the context of the takeover debate, proponents argue that anti-takeover provisions allow managers to negotiate a higher price in the event of a hostile bid, encourage more long-term investment, and hence create value (Stein, 1988, Harris, 1990). However, they may also reduce or delay the possibility of a takeover (Ryngaert, 1988; Pound, 1987; Malatesta and Walkling, 1988; Comment and Schwert, 1995; Karpoff et al., 2017). The trade-off between the price (the premium) and the probability of a takeover has become common wisdom and accepted as fact.

The goal of this paper is to provide causal estimates that allow us to assess if there is effectively a trade-off between price and probability, and to identify the type of mergers that anti-takeover provisions deter/allow to happen. We also aim to identify the channels through which firm-level anti-takeover provisions create or destroy value for firms and the economy as a whole. Establishing causal effects is important given the evidence of endogeneity of governance structures (Schoar and Washington, 2011; Karpoff et al., 2017) and the potential of mergers and acquisitions to create or destroy value (Morck, Schleifer and Vishny, 1990; Maksimovic and Phillips, 2001; Burkart, Gromb and Panunzi, 1998; Schoar, 2002).

[1] Reducing the threat of a takeover has been shown to destroy value by weakening managerial discipline (Scharfstein, 1988; Bertrand and Mullainathan, 2003; Gormley and Matsa, 2016).

We start by showing that the expected gains from adopting anti-takeover provisions can accrue to shareholders in three ways. The first is the effect of such provisions on the *probability* of being acquired (i.e. the deterrent effect). The second is the effect on the *premium* paid conditional on a successful acquisition. This is based on the *price* paid for the target at auction, which is determined by the relative bargaining power of the parties, the degree of competition (number of bidders), and the potential for synergies of the deal. Note that the effect is ex-ante ambiguous: for example, if anti-takeover provisions give managers more bargaining power, removing them should imply a negative effect on the premium; if they attract less competition, removing them implies a positive effect (Bulow and Klemperer, 1996). The third is the *selection* effect, whereby an anti-takeover provision changes the population of firms that become targets. For example, the additional firms that end up being taken over because they dropped an anti-takeover provision may be those with lowest (highest) potential for value creation, implying negative (positive) selection. The first two effects have been the focus of existing research as they are important determinants of shareholder value. The third, albeit seldom discussed, is essential inasmuch as one cannot infer the takeover premium by comparing firms that are taken over with and without anti-takeover provisions because the population of target firms changes when such provisions are in place.[2]

To provide causal estimates of the components of the expected premium in this setting requires some form of random assignment in the adoption of anti-takeover provisions to establish

[2] Note that most existing studies focus on effective (conditional) takeover premiums, that is, conditional on a takeover offer being made. Since premiums do not exist in the absence of a takeover bid, changes in these "conditional" premiums are subject to selection bias, as discussed later.

causality. For this we report two different specifications. The first is a regression discontinuity design for the takeover probability and expected premium. We use data on all shareholder-sponsored proposals (2,882 proposals in 927 different firms) to remove an anti-takeover provision voted on at annual meetings of S&P 1500 firms between 1994 and 2013. We rely on vote outcomes being random in a narrow interval around the majority threshold, leading to a discrete change in the probability of dropping a provision (see Cuñat, Gine, Guadalupe, 2012, 2013). The second specification is a matching estimator that is validated using the identification strategy proposed by Angrist and Rokkanen (2015).[3] Since each estimation strategy relies on a different set of maintained assumptions, our study adds value by providing consistent results across various techniques evaluated at different points of the sample.

Even when armed with a source of exogenous variation, we still need to correct for inherent problems of selection in the estimation of the conditional premium given the co-determination of premiums with the population of firms taken over. We use the bounding estimation strategy proposed by Lee (2009) to estimate upper and lower bounds for the effect of anti-takeover provisions on the takeover premium. As we need a distribution of premiums in order to apply the bounding technique, which cannot be done at the exact discontinuity, we provide two sets of results for the conditional premium using Lee bounds: one on the full sample using validated matching (Angrist and Rokkanen, 2015) and a second in an interval around the discontinuity.

[3] Angrist and Rokkanen (2015) build on the fact that in the regression discontinuity design we observe the assignment variable – the vote in our case – which is the only source of heterogeneity. They propose a matching estimator and use the regression discontinuity approach as a tool for validating the conditional independence assumption of the model. We explain further the method and intuition in Section III.

Across specifications and samples, we find that voting to remove an anti-takeover provision has a significant positive impact on the probability of a firm being taken over in the future. At the majority threshold (classic regression discontinuity design), passing a proposal to drop an anti-takeover provision increases the likelihood of a takeover within five years by 9.2% (1.8% per year), and also increases the expected value of future takeover premiums by 4.1%. For firms away from the discontinuity the effects are smaller but also positive and significant: voting to remove an anti-takeover provision increases the probability of a takeover within five years by 4.1% (0.8% per year) and increases the expected value of future takeover premiums by 2.6%. These are intent to treat effects (ITT) that measure the effect of passing a proposal. We discuss possible calculations of the effect of the provision itself (treatment on the treated) in section V. We also show that the results are similar across different definitions of the share of votes passed variable (in particular the different treatment of abstentions across votes).

Total shareholder gains can be expressed as an unconditional premium that includes both firms that experience a takeover (and realize a takeover premium) and those that do not (with a takeover premium of zero). The effect on the expected unconditional premium is not subject to the inherent selection problem of the conditional (i.e. realized) takeover premium, because the populations of the treatment and control groups are comparable. However, we also seek to know whether a given firm is able to obtain a higher or lower premium if it drops the anti-takeover provision and a merger does happen. For this, we cannot just compare the premium of firms that are taken over with or without anti-takeover provisions, as we can only observe takeover premiums for the firms that are taken over and we need to account for different selection patterns in the two groups. We use the bounds methodology developed by Lee (2009) to provide estimates that account for selection. Across different specifications and samples (for different intervals around the discontinuity, and for the full sample) we find that the effect of voting to remove a provision on the conditional premium is never negative and can be as high as a 15% four-week premium, suggesting that more shareholder value is created in less protected firms. This is a relatively large impact when compared to other effects that the literature has found relevant. For example, Eckbo (2009) finds that, in the cross-section, the average difference in the premium between a hostile and a friendly takeover is 5.8%, that between a public and a private acquirer 4.9%, and that between a multiple and single bidder contest 7.8%.

The takeover probability effect is in line with the findings of Karpoff et al. (2017), who, with a different sample and a different local instrument, also find a deterrent effect of anti-takeover provisions. This consistency between two different causal approaches contrasts with the largely inconclusive prior literature that does not address the endogeneity of adoption.[4]

The positive premium effect is in contrast to the accepted wisdom that anti-takeover provisions allow managers to obtain higher premiums; we find the opposite to be true of our sample, where there appears to be no trade-off between price and probability. This is all the more important given that studies of the correlation between anti-takeover provisions and takeover premiums have often found that adopting an anti-takeover provision has a negligible or positive

[4] For example, Pound (1987) documents that anti-takeover provisions reduce the probability of a takeover bid; Ryngaert (1988) finds that firms with a poison pill are more likely to reject a hostile takeover bid. In contrast, Comment and Schwert (1995) find that poison pills have no effect on takeovers; Bates, Becher and Lemmon (2008) find that having a staggered board does not preclude the completion of a takeover once a firm has already received a bid, though it may reduce the likelihood of receiving a bid in the first place.

effect on the premium (Comment and Schwert, 1995; Bange and Mazzeo, 2004; Bebchuk, Coates, and Subramanian, 2002; Bates, Becher & Lemmon, 2008; Cotter, Shivdasani, Zenner, 1997). However, to date no analysis has dealt with both the endogeneity of adoption and the selection of targets as we do here.

Next, we investigate the determinants of the positive premium result (of dropping a provision), which challenges the argument that anti-takeover provisions give managers bargaining power to extract a higher premium. We find that across specifications, the total shareholder value creation (adding up the dollar value of the acquirer and target premiums) is positive. This net value creation in the economy seems to come partly from more acquisitions in related industries (with higher potential for synergies) and partly from targets being matched to more valuable acquirers (positive selection).

Across the entire sample of firms, the higher premium is also linked to more competition for less protected firms: they have more bidders, more unsolicited bids, more challenged deals and more deals paid in cash. The competition amongst bidders increases the overall bargaining power of the target and seems to trump any loss in bilateral bargaining power with each individual bidder. This is consistent with the auction literature which suggests that the surplus of the seller is largely determined by the number of bidders rather than the individual bargaining power/negotiating skills of each of them (Bulow and Klemperer, 1996). However, for the more contested votes (around the discontinuity) we find a decrease in competition in terms of number of acquirers and contested deals and instead find some evidence of a higher acquirer premium, suggesting that for close-call deals the absence of competition allows acquirers to capture some of the surplus generated.

Lastly, we use the empirical framework to determine what fraction of the total increase in value from removing anti-takeover provisions comes from its different components. We find that the increase in value operates largely via quantities: over 50% comes from the increased probability of mergers. The premium effect is positive and potentially large. The selection effect is positive and between a quarter and half of overall value created when we focus on the whole vote support. We cannot assess the direction of selection when we focus on close-call votes but the bounds suggest that it is potentially large. This confirms that accounting for selection is important to understand how takeovers create value in the market.

In terms of the generalizability and external validity of our results, our data include over one third of firms in the S&P 1500 between 1994 and 2013. However, while our results apply to a significant share of firms, we cannot extrapolate the results – without further assumptions – to firms that are never subject to such shareholder votes. In particular, we cannot rule out that firms in which the premium effect is negative never hold a vote on anti-takeover provisions. But even in such a case we note that despite the proposals in our sample creating value for all firms on average, some failed to garner strong shareholder support, and the vast majority were opposed by management. This raises questions about governance that are beyond the scope of this paper.

In the next section we provide a framework to decompose the unconditional premium. In Section III we discuss the identification strategies underlying the two specifications used in the paper. In Section IV we present the data, and in Section V the results on unconditional premiums and takeover probabilities. In Section VI we provide bounded estimates for the treatment effect on the premium and use all the estimates in our decomposition. In Section VII we offer potential explanations for the positive premiums. Section VIII concludes.

II. Framework: Decomposing the Unconditional Premium

II.1. Dealing with Endogeneity and Selection

We start by providing an analytical framework to examine the effect of anti-takeover provisions on the expected shareholder gains via takeover probabilities and premiums. This allows us to establish the elements required for the decomposition of the unconditional premium in Section II.2 and to assess all possible sources of bias we need to deal with empirically.

We define the treatment dummy variable D, which takes the value D=1 if shareholders vote to drop an anti-takeover provision, and D=0 if they vote to keep it. Empirically, we observe the realized premium variable Y, which equals the premium paid if a takeover takes place, and zero otherwise. The realized premium measures the shareholder gains from the whole population of firms at risk of a takeover. In order to understand selection issues, we define two latent variables. Y^* is the potential premium offered for a firm, which is only observed if a takeover takes place. Z^* is a measure of the latent merger propensity of a firm; a merger happens whenever $Z^* > 0$. Therefore, we can write the unconditional premium (i.e. not conditional on whether the merger occurred) as: $Y = 1[Z^*>0] \cdot Y^*$, where 1[.] is the indicator function.

This structure gives rise to the classic selection model, which in standard notation and assuming a linear structure, is written as (Heckman, 1979; Lee, 2009):[5]

$$Y^* = D\beta + X\mu_1 + U \qquad \text{(underlying premium)}$$

[5] This model can be generalized to a non-linear structure.

$Z^* = D\gamma + X\mu_2 + V$ (latent merger propensity)

$Y = 1[Z^*>0] \cdot Y^*$ (unconditional premium)

The first challenge is to find a way to randomly assign the treatment dummy D. If D is randomly assigned, then we can recover the effect of an anti-takeover provision on the unconditional premium, ΔY, and on the takeover probability, ΔP:

$$\Delta P = \Pr[Z^*>0 \mid D=1] - \Pr[Z^*>0 \mid D=0] \qquad (1a)$$

$$\Delta Y = E[Y \mid D=1] - E[Y \mid D=0] \qquad (1b)$$

However, even with a randomly assigned D, one cannot recover β. Nevertheless, β is the parameter of interest to assess the effect of anti-takeover provisions conditional on a merger taking place; it is the difference in the price paid for a specific target with or without an anti-takeover provision in place.

The reason why we cannot recover this causal parameter even when we have an instrument for D is the selection of targets: the observed Y is conditional on a merger occurring ($Z^*>0$), which is itself affected by treatment $E[Y \mid D, X, Z^*>0] = D\beta + X\mu_1 + E[U \mid D, X, V> - D\gamma - X\mu_2]$

Typically, existing premium studies compare premiums conditional on a merger happening for firms with and without anti-takeover provisions, which we can write as:

$$E[Y \mid D=1, X, Z^*>0] - E[Y \mid D=0, X, Z^*>0]$$
$$= \beta + E[U \mid D=1, X, V> -\gamma - X\mu_2] - E[U \mid D=0, X, V> -X\mu_2] \qquad (2)$$

This shows that even with a randomly assigned D (and if U and V are not independent) one cannot recover the causal effect on Y^* because of the sample selection term $E[U \mid D=1, X, V> -\gamma -X\mu_2] - E[U \mid D=0, X, V> - X\mu_2]$.

Hence, we need an identification strategy that not only provides exogenous assignment to treatment but also corrects for selection. Section III describes how we address both requirements.

II.2. Decomposing the Unconditional Premium: Probability, Price and Selection Effects

Recall from (1b) above:

$$\begin{aligned}\Delta Y &= E[Y \mid D=1] - E[Y \mid D=0]\\ &= \Pr[Z^*>0 \mid D=1] * E[Y \mid D=1, Z^*>0] - \Pr[Z^*>0 \mid D=0] * E[Y \mid D=0, Z^*>0]\end{aligned}$$

One can rewrite this equation, after some manipulation, as:

$$\begin{aligned}\Delta Y &= \Pr[Z^*>0 \mid D=1] * \beta && \textit{(premium)}\\ &+ E[Y \mid D=0, Z^*>0] * \Delta P && \textit{(probability)}\\ &+ \Pr[Z^*>0 \mid D=1] * \{ E[Y \mid D=1, Z^*>0] - E[Y \mid D=1, V > -X\mu_2] \} && \textit{(selection)}\end{aligned}$$

Each of the terms in the expression represents a different effect of a provision on shareholder value. The first term measures the direct impact on takeover premiums β (times the baseline probability of a merger for the treated group). This is the effect on the premium *conditional* on a takeover taking place, or in other words, how much more/less would an acquirer pay for a given firm. Note that if anti-takeover provisions give managers more bargaining power, removing them should lead to negative premiums. On the other hand, if removing them attracts more competition for the target or it induces better matching between bidder and target, it may lead to positive premiums. The second term captures the change in merger probabilities (times the premium for the untreated group). This reflects the change in the merger probability from the presence of an anti-takeover provision, and the strength of the provision as an anti-takeover device. The third is a selection term that captures the change in the population of firms subject to a takeover offer (reflecting the fact that anti-takeover provisions change the population of firms that become targets). For example, firms that end up being taken over may be the weakest (strongest) firms in the economy as determined by potential value creation, leading to negative (positive) selection.

The remainder of the paper explains how we obtain each of the terms, and estimates the contribution of each to the overall unconditional premium, as reported in Section VI.

III. Identification Strategies

To identify the impact of an additional anti-takeover measure on the two outcomes of interest, we can directly estimate the takeover probability Δ P and the unconditional takeover premium Δ Y (as defined in 1a and 1b above). We then move on to the estimation of conditional premiums and their determinants, which requires us to examine how the sample of firms that become a target is selected.

We define y_{ft} as the outcome of interest for firm f at time t, v_{ft} as the votes in favor of a shareholder-sponsored anti-takeover proposal, v_f* as the majority threshold for a proposal to pass in firm f and an indicator $D_{ft} = 1(v_{ft} \geq v_f*)$ that takes value of 1 when a proposal passes. K is a constant term. We can then express the relationship of interest as:

$$y_{ft} = K + D_{ft}\theta + u_{ft} \tag{3}$$

The effect of interest is captured by the coefficient θ, while the error term u_{ft} represents all other determinants of the outcome. However, using this expression in a regression is unlikely to give a consistent estimate $\hat{\theta}$ because passing a proposal that induces dropping an anti-takeover provision is correlated with omitted variables that are themselves correlated with the probability and characteristics of a takeover, so that $E(D_{ft}, u_{ft}) \neq 0$. The next section covers two possible approaches to deal with this problem.

III.1. Effect of ATPs on Unconditional Premiums and Merger Probabilities

In our setting we use two different and complementary approaches to calculate the effect of an additional anti-takeover measure on the total unconditional premium and on the probability of a merger i) a classic regression discontinuity design, or ii) a matching model that uses the RDD setting to validate the Conditional Independence Assumption (CIA)

III.1.1 Classic regression discontinuity design

Identification in the classic regression discontinuity design setting exploits the fact that the assignment into treatment is governed by the running variable (votes) and that treatment probability changes discretely around the majority threshold.[6] However, the distribution of other observable and unobservable firm characteristics is continuous around the threshold. In an arbitrarily small interval around the majority threshold, assignment to either side can be considered as random. Therefore, a discontinuous increase in the outcome variable around the passing threshold can be interpreted as caused by the treatment.

To estimate this discrete change in the outcome variable, one can use the whole data, fitting flexible functional forms for the relationship between the vote and the dependent variable in different ways. Lee and Lemieux (2010) suggest the use of different polynomials for observations on either side of the threshold.[7] Alternatively, one can run a local regression on an optimally calculated interval around the discontinuity, as proposed by Imbens and Kalyanaraman (2012) (IK)

[6] Evidence for the fact that implementation probabilities change discretely at the discontinuity can be found in Cuñat, Gine, Guadalupe (2012, 2016), Popadak (2014) and Bach and Metzger (2015).

[7] If votes are stochastic, the estimator can be interpreted as a weighted average treatment effect that uses all the observations, with weights directly proportional to the probability of each firm having a realized vote near the discontinuity (Lee and Lemieux, 2010).

for a local linear regression approach, and extended by Calonico, Cattaneo and Titiunik (2014) (CCT) to second-order weighted polynomial regression over an optimal bandwidth that balances efficiency and bias.[8]

III.1.2 A matching model with validated CIA in an RDD setting

The downside of the classic regression discontinuity design is that identification is local and comes from firms with vote outcomes around the discontinuity. In order to obtain arguably causal estimates for firms with vote outcomes away from the majority threshold, we use a matching estimator. Following the identification strategy in Angrist and Rokkanen (2015) we estimate a matching model in which we validate the conditional independence assumption using a test that relies only on the standard underlying assumption of regression discontinuity designs; namely, that all the heterogeneity in the treatment comes from the running variable.

Angrist and Rokkanen (2015) note that a unique feature of a regression discontinuity setting is that one observes the running variable (v_{ft} in our case) which is the only factor that determines "treatment" (D=1), or as they put it, "RD design takes the mystery out of treatment assignment." In classic matching model applications (without random variation in D), researchers match treated and control firms and assume that the set of controls they match on is sufficiently rich so that any difference between outcomes is driven only by treatment (D). However, unless

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[8]The weights are computed by applying a kernel function on the distance of each observation's score to the cutoff. θ is then estimated as the difference between these non-parametric regression functions on either side of the majority threshold.

treatment is randomly assigned, there is no way to know whether one has the right set of matching criteria or whether omitted variables are correlated with assignment and the outcomes.

In regression discontinuity, the running variable is the (only) assignment variable. For example, in our setting we know that $D_{ft} = 1[\, v_{ft} > v_{ft}^{*}]$. So, we know the nature of the possible omitted variables bias: anything that is correlated with v_{ft} that also determines outcomes. Therefore, one can identify the coefficients of interest under a conditional independence assumption (CIA):

$$E[y_{ft} \mid v_{ft}, x_{ft}] = E[y_{ft} \mid x_{ft}] \; ; D = 0, 1$$

The CIA states that potential outcomes are mean-independent of the running variable, conditional on a set of controls x_{ft} . If the CIA holds, then the model is identified.

In standard matching models, the CIA is implicit but never tested because the assignment process is not observed. But the RDD setting gives us the running variable, which allows us to test the CIA outside the discontinuity.[9] In practice, testing the CIA amounts to making sure that while there may be a significant relationship between the y_{ft} and v_{ft} , the two are mean-independent conditional on x_{ft}. This can be tested by showing that they are mean independent outside the discontinuity threshold.

In other words, the regression discontinuity design provides a diagnostic tool to test the validity of the model that is used in a matching estimator. In this sense, the proposed estimator is formally very different from an RDD, but uses the RDD setting design to validate the set of

[9] A further condition required in this identification strategy is the existence of common support, so that the treatment status (removing an anti-takeover proposal) retains meaningful variation after we condition on X.

variables that participate in the model. This approach stems from the assumption that the only source of heterogeneity in assignment is the vote and relies on an auxiliary regression to test it.

Ideally one wants the CIA to hold in the full support of the running variable.[10] However, this may not be feasible in some cases. In such circumstances, some researchers have proposed to limit the sample to some interval from the threshold where CIA holds (this was called Bounded CIA in the NBER working paper version of Angrist Rokkanen 2015 and is used for example in Hainmueller et al, 2015).[11]

Imposing this additional structure has several advantages. The strategy allows us to estimate a matching model using the full sample, and test the CIA that underlies identification. This means we can provide estimates for firms with vote outcomes away from the discontinuity, while retaining a causal interpretation.[12] Moreover, using our estimates we can build counterfactuals at each vote level that predict what would have happened had that firm voted

[10] In Angrist Rokkanen (2015) the full sample CIA is done in the +/-20 interval for each school, because this is the interval where the samples are "clean" in that the counterfactuals are clear: it avoids having a student in the "accepted" sample of O'Bryant (the less selective school) who would also be accepted into BLS (the more selective school) and that someone who is not accepted to BLS would have been below the O'Bryant threshold for acceptance.

[11] The tension between these two strategies, is that, on the one hand, it is preferable not to select the interval based on post-estimation results (this favors testing for the CIA on the full support only). On the other hand, testing the CIA in smaller subsamples because it is less likely to be satisfied as we move away from the discontinuity, provides with a gradual sense of to what extent the CIA is satisfied (this is the Bounded CIA strategy).

[12] Angrist and Rokkanen (2015) also show how to extend this to the fuzzy regression discontinuity design. Note that throughout the paper, since we do not have information on implementation, we present reduced-form estimates of the intent to treat of such approach. One can estimate the impact of implementation by rescaling the intent to treat estimator. The rescaling factor is one over the change of the probability of implementation when a proposal passes. This conversion factor when estimated for the whole vote support ranges between 1.2 (using estimates from Popadak, 2014, which finds that the difference in implementation between pass and fail is 84%) and 1.7 (using estimates from Bach and Metzger (2015) who find that average implementation conditional on passing is 59%). Note also that one does not need CIA on implementation to hold in order to interpret our estimates as causal ITT estimates: the CIA test already takes into account any heterogeneity in implementation on the outcome variables.

differently. This implies that we can assess whether there are heterogeneous effects of anti-takeover provisions for different levels of vote support. Overall, this approach allows us to test the validity of the matching in a theoretically sound way, subject only to the limitations of applying an asymptotic result to a finite sample. The limit of interpreting the results using this identification strategy as causal is the extent to which the CIA is satisfied. We discuss this further below. See Section 3 in Angrist and Rokkanen (2015) for a related discussion.

III.2. Estimating the Conditional Premium using Lee (2009) Bounds

The existing literature focuses on the effect of anti-takeover provisions on the takeover premium conditional on a merger happening. However, as noted in Section II, a remaining challenge is to disentangle which part of this effect is a causal effect, fixing the characteristics of the target firm (e.g. effects that arise from changes in bargaining power, matching with different bidders, changes in competition for target firms, etc.), and which part of the effect is due to selection (i.e. when anti-takeover provisions are dropped a different population of firms experience takeovers).

This is a form of selection that is inherent to the problem studied rather than a sampling issue. To correct it we could have an excluded variable in a Heckman selection model, but these are virtually impossible to find in this setting since any variable that predicts takeovers will also determine the premium. The alternative is to provide bounds for the parameters of interest.

Lee (2009) shows how to use the structure of the underlying model to recover upper and lower bounds for β: If one observes $E[Y \mid D=1, X, V> - X\mu_2]$ (which is the premium from the sample that would have merged even without the anti-takeover provision, but that actually removed it), then one could estimate β from $E[Y \mid D=1, X, V> - X\mu_2]- E[Y \mid D=0, X, V> - X\mu_2]$. However, this is never observed. But notice that the sample for which $V> - X\mu_2$ is included in $V> -\gamma -X\mu_2$. This gives us a strategy to provide an upper (lower) bound for β under a monotonicity assumption: If one considers that all counterfactual observations for which we do not see Y are drawn from the lower (upper) end of the Y distribution, we can obtain a lower (upper) bound for β by trimming a proportion p (1-p) from the observations for Y, where $p=Pr(-\gamma -X\mu_2<V<-X\mu_2)/Pr(-\gamma -X\mu_2<V)$. In what follows, we will call these "sharp Lee bounds" (Lee, 2009).

Note that in order to apply Lee (2009) bounds we need an empirical distribution of the conditional premium, so it is not possible to estimate bounds at the discontinuity without making additional assumptions. One possibility is to assume that the distribution of premiums on an interval around the discontinuity is a good approximation for the distribution at the discontinuity. One needs to achieve a compromise between a narrow interval (e.g. 10 percentage points around the discontinuity), that brings the results closer to a causal interpretation or a broader interval that would produce a more meaningful distribution and increase the power of the test. We use this approach when we decompose the unconditional premium into its components in an interval around the discontinuity. Alternatively, we also extend the results beyond the discontinuity using the CIA-validated matching so we can implement the bounding strategy on the population distribution of premiums using all available observations adequately weighted. Under the CIA assumption this produces results that can be interpreted as causal and that contain the whole distribution of premiums. Our study thus adds value by combining these various techniques, for different subsamples, each with its own maintained assumptions, to show similar results.

IV. Data Description and Sample Characteristics

We construct a dataset that spans 20 years of voting data from ISS-Riskmetrics (ISS-Shareholder Proposals database).[13] This provides information on all the proposals voted in the S&P1500 universe and an additional 500 widely held firms. We restrict the analysis to the set of anti-takeover provisions that make up the G-index as defined by Gompers et. al. (2003). Our main sample consists of 2,881 shareholder-sponsored proposals voted on at annual meetings to change the anti-takeover structure of the firm.

Based on this sample, we construct two different vote metrics. The first one is a *Simple Vote rule*. It computes Votes For/(Votes For + Votes Against) whenever the pass rule is defined over votes cast. The second one is a *Vote Adjusted for Abstentions* vote metric. It starts from the simple majority vote rule and computes Votes For/(Votes For + Votes Against + Abstentions) in those cases in which the firm or the state rules determine that the cast votes include abstentions. In both cases we use Votes For/(Shares Outstanding) whenever the pass rule is defined over votes outstanding. To gather information about how votes are computed, we fully merge the ISS-Shareholder Proposals database with the ISS-Voting Results database for the period 1997-2006, we also merge it with Voting Analytics for the 2007-2013 period and are able to match around half of our observations in that period. Unfortunately, within our universe, there is no reliable information about the treatment of abstentions for observations before 1997 and for the unmatched

[13] For the period 1997-2013 we use the ISS-Shareholder Proposals dataset formerly known as Riskmetrics, now part of ISS. For the period 1994-1996 we use data from ISS tapes. We would like to thank Ernst Maug and Kristian Rydqvist for providing us with this data (Maug and Rydqvist, 2009).

observations post 2006.[14] The advantage of the *Simple Vote rule* is that it is consistently defined across all observations and comparable with previous studies that use the ISS-Shareholder Proposals dataset. The simple majority rule is also very focal and used by ISS, investors and the SEC to justify their rules and voting recommendations. The advantage of the *Vote Adjusted for Abstentions* is that it is closer to what managers publicly report as the vote outcome and hence more directly determinant for implementation. The disadvantage is that the inclusion of abstentions and broker non-votes may make this measure easier to manipulate. Bach and Metzger (2019) argue that there is manipulation using the *Adjusted Vote* metric in the Voting Analytics dataset. As we show below we find no evidence of manipulating in our sample with either metric. Throughout the paper we use the *Simple Vote rule* as our main specification, but we report our main results for both rules and show they are very similar across measures.[15]

To obtain our treatment indicator (D), we use information on vote outcomes adjusted by majority rules (simple majority – supermajority) and votes base (votes cast or outstanding). If this information is not available, we use a simple majority rule of 50% of votes cast. We define the distance to the vote as the difference between the vote outcome and the majority threshold (v_{ft} - v_f*).

We match this sample of firms to the SDC platinum database to identify which firms were taken

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[14] This translates in not knowing the exact treatment of abstentions for 1/3 of the sample. On average, two thirds of the firms use the simple majority rule so we expect that for 1/9 of the *Adjusted Vote* observations we use the Simple rule even though we should have accounted for abstentions. The difference between the two vote criterions is small: on average abstentions represent 1.3% of the votes and out of 1851 observations where we know the exact voting rule using the different dataset, only 30 observations change from pass to no pass.
[15] We also report all the specification tests and post-estimation calculations for the *Vote Adjusted for Abstentions* in the appendix.

over following a vote. We consider whether a firm is taken over within five years of the vote if at least 50% of its ownership is acquired by a bidder. For firms with multiple votes we treat these as separate events, but cluster standard errors by firm in our estimates.[16] In most of our analysis we define the merger premium for firms that are taken over as the cumulative return from four weeks prior to the takeover announcement up to the completion date (as reported by SDC) which, as we will see later, gives the most conservative estimates of our effect when we compare it to a range of alternative measures for robustness (See Table 5). We also obtain information from SDC on the acquirer's premium (only available for listed acquirers), number of bidders, number of unsolicited bids, whether the deal was challenged, the percent that was paid in stock, and whether both firms belong to the same two-digit SIC industry. Financial information comes from Compustat and ownership information from Thomson 13F.

Table A1 in the External Appendix presents information on the evolution of the votes to remove an anti-takeover provision used in the paper, as well as the takeover probabilities and premium over time. The average probability of a firm experiencing a takeover over the five years following a shareholder vote is 14%. We have a total of 138 (81) targets within 10 (5) percentage points of the majority threshold. The mean conditional premium (the premium paid conditional on a successful merger) is 32.7% and the mean unconditional premium (that assigns zero premium to the unsuccessful mergers) is 4.83%.[17]

[16] In an earlier version we analyzed whether there were differential effects for firms that voted on an issue the first time, the second time, third time etc., and found no significant differences, so we decided to pool the effects. Results available upon request.

[17] Note that throughout the paper we will treat all anti-takeover provisions as if they were identical, although in reality they may not be. External Appendix Table A4 shows that the most frequent provisions, which play a major role in

Table 1 presents basic descriptive statistics of the firms in our sample. In order to assess how firms subject to a shareholder proposal differ from their sampling population, we present the characteristics of the average S&P1500 firms and compare them to firms in our sample. One of the most noticeable differences is that firms in our sample are three times larger than the average S&P1500 firm. In addition, firms in the voting sample have lower Tobin's Q, slightly higher levels of leverage ratio, and relatively less cash liquidity. However, they are not that different in terms of profitability, return on equity, cash flows, capital expenditures and overheads. This suggests that while we obtain results for all the firms subject to an anti-takeover removal proposal (roughly one third of the population of S&P 1500 firms) one should exercise caution in extrapolating the results to firms that have never had such a shareholder proposal. In other words, as with any identification strategy, we cannot extrapolate the results outside the sample without making further assumptions.

V. The Effect of Anti-takeover Provisions on Takeover Probability and Unconditional Premiums

V.1 Regression Discontinuity Design (RDD) Estimates of Unconditional Premiums and Takeover Probabilities

V.1.1 Preliminary tests to validate the RDD identification strategy

Before presenting results using the classic regression discontinuity design and the validated matching model, we need to run a series of tests to confirm that this is a good setting to use these methods. First, we show there are no pre-existing differences in firm characteristics (or trends in

identifying the effect are: repeal classified board (35%), adopt cumulative voting (16%) and repeal poison pill (14%). Given that there is not enough power at the provision-type level to identify the separate effects of each proposal type within our methodology, we do our analysis for all G-index proposals pooled –a conventional measure of ATPs.

firm characteristics) around the majority threshold, which is an assumption of the regression discontinuity design (see External Appendix Table A2).

Second, we test that the distribution of the frequency of votes is continuous around the discontinuity. A discrete and significant jump in density to either side of the discontinuity would be indicative of strategic behavior around the majority threshold such that the continuity assumption would be violated. This does not appear to be true in our data and we believe the main differences with respect to Bach and Metzger (2019) arise from the limited overlap between the two sample where only 16% of observations are common – due to different proposal coverage, years and sampling (See External Data Appendix for more detail on differences between the samples).[18] In fact, External Appendix Figure A1a shows a smooth overall distribution of votes for the simple vote. Figure A1b shows the formal continuity test proposed by McCrary (2008) that rejects the discontinuity of the density function at the majority threshold. External Appendix Figure A1c shows the discontinuity test for the adjusted vote, and find no statistical difference at the threshold (note that, visually, there is a small discrete change, but this does not resemble in any way the large discontinuity documented by Bach and Metzger (2019), Figure 1). We also tested for discontinuity in the votes for sub-periods and by proposal using both vote definitions, and found no evidence of manipulation in any subsample (see External Appendix Tables A5a and

[18] There are important sample differences with respect to Bach and Metzger (2019): we focus on G-index proposals for S&P 1500 firms, their main sample focuses on a broader set of proposals of the Russell 3000 index which only includes 16% of takeover-related proposals; they also focus on a different time period. When Bach and Metzger (2019) uses ISS data for 2003-2011, they focus on the 10 proposals with most favorable votes. Only 6 of them belong to the G-index. They also have incomplete sampling due to missing information. The fraction of observations in our sample present in their sample ranges from 9% to 25% depending on subsamples. Section III in the External Data Appendix describes the main differences between these two databases for our time period.

A5b). These tests confirm that this dataset is a good setting in which to apply the classic regression discontinuity design using either both vote definitions.

V.1.2 Results using the Regression Discontinuity Design

We now present the estimates of the effect of passing a proposal to remove an anti-takeover provision on the takeover probability and the expected premium using the regression discontinuity design.

We begin by presenting graphical evidence using all of our data. Figure 1a shows the relationship between the merger probability and the distance from the majority threshold (% votes above pass in the horizontal axis) for the *Simple Vote* rule. The dots represent simple means in bins of 2% vote intervals, and the solid line is a running linear regression using the Imbens and Kalyanaraman (2012) approach to select the bandwidth. Overall, the downward sloping line suggests that higher shareholder support for dropping anti-takeover proposals is associated with a lower likelihood of a takeover. On the basis of this evidence alone we would wrongly conclude from the correlation that the more likely firms are to drop the provision, the less likely they are to be taken over. However, this is driven by unobserved characteristics. In fact, at the majority threshold we see a discrete change upwards in the function, suggesting a positive causal effect of voting to drop the provision on the takeover probability. The size of this discrete change is the regression discontinuity estimate, i.e. the local causal effect of the vote outcome.

Figure 1b shows the same graph with the unconditional premium in the vertical axis. Again, we observe a negative overall relationship between the two variables but a clear positive change at the discontinuity, suggesting that voting to drop a provision increases the unconditional premium firms expect to receive.[19]

Table 2 presents regression estimates of the effect at the discontinuity seen in Figures 1a and 1b using four different estimating methods. Columns 1 to 4 of Table 2 show the non-parametric test, which is a means test of the outcome variable, calculated on an increasingly narrow interval of votes around the majority threshold. Columns 5 and 6 show the regression discontinuity estimate using polynomial controls of order two and three (respectively) to each side of the discontinuity. Columns 7 and 8 report the results of running local regressions on an optimal bandwidth around the discontinuity. Column 7 reports the Imbens and Kalyanaraman (2011) local regression analysis, column 8 reports the Calonico, Cattaneo and Titiunik (2014) estimate. Columns 9 to 12 replicate the specifications of Columns 5 to 8 using the Vote Adjusted for Abstentions.

Panel A shows the results for the probability of a takeover within 5 years of a shareholder vote. The results show no effect on average of passing a proposal when all observations are included (Column 1). The differential probability of experiencing a takeover within five years of the vote is 4.76% in the 10% interval and between 7.7% and 9.6% in the narrower intervals.[20] Using the specifications in Columns 5 to 8 this effect ranges from 8.7% to 11.7%. The results when using the Vote Adjusted for Abstentions in columns 9 to 12 are very similar with estimates ranging

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[19] The graphs are very similar when using the Vote Adjusted for Abstentions. See External Appendix Figures A3a and A3b

[20] A possible explanation for the difference in the size of the effects is that the estimation of θ in a broad interval is biased due to the endogenous adoption of proposals. For example, if firms with a lower ex-ante likelihood of receiving an offer are more likely to drop anti-takeover proposals, a sample-wide estimate like the one in Column 1 would be biased downwards.

from 7% to 9.2%. These are sizeable effects when compared with the sample-wide average five-year probability of a takeover of 13%

In Panel B of Table 2 we explore the effects of anti-takeover provisions on the unconditional expected premium received by shareholders in subsequent takeover transactions over five years. We focus on unconditional premiums (we assign zero premium to firms that do not undergo a merger within five years).

The results in Columns 1 to 4 of Table 2 Panel B show the fully non-parametric means comparison approach. The effect of dropping an anti-takeover provision is an increase in the expected premium of between 2.6% (in the 10% interval) and 4.3% (closer to the threshold). Columns 6 and 7, using the flexible polynomial approach, show expected premiums of about 5%. The local regression approach produces slightly smaller estimates of 4.1% and 3.6% (IK and CCT). The results with the Vote Adjusted for Abstentions in columns 9 to 12 are qualitatively and quantitatively similar, although the result in column 12 is not statistically distinguishable from zero. Again, these are substantial effects against an average unconditional premium of 4.8% in the sample as shown in Table A1-B.

The estimates are by construction local and since they are quite large it is sensible to wonder how much they can actually be extrapolated to the rest of the sample. It is possible that the very large estimates only apply to firms with close-call votes. To answer this question we turn to a validated matching model as a complementary estimation approach in the next section.

V.2. Estimating Unconditional Premiums and Takeover Probabilities using a Validated Matching Model

V.2.1. Testing the Conditional Independence Assumption and Preliminary Results

As described in Section II.2, the Regression Discontinuity setting is an ideal setting to test the Conditional Independence Assumption (CIA) that underlies any matching model. This is what we do in Table 3.

The goal of Table 3 is to test whether conditioning on an explicit model for the determinants of takeover allows us to eliminate the relationship between the running variable (the vote) and the outcome variables (takeover probability and unconditional premium) at each side of the discontinuity. In order to satisfy the CIA, we use a model in the remainder of the paper that includes as regressors natural variables capturing the takeover probability and premium. These are firm size and performance the year before the vote (in sales, market value, profit margin, cash liquidity), firm governance the year before the vote (percentage of equity controlled by institutional owners and E-index), measures relating to market performance the year before the vote (average Tobin's Q in the industry and average market value in the industry) and year dummies.

Columns 1 and 3 (5 and 7) of Table 3 show that there is a negative correlation between the vote and the takeover probability (unconditional premium) on either side of the threshold (D=0 and D=1) that is in most instances highly significant. The effect is also rather large in most cases. For example, the coefficient in column 1 implies that a 10% increase in the vote outcome reduces the takeover probability by 2.51 percentage points (out of a 13% mean takeover probability). This reflects the fact that the vote outcome and our dependent variables are highly correlated. However, once we condition on our model (in even numbered columns of Table 3), the point estimates drop by a full order of magnitude, getting closer to zero, and the correlation becomes statistically insignificant. For example, the 2.51 percentage point effect in Column 1 drops to 0.12 percentage points in Column 2 (and is highly insignificant, with standard errors remaining of a similar magnitude as in Column 1). This shows that the outcomes and the vote are mean-independent conditional on a number of variables which supports the assumption that vote and takeover probability are conditionally independent in the D=0 (votes that did not pass) region.[21] Column 4 shows that vote and takeover probability are conditionally independent also in the D=1 (votes passed) region. A similar pattern emerges for the unconditional premium in Columns 5 (large, significant effect) and 6 (much smaller, insignificant effect). In Columns 7 and 8, for D=1 there is no significant relationship between unconditional premium and vote to start (column 7), and the coefficient is still insignificant in column 8. Appendix Table A9 shows a similar Table for the Adjusted votes.

We complement the formal CIA testing with a graphical tool, shown in Figure 2, which plots the residuals of regressions that include the covariates in Table 3 excluding shareholder votes. If the CIA holds once we condition on our model, the remaining relationship between firm outcomes (takeover probability or premiums) and the vote outcome should be relatively flat. Figure 2 shows outcomes (takeover probability in Figure 2a and unconditional premium in Figure 2b) against the residuals obtained from regressing the outcomes on our model, on each side of the threshold. The figure plots the residual means in 2% bins and a local linear regression estimation of the outcome variables as a function of the vote. We see that the estimated relationship is

[21] Note that the R^2 in odd-numbered columns is low, meaning that there are many other things that explain whether a firm is taken over besides the vote outcome, but what is important for this test is that those "other things" are not omitted variables in our regressions that would determine assignment (i.e. correlated with outcomes and the vote).

statistically flat on both sides of the threshold for both variables (and within the confidence bands), indicating that the model does a good job of making the running variable uncorrelated with potential outcomes along the vote support. The same is true for the Adjusted vote as can be seen in Appendix Figures A4a and A4b.

Once we have made the running variable –which determines assignment to treatment— conditionally independent of outcomes, we move on to using matching methods to compare treated to control groups. We first test whether the calculated propensity scores for treatment and control groups pass the common support test. The logit model for the propensity score is calculated using the same model as before (used in the CIA tests). (See the common support test in External Appendix Figure A2; the formal (Dehejia and Wahba, 1999) balancing test also shows that covariates are balanced).

V.2.2. Results using the CIA-validated Matching Model

After testing for the CIA, and establishing that we have common support we can match firms on either side of the discontinuity based on our model. First, as in the Angrist and Rokkanen (2015) paper, we use the estimated propensity score (see External Appendix Table A6) to provide a propensity-score-weighted matching estimator of the effect of passing a shareholder-sponsored proposal to remove an anti-takeover provision. This amounts to weighting treated (D=1) observations by 1/p and control (D=0) observations by 1/(1-p) where p is the estimated propensity score using our model. We also add to the reweighted regression the variables included in the CIA model as control to reinforce the matching procedure. Results are shown in Table 4 panel A. For the *Simple Vote* rule we find that passing an anti-takeover provision leads to a 4.1% increase in the probability of takeover (Column 1) and a 2.6% increase in the unconditional premium (Column 2). The equivalent results for the *Vote Adjusted for Abstentions* are 3.6% and 2.3% respectively.

We also get very similar results if we use a different matching estimator, like the nearest neighbor matching estimator with replacement (Table 4 panel B) with a 3.4% additional takeover probability and a 2.5% increase in the unconditional premium. The results for the vote adjusted for abstentions are virtually unchanged with estimates of 3.25% and 2.4% respectively.

Note that, in general, while the adjustment for the treatment of abstentions is small, it can still have an impact the RDD estimates which are largely based on the observations near the majority threshold.[22] However, this adjustment has almost no effect on the estimators based on matching that rely on the full set of available observations .

Three results are noteworthy here. First, confirming what we saw earlier when comparing the results at the 10% interval relative to those at the discontinuity, the estimates away from the discontinuity are smaller than the discontinuity estimates, suggesting that firms around the discontinuity (with contentious votes) stand to benefit more from removing anti-takeover provisions than firms away from the discontinuity, on average.[23] Second, the results away from the discontinuity are still positive, significant and economically large. The mean (within five years) takeover probability in this sample is 13%, and voting to remove an anti-takeover provision

[22] Within our sample, abstentions account for an average of 4.8% of the votes (2.5% median) for those observations for which we have full information about them.

[23] Unfortunately, one cannot apply this estimation strategy to returns (CAR) on the day of the vote itself. This is because, while the CAR for firms at the discontinuity is the surprise outcome that reveals information (thus reflecting the full value of the vote, which the paper estimates), returns away from the discontinuity are likely expected by the market, and therefore contain no information on the vote (See Section 1B in Cuñat, Gine and Guadalupe, 2012).

increases that probability by 4.1 percentage points. Correspondingly, the mean unconditional takeover premium is 4.8%, and voting to remove an anti-takeover provision increases the premium by 2.6 percentage points. Third, these matching estimates can be interpreted as *causal* on a broad set of firms under stronger identification assumptions than the RDD ones. While our sample is not the full set of listed firms in the USA, it represents a substantial share of the S&P 1500 index (931 distinct firms). See Section II in the External Appendix for further discussion and tests of heterogeneous effects along the voting support.[24]

Before moving on to analyzing conditional premiums, it is worth noting that our estimates capture the effect of voting to remove the provision but since votes are not binding (whether to follow the shareholders' recommendation is left to managers' discretion), they are estimated on the basis of those firms that implemented the proposal because it passed and would not have implemented it otherwise. In other words, these are intent to treat (ITT) effects. To obtain the effect of treatment on the treated (the effect on outcomes that operates through the actual removal of the provision), they need to be re-scaled by the inverse of the change in the probability of removing the provision following a positive vote. Although we cannot estimate these conversion factors directly from our data, we can approximate them using the reported implementation differentials

[24] As additional robustness checks, we evaluate whether these effects are the result of voting on any proposal – rather than of voting to remove an anti-takeover provision. External Appendix Tables A7.a and A7.b replicate the analysis in Tables 2 and 3 using other (non anti-takeover) shareholder votes, and finds no effect of those proposals on either the takeover probability or the premium. The results are particularly different for Table A7.b which estimates the effects through the whole vote support, while they are quite heterogeneous and imprecise in Table A7.a near the discontinuity where we cannot always rule out that they are different from those in Table 2. Taken as a whole, they suggest that what drives our main results is not just some signal around shareholder activism (as proposed by Bach and Metzger, 2015), which should arise after any type of vote, but an effect that only appears after the removal of an anti-takeover provision.

from the pre-existing literature. This would imply multiplying the regression discontinuity results by a factor of 3 and the matching results by a factor between 1.2 to 1.7 to obtain a Wald estimate of the treatment on the treated.[25]

Finally, we find that these effects only emerge when voting to remove anti-takeover provisions; voting to drop other types of provisions has no effect on takeover probabilities or premiums, which suggests the results are not related to "voting" per se but are specifically related to the takeover channel (see External Appendix Table A7).

VI. Effect on the Conditional Premium

In Sections V.1 and V.2 we obtained causal estimates for the effect of treatment on the unconditional premium ΔY and the takeover probability ΔP. However, we also seek to recover the effect on the premium itself, β; that is, the expected premium that a given firm (i.e. accounting for selection) would get if it removed the anti-takeover provision. Given the potentially quite strong selection in the data (our estimated ΔP is rather large) it is not possible to infer the value of β from either ΔY, or from the difference in realized premiums.

The value of β can be bounded using the method in Lee (2009). The proposed bounds rely on an assumption of monotonicity of the effect of anti-takeover provisions on the selection criterion.

[25] The conversion factors take into account in which part of the distribution of votes they are estimated, to match the estimation sample in our paper: The conversion factor at the discontinuity is from Cuñat, Gine, Guadalupe (2012). For results outside the discontinuity we use estimates from Popadak (2014) (using data from Shark Repellent) and Bach and Metzger (2015) (using data from Voting Analytics). Note that the data used for the calculation of the conversion factors differs from ours in terms of the time period, type of provisions and vote measurement, so they have to be taken as approximations, given that the marginal firms that implement proposals may differ across them.

That is, the assumption that the causal effect of anti-takeover provisions on the probability of a takeover can be heterogeneous across firms, but it must go in the same direction: either always positive or always negative.[26] The bounds are calculated by trimming the distribution of premiums of the treated group. The trimming procedure can be seen as implementing the best and worst case scenario of selection, given the estimated change in the probability of a takeover.[27]

VI.1 Bounding β in the whole vote support

In Table 5 panel A, Column 1 estimates the bounds proposed by Lee (2009) calculated for the whole vote support. The procedure requires that the estimates of the coefficients that determine selection in the first stage can be interpreted as causal. We achieve this by using the same linear reweighting as in Table 4, i.e. we use the weights obtained using the propensity score and we assume that the same Conditional Independence Assumption holds. This method yields estimates of β for the 4-week premium that are bounded between a non-statistically significant -2.3% and a significant 5.8%. This means that the direct premium effect of dropping an anti-takeover provision on a given targeted firm is positive.

The remaining columns in panel A of Table 5 use additional premium measures in different windows for the purposes of robustness. Column 2 reports the effect on the target premium

[26] Note that this is a weaker assumption than the standard monotonicity assumption necessary for instrumental variable regressions.

[27] In our application, the calculation of the bounds involves first calculating the increase in the probability of a takeover induced by the treatment, relative to the probability of the treated firms $q=[(Pr(Z^*>0|D=1)) - Pr(Z^*>0|D=0)]/Pr(Z^*>0|D=1)$. Then, from the observed population of mergers in the treated group (the ones for which the anti-takeover provision proposal passes) we compute the upper (q) and lower (1-q) quantile of observed premiums. The upper (lower) bound of β is then calculated as the average of observed takeover premiums above (below) the lower (upper) quantile minus the average premium of the control group (firms that did not pass the anti-takeover provision proposal).

computed as the change in price one week before announcement until completion (i.e. a shorter run-up) and Column 3 shows abnormal returns using the FFM factors in a short window (+/- 5 days) around the announcement. These shorter premium windows both show significant positive bounds for the lower and the upper bound (between 5% and 11.5%). Columns 4, 5 and 6 focus on longer time windows. Column 4 shows cumulative abnormal returns in a very long window: from the day of the vote to the takeover announcement day plus one. This addresses the fact that some of the expected effect of the merger could have been incorporated as early as on the day of the vote. Columns 5 and 6 report the cumulative abnormal returns using FFM factors from (-42, 5) trading days around announcement and (-42, until completion) respectively. This allows us to assess the difference between using announcement and completion dates. All estimates unambiguously show a positive premium effect. What is noteworthy here is that while, by construction, a bounding strategy such as Lee (2009) or any other is likely to give broad bounds, the set of results we obtain allows us to reject that the effect on the conditional premium is negative.[28] The bound estimates solve the selection problem and rely on very weak assumptions at the cost of not determining the coefficient with precision. However, in our case the bounds rule out that the conditional premium is negative. We explore a number of hypotheses that may explain this non-negative effect in Section VII.

[28] All the results in Table 5A and 5B are based on the Simple vote rule, results are very similar using the Adjusted vote. See Appendix Tables A10.a and A10.b.

So far, the results of this section use the full distribution of votes as supported by the CIA assumption tests of section V.2.1. These are necessary inputs that allow us to calculate the decomposition of the unconditional premium in Section VII.

VI.1 Bounding β near the discontinuity threshold

An alternative approach is to perform the decomposition of the unconditional premium using results at the majority threshold. Unfortunately, the bounding technique relies on having an empirical distribution of votes and mergers when vote outcomes tend to the majority threshold, which does not exist in a finite sample. Alternatively, we can rely on a bandwidth around the discontinuity that is sufficiently narrow to approach the RDD intuition, but sufficiently broad to get meaningful results.

In panel B of Table 5 we report results on a 10% vote interval around the discontinuity. The results are generally consistent with those in panel A but sometimes less precise at the lower bound. There are two mechanical reasons for this loss of precision. First, as we approach the discontinuity, we have fewer observations and fewer mergers and hence lose power. Second, the effect on the probability of a merger is higher in the RDD results than for the matching estimator, resulting in more severe selection problems that take the bounds further apart. In general, it is not possible to keep a reasonable sample size and take the limit of the bounds to the discontinuity without imposing additional functional assumptions.[29] [30]

The results show that the upper bound is consistently positive and significant. The lower bound is, in general, statistically indistinguishable from zero, except for the most volatile premium measure (the one measuring returns from the vote until merger completion in Column 4), which yields very broad and uninformative bounds. [31]

VII. Decomposing the Unconditional Premium: Takeover Probability, Takeover Premium, and Selection Effects

We now have all the elements necessary to evaluate the contribution of price, probability and selection effects to the overall estimated unconditional takeover premium ΔY using the decomposition in equation (2).[32] We do the decomposition for the full sample (based on the CIA-validated matching model) and for the +/- 10% interval. Results can be seen in Table 6.

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[29] In Table 5B we obtain bounds for the conditional merger premium (4 week and 1 week premium) for different bandwidths around the discontinuity. These include the intervals used by the local regression estimators in Table 3 with their relevant weights. At very close intervals, the combination of a smaller sample and more severe selection problems generate very broad bounds. These are mostly distributed on the positive side, especially for the one week premium, which is less volatile, but a negative lower bound cannot be ruled out.

[30] See also External Appendix Table A8 for an RDD calculation of the unconditional premiums in Table 6.

[31] Throughout the paper we use raw returns after checking that our results are not driven by outliers. We replicated all our main results using winsorized variables, with similar results. Results available upon request, can also be found in earlier versions of this paper.

[32] We take the estimates for ΔP and ΔY from Column 1 of Table 2 (RDD) and Columns 1 and 3 of Table 4 (matching). We compute $\Pr[Z^*>0 \mid D=1] = 13.5$ using the probabilities of each observation being treated and $E[Y \mid D=0 , Z^*>0] =29.6$ using the probabilities of each observation non being treated, from the matching model. The bounds on β and the selection term come from Column 1 in Table 5.

For the whole sample we find that 52% of the premium is driven by the takeover probability effect (note the treatment effect on the takeover probability is estimated without selection bias, so this number does not change with the bounding exercise). Using our lower bound estimate for β (-2.3), we find that selection accounts for (61%) and that the premium contributes negatively (-13%). However, note that from Table 5, the lower bound premium is not statistically distinguishable from zero and that other windows for the premium yield unambiguously positive estimates. With our upper bound estimate for β (5.7), 32% of the unconditional premium is explained by the effect on premiums holding the population constant, and 17% by selection.

At the discontinuity (Panel B), the effect of takeover probabilities is also the largest (57%) and the premium effect ranges from 84% to -12% (although, this negative estimate uses a negative premium effect that is statistically indistinguishable from zero). The selection effect at the discontinuity cannot be signed, although it is potentially large, ranging between 55% and -41%.

This implies that while half of the value implications of dropping an anti-takeover provision can be attributed to an increased probability of experiencing a takeover, non-negligible amounts are driven by the positive premium. Selection effects are positive and large for the whole sample and imprecisely estimated for the RDD sample, although still potentially very large. This paints a very different picture from the existing literature (where there is no strategy to deal with selection and endogeneity) and confirms that failing to account for the endogenous selection of targets induces substantial bias.

VII. Understanding Positive Target Premiums

We next explore the possible drivers for the positive effect on the conditional target premium by looking at what else changes when firms pass a proposal. Given that the population of merged companies changes with the removal of an anti-takeover provision, we analyze these effects using Lee bounds (full descriptive statistics of all the variables in this section can be found in External Appendix Table A1). We start by analyzing the effects for the *Simple Vote* in whole matched sample in Table 7A and then focus on the effects closes to the discontinuity (in the +/- 10% interval) in Table 7B. Note that these are not only different estimation strategies but also that the effects are evaluated at different points of the distribution of votes (weighted average of the full sample vs. close-call votes) which can yield different point estimates.[33] Tables A12A and A12B in the Appendix replicate the results using the *Adjusted* vote. Since the results are extremely similar for both vote definitions, we discuss only the *Simple* vote here and refer the reader to the Appendix for the exact point estimates using the *Adjusted* vote.

We first evaluate the total value/synergies created by the deals in less protected firms versus more protected firms. We find that voting to remove an ATP leads to more value-creating deals using a number of different measures, both for the full sample (panel A of Table 7A) and for close-call votes in the in the +/- 10% interval (panel A of Table 7B). For example, for the full sample (for close-call votes) bidder and target firms are between 16% and 28% (between 14% and 32%) more likely to belong to the same two-digit SIC industry (relative to a mean of 63% in the sample). This indicates that the deals are more likely to be related mergers with higher synergies

[33] See Section II in the External Appendix for a discussion on the heterogeneous effects of ATPs at different levels of vote support and an analysis of the difference between the two samples.

than financial mergers or diversifying mergers. Column 2 shows that in less protected deals, targets are matched with relatively larger acquirers, as measured by their market capitalization. The ratio of target to acquirer market capitalization four weeks before the announcement is reduced by between 0.7 to 1.3 in the full sample (relative to a mean of 1.11 and standard deviation of 4.35; the interval is not significant for close-call votes). This suggests that the positively selected targets on average are matched to relatively more valuable and potentially productive acquirers, but this effect is not present closer to the discontinuity. If we add up the dollar value of the premium of the target and the acquirer, which is a measure of the total value/synergies created by the deal economy-wide, the upper bound estimate of the effect is quite large and positive in dollar value (up to US$7.3 billion higher for the full sample and US$9.2 billion for close-call votes, Column 3) and as a share of the total market cap (up to 14% of the target and acquirer's value for the full sample and 22% for close-call votes, Column 4). The lower bound of the synergy estimates is not significant in either sample, so one has to take this into account, but the results suggest that, if anything, there is net additional value creation in the market when anti-takeover provisions are removed, and this is true both for close-call votes as well as the full sample. This is important since it suggests that the presence of anti-takeover provisions hinders the realization of deals that have more value-creating potential, and hence potentially represent a net loss to the economy.

So we find that less protected firms receive a higher premium and those deals create more market value. But what happens to the acquirer premium? Here we find differences between the two samples, which are shown in Columns 5 to 8 in Tables 7A and 7B: in the full sample we cannot clearly sign the acquirer premium. It does not seem that acquirers are systematically able to extract a higher share of the synergies created given that the results depend heavily on the measure of premium used. In contrast, for close-call votes (Table 7B) we do find a systematic pattern: the upper bound of the effect on the acquirer premium is always significant, large and positive, and the lower bound is insignificant but also positive in several cases. We interpret this as reflecting that acquirers can extract/appropriate some of the value created in close-call votes, but less clearly so for the full sample of deals.

The share of the surplus extracted by acquirers can change because of changes in relative bargaining power when targets are less protected, or with changes in the extent of competition for targets. We cannot measure bargaining power directly, but we can measure competition through several proxies. We do this in panel B Columns 1 to 4 of Tables 7A and 7B and find that for the full sample competition increases, whereas it does not – and actually seems to decrease – for the close-call votes sample, which would explain the difference in results on the acquirer premium. In the full sample, less protected deals receive between 0.14 and 0.32 more bidders, (the mean number of bidders is 1.24 and over 90% of firms have only one or two). In addition, the probability of receiving a challenged deal is between 10% and 22% higher (sample mean is 15%). These are statistically and economically significant effects. They also seem to be the target of more unsolicited deals (with 4% to 12% higher probability, although the lower-bound result is not significant) and cases where a higher fraction of the deal is settled in cash (indicating more competition as in Offenberg and Pirinsky, 2015).[34] For close-call votes, the lower bounds are

[34] The use of cash in takeovers has also been linked to overvalued targets (see Shleifer and Vishny, 2003 or Malmendier et. al., 2016). Given that we are isolating the effect of anti-takeover provisions on a given target, our results would suggest that dropping an ATP may cause the overvaluation of the target. However, over- and

negative and significant but the upper bound is of the opposite sign and equally large, which may have to do with the lack of power to estimate effects in this subsample. However, the overall picture is not of an increase in competition but if anything, a decline.

We interpret this evidence as suggesting that voting to remove a provision makes entering the bidding contest less attractive in close-call votes, that the competition is less intense, and that the acquirer can appropriate some of the surplus created. In contrast, for non close-call votes competition between bidders erodes any premium for the acquirer.

Finally, we explore the potential role of activist investors in delivering higher premiums.[35] Greenwood and Schor (2009) provide evidence of the ability of activists to put firms into play, and therefore to collect high takeover premiums. We find that, in the full sample, among firms that are taken over, the likelihood of a 13D activist campaign prior to a merger announcement is higher for firms that passed a proposal to get rid of an anti-takeover provision.[36] Activist investors may be an additional channel through which higher premiums are observed for the full sample, but not for the close-call votes sample.

VIII. Conclusion

undervaluation of targets is more likely to operate through the selection of targets, and its effects are captured in the selection part of our decomposition (see Section VI.2).

[35] An activist investor who acquires more than 5% beneficial ownership is required to disclose in the Schedule 13D within 10 days of crossing 5% if it intends to influence control.

[36] We found that the unconditional probability of being the target of activists after passing a vote is unchanged (unreported).

In spite of the attention devoted to the consequences of anti-takeover provisions there is limited causal evidence of their impact on takeovers in terms of both probability and premiums, and the implications for value creation/destruction for the economy as a whole. To investigate the effect of anti-takeover provisions, one must first jointly assess their impact on takeover probabilities, merger premiums, and the selection of targets.

This paper provides estimates – dealing with the endogenous adoption of provisions and selection of targets – of the effects of anti-takeover provisions and identifies several channels through which they destroy value. First, having an anti-takeover provision in place reduces the likelihood of a takeover happening. Second, the deals that take place when a firm is protected by ATP are "worse" on several dimensions: they involve worse targets, smaller acquirers, are more likely to be between firms in unrelated businesses (and hence are less likely to create value) and create fewer synergies.

The more protected the firm, the lower the premium paid for the target; we find evidence that for the full sample this is at least partly driven by the fact that more protected firms attract less competitive bidding. This is likely because ATPs directly deter bidders, but also because the worse the firm's governance, the more difficult it will be to realize synergies. This mechanism does not apply, however, to close-call votes, where we see no increase in competitive bidding when less protected, and where bidders can also realize a positive premium.

Therefore, we find no apparent trade-off between takeover price (the premium) and takeover probability – a trade-off typically presented by managers as the rationale for the adoption of anti-takeover provisions. In our results, both price and probability are significantly lower when an anti-takeover provision is in place. In fact, the gains from dropping an anti-takeover provision accrue almost exclusively to the target shareholders.

We find very similar overall effects when using different identification strategies, with different vote definitions and in different subsamples, each with its own strengths and weaknesses. We obtain causal effects at the vote discontinuity for takeover probabilities and unconditional premiums. We also are able to bound the effects on the conditional premium for an interval around the discontinuity as well as away from the discontinuity using CIA validated matching. When using the latter, we find that our results apply to most of the support of vote outcomes. So while our results cannot necessarily be extrapolated to firms that never hold such shareholder votes, the evidence suggests that they do apply to the majority of firms in this population, which is about one third of the S&P 1500.

Finally, the existing literature fails to account for selection when computing the takeover premium. We show that this selection effect can be quite large and provide a framework to assess how much of the overall expected premium of removing an anti-takeover provision is driven by probability, price (premium) and selection effects.

While we present new results and answer a number of previously unanswered questions, our analysis leaves a number of open questions. For example, we take all anti-takeover provisions as identical and do not consider heterogeneity of effects for different types of proposal or different kinds of firms. Furthermore, if these deals are good for the shareholders of target firms and for the economy as a whole, why do so many firms keep anti-takeover provisions in place? Given that the firms in our sample tend to be large listed companies, our results would be consistent with the view that ATPs have a positive role in young entrepreneurial firms, but become value-destroying for more mature firms (see Johnson et al., 2015, 2017). There is ample evidence of the inefficiencies in internal governance and the political economy of decision-making within firms that make such provisions sticky, and mature firms may find themselves off equilibrium with an above-optimal level of anti-takeover protection. These are important avenues to explore and are left to future research.

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Figures

Figure 1a: Merger Probability

Linear regression using the Imbens and Kalyanaraman (2012) approach to select the bandwidth. Dots represent the simple means by bins of 2% vote intervals.

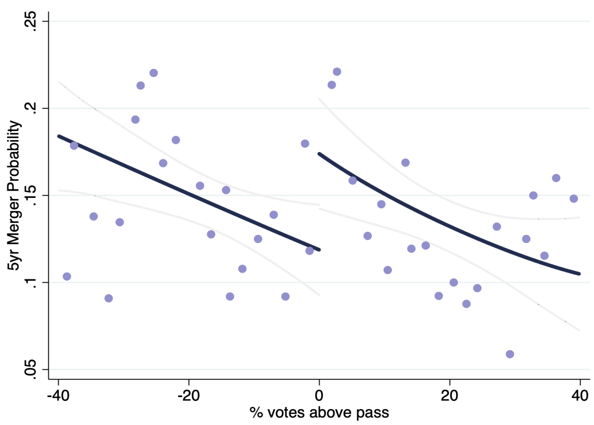


Figure 1b: Unconditional Premium

Linear regression using the Imbens and Kalyanaraman (2012) approach to select the bandwidth. Dots represent the simple means by bins of 2% vote intervals.

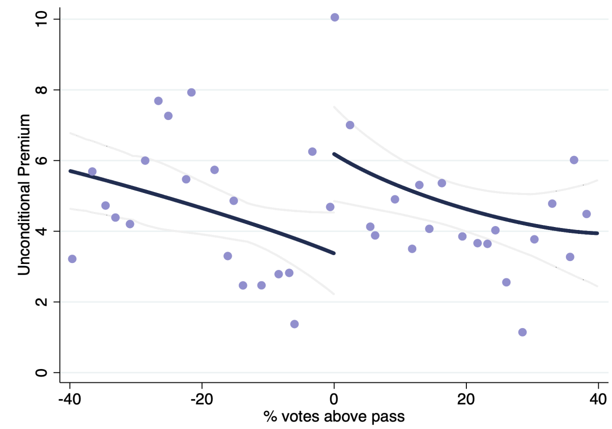


Figure 2a: Conditional Independence Test Merger Probability

Residuals of two independent linear models (one to each side of the discontinuity) using the same covariates as in the matching model. Dots represent the simple means by bins of 2% vote intervals.

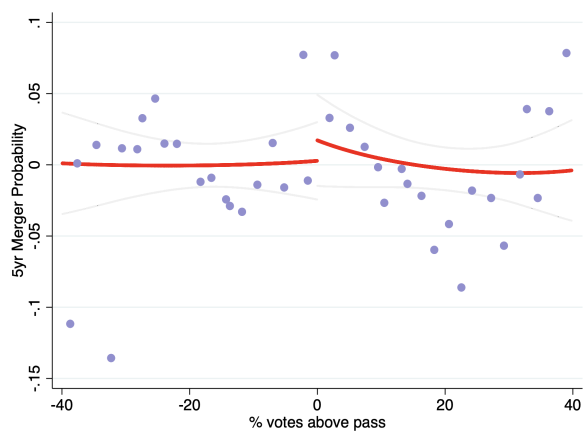
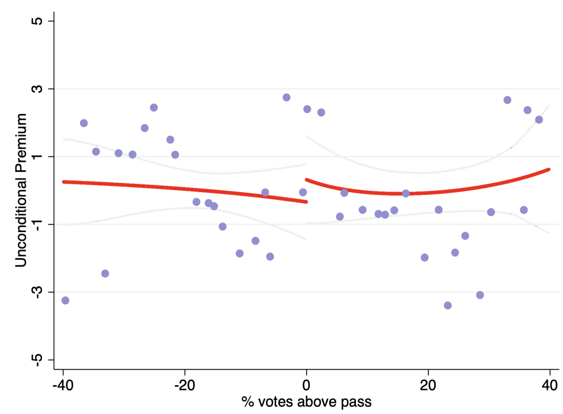


Figure 2b: Conditional Independence Test Premiums

Residuals of two independent linear models (one to each side of the discontinuity) using the same covariates as in the matching model. Dots represent the simple means by bins of 2% vote intervals.



Tables

Table 1
Descriptive Statistics

This table describes the sample of 2,882 voted G-index proposals one period before the vote. All accounting variables are obtained from Compustat: Market Value (mkvalt_f), Tobin's Q defined as the market value of assets (AT+mkvalt_f-CEQ) divided by the book value of assets (AT), and balance sheet Deferred Taxes and Investment Tax Credit (TXDITC), Return on Equity (NI/(CEQ+TXDITC)), Return on Assets (NI/AT), Profit Margin (EBITDA/Sale), Liquidity (CHE/Sales), Leverage ((DLTT+DLC)/ AT), Capital Expenditures (Capx/AT), Overheads (XSGA/XOPR). Ownership variables are generated from Thomson 13F database. All monetary values are in 2012 US$. Note that the number of observations may change due to missing values in some of the variables.

	N	Mean	Median	Std. dev.	10th Per.	90th Per.	Mean SP1500	t-test
Market Value ($mil)	2860	28,928	9,476	58,068	595	73,761	9,561	15.4
Tobin Q	2755	1.58	1.25	0.98	0.95	2.59	1.96	-14.8
Return on Equity	2863	0.153	0.107	0.220	-0.070	0.292	-0.04	1.25
Return on Assets	2861	0.031	0.031	0.087	-0.023	0.109	0.12	-7.4
Profit Margin (EBITDA/Sales)	2805	0.159	0.169	0.167	0.055	0.385	0.13	0.3
Cash Liquidity (CHE/Sales)	2861	0.091	0.052	0.109	0.006	0.221	0.13	-17.8
Leverage (DLTT+DLC)/AT	2859	0.288	0.277	0.165	0.078	0.505	0.223	18.5
Capital Expenditures (Capx/ AT)	2744	0.053	0.043	0.048	0.003	0.109	0.052	0.12
Overheads (SGA/Op.Exp.)	2175	0.282	0.251	0.184	0.077	0.521	0.314	-5.79
Ownership Inst. Shareholders	2696	0.638	0.655	0.193	0.378	0.864	0.680	-4.85
Ownership Herfindahl	2698	0.054	0.041	0.054	0.022	0.089	0.063	-7.61
Vote Simple rule	2882	48.14	47.03	21.83	20	78.1	n.a.	n.a.
Vote Adjusted for Abstentions	2882	47.80	46.7	21.56	20	78.8	n.a.	n.a.

Table 2
Takeover Probability and Premiums around the Majority Threshold

This table presents the effect of passing an anti-takeover proposal on the probability of becoming a target and on premiums. Panel A displays the probability of becoming a target which is estimated over the next 5 years after the vote using SDC data. Panel B displays the unconditional premium of becoming a potential target. Premiums are computed as the price offer to target 4 weeks prior to announcement until completion. Column 1 estimates are based on the whole sample. Column 2 restricts the sample to observations with a vote share within ten points of the threshold, column 3 to five points and so forth. Column 5 and 6 introduces a polynomial in the vote share of order 2 and 3 (Lee and Lemieux, 2010), one on each side of the threshold, and uses the full sample. Column 7 uses the local linear regression approach by Imbens Kalyanaraman (2012) with a triangular kernel function. Column 8 uses the non-parametric approach proposed by Calonico, Cattaneo and Titiunik (2014). Columns 9 to 12 are estimated using an alternative vote measure that adjusts for abstentions. All columns control for year fixed effects; standard errors are clustered by firm. The reported bandwidth is expressed in percent vote. Significance at the 10%, 5%, and 1% levels are indicated by *, **, and *** respectively.

Panel A: Probability of becoming a takeover target over the next 5 years

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Simple Vote Rule								Vote Adjusted for abstentions			
	Full	+/-10	+/-5	+/-2.5	Poly. 2	Poly. 3	IK	CCT	Poly. 2	Poly. 3	IK	CCT
yes Simple	-1.24%	4.76%	7.71%**	9.60%*	8.80%**	11.7%**	9.18%***	8.69%**	9.2%**	9.01%*	8.08%**	7.03%*
	(2.11)	(3.26)	(3.81)	(5.34)	(4.25)	(5.11)	(3.11)	(4.25)	(4.34)	(5.18)	(3.39)	(4.72)
bandwidth							27.7	13.2			23.3	13.5
Obs	2,881	883	457	249	2,881	2,881	2,882	2,882	2,881	2,881	2882	2882
R-sq/Z	0.000	0.004	0.010	0.015	0.006	0.007	2.95	2.05	0.005	0.006	2.38	1.65

Panel B: Unconditional Premium 4 Weeks

	Simple Vote Rule								Vote Adjusted for abstentions			
	Full	+/-10	+/-5	+/-2.5	Poly. 2	Poly. 3	IK	CCT	Poly. 2	Poly. 3	IK	CCT
yes Simple	0.194	2.616**	3.012*	4.321	5.331***	5.249**	4.12***	3.59*	5.016***	3.487	3.27**	2.66
	(0.794)	(1.212)	(1.707)	(2.662)	(1.711)	(2.251)	(1.52)	2.02)	(1.746)	(2.294)	(1.60)	(2.09)
bandwidth							24.6	14.3			22.6	13.8
Obs	2,881	883	457	249	2,881	2,881	2,882	2,882	2,881	2,881	2,882	2,882
R-sq/Z	0.000	0.007	0.008	0.013	0.035	0.036	2.69	1.76	0.035	0.036	2.04	1.27

Table 3

Conditional Independence Tests

This table reports the tests of the conditional independence assumption for our two outcome variables: Takeover Probability and Unconditional Premium. Columns 1,3,5 and 7 present the initial relationship between the running variable i.e the vote and the two outcome variables for observations to the left or right of the cutoff. Columns 2,4,6,8 display the model that controls for firm characteristics one year prior to the vote including Sales, Profit Margin, Market Value, Cash Liquidity, Percentage of Institutional Ownership, Average Industry Tobins'Q, Average Industry Market value and the Entrenchment Index. Significance at the 10%, 5%, and 1% levels are indicated by *, **, and *** respectively.

	Simple Vote Rule							
	Takeover Probability				**Unconditional Premium**			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	D=0		D=1		D=0		D=1	
	[-50,0)		[0,50]		[-50,0)		[0,50]	
Vote	-0.00251***	-0.000127	-0.00160*	-0.000470	-0.0825***	-0.0417	-0.0249	-0.0400
	(0.000848)	(0.000878)	(0.000838)	(0.000951)	(0.0313)	(0.0300)	(0.0348)	(0.0439)
ln Sales		-0.0135		-0.0261*		-1.446***		-0.654
		(0.0146)		(0.0147)		(0.500)		(0.677)
Profit Margin		0.235***		0.00846		-0.536		0.210
		(0.0748)		(0.0807)		(2.560)		(3.723)
Ln Market Value		-0.0173		-0.0149		0.190		-1.102*
		(0.0137)		(0.0138)		(0.468)		(0.638)
Cash Liquidity		0.110		0.192*		0.721		7.674*
		(0.104)		(0.0984)		(3.543)		(4.540)
Inst. Own.		0.0241		-0.183**		4.103*		-11.89***
		(0.0634)		(0.0796)		(2.168)		(3.674)
Av. Ind. Tobins'Q		0.00581		0.0163		0.274		1.116**
		(0.0109)		(0.0105)		(0.373)		(0.486)
Av.Ind.Mkt.Value		0.0699***		0.0249*		1.337***		0.321
		(0.0113)		(0.0128)		(0.387)		(0.590)
Entrench. Index		0.0156**		0.0135		0.446		1.187**
		(0.00793)		(0.0100)		(0.271)		(0.463)
Year Dummies		Y		Y		Y		Y
Obs	1,284	1,284	1,095	1,095	1,284	1,283	1,095	1,095
R-sq	0.007	0.139	0.003	0.089	0.005	0.094	0.000	0.085

Table 4

Matching Estimates of the Unconditional Premium in the Full Sample

This table reports CIA estimates of the effect of passing a G proposal on the takeover probability and the unconditional premium (4weeks before Announcement to Completion). Panel A reports the results from a linear reweighting estimator and Panel B reports results from a nearest neighbor matching procedure with replacement and two matches per observation. Controls are the same as in Table 3: ln Sales, Profit Margin, Market Value, Cash Liquidity, Percentage of Institutional Ownership, Average Industry Tobins'Q, Average Industry Market Value and the Entrenchment Index. Significance at the 10%, 5%, and 1% levels are indicated by *, **, and *** respectively.

Panel A: Propensity Score Weighting

	(1)	(2)	(3)	(4)
	Simple Vote Rule		Vote Adjusted for Abstentions	
	Takeover Probability	Unconditional Premium	Takeover Probability	Unconditional Premium
yes	4.1%**	2.59***	3.62%*	2.34**
	(1.97)	(0.97)	(1.9)	(1.00)
t stat	2.05	2.66	1.83	2.33
Model	Y	Y	Y	Y
Obs	2,123	2,123	2,129	2,129

Panel B: Nearest Neighbor Matching

	(1)	(2)	(3)	(4)
	Simple Vote Rule		Vote Adjusted for Abstentions	
	Takeover Probability	Unconditional Premium	Takeover Probability	Unconditional Premium
yes	3.36%*	2.53***	3.25%*	2.39***
	(1.92)	(0.81)	(1.92)	(0.80)
t stat	1.74	3.10	1.69	2.98
Obs	2,379	2,379	2,379	2,379

Table 5

Target Conditional Premiums, Lee Bounds Estimates

This table reports the effect of passing a G proposal on different premium measures for the target company. Panel A estimates are obtained using Lee (2009) methodology and propensity score matching to account for selection in the universe of targeted companies. Panel B estimates restrict the sample to votes within the (-10,10) interval. Column 1 reports the effect on the Target Premium computed as the change in price 4 weeks before announcement until completion. Column 2 reports the effect on the Target Premium computed as the change in price 1 week before announcement until completion. Columns 3 and 4 report premiums based on the cumulative abnormal returns using the FFM factors for different windows (-/+ 5 days,) and (Vote/+1days) both relative to the announcement date. The runups for the target company are computed as the abnormal returns from (-42,5) trading days around announcement in column 5 and in column 6 as the abnormal return (-42, until Completion), always using the FFM factors. Significance at the 10%, 5%, and 1% levels are indicated by *, **, and *** respectively

Simple Vote Rule

Panel A: Upper and Lower Sharp Lee Bounds (with prop. score weights, full sample)

	(1)	(2)	(3)	(4)	(5)	(6)
	Premium 4 weeks before Announce. to Completion	Premium 1 week before Announce. to Completion	CAR(-5,5) FFM	CAR (Vote,Ann+1)	Runup (-42,5) FFM	Runup (-42, Completion) FFM
Lower Bound Estimation						
yes	-2.3	4.95*	5.27**	2.6	2.72	3.29
	(3.23)	(2.9)	(2.11)	19.3	(2.93)	(3.39)
Z	-0.71	1.71	2.49	0.13	0.93	0.84
Upper Bound Estimation						
yes	5.75**	11.49***	10.12***	52.24**	9.17***	16.52***
	(2.89)	(2.48)	(1.92)	(16.16)	(2.61)	(3.93)
R-sq/Z	1.99	4.62	5.25	3.23	3.51	4.80
# sel.obs	418	416	418	406	418	408
Obs	2,379	2,379	2,379	2,379	2,379	2,379

Panel B: Upper and Lower Sharp Lee Bounds in (-10, 10) interval

	(1)	(2)	(3)	(4)	(5)	(6)
Lower Bound Estimation						
yes	-2.02	-1.18	-1.79	-52.24**	2.53	-0.27
	(4.57)	(4.56)	(3.23)	(23.1)	(5.27)	(7.45)
Z	-0.44	-0.26	-0.55	-2.25	0.48	-0.04
Upper Bound Estimation						
yes	14.63**	22.93***	11.06***	47.73*	20.96***	27.39***
	(5.99)	(6.80)	(4.11)	(26.96)	(5.71)	(8.279)
R-sq/Z	2.44	3.37	2.69	1.77	3.67	3.30
# sel.obs	135	134	133	124	133	130
Obs	883	883	883	883	883	883

Table 6

Decomposing the Shareholder Value Effect

This table provides a decomposition of the Change in Shareholder Value induced by the passing of a proposal to eliminate an anti-takeover provision. We provide an estimate of the three different components that affect shareholder value via changes in the premium, changes in the probability of a takeover and changes in the population of firms that are put into play. We provide both the lower and upper bound values since we use Lee (2009) to estimate the change in Takeover Premium β. Column 1 estimates the Change in Shareholder's Value as the unconditional takeover premium under the CIA model for panel A, and using the RDD IK estimate for panel B. Column 2 "Premium Effect" is the result of the change in Takeover Premium β times the Baseline Probability of Merger (Pr[Z*>0 | D=1]). Column 3 "Takeover Probability Effect" is the result of the change in the Probability of Merger ({Pr[Z*>0 | D=1] - Pr[Z*>0 | D=0]} times the Baseline Premium (E[Y | D=0 , Z*>0]). Column 4 provides an estimate of the Selection Effect. Using the probabilities of the matching model we calculate that the Baseline Probability is 14.4 and the Baseline Premium is 32.7

Simple Vote Rule			
(1)	(2)	(3)	(4)
Change in Shareholder Value	**Premium Effect**	**Takeover Probability Effect**	**Selection Effect**
Δ Y	β * Pr[Z*>0 | D=1]	{Pr[Z*>0 | D=1] - Pr[Z*>0 | D=0]} * E[Y | D=0 , Z*>0]	Pr[Z*>0 | D=1] * { E[Y | D=1 , Z*>0]- E[Y | D=1 , V > - μ2]}
Panel A: Using Validated Matching for Uncond. Premium & Probability from Table 4			
Lower Bound Estimation of β = -2.3			
2.59%	-0.33	1.34	1.59
	-13%	***52%***	***61%***
Upper Bound Estimation of β = 5.7			
2.59%	0.82	1.34	0.43
	32%	***52%***	***17%***
Panel B: Using RDD-IK for Unconditional Premium and Probability from Table 2 (-10,10)			
Lower Bound Estimation of β = -2.02			
2.61%	-0.30	1.48	1.42
	-12%	***57%***	***55%***
Upper Bound Estimation of β = 14.6			
2.61%	2.19	1.48	-1.06
	84%	***57%***	***-41%***

Table 7 A

Merger Effects -- Lee bounds with prop. score weights (full sample)

This table reports the effect of passing a G proposal on different merger outcomes. All estimates are obtained using Lee (2009) methodology to account for selection in the universe of targeted companies. Panel A presents results for different measures of Matching and Acquirer Premium. Column 1 reports the effect on the likelihood of Target and Acquirer being in the same 2-digit SIC code, column 2 on the relative size of Target versus Acquirer, column 3 reports a measure of Total Synergies and column 4 Total Synergies as a percentage of total market capitalization. Column 5 reports the effect on the Acquirer Premium (change in price 4 weeks before announcement until one day after). Column 6 reports a premium based on the abnormal returns (FFM) on a (-5/+5) window around announcement. The run-up of the acquirer is measured as the abnormal returns on a (-42/+5) window around announcement in column 7 and as (-42/Completion) in column 8. Panel B Columns 1, 2 and 3 report the effect on the number of bidders, the deal being unsolicited and the deal being challenged. Column 4 reports the effect on the percentage of stock paid for the target. Columns 5 and 6 present the effects on Activism events.

Simple Vote Rule

Panel A

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Matching				**Acquirer Premium**			
	Same 2Digit SIC	Size Target Rel. to Acquiror	Total Synergies FFM	Total Synergy/ Total Mkt Cap	Acquirer Premium	Acquirer CAR(-5,5)	Runup CAR(-42,5)	Runup CAR (-42,Comp)
Lower Bound Estimation								
yes	0.161**	-1.31**	-321,515	-0.03	-8.81***	-4.46***	-7.14**	-3.12
	(0.06)	(0.51)	(2,446,489)	(0.03)	(3.18)	(1.37)	(2.90)	(4.45)
Z	2.30	-2.57	-0.13	-1.01	-2.77	-3.25	-2.46	-0.70
Upper Bound Estimation								
yes	0.28***	-0.73	7,317,733***	0.14***	-0.25	1.06	5.78**	17.6***
	(0.07)	(0.48)	(1,854,619)	(0.03)	(2.35)	(1.38)	(3.27)	(3.74)
Z	3.95	-1.53	3.95	4.63	0.11	0.77	1.76	4.7
#sel	418	356	262	262	281	278	278	271
Obs	2,379	2,379	2,379	2,379	2,379	2,379	2,379	2,379

Panel B

	(1)	(2)	(3)	(4)	(5)	(6)
	Competition				**Activism**	
	Number of Bidders	Unsolicited Deal	Challenged Deal	Stock Percent	Num 13D Events two years prior	Dummy 13D Event two years prior Annoucement
Lower Bound Estimation						
yes	0.14**	0.039	0.10***	-26.83***	0.051	0.072
	(0.06)	(0.03)	(0.03)	(7.22)	(0.077)	(0.044)
Z	2.20	1.27	2.66	-3.71	0.66	1.62
Upper Bound Estimation						
yes	0.315***	0.12***	0.22***	-3.46	0.445***	0.289***
	(0.04)	(0.02)	(0.03)	(5.39)	(0.083)	(0.074)
Z	6.40	5.01	7.26	-0.64	5.33	3.87
#sel	418	418	418	217	639	639
Obs	2,379	2,379	2,379	2,379	2,379	2,379

Table 7 B

Merger Effects --Lee bounds in (-10,+10) interval

This table reports the effect of passing a G proposal on different merger outcomes. All estimates are obtained using Lee (2009) bounds on the restricted (-10,10) vote interval. Panel A presents results for different measures of Matching and Acquirer Premium. Column 1 reports the effect on the likelihood of Target and Acquirer being in the same 2-digit SIC code, column 2 on the relative size of Target versus Acquirer, column 3 reports a measure of Total Synergies and column 4 Total Synergies as a percentage of total market capitalization. Column 5 reports the effect on the Acquirer Premium (change in price 4 weeks before announcement until one day after). Column 6 reports a premium based on the abnormal returns (FFM) on a (-5/+5) window around announcement. The run-up of the acquirer is measured as the abnormal returns on a (-42/+5) window around announcement in column 7 and as (-42/Completion) in column 8. Panel B Columns 1, 2 and 3 report the effect on the number of bidders, the deal being unsolicited and the deal being challenged. Column 4 reports the effect on the percentage of stock paid for the target; columns 5 and 6 show the effects on Activism events.

Simple Vote Rule

Panel A

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Matching				**Acquirer Premium**			
	Same 2Digit SIC	Size Target Rel. to Acquiror	Total Synergies FFM	Total Synergy/ Total Mkt Cap	Acquirer Premium	Acquirer CAR(-5,5)	Runup CAR (-42,5)	Runup CAR (-42,Comp)
Lower Bound Estimation								
yes	0.14	-0.24	5,461,211*	0.07	-1.81	-0.42	3.20	8.44
	(0.094)	(1.03)	(3,003,063)	(0.074)	(3.35)	(2.28)	(4.67)	(7.33)
Z	1.46	-0.23	1.83	0.94	-0.54	-0.19	0.69	1.15
Upper Bound Estimation								
yes	0.38***	-0.05	9,208,777***	0.22***	7.20**	4.68**	13.4***	22.94***
	(0.061)	(0.34)	(2,876,863)	(0.049)	(3.52)	(2.65)	(4.82)	(6.36)
Z	6.18	-0.15	3.20	4.42	2.04	2.04	2.78	3.61
#sel	135	140	101	101	103	103	104	103
Obs	883	883	883	883	883	883	883	883

Panel B

	(1)	(2)	(3)	(4)	(5)	(6)
	Competition				**Activism**	
	Number of Bidders	Unsolicited Deal	Challenged Deal	Stock Percent	Num 13D Events two years prior Annouce.	Dummy13D Event two years prior Ann.
Lower Bound Estimation						
yes	-0.30**	-0.12**	-0.22***	-28.08**	-0.142	-0.054
	(0.08)	(0.042)	(0.051)	(11.04)	(0.200)	(0.096)
Z	-3.75	-3.0	-4.21	-2.54	-0.71	-0.56
Upper Bound Estimation						
yes	0.16	0.04	0.076	-10.43	-0.049	-0.025
	(0.15)	(0.072)	(0.09)	(11.03)	(0.132)	(0.076)
Z	1.05	1.05	0.79	-0.94	-0.38	-0.34
#sel	135	135	135	88	240	240
Obs	883	883	883	883	883	883

EXTERNAL APPENDIX (Not for Publication)

Section I: Figures and Tables

Figure A1a: Distribution of Votes

Histogram of the percentage of votes above majority threshold using 2 percentage point bins.

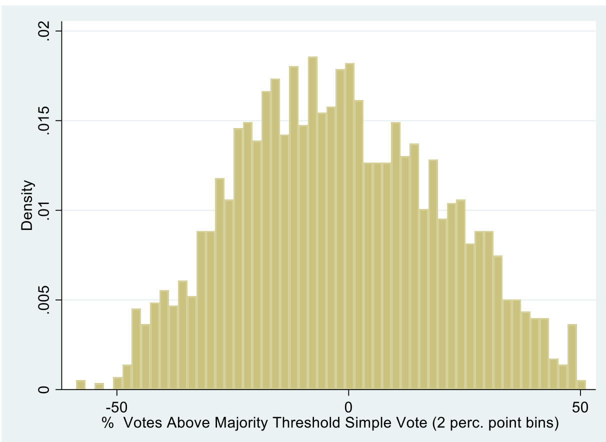


Figure A1b: Continuity of Vote, McCrary 2008

Continuity test in the density of the percentage of votes above majority threshold.

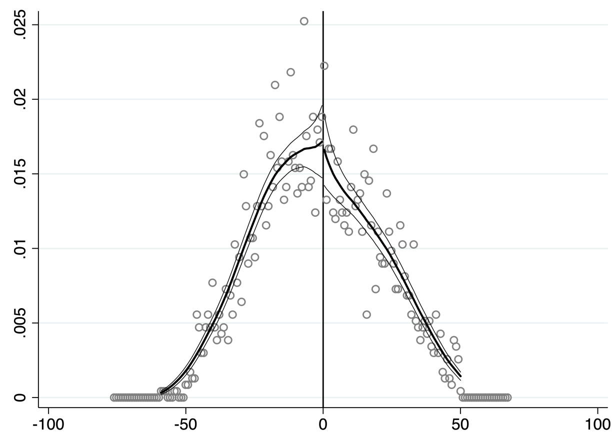


Figure A1c: Continuity of Vote, Votes Adjusted for Abstentions, McCrary 2008

Continuity test in the density of the percentage of votes above majority threshold.

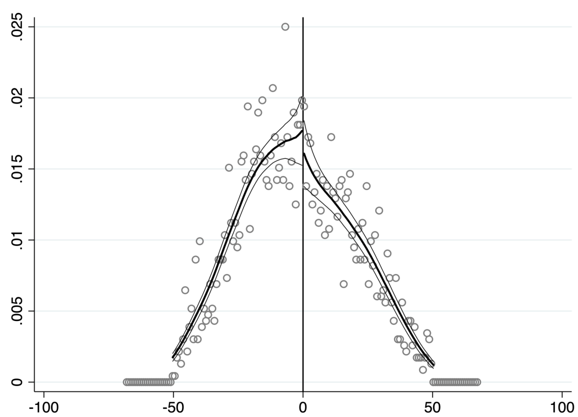


Figure A2: Histogram of Estimated Propensity Scores – Simple Vote Rule

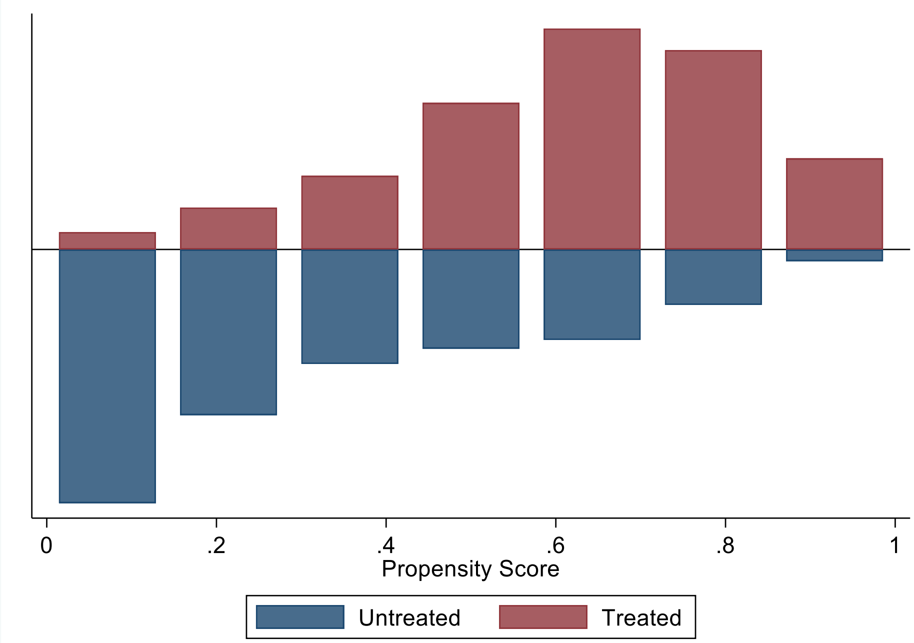


Figure A3a: Merger Probability

Linear regression using the Imbens and Kalyanaraman (2012) approach to select the bandwidth. Dots represent the simple means by bins of 2% vote intervals. Vote Adjusted for Abstentions

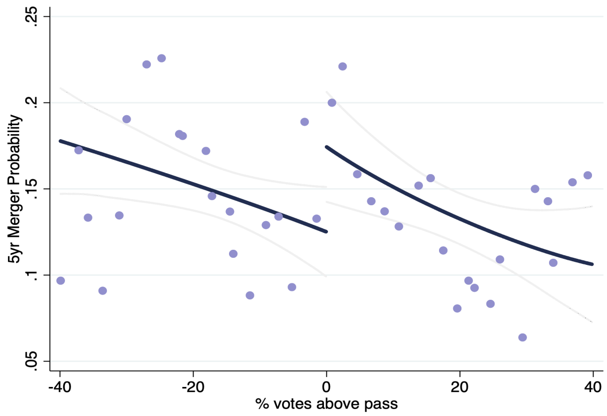


Figure A3b: Unconditional Premium

Linear regression using the Imbens and Kalyanaraman (2012) approach to select the bandwidth. Dots represent the simple means by bins of 2% vote intervals. Vote Adjusted for Abstentions

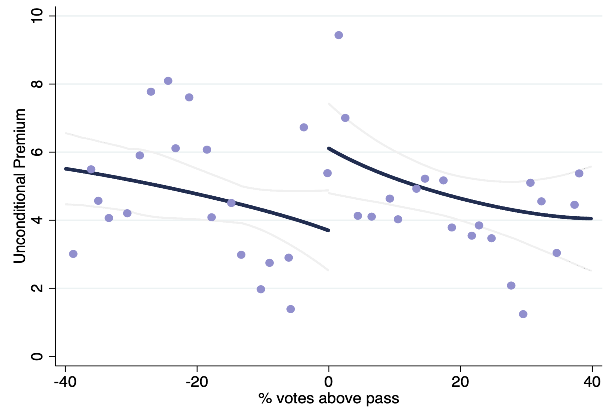


Figure A4a: Conditional Independence Test Merger Probability

Residuals of two independent linear models (one to each side of the discontinuity) using the same covariates as in the matching model. Dots represent the simple means by bins of 2% vote intervals. Vote Adjusted for Abstentions

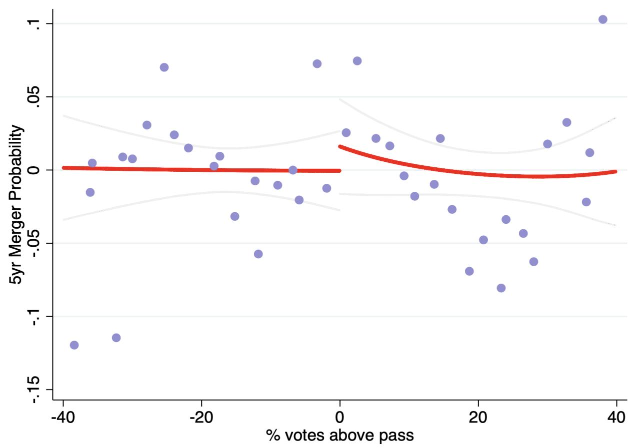


Figure A4b: Conditional Independence Test Premiums

Residuals of two independent linear models (one to each side of the discontinuity) using the same covariates as in the matching model. Dots represent the simple means by bins of 2% vote intervals. Vote Adjusted for Abstentions

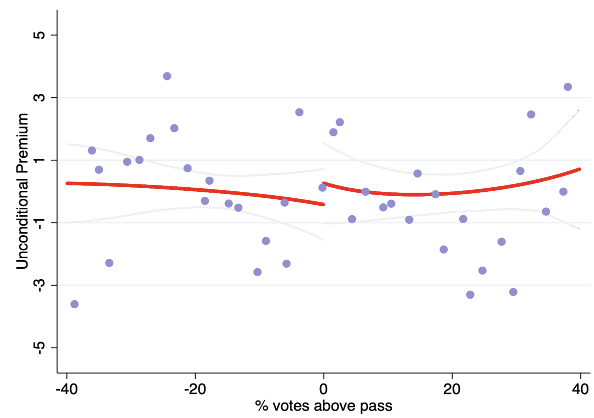


Table A 1- A

Shareholder Anti-takeover Proposals

This table displays the frequency of Anti-takeover (G) voted proposals, the percent of proposals passed and the average support over time. Data is collected by Riskmetrics on all shareholders proposals from 1994 until 2013 for all S&P 1,500 companies plus an additional 500 firms widely held. We have a sample of 2882 voted proposals.

Year	Voted Proposals	Passed Proposals	Percent. Passed Proposals	Average Vote Outcome	Num. Proposals (-5, +5)	Num. Proposals (-10, +10)
1994	157	9	5.73%	28.1%	15	31
1995	207	16	7.73%	28.2%	18	42
1996	169	16	9.47%	32%	24	47
1997	104	26	25.00%	39.7%	21	38
1998	119	33	27.73%	41.1%	17	35
1999	139	49	35.25%	43.7%	37	53
2000	127	61	48.03%	46.6%	33	50
2001	126	65	51.59%	48.1%	34	63
2002	144	91	63.19%	53.5%	24	48
2003	180	128	71.11%	57.8%	35	69
2004	136	87	63.97%	57.4%	17	35
2005	131	83	63.36%	56.1%	13	40
2006	146	83	56.85%	56.7%	15	49
2007	140	70	50.00%	51.6%	13	30
2008	145	86	59.31%	57.7%	19	45
2009	192	98	51.04%	54.2%	40	61
2010	158	80	50.63%	53.3%	33	61
2011	147	61	41.50%	50.45%	27	43
2012	125	76	60.80%	59.07%	19	29
2013	90	46	51.11%	57.11%	14	29
Total	**2,882**	**1264**	**43.86%**	**48.2%**	**468**	**898**

Table A 1- B

Mergers Announcements and Premiums

This table displays the probability of becoming a target over time and the corresponding premiums. The probability is computed over a window of 5 years after the vote. The table also displays the conditional premium for those firms that did merge, as well as the unconditional premium which includes the whole sample of firms. Data is from Thomson SDC.

Year	Prob Merger Announ. over next 5 Years	Merger Announ. over next 5Y Full Sample	Mergers Announ. over next 5Y in (-5,5)	Mergers Announ. over next 5Y in (-10,10)	Conditional Premium			Unconditional Premium	
					Mean	Median	Std Dev	Mean	Std Dev
1994	18%	29	2	3	28.7	24.5	21.1	5.30	14.33
1995	29%	61	2	9	32.1	32.2	22.8	9.46	19.1
1996	29%	50	10	19	35.4	32.2	38.2	10.4	26.2
1997	20%	21	8	11	31.9	32.4	23.7	6.45	16.6
1998	17%	21	3	5	31.2	32.4	14.3	5.51	13.3
1999	13%	18	7	7	36.8	32.6	36.5	4.77	17.8
2000	14%	18	12	12	33.1	37.7	12.6	4.69	12.48
2001	9%	11	4	7	31.7	32.0	13.9	2.77	9.83
2002	16%	23	2	9	25.7	27.6	15.2	4.11	11.2
2003	15%	28	7	12	28.1	25.5	20.7	4.38	13.03
2004	9%	13	1	3	42.8	37.9	39.6	4.09	17.32
2005	13%	17	0	2	40.1	41.5	16.8	5.19	14.75
2006	17%	25	4	13	21.9	21.7	27.1	3.75	13.78
2007	13%	18	2	5	36.4	33.3	29.2	4.68	15.94
2008	12%	17	2	3	34.5	32.3	23.0	4.05	13.54
2009	12%	23	8	9	32.9	29.5	12.6	3.69	11.08
2010	10%	16	6	7	46.4	44.8	41.3	4.69	18.9
2011	3%	5	1	1	26.0	31.4	13.4	0.88	5.22
2012	1%	1	0	0	40.6	40.6	.	0.32	3.63
2013	2%	2	0	1	28.5	28.5	4.1	0.63	4.25
Total	**14%**	**417**	**81**	**138**	32.7	32.2	25.9	4.83	15.22

Table A2

Pre-differences in Firm Characteristics as a Function of the Vote Outcome

This table tests whether a vote to drop an anti-takeover proposal passes is systematically related to firm characteristics prior to the meeting. Each row corresponds to a different dependent variable and each entry comes from a separate regression. Each entry in the table reports the coefficient on whether a proposal passed. Columns 1 and 2 (3 and 4) report the estimated effect of passing a vote on outcome variable levels (changes) the year before the annual meeting, t-1 (between t-2 and t-1). Columns 1 and 3 present estimates without controlling for a polynomial in the vote share and, therefore, estimate the average effect of passing relative to not passing. Columns 2 and 4 include the polynomial in the vote share of order 3 on each side of the threshold such that it effectively estimates the effect at the discontinuity. All columns control for year fixed effects and standard errors (in parenthesis) are clustered at the firm level. Significance at the 10%, 5%, and 1% levels are indicated by *, **, and *** respectively.

	Vote Simple				**Vote Adjusted**			
	Before meeting (t-1)		Change, from (t-2) to (t-1)		Before meeting (t-1)		Change, from (t-2) to (t-1)	
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
A.								
Tobin Q	0.071	-0.036	-0.048**	-0.004	0.064	-0.096	-0.061***	-0.071
	(0.062)	(0.127)	(0.022)	(0.070)	(0.065)	(0.202)	(0.021)	(0.071)
Return on Assets	-0.005	-0.007	-0.006*	-0.005	-0.005	-0.009	-0.007**	-0.006
	(0.004)	(0.010)	(0.003)	(0.009)	(0.005)	(0.012)	(0.003)	(0.009)
Return on Equity	-0.161	-0.241	-0.082	-0.182	-0.155	-0.246	-0.081	-0.160
	(0.125)	(0.346)	(0.116)	(0.359)	(0.123)	(0.361)	(0.114)	(0.376)
Profit Margin	-0.095	0.128	0.012	-0.014	-0.099	0.127	0.012	0.008
	(0.086)	(0.152)	(0.015)	(0.035)	(0.090)	(0.170)	(0.015)	(0.043)
Cash Liquidity	0.001	0.009	0.001	-0.001	0.003	0.011	0.001	0.001
	(0.007)	(0.014)	(0.002)	(0.007)	(0.007)	(0.014)	(0.002)	(0.007)
Leverage/ Assets	-0.031**	0.011	0.007***	0.007	-0.035***	0.003	0.008***	0.008
	(0.013)	(0.022)	(0.003)	(0.008)	(0.013)	(0.022)	(0.003)	(0.008)
SG&A/Op. Exp.	-0.016	0.001	-0.002	-0.007*	-0.012	0.008	-0.002	-0.003
	(0.014)	(0.029)	(0.002)	(0.004)	(0.014)	(0.029)	(0.002)	(0.004)
CAPX/Assets	0.005	0.002	-0.003**	-0.001	0.005	0.001	-0.003**	-0.003
	(0.004)	(0.006)	(0.001)	(0.003)	(0.004)	(0.006)	(0.001)	(0.003)
Log Total Assets	-0.970***	-0.235	0.005	-0.009	-0.918***	-0.110	0.007	-0.006
	(0.152)	(0.217)	(0.009)	(0.025)	(0.149)	(0.224)	(0.009)	(0.026)
B.								
Institutional Owners %	0.085***	0.013	0.002	-0.008	0.086***	0.029	0.002	-0.009
	(0.012)	(0.022)	(0.003)	(0.009)	(0.012)	(0.023)	(0.003)	(0.010)
Herfinal Index	-0.012***	-0.003	0.003	0.001	-0.012***	-0.004	0.003	0.001
	(0.003)	(0.006)	(0.002)	(0.003)	(0.003)	(0.006)	(0.002)	(0.003)
Number Proposals	-0.033	-0.003			-0.025	0.082		
	(0.043)	(0.095)			(0.043)	(0.122)		
Polynomial in the vote share	**no**	**yes**	**no**	**yes**	**no**	**yes**	**no**	**yes**

Comments: Column 1 (3) shows the difference in average characteristics (trends) for firms that pass versus firms that do not pass an anti-takeover proposal. Firm characteristics are measured the year before the meeting when the vote takes place. We find that the two sets of firms are different: firms that pass an antitakeover proposal have lower leverage, more institutional shareholders, lower Tobin's Q growth and ROA growth. This suggests that the adoption of anti-takeover provisions is correlated with observed and possibly unobserved firm characteristics such that an approach that deals with endogeneity bias is necessary. However, when we restrict the analysis to firms that are close to the majority threshold (by controlling for a third order polynomial to each side of the discontinuity in columns 2 and 4) those differences disappear, confirming that characteristics are smooth across the majority threshold.

Table A3
Descriptive Statistics for Premium and Merger Variables

This table presents descriptive statistics for the variables in Tables 5 Premium Effects & Table 7 Merger Effects for the sample of 417 mergers.

	N	Mean	Median	Std. dev.	10th Per.	90th Per.
Target Premium						
Premium 4w Ann. to Completion	417	32.71	32.24	25.95	6.22	56.91
Premium 1w Ann. to Completion	416	31.75	28.36	25.50	5.01	58.16
CAR(-5,5) FFM	415	15.17	14.71	19.13	-2.98	32.25
CAR (Vote,Ann+1)	399	28.22	16.64	127.95	-98.80	141.33
Runup (-42,5) FFM	415	17.65	16.31	25.93	-4.47	42.48
Runup (-42, Completion) FFM	408	20.78	20.19	45.05	-12.29	63.93
Aquirer Premium						
Acquirer Premium	311	0.11	-0.23	12.72	-14.24	14.77
Acquirer CAR(-5,5) FFM	325	-1.19	-0.49	9.04	-11.63	6.73
Runup Acquirer (-42,5) FFM	325	-2.62	-2.01	16.97	-19.92	13.30
Runup Acquirer (-42,Comp) FFM	315	-7.34	-5.31	29.59	-43.57	24.41
Competition						
Number of Bidders	417	1.24	1	0.65	1	2
Unsolicited Deal	417	0.101	0	0.301	0	1
Challenged Deal	417	0.161	0	0.367	0	1
Stock Percent	247	79.34	100	28.50	31.65	100
Matching						
Same 2Digit SIC	417	0.631	1	0.483	0	1
Size Target Rel. to Acquiror	421	1.11	0.336	4.35	0.074	1.110
Total Synergies FFM	311	-3,455,048	441,359	1.85e+07	-1.61e+7	5,694,756
Total Synergy/ Total Mkt Cap	311	0.325	1.659	24.792	-35.21	28.75

Table A4
Proposal Types

This table displays the type of proposals that belong to the G-index. The sample includes all shareholders proposals from 1994 until 2013 for all S&P 1,500 companies plus an additional 500 firms widely held from Riskmetrics.

Code	Type of Resolution	# Proposals	[-10,10]	[-5,5]	Mergers
2300	Repeal Classified Board	996	339	175	167
2220	Adopt Cumulative Voting	475	61	28	56
2310	Repeal Poison Pill	396	155	93	63
2320	Repeal Supermajority	218	54	25	20
2414	Golden Parachutes	206	72	26	31
2325	Call Special Meeting	202	107	57	11
2326	Written Consent	98	65	42	3
2100	Confidential Voting	90	31	16	16
2202	Director's Duties	91	2	0	28
2902	Eliminate Unequal Voting	60	6	2	9
2901	Director's Contracts	22	0	0	8
2350	Antigreemail	10	3	1	3
2240	Director's Liabilities	7	1	1	2
2906	Amend bylaws	5	2	2	0
2342	Change bylaw -- incorporate out Delaware	4	0	0	0
2120	Repeal Advance Notice Bylaw	2	0	0	0
Total		2,882	898	468	417

Table A5 A
Continuity Tests – Simple Vote Rule

This table proposes shows tests for vote manipulation McCrary (2008). The local density at the cut-off is estimated based on a third order polynomial regression and a fourth order bias correction polynomial in an optimally determined boundary as proposed in Cattaneo, Jason and Ma (2014). Panel A shows the results for the full sample and disaggregated results over different sample periods. Panel B shows results for the most voted proposals coded as follows:(2220) Adopt Cumulative Voting, (2300) Repeal Classified Board, (2310) Redeem or Vote on Poison Pill, (2320) Eliminate Supermajority Provision, (2325) Shareholders Call of Special Meeting, (2414). Column 7 in Panel B runs the test for the rest of the proposals included in the G-Index.

Panel A

	(1) All years	(2) 1996-2000	(3) 2001-2005	(4) 2006-2010	(5) 2011-2013	(6) 1996-2003	(7) 2003-2013	(8) 2003-2011
Density difference (left - right)	0.0011	-0.0153	0.0017	0.004	0.007	-0.0087	0.003	0.0054
P-value	0.715	0.0331*	0.886	0.8860	0.1330	0.8380	0.5670	0.7590
Observations	2872	658	775	775	361	1472	1580	1366
Left Boundary (% votes)	26.9	17.4	27.3	27.3	35.6	18.3	24.5	23.6
Right Boundary (% votes)	24.4	16.2	32.5	32.5	37.2	20.1	25.5	24.2

Panel B

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Provision type >	2220	2300	2310	2320	2325	2414	other
Density difference (left - right)	0.0072	-0.0017	0.0025	0.0083	-0.0117	0.0022	-0.0037
P-value	0.795	0.361	0.994	0.561	0.468	0.095*	0.300
Observations	474	989	396	206	202	206	399
Left Boundary (% votes)	5.5	14.1	13.4	16.4	12.1	12.4	13.6
Right Boundary (% votes)	4.4	18.5	14.1	17.5	11.8	16.9	16.1

Table A5 B
Continuity Tests - Vote Adjusted For Abstentions

This table proposes shows tests for vote manipulation McCrary (2008). The local density at the cut-off is estimated based on a third order polynomial regression and a fourth order bias correction polynomial in an optimally determined boundary as proposed in Cattaneo, Jason and Ma (2014). Panel A shows the results for the full sample and disaggregated results over different sample periods. Panel B shows results for the most voted proposals coded as follows:(2220) Adopt Cumulative Voting, (2300) Repeal Classified Board, (2310) Redeem or Vote on Poison Pill, (2320) Eliminate Supermajority Provision, (2325) Shareholders Call of Special Meeting, (2414). Column 7 in Panel B runs the test for the rest of the proposals included in the G-Index.

Panel A

	(1) All years	(2) 1996-2000	(3) 2001-2005	(4) 2006-2010	(5) 2011-2013	(6) 1996-2003	(7) 2003-2013	(8) 2003-2011
Density difference (left - right)	-0.0028	-0.0153	0.0033	0.0036	0.006	-0.009	0.003	0.0058
P-value	0.882	0.029**	0.886	0.7710	0.07*	0.7770	0.6170	0.8680
Observations	2882	658	717	781	362	1472	1590	1375
Left Boundary (% votes)	20.2	17.4	19.9	25.2	34.1	17.9	23.0	22.3
Right Boundary (% votes)	21.3	16.1	21.9	31.1	32.1	19.7	23.9	22.4

Panel B

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Provision type >	2220	2300	2310	2320	2325	2414	other
Density difference (left - right)	0.0058	0.0006	0.0026	-0.0024	-0.0129	-0.0025	0.0008
P-value	0.879	0.215	0.991	0.807	0.580	0.029**	0.055*
Observations	475	996	396	208	202	206	399
Left Boundary (% votes)	5.8	15.2	13.4	11.9	12.7	13.6	17.7
Right Boundary (% votes)	4.8	18.4	13.9	12.1	12.1	19.1	24.6

Table A6

Propensity Score Model

This table reports the propensity score model. All firm characteristics are one year prior to the vote including ln Sales, Profit Margin, Ln Market Value, Cash Liquidity, Percentage of Institutional Ownership, Average Industry Tobin's Q, Average Industry Market value and the Entrenchment Index. Significance at the 10%, 5%, and 1% levels are indicated by *, **, and *** respectively.

	Vote Simple	**Vote Adjusted**
ln Sales	-0.203***	-0.197***
	(0.0447)	(0.0444)
Profit Margin	-0.864***	-0.933***
	(0.272)	(0.270)
Ln Market Value	0.0516	0.0714*
	(0.0425)	(0.0422)
Cash Liquidity	-0.579*	-0.378
	(0.303)	(0.300)
Percent Institutional Ownership	1.706***	1.733***
	(0.206)	(0.205)
Av. Ind. Tobins'Q	0.00423	-0.0433
	(0.0324)	(0.0321)
Av. Ind. Market Value	-0.0633*	-0.0709**
	(0.0360)	(0.0359)
Entrenchment Index	0.226***	0.230***
	(0.0257)	(0.0257)
Year Dummies	Y	Y
Obs	2,379	2,379
Pseudo R-sq	0.251	0.238

Table A7 A

Takeover Probability and Premiums around the Majority Threshold for Other Proposals

This table presents the effect of passing Other (i.e. non anti-takeover proposal) on the probability of becoming a target and on premiums. Panel A displays the probability of becoming a target over the next 5 years after the vote using SDC data. Panel B displays the unconditional premium of a potential target. Premiums are computed as the price offer to target 4 weeks prior to announcement until completion. Column 1 estimates are based on the whole sample. Column 2 restricts the sample to observations with a vote share within ten points of the threshold, column 3 to five points and so forth. Column 6 and 7 introduces a polynomial in the vote share of order 2 and 3 (Lee and Lemieux, 2010), one on each side of the threshold, and uses the full sample. Column 9 uses the local linear regression approach by Imbens Kalyanaraman (2012). Column 10 uses the non-parametric approach proposed by Calonico, Cattaneo and Titiunik (2014). All columns control for year fixed effects; standard errors are clustered by firm. The reported bandwidth is expressed in percent vote. Significance at the 10%, 5%, and 1% levels are indicated by *, **, and *** respectively.

Simple Vote Rule

Panel A: Probability of becoming a takeover target over the next 5 years

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(9)	(10)
	Full	+/-10	+/-5	+/-2.5	+/-1.5	poly hl 2	poly hl 3	IK	CCT
yes	-0.00972	0.0328	0.0519	0.0599	0.0502	0.0734*	0.0257	0.047	0.065
	(0.0193)	(0.0257)	(0.0331)	(0.0534)	(0.0623)	(0.0378)	(0.0488)	(0.033)	(0.053)
bandwidth								32.8	10.6
Obs	3,593	727	339	160	98	3,593	3,593	3,598	3,598
R-sq/Z	0.000	0.003	0.007	0.009	0.008	0.003	0.004		

Panel B: Unconditional Premium

	Full	+/-10	+/-5	+/-2.5	+/-1.5	poly hl 2	poly hl 3	IK	CCT
yes	-0.215	1.516	0.518	-0.685	-0.187	1.918	-1.195	0.64	-1.24
	(0.944)	-1.345	-1.098	-1.750	-1.577	-1.478	-1.543	(1.38)	(1.65)
bandwidth								15.7	10.3
Obs	3,593	727	339	160	98	3,593	3,593	3,598	3,598
R-sq/Z	0.000	0.004	0.001	0.001	0.000	0.003	0.004		

Table A7 B

CIA Estimates and Propensity Score Matching -- Other Proposals

This table reports CIA estimates of the effect of passing Other (non-G) proposals on the takeover probability and the unconditional premium (4 weeks before Announcement to Completion). Panel A reports the results from a linear reweighting estimator and Panel B reports results from a nearest neighbor matching procedure. Controls are the same as in Table A6: Log Sales, Profit Margin, Ln Market Value, Cash Liquidity, Percentage of Institutional Ownership, Average Industry Tobins'Q, Average Industry Market Value and the Entrenchment Index. Significance at the 10%, 5%, and 1% levels are indicated by *, **, and *** respectively.

Panel A: Propensity Score Weighting

	(1)	(2)	(3)	(4)
	Takeover Probability		Unconditional Premium	
yes	-0.056***	-0.058***	-1.443***	-1.377***
	(0.015)	(0.014)	(0.524)	(0.493)
t stat	-3.67	-4.11	-2.75	-2.80
Model	Y	N	Y	N
Obs	2,909	2,909	2,909	2,909

Panel B: Nearest Neighbor Matching

	(1)	(2)
	Takeover Probability	Unconditional Premium
yes	-0.041*	-1.01
	(0.023)	(0.84)
Obs	3,067	3,067

Table A8 - Simple Vote Rule

Unconditional Premiums around the Majority Threshold

This table presents the effect of passing an anti-takeover proposal on different premium measures. The two first measures come from SDC. The next two measures compute the cumulative abnormal returns using the FFM factors for different windows (-/+ 5 days,) and (-/+1days) both relative to the announcement date. We use two estimation methodologies: the local linear regression approach by Imbens Kalyanaraman (2012) and the non-parametric approach proposed by Calonico, Cattaneo and Titiunik (2014). Significance at the 10%, 5%, and 1% levels are indicated by *, **, and *** respectively.

	1 week before Announcement to Completion		**1 day before Announcement to Completion**		**(-5,5) CAR Announcement FFM**		**(-1,1) CAR Announcement FFM**	
	IK	CCT	IK	CCT	IK	CCT	IK	CCT
yes	4.19**	4.15**	4.34***	4.69**	2.70**	2.70**	2.56***	2.54**
	(1.91)	(1.89)	(1.65)	(2.07)	(1.17)	(1.17)	(0.98)	(1.17)
Obs	2,881	2,881	2,881	2,881	2,880	2,880	2,882	2,882

Table A9

Conditional Independence Tests on Vote Adjusted for Abstentions

This table reports the tests of the conditional independence assumption for our two outcome variables: Takeover Probability and Unconditional Premium. Columns 1,3,5 and 7 present the initial relationship between the running variable i.e. the vote and the two outcome variables for observations to the left or right of the cutoff. Columns 2,4,6,8 display the model that controls for firm characteristics one year prior to the vote including Sales, Profit Margin, Market Value, Cash Liquidity, Percentage of Institutional Ownership, Average Industry Tobin's Q, Average Industry Market value and the Entrenchment Index. Significance at the 10%, 5%, and 1% levels are indicated by *, **, and *** respectively.

	Vote Adjusted for abstentions							
	Takeover Probability				**Unconditional Premium**			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	D=0		D=1		D=0		D=1	
	[-50,0)		[0,50]		[-50,0)		[0,50]	
Vote	-0.00237***	-2.78e-05	-0.00169*	-0.000477	-0.0748**	-0.0382	-0.0268	-0.0424
	(0.000854)	(0.000882)	(0.000863)	(0.000983)	(0.0316)	(0.0303)	(0.0359)	(0.0454)
ln Sales		-0.0135		-0.0264*		-1.446***		-0.661
		(0.0144)		(0.0148)		(0.495)		(0.686)
Profit Margin		0.235***		-0.000834		-0.493		-0.121
		(0.0744)		(0.0812)		(2.553)		(3.755)
Ln Market Value		-0.0162		-0.0163		0.237		-1.186*
		(0.0135)		(0.0140)		(0.464)		(0.645)
Cash Liquidity		0.104		0.183*		0.589		6.902
		(0.102)		(0.101)		(3.488)		(4.688)
Percent Inst. Own.		0.0206		-0.187**		3.896*		-12.27***
		(0.0625)		(0.0811)		(2.144)		(3.750)
Av. Ind. Tobins'Q		0.00304		0.0248*		0.133		1.643***
		(0.00946)		(0.0129)		(0.325)		(0.599)
Av.Ind.Mkt.Value		0.0661***		0.0302**		1.191***		0.566
		(0.0112)		(0.0130)		(0.384)		(0.599)
Entrench. Index		0.0159**		0.0126		0.469*		1.161**
		(0.00786)		(0.0102)		(0.270)		(0.471)
Year Dummies		Y		Y		Y		Y
Obs	1,308	1,308	1,071	1,071	1,308	1,307	1,071	1,071
R-sq	0.006	0.138	0.004	0.090	0.004	0.093	0.001	0.087

Table A10

Target Conditional Premiums, Lee Bounds Estimates on Vote Adjusted for Abstentions

This table reports the effect of passing a G proposal on different premium measures for the target company. All estimates are obtained using Lee (2009) methodology to account for selection in the universe of targeted companies. Column 1 reports the effect on the Target Premium computed as the change in price 4 weeks before announcement until completion. Column 2 reports the effect on the Target Premium computed as the change in price 1 week before announcement until completion. Columns 3 and 4 report premiums based on the cumulative abnormal returns using the FFM factors for different windows (-/+ 5 days,) and (Vote/+1days) both relative to the announcement date. The runups for the target company are computed as the abnormal returns from (-42,5) trading days around announcement in column 5 and in column 6 as the abnormal return (-42, until Completion), always using the FFM factors.

Vote Adjusted for abstentions

Panel A: Lee Bounds with weights full sample

	(1) Premium 4 weeks before Announce. to Completion	(2) Premium 1 week before Announce. to Completion	(3) CAR(-5,5) FFM	(4) CAR (Vote,Ann+1)	(5) Runup (-42,5) FFM	(6) Runup (-42, Completion) FFM
Lower Bound Estimation						
yes	-0.85	5.86*	5.46**	9.86	3.27	3.87
	(3.37)	(3.31)	(2.14)	(21.95)	(2.94)	(3.92)
Z	-0.25	1.77	2.54	0.45	1.11	0.99
Upper Bound Estimation						
yes	5.99**	11.3***	10.02***	51.14***	9.78***	15.4***
	(2.89)	(2.54)	(1.93)	(17.50)	(2.78)	(4.05)
R-sq/Z	2.07	4.45	5.18	2.92	3.15	3.81
# sel	408	406	408	395	408	397
Obs	2.379	2.379	2.379	2.379	2.379	2.379

Panel B: Lee Bounds without weights in (-10, 10) interval

	(1)	(2)	(3)	(4)	(5)	(6)
Lower Bound Estimation						
yes	-2.81	-1.38	1.2	-43.17*	4.29	-0.54
	(4.45)	(4.76)	(3.50)	(23.4)	(5.56)	(7.74)
Z	-0.63	-0.29	-0.34	-1.84	0.77	-0.07
Upper Bound Estimation						
yes	13.42**	21.7***	11.22***	51.43*	21.02***	25.64***
	(5.96)	(6.66)	(4.08)	(26.4)	(5.61)	(8.38)
R-sq/Z	2.25	3.26	2.75	1.94	3.74	3.06
# sel	137	136	135	126	135	132
Obs	893	893	893	893	893	893

Table A11
Decomposing the Shareholder Value Effect for Vote Adjusted for Abstentions

This table provides a decomposition of the Change in Shareholder Value induced by the passing of a proposal to eliminate an anti-takeover provision. We provide an estimate of the three different components that affect shareholder value via changes in the premium, changes in the probability of a takeover and changes in the population of firms that are put into play. We provide both the lower and upper bound values since we use Lee (2009) to estimate the change in Takeover Premium β. Column 1 estimates the Change in Shareholder's Value as the unconditional takeover premium under the CIA model for panel A, and using the RDD IK estimate for panel B. Column 2 "Premium Effect" is the result of the change in Takeover Premium β times the Baseline Probability of Merger (Pr[Z*>0 | D=1]). Column 3 "Takeover Probability Effect" is the result of the change in the Probability of Merger ({Pr[Z*>0 | D=1] - Pr[Z*>0 | D=0]} times the Baseline Premium (E[Y | D=0 , Z*>0]). Column 4 provides an estimate of the Selection Effect. Using the probabilites of the matching model we calculate that the Baseline Probability is 14.4 and the Baseline Premium is 32.7

Vote Adjusted for abstentions			
(1)	(2)	(3)	(4)
Change in Shareholder Value	**Premium Effect**	**Takeover Probability Effect**	**Selection Effect**
Δ Y	β * Pr[Z*>0 | D=1]	{Pr[Z*>0 | D=1] - Pr[Z*>0 | D=0]} * E[Y | D=0 , Z*>0]	Pr[Z*>0 | D=1] * { E[Y | D=1 , Z*>0]- E[Y | D=1 , V > - μ2]}
Panel A: Using AR for Unconditional Premium & Probability from T4			
Lower Bound Estimation of β = -0.85			
2.3%	-0.12	1.18	1.25
	-5%	***51%***	***54%***
Upper Bound Estimation of β = 5.99			
2.3%	0.86	1.18	0.26
	38%	***51%***	***11%***
Panel B: Using RDD-IK for Unconditional Premium and Probability from T2 (-10,10)			
Lower Bound Estimation of β = -2.8			
2.24%	-0.46	1.26	1.44
	-21%	***56%***	***65%***
Upper Bound Estimation of β = 13.4			
2.24%	2.20	1.26	-1.21
	98%	***56%***	***-54%***

Table 12 A

Merger Effects -- Lee bounds Full distribution with weights from CIA model

This table reports the effect of passing a G proposal on different merger outcomes. All estimates are obtained using Lee (2009) methodology to account for selection in the universe of targeted companies. Panel A presents results for different measures of Matching and Acquirer Premium. Column 1 reports the effect on the likelihood of Target and Acquirer being in the same 2-digit SIC code, column 2 on the relative size of Target versus Acquirer, column 3 reports a measure of Total Synergies and column 4 Total Synergies as a percentage of total market capitalization. Column 5 reports the effect on the Acquirer Premium (change in price 4 weeks before announcement until one day after). Column 6 reports a premium based on the abnormal returns (FFM) on a (-5/+5) window around announcement. The runup of the acquirer is measured as the abnormal returns on a (-42/+5) window around announcement in column 7 and as (-42/Completion) in column 8. Panel B Columns 1, 2 and 3 report the effect on the number of bidders, the deal being unsolicited and the deal being challenged. Column 4 reports the effect on the percentage of stock paid for the target; columns 5 and 6 present the effects on Activism events.

Vote Adjusted of abstentions

Panel A

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Matching				**Acquirer Premium**			
	Same 2Digit SIC	Size Target Rel. to Acquiror	Total Synergies FFM	Total Synergy/ Total Mkt Cap	Acquirer Premium	Acquirer CAR(-5,5)	Runup CAR (-42,5)	Runup CAR (-42,Comp)
Lower Bound Estimation								
yes	0.179***	-1.3**	7.085	-0.02	-7.89**	-4.13***	-5.17*	-0.9
	(0.06)	(0.50)	(2,436,535)	(0.03)	(3.14)	(1.38)	(2.85)	(4.44)
Z	2.59	-2.54	0.00	-0.56	-2.51	-2.99	1.81	-0.22
Upper Bound Estimation								
yes	0.27***	-0.70	7,409,240***	0.15***	0.02	1.04	6.21**	19.38***
	(0.07)	(0.48)	(1,845,106)	(0.03)	(2.35)	(1.39)	(3.25)	(3.84)
Z	3.87	-1.45	4.02	4.68	0.01	1.03	1.91	5.05
Obs	2379	2379	2379	2379	2379	2379	2379	2379

Panel B

	(1)	(2)	(3)	(4)	(5)	(6)
	Competition				**Activism**	
	Number of Bidders	Unsolicited Deal	Challenged Deal	Stock Percent	Num 13D Events two years prior Announce.	Dummy 13D Event two years prior Announce.
Lower Bound Estimation						
yes	0.15**	0.045	0.11***	-23.99***	0.040	0.061
	(0.06)	(0.03)	(0.03)	(7.04)	(0.075)	(0.043)
Z	2.26	1.44	2.68	-3.41	0.54	1.41
Upper Bound Estimation						
yes	0.30***	0.12***	0.20**	-4.69	0.365***	0.216***
	(0.10)	(0.02)	(0.09)	(6.66)	(0.079)	(0.070)
Z	2.88	5.08	2.08	-0.70	4.59	3.06
Obs	2379	2379	2379	2379	2379	2379

Table A12b

Merger Effects --Lee bounds without weights for sample in narrow interval (-10,10)

This table reports the effect of passing a G proposal on different merger outcomes. All estimates are obtained using Lee (2009) methodology to account for selection in the universe of targeted companies. Panel A presents results for different measures of Matching and Acquirer Premium. Panel B reports the effect on Competition and activism events.

Vote Adjusted for abstentions

Panel A

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Matching				**Acquirer Premium**			
	Same 2Digit SIC	Size Target Rel. to Acquiror	Total Synergies FFM	Total Synergy/ Total Mkt Cap	Acquirer Premium	Acquirer CAR(-5,5)	Runup CAR (-42,5)	Runup CAR (-42,Comp)
Lower Bound Estimation								
yes	0.142	-0.08	5,367,368*	0.084	-1.52	-0.91	4.2	9.43
	(0.09)	(0.33)	(2,922,611)	(0.07)	(3.91)	(2.19)	(4.39)	(7.41)
Z	1.53	-0.26	1.84	1.19	-0.39	-0.04	0.96	1.27
Upper Bound Estimation								
yes	0.378***	-0.014	8,807,647***	0.20***	6.19	4.6*	12.8***	22.64***
	(0.060)	(1.00)	(2,813,556)	(0.062)	(4.2)	(2.57)	(4.67)	(6.88)
Z	6.30	-0.01	3.13	3.25	1.46	1.81	2.75	3.29
Obs	893	893	893	893	893	893	893	893

Panel B

	(1)	(2)	(3)	(4)	(5)	(6)
	Competition				**Activism**	
	Number of Bidders	Unsolicit Deal	Challenged Deal	Stock Percent	Num 13D Events two years prior Announce.	Dummy13D Event two years prior Announce.
Lower Bound Estimation						
yes	-0.28**	-0.12**	-0.21***	-27.1**	-0.19	-0.075
	(0.072)	(0.040)	(0.051)	(10.41)	(0.206)	(0.99)
Z	-3.73	-2.99	-4.18	-2.61	0.95	-0.76
Upper Bound Estimation						
yes	0.16	0.043	0.07	-12.63	-0.014	-0.003
	(0.15)	(0.070)	(0.09)	(10.63)	(0.136)	(0.078)
Z	1.11	0.61	0.85	-1.19	-0.11	-0.04
Obs	893	893	893	893	893	893

Section II: Further Results Appendix: Heterogeneous effects across vote levels

The results of the RDD and the matching specification are qualitatively very similar, but the RDD specification yields higher estimates. This is because we are evaluating the effects for a different population of firms and, even within the causal interpretation of the coefficients, the effects of ATPs may be heterogeneous across these two populations. However, it is important to note that in both specifications, these direct or indirect effects affect the type of firm for which we are measuring the effect, but not the causal interpretation of the coefficient.

Using our specifications, we can also provide values of the estimated effect at each vote level, such that we can identify heterogeneous effects at different points of the vote distribution. To do so, we start by fitting two linear models that use the same variables and coefficients as in Table 3, estimated separately on each side of the discontinuity. Then we extrapolate each model by predicting the observations on the other side of the discontinuity. Finally, we show in Figures A3 the smoothed functions of the real data (dark/black lines) and the predicted observations (light/red line).[37]

Given that the predicted observations do not take into account the implementation jump at the majority threshold, they can be interpreted as counterfactuals. Focusing on Panels A and C of Figure A3, the light/red lines are the counterfactuals, had the proposal passed for firms for which it did not pass. Comparing the lighter/red line with the left-hand-side dark/black line amounts to asking: what would have been the takeover probability and the unconditional premium at each vote outcome for firms that did not pass a provision had they passed one?

We find that the effect is positive and quite constant for all firms, suggesting that, if anything, firms with very low votes have slightly bigger takeover and premium effects. This begs the question for why shareholders do not support more strongly a proposal that would increase their returns. Earlier literature has highlighted that shareholders may have goals other than shareholder value maximization. For example, it has been shown that banks and insurance companies tend to side with management by voting against these proposals, while mutual funds, unions, advisors, and pension funds tend to support the proposals (Brickley, Lease, and Smith, 1988; Agrawal, 2011).

Similarly, the right-hand-side lighter/red lines show the counterfactual, had the proposal not passed for firms in which it passed. Therefore, panels B and D of Figure A3 answer the question: What would the takeover and unconditional premium have been had the firms that passed a provision not passed it? And does this effect vary for different vote outcomes? Here we find some interesting heterogeneous responses. The effect (the

[37] More specifically, we predict the outcome on the right-hand-side using the left-hand-side model (and vice-versa) for each observation. We then smooth the prediction and the real data that estimates the model using the same procedure as in Figure 1. Note the dark lines in the figure are the original data, and identical to those in Figure 1.

difference between the two lines) is declining in the distance to the threshold. It is largest for firms around the discontinuity: firms up to 25% from the discontinuity would have had a lower takeover probability and expected premium had they not passed the provision. For firms with votes 25% higher than the majority threshold, the difference disappears. Although these firms represent a small part of the sample (13%), and contribute little to the average treatment effect, the results suggest that these firms are different from the rest. Whenever a proposal attracts such high shareholder support, the takeover probability and premium for these firms seem independent of the actual passing of the proposal.

Figure A3a: Extrapolate Merger Probabilities LHS

The lighter/red line shows the extrapolation of the linear model of the right hand side to the left hand side. Dots show simple means 2.5% vote intervals. Comparing the lighter/red line with right-hand-side dark/black line shows what would have been the takeover probability at each vote outcome for firms that did not pass a provision had they passed one.

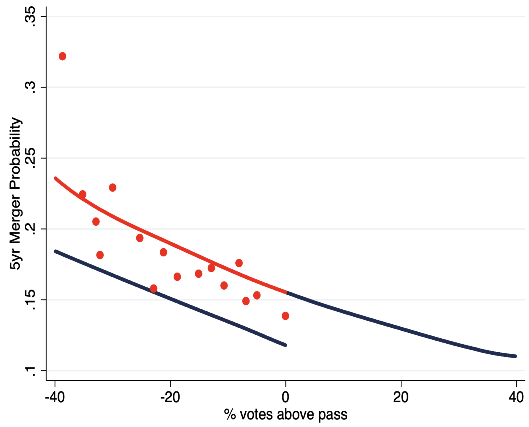


Figure A3b: Extrapolate Merger Probabilities RHS

The lighter/red line shows the extrapolation of the linear model of the left hand side to the right hand side. Dots show simple means 2.5% vote intervals. Comparing the lighter/red line with the left-hand-side dark/black line shows what would have been the takeover probability at each vote outcome for firms that passed a provision had they not passed one.

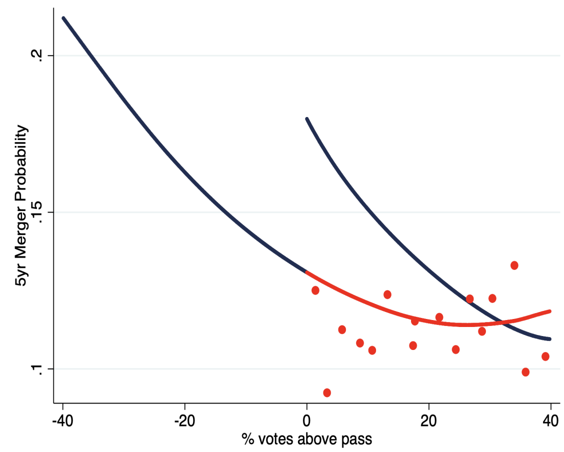


Figure A3c: Extrapolate Unconditional Premium LHS

The lighter/red line shows the extrapolation of the linear model of the right hand side to the left hand side. Dots show simple means 2.5% vote intervals. Comparing the lighter/red line with right-hand-side dark/black line shows what would have been the unconditional premium at each vote outcome for firms that did not pass a provision had they passed one.

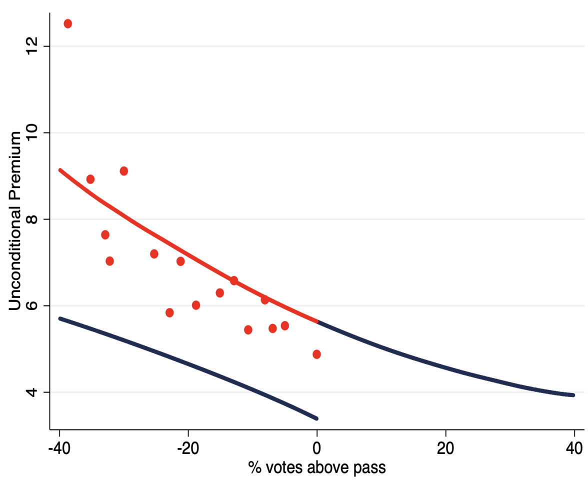
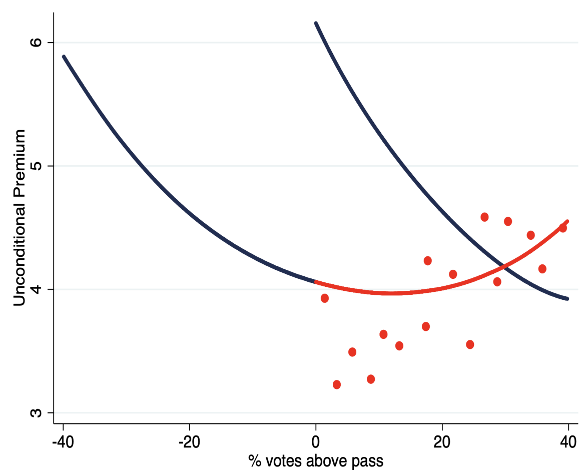


Figure A3d : Extrapolate Unconditional Premium RHS

The lighter/red line shows the extrapolation of the linear model of the left hand side to the right hand side. Dots show simple means 2.5% vote intervals. Comparing the lighter/red line with left-hand-side dark/black line shows what would have been the unconditional premium at each vote outcome for firms that passed a provision had they not passed one.



Section III: External Data Appendix: Differences between ISS-Shareholder Proposals/Voting Results and Voting Analytics

The two ISS and Voting analytics datasets are different. The ISS datasets (formerly known as Riskmetrics), use analysts that aim to capture the vote outcome on the day the vote takes place or the first available date, and those vote outcomes are not updated. The Shareholder Proposals dataset gives the *Simple* vote for all observations (since 1997), and the Voting Results (1997-2006) dataset provides the voting rule and allows to adjust for abstentions and cast/outstanding rules (*Adjusted*) when necessary.

Voting Analytics captures the vote, not on the day of the vote but on the day it is published in 8k or 10Q. The data starts in 2003. In addition, we were informed by the data provider that Voting Analytics updates the dataset over time when management releases revisions to the initial published number. Before 2010, this publication must happen within 3 months after the end of the quarter which, in practice, is between a few days and up to 5 months after the vote. Voting Analytics includes adjustments that happen in the final tabulation of the votes.[38] Voting Analytics also reports the treatment of abstentions consistently within its universe.

The differences between the votes recorded in ISS-Shareholder Proposals and Voting Analytics reflect the different treatment of abstention votes, the different timing of the recording of the vote and possibly mistakes in coding the votes.

Using the Voting Analytics dataset, Bach and Metzger (2019) report a strong discontinuity in the vote distribution at the majority threshold. We do not find such discontinuity either in the *Simple* or *Adjusted* votes in our sample. Bach and Metzger (2019) reports that 16% of the votes in their sample are anti-takeover proposals.

[38] Among these are changes in the voting threshold, recounts, changes in the way that firms deal with abstentions, the treatment of absent votes, over-voting adjustments (see Smith, 2015 and Kahan and Rock 2008).

When we match our sample to the Voting Analytics dataset, the overlap between our main sample and Bach and Metzger (2019) is 16% (711 matched observations over 4442 observations Voting Analytics in the period 2007-2013).

Bach and Metzger (2019) also report results on some ISS based subsamples. The overlap between our sample and each of their ISS subsamples is more difficult to calculate. One needs to take into account that: they focus on the 10 proposals with most favorable votes out of which only 6 are part of the G index; they restrict their sample to proposals for which they can measure implementation (58% coverage); they have a different time span to ours; they only report results on specific subsamples. According to this, we calculate that the fraction of our observations in each of their subsamples represents between 8%-9% of our sample.

So overall, the overlap between the two samples is quite small and we show that there is no evidence of density discontinuity change in our sample using either the *Simple* or *Adjusted* votes (see Figures A1b and A1c as well as External Appendix Tables A5A and A5B).