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

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April 2016

Abstract

This paper studies the deterring effects of anti-takeover provisions on takeovers and identifies the channels through which they create or destroy value for firms, as well as for the economy as a whole. We provide causal estimates – that also deal with the endogenous selection of targets – showing that voting to remove an anti-takeover provision increases the probability of a takeover by 4.5% and leads to a 2.8% higher-than-expected premium, the latter resulting from increased competition for less protected targets. We also find evidence of net value creation in the economy as stemming from more related acquisitions and targets being matched to more valuable acquirers.

* We would like to thank Laurent Bach, Bernard Black, Marco Becht, Francois Derrien, Andrey Golubov, Denis Gromb , Nadia Malenko, Marco Pagano, Miikka Rokkanen, Yishay Yafeh and seminar/conference participants at LSE, HEC, LBS, Exeter, Carlos III, INSEAD, Tilburg University, University of Rotterdam, CEPR Adam Smith workshop, SFS Cavalcade, ECGI, CGCG Colloquia, FMA Europe for helpful comments and suggestions.

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I. Introduction

Anti-takeover provisions (poison pills, staggered boards etc) are major governance mechanisms that affect firm value (e.g. Gompers, Ishii, Metrick, 2003; Cuñat, Gine, Guadalupe, 2012). It is argued that they allow managers to bargain for a higher price in the event of a hostile takeover, and that they allow for more long-term investment and hence value creation (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 2015). This trade-off can have differential effects on aggregate economic value and on the distribution of gains between acquirers and targets, depending on whether they deter or encourage value-creating takeovers (Morck, Schleifer and Vishny, 1990; Maksimovic and Phillips 2001; Burkart, Gromb and Panunzi, 1998; Schoar, 2002). It has also been shown that reducing the threat of a takeover can destroy value by weakening managerial discipline (Scharfstein, 1988; Bertrand and Mullainathan, 2003). Given the importance for value creation or destruction at the firm and economy-wide levels, substantial theoretical and empirical attention has been devoted to the effects of anti-takeover provisions. However, there is little causal evidence on the effect anti-takeover provisions on the takeover premium, on the takeover probability itself, and on the type of mergers that they deter/allow to happen.

The goal of this paper is to provide causal estimates that allow us to assess the effects of anti-takeover provisions on deterring takeovers, as well as identifying the channels through which they create or destroy value for firms and the economy as a whole.

The expected takeover premium from adopting anti-takeover provisions can be broken down into three components: the causal effect that the anti-takeover provision has on the *probability* of being acquired (i.e. the deterrent effect); the causal effect of the anti-takeover provision on the *premium* paid if the acquisition is successful (i.e. the price effect); and lastly the *selection* effect arising from the fact that adopting an anti-takeover provisions changes the population of firms that become targets. The first two are important in themselves and each has given rise to a substantial literature. The third, albeit seldom discussed, is essential in as much as one cannot infer the takeover premium from comparing firms that are taken over with and without anti-takeover provisions because the population of target firms changes when anti-takeover provisions are in place.[1] It is important that we control for this form of selection, which is inherent to the problem studied (rather than an issue with the data) since knowing the type of selection at work allows us to understand which deals are favored when anti-takeover provisions are present. This empirical decomposition reveals the different elements at play and allows us to structure the analysis to provide a comprehensive answer to the question of how anti-takeover provisions affect the takeover market and subsequent value creation.

Our data consists of all shareholder-sponsored proposals to remove an anti-takeover provision voted on at annual meetings of S&P 1500 firms between 1994 and 2013 (shareholder-sponsored proposals to adopt those are virtually nonexistent). The

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

total sample covers 2,820 proposals in 931 different firms. To identify our effects we use the vote outcome on proposals to *remove* anti-takeover provisions.

Two different components are necessary to provide causal estimates in this setting. First, we need some form of random assignment in the adoption of anti-takeover provisions to establish causality. For that, we start present regression discontinuity results for the takeover probability and expected premium. We rely on vote outcomes being random in a narrow interval around the majority threshold but leading to a discrete change in the probability of dropping a provision (see Cuñat, Gine, Guadalupe, 2012, 2013). We then generalize the results and extrapolate them to firms with vote outcomes away from the discontinuity, using the estimation strategy proposed in Angrist and Rokkanen (2014).[2] Second, we need a way to correct for inherent problems of selection in the estimation of the premium effect. For this we estimate sharp upper and lower bounds for the causal effect of anti-takeover provisions on the takeover premium (Lee, 2009). This addresses the co-determination of premiums with the population of firms taken over. Accounting for these two components allows us to estimate the effect of anti-takeover provisions on the different elements that contribute to shareholder value, obtaining their causal effect for all firms that hold a shareholder vote on the issue, not just those around the vote discontinuity.

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, both for firms around and away from the majority threshold. Around the majority threshold

[2] Angrist and Rokkanen (2014) 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 that uses 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.

(classic regression discontinuity), passing a proposal to drop an anti-takeover provision increases the likelihood of experiencing a takeover within five years by 9%. It also increases the shareholder value of future expected takeover premiums by 4.2%. 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 takeover within five years by 4.5% and increases the expected takeover premium by 2.8%.[3]

Going beyond average effects, we also are able to state for which types of firms (as in firms with different vote outcomes) the effects of passing a proposal are largest and smallest. This is important given that firms with little (versus substantial) support to remove an anti-takeover provision need not benefit from it in the same way a priori. We find that all types of firms that fail to pass a proposal to remove an anti-takeover provision would have benefitted from removing it. The largest benefits from passing a proposal accrue to firms that passed them by small or moderate margins (up to 20% above the majority threshold); little benefit accrues to firms with more than 20% votes in favor above the threshold. We also 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, so this is not about "voting" per se but about the takeover channel.

[3] These values are all intent to treat (ITT) values of voting to remove the provision. For the treatment on the treated that goes through implementation, they need to be re-scaled by the inverse of the jump in the probability of the implementation of the proposal. In practice, at the discontinuity this implies multiplying them by a factor of 2 (using estimates from Cuñat, Gine and Guadalupe, 2012, 2016). Outside the discontinuity the conversion factor ranges between 1.2 (using estimates from Popadak, 2014) and 1.7 (using estimates from Bach and Metzger, 2015).

The causal effect on the expected/unconditional premium is not subject to the inherent selection problem of anti-takeover provisions altering the population of firms that are taken over. This is because the unconditional premium includes both firms that experienced a takeover (and realized a takeover premium) and those that did not (with a takeover premium of zero), so the populations of the treatment and control groups are comparable. However, we would also like to know whether a given firm is able to obtain a higher or lower premium if it drops the anti-takeover provision. For this, we cannot just compare the premium of firms that are taken over with or without anti-takeover provisions, since we need to account for different selection patterns in the two groups. In the absence of a second instrument that affects the premium but not the probability of a takeover, we provide estimates that account for selection using the sharp bounds methodology developed by Lee (2009) and apply these bounds to our estimates. We find that the causal effect of voting to remove a provision on the conditional premium is between 0.3% and 5.5%: i.e. it is always positive, suggesting that more shareholder value is created in less protected firms. To benchmark these effects, consider that Eckbo (2009) finds that the average difference in the premium between a hostile and a friendly takeover to be 5.8%, that between a public and a private acquirer 4.9%, and that between a multiple and single bidder contest 7.8%. Overall we find that having anti-takeover provisions reduce takeover probabilities and premiums.

Next we investigate the determinants of the positive premium result, which is inconsistent with the argument that anti-takeover provisions give managers bargaining power to extract higher premiums. Our evidence suggests that the higher premium is linked to more competition for less protected firms: they have more bidders, more unsolicited bids, more challenged deals and more deals paid in cash. We also find that while we cannot sign the acquirer premium, the total value creation (adding up the dollar value of the acquirer and target premia) is positive. This net value creation in the economy seems to come partly from the fact that acquisitions are more likely to take place in related industries (with higher potential for synergies) and partly from the fact the (positively selected) targets are matched to more valuable acquirers.

Lastly, we use the empirical framework/decomposition to obtain what fraction of the overall increase in value from removing anti-takeover provisions comes from its different components. We find that the increase in value operates largely via quantities: 49% of the shareholder value comes from the increased probability of mergers. In our preferred (and most conservative) specification, the premium effect represents between 1% and 27% of the shareholder value, and therefore the selection effect is positive and between 24% and 49% of the overall value created. Hence, accounting for selection is key to understanding how takeovers create value in the market.

An important contribution of this paper is that our methodology addresses the endogeneity of adopting/removing anti-takeover provisions as well as the sample selection of who becomes a takeover target. In fact, we are able to provide an estimate of the role of sample selection in overall value changes. The earlier literature on this question suggests that anti-takeover provisions are not randomly adopted, hence correlations are likely to be subject to endogeneity bias.[4] We are able to provide a quantitative estimate of the role of selection.

In contrast to the existing literature which provides scant causal evidence, we find large and significant effects of removing anti-takeover provisions. This is all the more important given that studies of the correlation between anti-takeover provisions and takeover probabilities and premiums have often found contradictory or no effects.[5] We also explain what seems to be driving the clear and strong premium effects (more competition, better matching) and that the types of mergers that take place under less protection lead to net economic value creation.

Our paper proceeds as follows: the next section provides a framework to decompose the unconditional premium. Section III discusses our main identification strategy; Section IV presents the data, and Section V the results on unconditional premia and takeover probabilities. In Section VI we provide bounded estimates for the treatment effect on the premium and uses all the estimates in our decomposition. Section VII concludes.

II. Framework: Decomposing the Unconditional Premium

II.1. Dealing with Endogeneity and Selection

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[4] For example, Malatesta and Walkling (1988) show that firms adopting poison pill defenses are much more likely to become the target of takeover activity; Comment and Schwert (1995) show that the proportion of pill adopters that are in play increases after adoption of a poison pill. Bange and Mazzeo (2004), also highlight the selection effects of anti-takeover measures.

[5] The literature generally finds that adopting an anti-takeover provision has a negligible or positive 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). Regarding takoevoer probabilitis, 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. Using an instrumental variable identification strategy Karpoff et al. (2015) find a causal negative effect of anti-takeover provisions on takeover probabilities.

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

We define the treatment dummy variable D, which takes 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):[6]

$$Y^* = D\beta + X\mu_1 + U \qquad \text{(underlying premium)}$$
$$Z^* = D\gamma + X\mu_2 + V \qquad \text{(latent merger propensity)}$$
$$Y = 1[Z^*>0] \cdot Y^* \qquad \text{(unconditional premium)}$$

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:

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

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

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

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 an anti-takeover provision in place.

The reason 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 D randomly assigned (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 (1) above:

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

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). The second term measures the impact of the change in merger probabilities (times the premium for the untreated group). The third is a selection term that captures the change in the population of firms that are subject to a takeover offer.

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

III. Identification Strategy

We now discuss how to identify the impact of an additional anti-takeover measure on the two outcomes of interest which we can directly estimate causally. These are the takeover probability ΔP and the unconditional takeover premium ΔY (as defined in 1a and 1b above).

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:

$$y_{ft} = K + D_{ft}\theta + u_{ft} \quad (3)$$

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, such that $E(D_{ft}, u_{ft}) \neq 0$.

In order to estimate the causal effect of anti-takeover provisions on the incidence of takeovers and unconditional premiums, we start presenting results from a classic regression discontinuity design, and then build on this using the Angrist Rokkanen (2014) identification strategy.

III.1. Classic regression discontinuity design

Identification in the classic regression discontinuity design setting exploits the fact that while, on average, firm characteristics and vote outcomes are likely correlated with unobserved variables, in an arbitrarily small interval around the majority threshold assignment to treatment can be considered as random. This assumes that the relationship between firm characteristics and shareholder votes is continuous around the threshold (which can be tested for observable variables), while the probability of implementing an anti-takeover proposal jumps discontinuously.[7] A discontinuous increase in the outcome variable around the passing threshold can therefore be interpreted as caused by the treatment. Therefore differences in y_{ft} between proposals

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

to drop anti-takeover defenses that either pass or do not pass by a narrow margin of votes give us a non-parametric causal treatment effect.

One can also estimate this using the whole data, by fitting flexible functional forms for the relationship between the vote and the dependent variable in different ways. Lee and Lemieux (2010) suggest using different polynomials for observations on either side of the threshold.[8] An alternative approach is to run a local regression on an optimally calculated interval around the discontinuity, as initially proposed by Imbens and Kalyanaraman (2012) (IK) for a local linear regression approach. Similarly, Calonico, Cattaneo and Titiunik (2014) (CCT) approximate the flexible regression function on either side of the majority threshold by a second order weighted polynomial regression over an optimal bandwidth that balances efficiency and bias.[9] Below, we report results using different methods: differences in means on an increasingly small vote interval, regressions with vote polynomial controls as in Lee and Lemieux (2010) and a hybrid method that involves a local weighted regression on an optimal bandwidth as proposed by Imbens and Kalyanaraman (2012) and Calonico, Cattaneo and Titiunik (2014).

III.2. Identification Strategy – Extrapolation beyond the discontinuity

The downside of the classic regression discontinuity design is that identification is local and identified from firms with vote outcomes around the discontinuity. However, as we would like to obtain causal estimates for other types of

[8] 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).

[9] 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.

firms as well, the identification strategy in Angrist and Rokkanen (2014) allows us to do that. Their approach exploits the fact that in the regression discontinuity setting, unlike in most applications, the variable that assigns observations to treatment is known. The problem with extrapolating beyond the discontinuity is that outcomes are not independent of the running variable (the vote outcome). Treatment in our case is passing the shareholder proposal (D=1), so that the only thing that determines assignment is the observed vote outcome. Angrist and Rokkanen (2014) remark that if one could eliminate the relationship between assignment variable and outcomes by conditioning on some covariates, then one could make a conditional independence assumption (CIA) to obtain causal estimates. That is, for causal inference, the outcome needs to be independent of the running variable, conditional on a set of controls x_{ft} :

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

Unlike an OLS regression (where one does not know the assignment variable), in a regression discontinuity setting this condition is testable by showing that the vote does not affect the outcome variable after controlling for an adequate set of x_{ft}. 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_i.

$$0 < P[D=1 \mid x_{ft}]$$

If we can find a model x_{ft} in which the conditional independence condition and the common support condition both hold, then a causal estimate of the effect of the treatment on the outcome variable can be obtained with standard matching estimators using the variables x_{ft} validated by the regression discontinuity setting. In this sense, the regression discontinuity design provides a diagnostic tool to test the validity of the conditional model that then can be used in a matching estimator. Once the matching samples are constructed, we can also measure heterogeneous effects for different vote outcomes.[10]

There are several advantages of using this matching method. First, it allows us to determine (and test) a valid model to extend the analysis to a broader sample of firms: We can go beyond the local interpretation of the regression discontinuity estimator while retaining a causal interpretation. Second, using our estimates we can build counterfactuals at each vote level that predict what would have happened had that firm voted differently. This implies that we can assess whether there are heterogeneous effects of anti-takeover provisions for different levels of vote support. Finally, we are able to use the available sample in a more efficient way. This is particularly valuable when studying a relatively rare event such as the takeover of a large listed firm.

III.3. Bounding the causal and the selection effects.

The existing literature focuses on the effect of anti-takeover provisions or other treatments 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 comes from 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

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[10] Angrist and Rokkanen (2014) 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 use a rescaling factor of the probability of implementation conditional on passing to obtain an estimate of the impact of implementation. This conversion factor ranges between 1.2 (using estimates from Popadak, 2014) and 1.7 (using estimates from Bach and Metzger, 2015).

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 experiences 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 causal 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 observed $E[Y \mid D=1, X, V> - X\mu_2]$ (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 making 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).

IV. Data Description and Sample Characteristics

We construct a dataset that spans 20 years of voting data from ISS-Riskmetrics.[11] The data provides information on all the proposals voted in the S&P1500 universe and an additional 500 widely held firms. Our sample consists of 2,820 shareholder-sponsored proposals voted on at annual meetings to change the anti-takeover structure of the firm. We restrict the analysis to the set of anti-takeover provisions that make up the G-index as defined by Gompers et. al. (2003). To obtain our treatment indicator (D), we use information on vote outcomes adjusted by majority rules and vote base (votes cast or votes 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 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. 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 8). We also obtain from SDC information 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

[11] For the period 1997-2013 we use the 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).

SIC industry. Financial information comes from Compustat and ownership information from Thomson 13F.

Table 1 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. Between 1994 and 2013 the share of votes in favor of the proposal went from 28% in 1994 to 65% in 2013, so that the percentage of proposals passed rose from 5%-7% in the mid-90s to 73% in 2013 (Table 1 A). The regression discontinuity estimate is identified out of proposals with a close-call outcome: 30% (15%) of the proposals in our sample fall within ten (five) percentage points on either side of the majority threshold.

Panel B of Table 1 shows that the average probability of a firm experiencing a takeover over the five years following a shareholder vote is 13%, with peaks in 1995 and 1996 due to many successful mergers in the 1998-2000 period. We have a total of 135 (79) targets within 10 (5) percentage points of the majority threshold. The mean conditional premium (the premium paid conditional on a successful merger) is 32.36% and the mean unconditional premium (that assigns zero premium to the unsuccessful mergers) is 4.83%.

Table 2 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. The size difference is likely to be driving some of the other differences in firm characteristics. For instance, firms in the voting sample have lower Tobin 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 the Angrist Rokkanen method allows us to obtain results for firms subject to an anti-takeover removal proposal, one should exercise caution in extrapolating the results to other firms that have never had such a shareholder proposal.

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

V.1. Preliminary tests to validate the identification strategy

Before presenting results using the classic regression discontinuity design and Angrist and Rokkanen (2014) identification strategies, we need to run a series of tests to confirm that this is a good setting to use these methods. First, Appendix Table A1 shows that there are no pre-existing differences in firm characteristics (or trends in firm characteristics) around the majority threshold, which is an assumption of the regression discontinuity design. To start, 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 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. The absence of observable differences around the discontinuity is important for our identification strategy.

Second, we test that the distribution of the frequency of votes is continuous around the discontinuity. A discrete 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.[12] Appendix Figure A1a shows a smooth overall distribution of votes. Figure A1b shows the formal continuity test proposed by McCrary (2008) that rejects the discontinuity of the density function at the majority threshold. We also tested for discontinuity in the votes for sub-periods and by proposal, and found no evidence of manipulation in any subsample (see Appendix Table A2). These tests confirm that this is a good setting to apply the classic regression discontinuity and the identification strategy proposed by Angrist Rokkanen (2014).

V.2. Classic Regression Discontinuity Design

We now present the estimates of the effect of passing a proposal to remove an anti-takeover provision on 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 threshold (% votes above pass in the horizontal axis). The dots represent simple means

[12] This is why one cannot use management proposals because these have serious manipulation as shown by Listokin (2008)

in bins of 2.5% 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 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 truncation upwards in the function, suggesting a positive causal effect of voting to drop the provision on the takeover probability. The size of this truncation 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 shift at the discontinuity, suggesting that voting to drop a provision increases the unconditional premium firms expect to receive.

Table 3 presents regression estimates of the effect at the discontinuity seen in Figures 1a and 1b using four different estimating methods. Columns 1 to 5 of Table 3 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 6 and 7 of Table 3 show the regression discontinuity estimate using polynomial controls of order two and three to each side of the discontinuity. Columns 9 and 10 report the results of running local regressions on an optimal bandwidth around the discontinuity.

Column 9 reports the Imbens and Kalyanaraman (2011) local regression analysis, column 10 reports the Calonico, Cattaneo and Titiunik (2014) estimate.

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). This effect becomes bigger and more significant when it is computed at increasingly narrow intervals. At the narrowest intervals, the differential probability of experiencing a takeover within five years of the vote is between 10% and 12%.[13] The estimates using the polynomial controls (columns 6 and 7) suggest that firms that pass a proposal to drop an anti-takeover provision have an additional cumulative probability between 10% and 12% to be the target of a takeover within five years following the vote. The point estimates in columns 9 and 10 are around 9%, which is about 2% below the previous ones but not statistically different.

Overall estimates range between 9.1% and 12%, which is a sizeable effect, when compared with the sample-wide average five-year probability of a takeover of 13% in the sample, or 18% within 5% of the majority threshold.

In Panel B of Table 3 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 5 of Table 3 Panel B show the fully non-parametric means comparison approach. The effect of dropping an anti-takeover provision is an

[13] 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

increase in the expected premium of between 4% and 6%. 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% (IK) and (CCT). Again, these are substantial effects against an average unconditional premium of 0.4% in the sample as shown in column 1.

While causal, 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 contested votes. To answer this question we turn to the Angrist and Rokkanen (2014) estimation approach in the next section.

V.3. Extrapolating the results beyond the vote threshold (Angrist Rokkanen, 2014)

V.3.1. Testing the Conditional Independence Assumption

As described in Section II.2, the first step to apply the Angrist and Rokkanen (2014) identification strategy is to test whether we can plausibly make a Conditional Independence Assumption (CIA). As mentioned earlier, an important advantage of this method is that in the regression discontinuity setting the CIA can actually be tested. This is what we do in Table 4.

The goal of Table 4 is to test whether conditioning on an explicit model for the determinants of takeover we can 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 Q in the industry and average market value in the industry) and year dummies.

Columns 1 and 3 (5 and 7) of Table 4 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. This reflects the fact that the vote outcome and our dependent variables are not independent. However, once we condition on our model (in even numbered columns of table 4), the correlation becomes statistically insignificant and the point estimates get closer to zero. For example, in column 3 of Table 4 there is a highly significant -0.0018 coefficient on the vote variable that drops to an insignificant -0.0001 when we include the variables in our model. This supports the assumption that vote and takeover probability are conditionally independent in the D=1 (votes that passed) region. Column 2 shows that vote and takeover probability are conditionally independent also in the D=0 (votes that did not pass) region. And the same is true for the unconditional premium (columns 6 and 8).

Following Angrist and Rokkanen (2014), we complement formal CIA testing with a graphical tool, shown in Figure 2, that plots the residuals of regressions that include the covariates in Table 4 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 2.5% bins and a local linear regression estimation of the outcome variables as a function of the vote. We see that the estimated relationship is quite 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.

Once we have made the running variable –which determines assignment to treatment—conditionally independent of outcomes, Angrist and Rokkanen (2014) propose 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). Figure 4 shows a substantial amount of overlap in the propensity score of treated and control groups. A formal test of balancing (Dehejia and Wahba, 1999) also shows that covariates are balanced.

V.3.2. Results beyond the discontinuity

After testing for the CIA, and establishing that we have common support so that we can match firms on either side of the discontinuity based on our model, we are in a position to extend our earlier regression discontinuity results to the sample of firms away from the discontinuity.

First, like Angrist and Rokkanen (2014), we use the estimated propensity score 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. Results are shown in Table 5 panel A. We find that passing an anti-takeover provision leads to a 4.5% increase in the probability of takeover (Column 1) and a 2.8% increase in the unconditional premium (Column 3).

We obtain similar estimates if we add to the reweighted regression the variables included in the CIA model as controls (columns 2 and 4). We also get very similar results if we use a different matching estimator, like the nearest neighbor matching estimator (Table 5 panel B) with a 3.4% additional takeover probability and a 2.5% increase in the unconditional premium.

Three results are noteworthy here. First, the matching estimates (away from the discontinuity) are smaller than the regression 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.[14] 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 increases that probability by 4.5 percentage points. Correspondingly, the mean unconditional takeover premium is 4.8%, and voting to remove an anti-takeover provision increases the premium by 2.5 percentage points. Third, these matching estimates can be interpreted as *causal* on a broad set of firms. While our sample is not the full set of

[14] Unfortunately, one cannot apply the Angrist and Rokkanen (2014) strategy to returns (CAR) on the day of the vote itself. This is because, as Cuñat, Gine and Guadalupe (2012) explain, while the CAR for firms at the discontinuity is causal and is the outcome a surprise 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.

listed firms in the USA, it represents a substantial share of the S&P 1500 index (931 distinct firms).

In addition, once the CIA is established, we can provide not only mean estimates of passing a provision (those in Table 5) but also an estimated effect at each point of the vote distribution, such that we can identify heterogeneous effects at different points of the vote distribution. We do this by fitting a linear model that uses the same variables and coefficients as in Table 4 and extrapolating the model to the other side of the discontinuity. This 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? This is shown in Figure 3 panels A and C. The dark/black lines are the empirical estimated takeover probability (Panel A) and premium (Panel C). The lighter/red line shows for each vote outcome below the majority threshold what the takeover probability (Panel A) and the unconditional premium (Panel C) would be had they passed the proposal (based on our model).[15] 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.

In turn, Panels B and D of Figure 3 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 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

[15] More specifically, we predict the outcome on the right-hand-side using the left-hand-side model (and vice-versa) for each observation and then smooth the prediction using the same procedure as in Figure 1 and the dark line of this figure.

probability and expected premium had they not passed the provision. For firms with votes 25% higher than the majority threshold, the difference tends to disappear. 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 for these firms seems independent of the actual passing of the proposal.

Finally, we evaluate whether these effects are the result of voting on any proposal--rather than about voting to remove an anti-takeover provision. Appendix Table A3 replicates the analysis in Table 5 using other (non anti-takeover) shareholder votes, and finds no effect of those proposals on either the takeover probability or the premium. This suggests that what drives our main result is not just some signal around shareholder activism (as proposed by Bach and Metzger 2015), but an effect that goes through the removal of an anti-takeover provision.

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

VI.1. Causal Effect on the Premium β

In Section V we obtained causal estimates for the effect of treatment on the unconditional premium ΔY and the takeover probability Δ P. However, we also seek to recover the effect on the premium itself, β. i.e., 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 Δ P is quite large) it is not possible to infer the value for the causal 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 the monotonicity of the effect of anti-takeover provisions. 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.[16] Table 6 Column 1 estimates the bounds proposed by Lee (2009). The procedure requires that the first stage that estimates a selection equation can be interpreted as causal. We achieve this by using the same linear reweighting as in Table 5. This method yields estimates of β that are bounded between 0.3% and 5.5%. This means that the direct premium effect of dropping an anti-takeover provision on a given targeted firm is positive. Although the bounds include the possibility of a very small positive premium effect (0.3%), the lower bound is not negative. We explore a number of hypotheses that may explain this non-negative effect below.

The remaining columns in Table 6 test additional premium measures using different windows for the purposes of robustness. Column 2 reports the effect on the target premium computed as the change in price one week before announcement until completion (i.e. a shorter run-up). Column (3) shows abnormal returns using the FFM factors in a short window (+/- 5 days) around the announcement. Column (4) shows cumulative abnormal returns in a very long window: from the vote to the takeover announcement day plus one. This addresses the fact that some of the expected effect of

[16] 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).

the merger could have been incorporated on the day of the vote. Columns (5) and (6) report the cumulative abnormal 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 unambiguously show a positive causal premium.[17]

VI.2. Decomposition

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).[18] Results can be seen in Table 7.

We find that 49% 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 (0.3), we find that the remainder of the takeover premium is driven by selection (49%) and 1% is driven by the causal premium itself. With our upper bound estimate for β (5.5), 27% of the unconditional premium is explained by the effect on premiums holding the population constant, and 24% by selection. Using other measures for the estimated premium in Table 8 yields even an larger contribution of the premium effect (given those estimates are larger especially at the lower bound) and also significant selection effects at both bounds.

[17] Throughout the paper we use raw returns after checking that our results are not driven by outliers. Appendix Table A4 replicates Table 6 with each premium variable winsorized at 1% with almost identical results.

[18] We take the estimates for Δ P and ΔY from Columns 1 and 3 of Table 5. 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 6.

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 and selection effects. 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.[19]

VII. Understanding Positive Target Premiums

We next explore (in Table 8) the possible drivers for the positive target premium that we found by looking at what else changes when firms pass such a proposal. Given that the population of merged companies changes with the removal of an anti-takeover provision, we also analyze these effects using Lee bounds (full descriptive statistics of all variables can be found in Appendix Table A6). [20]

First, we find no clear effect on the acquirer's premium (the estimates vary a lot depending on the premium measure used). In Panel A columns (1) to (4) we cannot sign the effect for different acquirer premium measures. This suggests that the higher target premium following the vote to remove the provision is not exclusively driven by a transfer of premium to the target from the acquirer. In columns (5) to (8), we find evidence consistent with the higher premium deriving from the presence of more competition for target firms: They receive between 1.5 and 2.6 more bidders (the mean number of bidders is 1.22) and the probability of receiving a challenged deal is between 11% and 17% higher (over a mean probability of 15%). These are statistically

[19] Our data also differ from the existing studies in that they cover a much more recent period, and a broader set of firms and exclude management proposals.
[20] For robustness, Appendix Table A5 replicates the results in Table 8 with dependent variable winsorized at 1% to confirm that none of the results in Table 8 is driven by extreme observations.

and economically significant effects. They also seem to be the target of more unsolicited deals (with 5% to 9% higher probability, although the result is not significant) and for a higher fraction of the deal to be settled in cash (indicating more competition as in Offenberg and Pirinsky, 2015). All these are consistent with these firms being more “attractive”, hence triggering more competition. Previous literature on auctions has shown that the effect of the number of bidders is expected to be more important than the individual bargaining power of each of them (Bulow and Klemperer 1996), consistent with our results (see Gilson and Schwartz 2015 for a specific application to defensive strategies).

We also find that these deals are potentially more “value creating”: Column 1 of Panel B shows that bidder and target firms are between 17% and 23% more likely to belong to the same two-digit SIC industry (relative to a mean of 62% in the sample) indicating that the deals are more likely to be related mergers rather than potentially value-destroying 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 (relative to a mean of 1.06 and standard deviation of 4.3). This suggests that the positively selected targets are matched to relatively more valuable and potentially productive acquirers. Interestingly, while we could not sign the effect on the acquirer’s premium, if we add up the dollar value of the premium of the target and the acquirer, the upper bound estimate of the effect is quite large and positive in dollar value (between US$306 million and US$7.2 billion higher in column 3) and as a share of the total market cap (between 3% and

14% of the target and acquirer's value column 4). The lower bound of the synergy estimates is not significant, so one has to take this into account, but the results suggest that, if anything, there is net value creation in the market when anti-takeover provisions are removed, and that this is not just one party gaining at the expense of the other. 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.

VIII- Conclusion

In spite of the attention devoted to the consequences of anti-takeover provisions, there is still little causal evidence on their impact on the takeover probability, the takeover premium, or on their potential for value creation/destruction for the economy as a whole.

This paper provides causal estimates – that also deal with the endogenous 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 more protected are "worse" on several dimensions: they involve worse targets, smaller acquirers, are more likely to be between firms in unrelated businesses (and hence 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 this is at least partly driven by the fact that more protected firms attract less competition. This is likely because the worse governance makes it more difficult for synergies to be realized, hence deterring bidders.

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

The Angrist and Rokkanen (2014) identification strategy allows us to obtain causal effects away from the discontinuity, as well as to explore the possible heterogeneity of our results for different kinds of firms. We find that our results apply through most of the support of vote outcomes. So, while our results cannot necessarily be extrapolated to firms that never hold such shareholder votes, 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? We conjecture that the answer has to do with internal governance and the political economy of decision making within firms. 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 2.5% vote intervals.

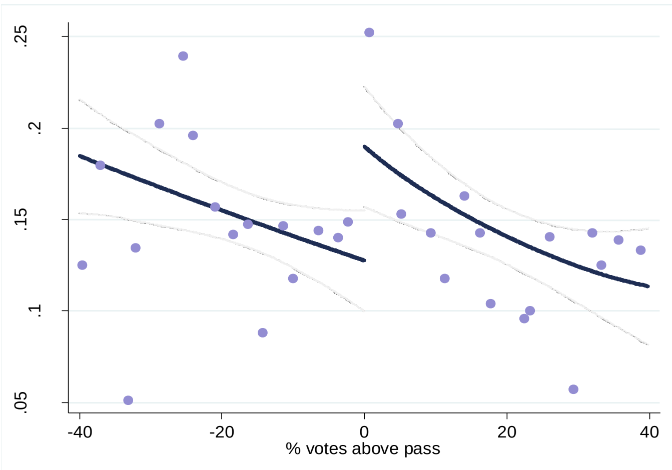


Figure 1b: Unconditional Premium

Linear regression using the Imbens and Kalyanaraman (2012) approach to select the bandwidth. Dots represent the simple means by bins of 2.5% vote intervals.

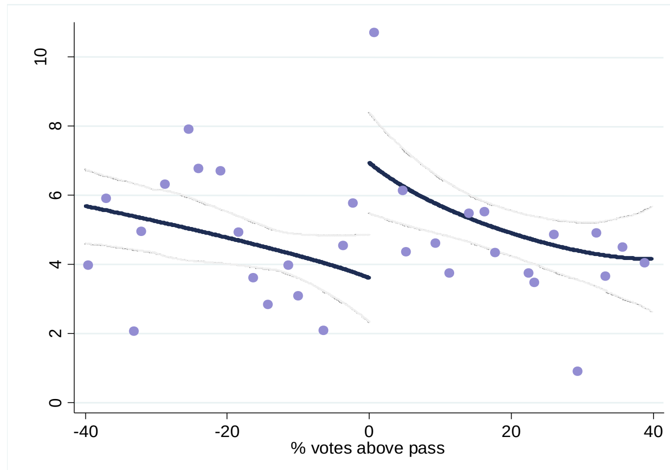


Figure 2a: Conditional Independence Test Merger Probability

Residuals of two independent linear models (one to each side of the discontinuity) using the same covariates as in the matching model

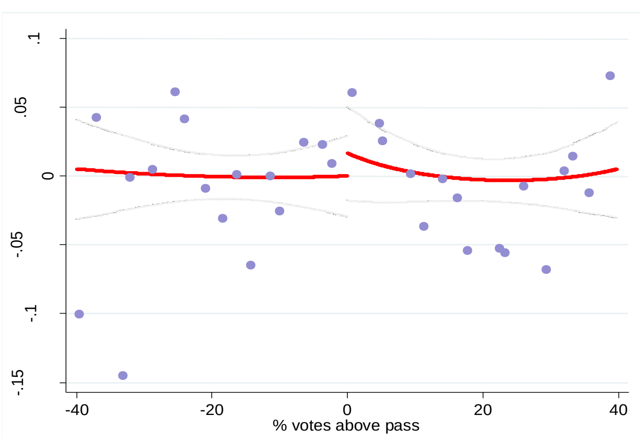


Figure 2b: Conditional Independence Test Premiums

Residuals of two independent linear models (one to each side of the discontinuity) using the same covariates as in the matching model

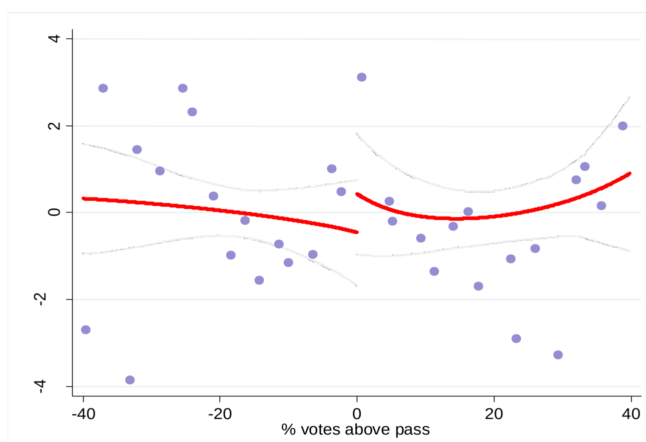


Figure 3a: Extrapolation Merger Probabilities LHS

Extrapolation of the linear model for merger probability of the right hand side to the left hand side

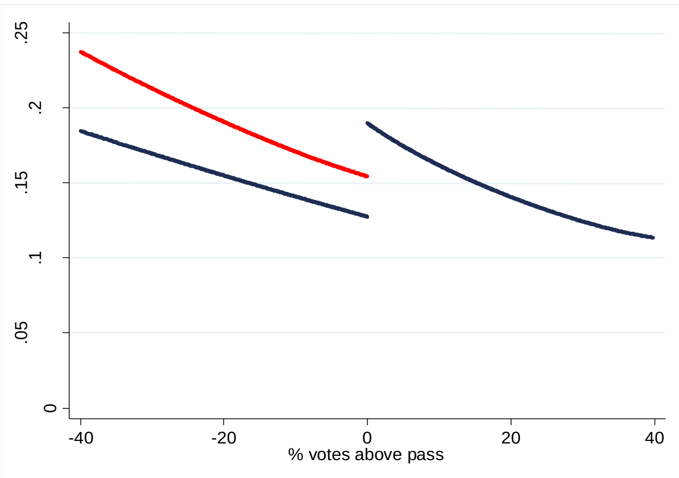


Figure 3b: Extrapolation Merger Probabilities RHS

Extrapolation of the linear model for merger probability of the left hand side to the right hand side

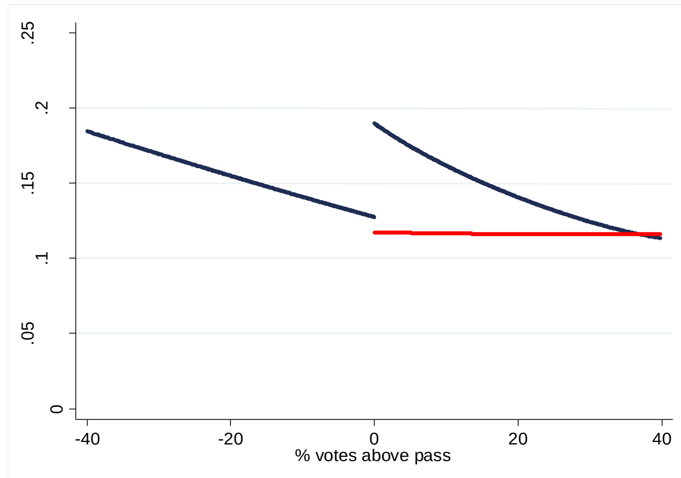


Figure 3c: Extrapolation Unconditional Premium LHS

Extrapolation of the linear model for the unconditional premium of the right hand side to the left hand side

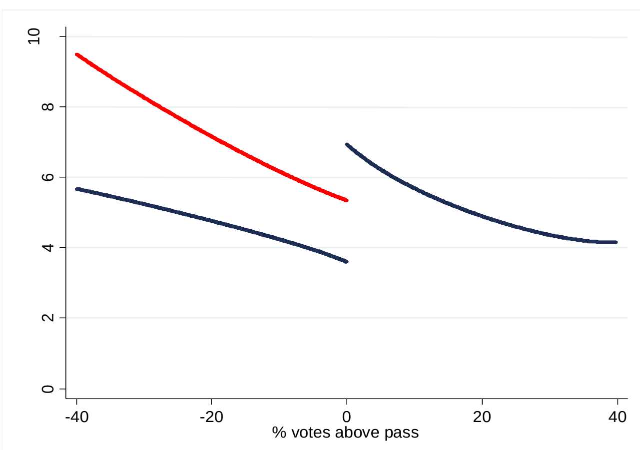


Figure 3d : Extrapolation Unconditional Premium RHS

Extrapolation of the linear model for the unconditional premium of the right hand side to the right hand side

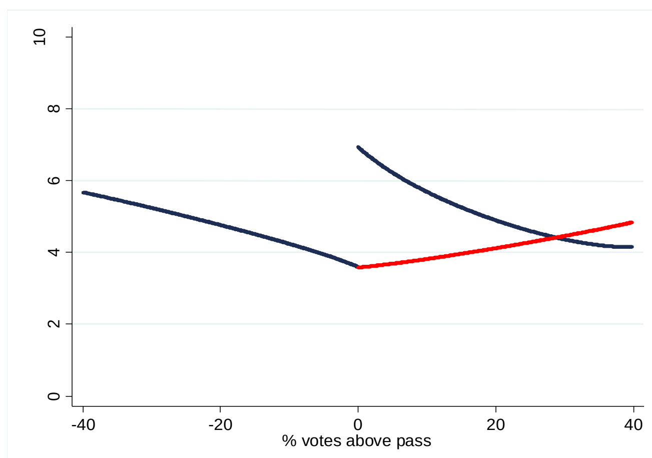
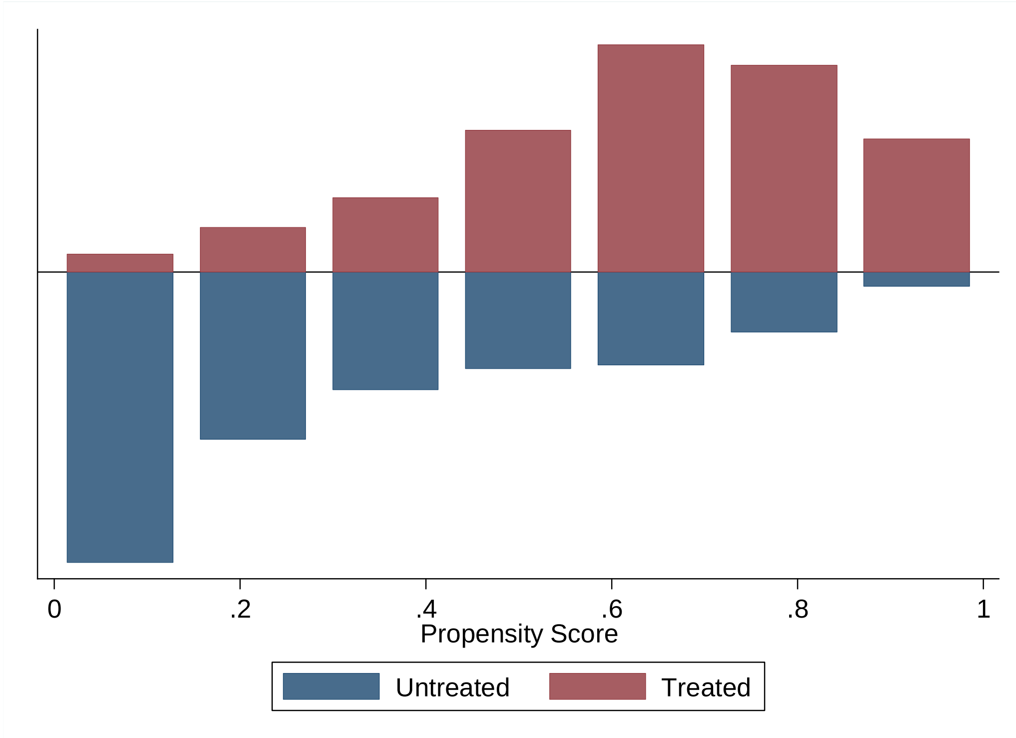


Figure 4: Histogram of Estimated Propensity Scores



Tables

Table 1 A
Shareholder Anti-takeover Proposals

This table displays the frequency of proposal to remove anti-takeover provisions, the percent of proposals passed and the average support over time. Data is collected by ISS-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 2820 voted proposals.

Year	Voted Proposals	Passed Proposals	Percent. Passed Proposals	Average Vote Outcome	Num. Proposals (-5, +5)	Num. Proposals (-10, +10)
1994	158	9	5.70%	27.9%	15	31
1995	209	15	7.18%	28.1%	18	42
1996	169	16	9.47%	32%	24	47
1997	114	33	28.95%	40.9%	22	41
1998	123	35	28.46%	41.3%	17	35
1999	144	51	35.42%	44%	38	56
2000	128	62	48.44%	46.8%	33	50
2001	127	65	51.18%	47.9%	34	63
2002	146	93	63.70%	53.7%	24	49
2003	183	129	70.49%	57.7%	35	70
2004	137	88	64.23%	57.6%	17	35
2005	131	86	65.65%	56.9%	13	40
2006	148	90	60.81%	56.5%	15	50
2007	140	73	52.14%	51.6%	13	30
2008	145	88	60.69%	57.6%	20	45
2009	190	102	53.68%	54.2%	40	61
2010	141	71	50.35%	53.1%	25	49
2011	117	51	43.59%	50.6%	11	22
2012	107	70	65.42%	61.%	7	14
2013	63	46	73.02%	64.9%	8	12
2014	Na	Na	Na	Na	Na	Na
Total	**2820**	**1273**	**45.21%**	**48.2%**	**429**	**842**

Table 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 frequency of mergers announcements for the full sample and for those votes within interval of (-5,5) and (-10,10) relative to the threshold. Column 5 presents the conditional premium for those firms that did merge, while column 6 presents the unconditional premium which includes the whole sample. 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		Num of Announ per Year
					Mean	Median	Std Dev	Mean	Std Dev	
1994	18%	29	2	3	28.74	24.58	21.13	5.27	14.29	38
1995	29%	62	2	9	31.89	32.24	22.72	9.46	19.09	45
1996	29%	50	10	18	35.42	32.24	38.16	10.48	26.22	39
1997	23%	27	9	13	33.74	34.26	23.17	7.99	18.20	44
1998	18%	23	3	5	30.14	32.24	14.17	5.63	13.24	158
1999	13%	19	7	8	35.43	32.42	35.99	4.67	17.54	90
2000	14%	18	12	12	33.1	37.77	12.64	4.65	12.44	110
2001	8%	11	4	7	31.78	32.05	13.92	2.75	9.79	26
2002	15%	23	2	9	25.77	27.6	15.2	4.06	11.13	17
2003	15%	28	7	12	28.18	25.54	20.74	4.31	12.93	19
2004	9%	13	1	3	42.88	37.91	39.67	4.06	17.26	38
2005	11%	15	0	2	38.58	40.88	17.45	4.4	13.59	127
2006	16%	25	4	13	21.94	21.72	27.07	3.70	13.70	96
2007	12%	18	2	5	36.42	33.34	29.25	4.68	15.94	107
2008	8%	17	2	2	34.56	32.29	23.02	4.05	13.54	110
2009	11%	22	8	9	31.88	27.41	12.79	3.69	11.08	79
2010	11%	15	5	6	48.01	49.02	42.28	5.10	19.98	126
2011	3%	4	0	0	24.65	23.13	15.06	0.84	5.11	74
2012	1%	1	0	0	40.6	40.6	0	0.38	3.92	29
2013	3%	2	0	0	25.61	25.6	0	0.81	4.52	99
2014	Na	Na	Na	Na	Na	Na	Na	Na	Na	19
Total	**13%**	**422**	**80**	**136**	32.36	32.05	25.69	4.83	15.22	1490

Table 2

Descriptive Statistics

This table describes the sample of 2,820 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), OROA (Cashflow/Total Assets), Profit Margin (EBITDA/Sale), Liquidity (CHE/Sales), Leverage ((DLTT+DLC)/ AT), Capital Expenditures (Capx/AT), Overheads (XSGA/XOPR), Property, Plant & Equipment (PPEGT/ AT). 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.

Panel A	**N**	**Mean**	**Median**	**Std. dev.**	**10th Per.**	**90th Per.**	**Mean SP1500**	**t-test**
Market Value ($mil)	2795	28,161	8,574	57,971	518.8	71,793	9,561	14
Tobin Q	2686	1.58	1.25	0.98	0.95	2.58	1.96	-14
Return on Equity	2799	0.135	0.105	1,708	-0.072	0.286	-0.04	1.1
Return on Assets	2797	0.030	0,030	0.089	-0.026	0.107	0.12	-7.6
OROA (Cashflow/Total Assets)	2723	0.073	0.074	0.083	0.007	0.159	0.084	-5.8
Profit Margin (EBITDA/Sales)	2741	0.157	0.168	1.689	0.054	0.384	0.13	0.3
Cash Liquidity (CHE/Sales)	2797	0.089	0.051	0.108	0.006	0.220	0.13	-18.2
Leverage (DLTT+DLC)/AT	2795	0.288	0.279	0.166	0.077	0.506	0.212	18.22
Capital Expenditures (Capx/ AT)	2676	0.054	0.043	0.048	0.003	0.109	0.052	0.5
Overheads (SGA/Op.Exp.)	2120	0.281	0.251	0.182	0.076	0.509	0.314	-5.58
Property, Plant, Equip / Assets	2488	0.652	0.644	0.395	0.146	1.16	0.52	12.4
Ownership Inst. Shareholders	2613	0.634	0.650	0.195	0.369	0.864	0.682	-5.6
Ownership Herfindahl	2615	0.055	0.041	0.057	0.022	0.091	0.063	-6.75

Table 3

Takeover Probability and Premiums around the Majority Thershold

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 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. 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)	(9)	(10)
	Full	+/-10	+/-5	+/-2.5	+/-1.5	poly hl 2	poly hl 3	IK	CCT
yes	-0.00807	0.0537	0.0816**	0.101*	0.116*	0.100**	0.119**	0.091**	0.093**
	(0.0219)	(0.0344)	(0.0411)	(0.0563)	(0.0650)	(0.0449)	(0.0542)	(0.035)	(0.04)
Obs	2,818	828	419	234	153	2,818	2,818	2,818	2,818
R-sq/Z	0.000	0.005	0.011	0.016	0.024	0.007	0.007	2.54	1.94

Panel B: Unconditional Premium

	Full	+/-10	+/-5	+/-2.5	+/-1.5	poly hl 2	poly hl 3	IK	CCT
yes	0.413	2.975**	3.349*	4.826*	5.854*	5.273***	5.173**	4.15**	4.095**
	(0.810)	(1.284)	(1.858)	(2.839)	(3.423)	(1.868)	(2.456)	(1.79)	(2.21)
Obs	2,818	828	419	234	153	2,818	2,818	2,818	2,818
R-sq/Z	0.000	0.009	0.009	0.015	0.021	0.006	0.007	2.3	1.80

Table 4

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 Entrechement Index. Significance at the 10%, 5%, and 1% levels are indicated by *, **, and *** respectively.

	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	-0.00227***	-0.000164	-0.00179**	-0.000156	-0.0737**	-0.0157	-0.0346	0.0309
	(0.000872)	(0.000905)	(0.000861)	(0.000997)	(0.0320)	(0.0337)	(0.0355)	(0.0413)
ln Sales		-0.0172		-0.0307**		-1.589***		-0.721
		(0.0150)		(0.0151)		(0.557)		(0.627)
Profit Margin		0.237***		0.00372		5.187*		-1.757
		(0.0763)		(0.0834)		(2.841)		(3.454)
Ln Market Value		-0.0140		-0.0130		-0.386		-0.700
		(0.0140)		(0.0142)		(0.520)		(0.586)
Cash Liquidity		0.0808		0.173*		2.450		10.24**
		(0.108)		(0.102)		(4.036)		(4.213)
Percent Inst. Own.		0.0350		-0.183**		3.205		-9.143***
		(0.0646)		(0.0815)		(2.407)		(3.376)
Av. Ind. Tobins'Q		0.00600		0.0150		0.408		0.442
		(0.0112)		(0.0108)		(0.418)		(0.447)
Av.Ind.Mkt.Value		0.0714***		0.0225*		1.575***		0.0733
		(0.0116)		(0.0132)		(0.433)		(0.547)
Entrechment Index		0.0128		0.0111		-0.166		0.526
		(0.00812)		(0.0103)		(0.302)		(0.425)
Year Dummies		Y		Y		Y		Y
Obs	1,227	1,227	1,069	1,069	1,227	1,227	1,069	1,069
R-sq	0.005	0.137	0.004	0.084	0.004	0.110	0.001	0.076

Table 5
CIA Estimates and Propensity Score Matching

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 clustering. Controls are the same as in Table 4: Log Sales, Profit Margin, Market Value, Cash Liquidity, Percentage of Institutional Ownership, Average Industry Tobins'Q, Average Industry Market Value and the Entrenchment Index. Weights trimmed at the top and bottom 5% in Panel A. 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	0.045**	0.046*	2.77***	2.75**
	(0.0208)	(0.025)	(1.03)	(1.16)
t stat	2.18	1.82	2.69	2.38
Model	Y	N	Y	N
Obs	2,063	2,063	2,063	2,063

Panel B: Nearest Neighbor Matching with clustering

	(1)	(2)
	Takeover Probability	Unconditional Premium
yes	0.0344*	2.505***
	(0.0209)	(0.910)
Obs	2,296	2,296

Table 6

Target Premiums

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.

	(1) Premium 4weeks 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	0.29	6.29*	6.85***	19.66	3.66	0.59
	(4.051)	(3.45)	(2.05)	(21.08)	(3.13)	(4.44)
Upper Bound Estimation						
yes	5.46**	9.94***	9.44***	42.11**	8.49**	16.02***
	(2.87)	(3.22)	(2.08)	(19.65)	(3.26)	(4.21)
Obs	2296	2296	2296	2296	2296	2296

Table 7

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. Column 1 estimates the Change in Shareholder's Value as the unconditional takeover premium under the CIA model in Table 4- column 3-. Columns 2 to 4 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. Panel A provides the lower bound values using the method in Lee (2009) to estimate the change in Takeover Premium (Δ Pi). Panel B provides the upper bound values. Column 2 estimates the change in Takeover Premium (Δ Pi) times the Baseline Probability of Merger. Column 3 estimates the change in the Probability of Merger (Δ Qi) times the Baseline Premium. Δ Qi is estimated under the CIA model in Table 6, column 1. Column 4 provides an estimate of the Selection Effect. Using the probabilites of the matching model we calculate that Pr[Z*>0 | D=1] = 13.5 and E[Y | D=0 , Z*>0] =29.6.

(1)	(2)	(3)	(4)
Change in Shareholder Value	Premium Effect	Takeover Probability Effect	Selection Effect
Δ Y	**β * Pr[Z*>0 | D=1]**	**E[Y | D=0 , Z*>0] * {Pr[Z*>0 | D=1] - Pr[Z*>0 | D=0]}**	**Pr[Z*>0 | D=1] * { E[Y | D=1 , Z*>0]- E[Y | D=1 , V > - μ2]}**
Panel A: Lower Bound Estimation of β = 0.3			
2.7%	0.040%	1.33%	1.34%
	(1%)	*(49%)*	*(49%)*
Panel B: Upper Bound Estimation of β = 5.5			
2.7%	0.73%	1.33%	0.65%
	(27%)	*(49%)*	*(24%)*

Table 8

Merger Effects

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, column 1 reports the effect on the Acquirer Premium (computed as the change in price 4 weeks before announcement until one day after). Column 2 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 3 and as (-42/Completion) in column 4. Columns 5, 6 and 7 report the effect on the number of bidders, the deal being unsolicited and the deal being challenged. Column 8 reports the effect on the percentage of stock paid for the target. Panel B presents results for different measures of Matching. 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 Total Synergies as the change in value for target and acquirer (computed as abnormal returns FFM (-42/Completion) times the market capitalization 42 days before announcement, units in thousands) and column 4 reports Total Synergies as a percentage of total market capitalization for target and acquirer 42 days before announcement.

Panel A

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Acquirer Premium				**Competition**			
	Acquirer Premium	Acquirer CAR (-5,5) FFM	Runup Acquirer (-42,5) FFM	Runup Acquirer (-42,Comp) FFM	Number of Bidders	Unsolicited Deal	Challenged Deal	Stock Percent
Lower Bound Estimation								
yes	-8.46**	-4.44***	-6.05**	-3.30	0.15**	0.037	0.11***	-26.87***
	(3.19)	(1.35)	(2.86)	(4.44)	(0.06)	(0.03)	(0.03)	(7.22)
Upper Bound Estimation								
yes	2.38	1.03	5.87**	17.64***	0.26**	0.09	0.17*	-3.15
	(2.35)	(1.38)	(3.26)	(3.78)	(0.10)	(0.09)	(0.09)	(5.39)
Obs	2296	2296	2296	2296	2296	2296	2296	2296

Panel B

	(1)	(2)	(3)	(4)
	Matching			
	Same 2Digit SIC	Size Target Rel. to Acquiror	Total Synergies FFM (thou.$)	Total Synergy/ Total Mkt Cap
Lower Bound Estimation				
yes	0.175**	-1.33**	-306,949	-0.03
	(0.06)	(0.51)	(2,428,514)	(0.03)
Upper Bound Estimation				
yes	0.23***	-0.69	7,209,041***	0.14***
	(0.06)	(0.48)	(1,844,029)	(0.03)
Obs	2296	2296	2296	2296