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

BY Vicente Cuñat, Mireia Giné and Maria Guadalupe *

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Abstract

This paper decomposes the expected takeover premium from adopting an anti-takeover provision into three components (a causal effect on the takeover probability; a causal effect on the premium paid; and a selection effect) and provides causal evidence on each of those, thus being able to ascertain the contribution of each to shareholder value creation from takeovers. Using data on shareholder-sponsored proposals to remove an anti-takeover provision voted on in annual meetings of S&P 1500 firms between 1994 and 2013, we extend the regression discontinuity design using the approach in Angrist and Rokkanen (2013) to provide causal estimates that do not rely only on firms around the discontinuity. In order to account for selection in observed mergers we estimate sharp bounds for the causal effect of anti-takeover provisions on the takeover premium (Lee, 2009). For an average firm, voting to remove an anti-takeover provision leads to a 4.5% higher probability of being taken over and a 2.8% higher expected unconditional takeover premium. We also find evidence that increased competition in takeover contests is one driver of the estimated increased premium for firms that remove an anti-takeover provision. Finally, we show that 53% of the shareholder gains come from the increased probability of a takeover, with also significant shares for selection and premium effects.

* Cuñat: LSE, Houghton St, London WC2 2AE, UK (v.cunat@lse.ac.uk); Gine: WRDS, University of Pennsylvania and IESE Business School, Av. Pedralbes 21, Barcelona, Spain (gine@wharton.upenn.edu); Guadalupe: INSEAD, CEPR and IZA, 3022 Boulevard de Constance, 77300 Fontainebleau, France (maria.guadalupe@insead.edu).

I. Introduction

Anti-takeover provisions (poison pills, staggered boards etc.) are considered as a major governance tool, and a mechanism to affect firm value (Gompers, Ishii, Metrick, 2003; Cuñat, Gine, Guadalupe, 2012). As such, they have received substantial academic and practitioner attention. [1] By reducing or delaying the threat of a takeover they are thought to reduce managerial discipline (Bertrand and Mullainathan, 2003). It is also often argued in their favor that they allow managers to bargain for a higher price in the event of a hostile takeover. However, in spite of the theoretical and empirical attention devoted to the effects of anti-takeover provisions, there is still little causal evidence on the effect of anti-takeover provisions on the takeover premium or even on the takeover probability itself.[2] Without a causal estimate one cannot assess whether and how much anti-takeover provisions work in deterring takeovers or their effect on shareholder value.

The goal of this paper is to provide arguably causal evidence on the effect of anti-takeover provisions on firm value through the takeover channel and a comprehensive framework to think about the possible effects. In order to structure our analysis, we first show how the expected future takeover premium from adopting an anti-takeover provision can be decomposed into three components: First, the causal effect that the anti-takeover provision has on the probability of being taken over; Second, the causal effect of the anti-takeover provision on the premium paid if the acquisition is successful; and Third, a selection effect arising from the fact that different types of firms become targets when they adopt an anti-takeover provision. The first two have each given rise to a substantial literature, which we discuss below. The third, more seldom discussed, is also essential to assess the overall expected effect of anti-takeover provisions and plays a significant role. Given that the anti-takeover provision affects the probability that the firm is taken over, firms that are taken over with an anti-takeover provision are likely to have different characteristics from firms that are taken over without an anti-takeover

[1] E.g. Malatesta and Walkling, 1988; Ryngaert, 1988; Jarrell and Poulsen, 1987; Brickley, Coles, and Terry, 1994

[2] Given the fact that the adoption/removal of anti-takeover provisions is driven by factors such as firm performance, the potential presence of a bidder, or other omitted factors, one cannot interpret existing evidence –mostly based on correlations-- as causal.

provision.[3] Therefore, one cannot infer the takeover premium from comparing firms that are taken over with and without anti-takeover provisions, because the selection changes when an anti-takeover provision is in place. Our empirical decomposition makes all the different elements at play clear and allows us to bring together hitherto related but relatively separate literatures into a comprehensive answer to the question of how anti-takeover provisions affect shareholder value through takeover probability and price effects.

We build our empirical analysis on this decomposition to structure our evidence on the overall effect of anti-takeover provisions on firm value. To be able to establish causality we extend the regression discontinuity design using the Angrist and Rokkanen (2013) identification strategy. This method essentially is a matching estimator that uses the regression discontinuity approach as a tool for validating the conditional independence assumption of the model. Furthermore, in order to account for selection we estimate sharp bounds for the causal effect of anti-takeover provisions on the takeover premium (Lee, 2009).

Our data consists of all shareholder-sponsored proposals to remove an anti-takeover provision voted on in annual meetings of S&P 1500 firms between 1994 and 2013. This is a total of 2809 proposals in 929 different firms. To identify our effects we use the vote outcome on shareholder-sponsored proposals to *remove* anti-takeover provisions voted on at annual meetings (shareholder-sponsored proposals to adopt those are virtually nonexistent). The fact that the vote outcome can be considered random in a narrow interval of the discontinuity but leads to a discrete change in the probability of removing the anti-takeover provision in the firm is what underlies the classic regression discontinuity identification (e.g. Cuñat, Gine, Guadalupe, 2012). The disadvantage of this method is that since the regression discontinuity estimate is obtained from firms with proposals close to the majority threshold, we don't know what is the external validity of the estimate (i.e. how much it applies to firms with uncontested vote outcomes).

[3] Note many existing studies focus on effective 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 conditional premiums are subject to selection bias, as we discus later.

The classic regression discontinuity design is based on the assumption that, other than the treatment, there are no observable or unobservable differences in two populations of firms with the same running variable (the vote outcome in our setting). Angrist and Rokkanen (2013) build on this assumption to obtain results beyond the discontinuity, by using the votes outside the discontinuity to test the conditional independence assumption of a matching model. [4] This strategy, allows us to extend the existing literature in several ways: First, we can go beyond close-call votes and extrapolate the results of the regression discontinuity approach to the full sample of shareholder-sponsored proposals to remove anti-takeover provisions. This means that we can evaluate what is the effect of anti-takeover provisions for all firms and not just those around the discontinuity as in the classic regression discontinuity design. Second, we can test for the presence of heterogeneous effects of anti-takeover provisions across firms according to their vote outcomes. 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.

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 (classic regression discontinuity), passing a proposal to drop an anti-takeover provision increases the likelihood of experiencing a takeover by about 2% per year over the following 5 years. It also increases the shareholder value of future expected merger premiums by 4.7%.

For firms away from the discontinuity, the effects are smaller but also positive and significant on average: removing an anti-takeover provision reduces the probability of takeover by 0.9% per year (4.5% over 5 years) and increases the expected takeover premium by 2.7%. Obtaining this average effect is very important since it is possibly the main policy-relevant variable: not knowing ex-ante how any firm will vote in the future, and whether they will be subject to a takeover, we need the average effect to decide on the value creating potential of a policy/shareholder vote.[5] The fact that the effect is

[4] We explain further the Angrist and Rokkanen (2013) method and intuition in Section III.
[5] These values are all intent to treat (ITT) values. For the treatment on the treated, they need to be re-scaled by the inverse of the jump in the probability of the implementation of the proposal at the discontinuity. In

smaller than for firms near the discontinuity suggests that contested votes take place at firms where shareholders can benefit more from removing the anti-takeover provision.

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. 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 effect on both the takeover probability and the expected premium is large and significant for all firms that failed to pass a proposal. The largest benefits from passing a proposal accrue to firms that actually passed them by small or moderate margins (up to 20%). In contrast, for firms that passed anti-takeover provisions by very large margins, the effects on value are limited, and closer to zero. Firms that vote massively in favor of a shareholder-sponsored proposal are rare in the sample, but they also seem to be different from the rest. This suggests that while the effects are never negative, they are heterogeneous and not all firms benefit to the same extent.

The causal effect on the expected (unconditional) premium is not subject to selection because it includes both firms that did experience a takeover and those that did not, so the population at risk is constant. However, we also would like to know the effect of removing an anti-takeover provision on a given firm, ceteris paribus. This estimate is what tells us whether a given firm is able to obtain a higher or lower premium if it drops the anti-takeover provision. However, as mentioned earlier, standard estimates for this effect are subject to selection bias. Even if we have an instrument for the anti-takeover provision, given that anti-takeover provisions affect both the premium and the probability of being taken over, the populations of firms that are effectively taken over with or without an anti-takeover provision are not comparable. In order to provide such estimate we will apply sharp bounds (as proposed by Lee, 2009) to the Angrist-Rokkanen (2013) estimates. We find that the causal effect on the conditional premium can be bounded between 0.2% and 5.3%: i.e. it is always positive.

Overall, while we confirm the established consensus that anti-takeover provisions reduce takeover probabilities (in our paper, voting to drop an anti-takeover provision

practice this implies multiplying them by a factor of 2 to 3 times, depending on estimates (see Popadak 2014 or Cuñat, Gine and Guadalupe, 2012).

increases takeover probability), we find that they also reduce (and not increase as is commonly thought) the takeover premium. Since this finding is the opposite to the most established intuition, we investigate possible channels behind why removing anti-takeover provision raises the expected takeover premium. We find that voting to remove an anti-takeover provision (causally) increases the number of bidders, the number of unsolicited bids, the number of challenged deals and the probability that the deal is paid in cash. This suggests increased competition is one plausible driver for the increased takeover premium.

Finally, we can use all these estimates in our decomposition and obtain what fraction of the overall increase in value from removing anti-takeover provisions comes from its three different components (the takeover probability, the premium and the selection effects). We show that the increase in value from removing an anti-takeover provision operates largely via quantities: 53% of the shareholder value is coming from the increased probability of mergers. The premium effect is between 1% and 35% and the selection effect is positive and between 12% and 46% 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 had already suggested that anti-takeover provisions are not randomly adopted, and hence correlations are likely to be subject to endogeneity bias.[6] We are able to provide a quantitative estimate of the role of selection.

By being able to provide causal estimates, we are able to contribute to a literature with scant causal evidence. This is all the more important given that papers studying the correlation between anti-takeover provisions and takeover probabilities and premia, have often found contradictory results.

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[6] For example, Malatesta and Walkling (1988) show that firms adopting poison pill defenses are much more likely to become the target of takeover activity than a randomly selected firm; and Comment and Schwert (1995) show that the proportion of pill adopters that are in play increases from 2.4% one month before to 19.4% one year after and most of this increase takes places within a month of adoption. Bange and Mazzeo (2004), also highlight the selection effects of anti-takeover measures.

Regarding the effect of anti-takeover provisions on the probability of a takeover, Pound (1987) documents that anti-takeover provisions reduce the probability of a takeover bid. Consistently, Ryngaert (1988) finds that firms with a poison pill are more likely to reject a hostile takeover bid (the analysis is done on a sample of 29 firms). In contrast, Comment and Schwert (1995) finds that poison pills have no effect on takeovers, once a bidder has made an offer. Similarly, 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. Using a different identification strategy we also find that anti-takeover provisions do deter takeovers, and we are able to show how the effect is different for different types of firms, and how much it contributes to the overall expected premium.

A substantial literature has analyzed the effect on the takeover premium conditional on a takeover taking place. While we find that voting to remove an anti-takeover provision has a positive effect on premiums, this 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). The challenge with this literature is that firms that are taken over are a selected set, and the selection is likely affected by the anti-takeover provision.[7] In this paper we explicitly deal with selection to provide an estimate for the causal effect on the premium and, we are also able to estimate the contribution of selection to the unconditional estimates.

The paper proceeds as follows: 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. Section VI provides our bounded estimates for the treatment effect on the premium and uses all the estimates in our decomposition. Section VII concludes.

[7] For example, in the presence of takeover defenses, only deals with high synergies may be subject to an offer, so premiums can change because the composition of targets is changing as a response to anti-takeover defenses. Simultaneously, anti-takeover defenses alter the negotiations between the bidder and the target and therefore can change takeover premiums keeping the characteristics of the firms constant.

II. Framework: Decomposing the Unconditional Premium

II.1. Dealing with Endogeneity and Selection

We start by providing an analytical framework to empirically examine the effect of anti-takeover provisions on the expected shareholder gains via takeover premiums, which will allow us to assess all the possible causes of bias we need to deal with empirically and establish all the elements needed to build the decomposition of the unconditional premium in Section II.2.

We are interested in the effect of having an anti-takeover provision on (i) the takeover probability and (ii) the takeover premium. In our setting, we will test the effect of voting to drop an anti-takeover provision, defined by the treatment dummy variable D, where D=1 if shareholders vote to drop the provision, D=0 if they vote to keep it. Empirically, we observe the realized premium, Y which equals the premium paid it a takeover takes place and zero otherwise. In order to understand selection issues, we define two latent variables. Y*, is the potential offered premium for any firm, which is only observed if a takeover takes place. Similarly, Z* is a measure of the latent merger propensity of a firm, and a merger happens whenever $Z^* > 0$. Therefore we can write the unconditional premium as: $Y = 1[Z^*>0] \,.\, Y^*$. 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):[8]

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

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:

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

[8] Generalizing the model to a non-linear structure is straightforward.

It is also useful to write ΔY using the law of iterated expectations as:

$$\Delta Y = \Pr[Z^*>0 \mid D=1] * E[Y \mid D=1 , Z^*>0] - \Pr[Z^*>0 \mid D=0] * E[Y \mid D=0 , Z^*>0] \quad (1)$$

However, even with a randomly assigned D, one cannot recover the effect on the premium holding firm characteristics constant, β. This is because the observed Y is conditional on a merger occurring ($Z^*>0$), which is itself affected by treatment:

$$E[Y \mid D, X, Z^*>0] = D\beta + X\mu_1 + E[U \mid D, X, V> - D\gamma -X\mu_2]$$

So that:

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

This shows that even with D randomly assigned (and unless U and V are independent) one cannot recover the causal effect on Y^* because of the sample selection term $E[U \mid D=1, X, V> -\gamma -X\mu_2] - E[U \mid D=0, X, V> - X\mu_2]$. However, typically, existing premium studies compare premiums conditional of a merger happening for firms with and without anti-takeover provisions: $E[Y \mid D=1 , X, Z^*>0] - E[Y \mid D=0 , X, Z^*>0]$. As equation (2) shows, without accounting for selection, this approach does not estimate β. Note that β is the parameter of interest if, for example, we want to assess the effect of anti-takeover provisions on the bargaining and negotiation process of a takeover premium: it reflects for a given firm how much more/less would they get by removing an anti-takeover provision.

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

With a randomly assigned D (which we will obtain by extending the classic regression discontinuity design with the Angrist Rokkanen (2013) identification strategy), and using Lee bounds to bound β, we will have a set of parameters of interest. But we are also interested in knowing how much of the overall estimated unconditional premium ΔY is driven by the takeover (Δ P), underlying premium (β) 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, after some manipulation this equation as:

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

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). The third is a selection term that captures the change in the population of firms that are subject to a takeover offer. Below we will report the estimates for these three components that make up the change in the unconditional premium.

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

III. Identification Strategy

We now turn to 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 Δ P and the unconditional takeover premium Δ Y. Let's 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 one when a proposal passes. K is a constant term. We can then express the relationship of interest as:

$$y_{ft} = K + D_{ft}\theta + u_{ft} \quad (1)$$

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 directly in a regression is unlikely to give a consistent estimate $\hat{\theta}$, for instance 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 their unconditional premiums, we start presenting results from a classic regression discontinuity design, and then build on this using the Angrist Rokkanen (2014) identification strategy.

II.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 into treatment can be considered as random. This assumes that the relationship between firm characteristics and shareholder votes is continuous around the threshold (which one can test for observable variables) while the probability of implementing an anti-takeover proposal jumps discontinuously.[9] 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 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), propose to use two different polynomials for observations on either side of the threshold.[10] An alternative approach is to run a local regression on an optimally calculated interval around the discontinuity. This approach

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

[10] 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).

was initially proposed by Imbens and Kalyanaraman (2012), (or IK) for a local linear regression approach. Similarly, Calonico, Cattaneo and Titiunik (2014), (or CCT) propose to 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. [11]

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).

II.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, one would like to obtain causal estimates for other types of firms as well. The identification strategy in Angrist and Rokkanen (2014) allows us to do that.

The Angrist and Rokkanen (2014) approach exploits the fact that in the regression discontinuity setting, unlike in most applications, the variable that assigns observations to treatment is known. In our case this is the vote outcome. In traditional regression discontinuity, one considers that assignment is random around the discontinuity and that is what allows us to provide causal estimates of the treatment. The problem with extrapolating beyond the discontinuity is that outcomes are not independent of the running variable (the vote outcome). Angrist and Rokkanen (2014) observe that if one could eliminate the relationship between assignment variable and outcomes by conditioning on some covariates then one can make a Conditional Independence Assumption (CIA) to obtain causal estimates. That is, the outcome needs to be independent of the running variable, conditional on a set of controls x_{ft} :

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

[11] 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.

Unlike in an OLS regression (where one does not know the assignment variable), in a regression discontinuity design 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) still retains meaningful variation after we condition on x_i.

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

If one 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. Once the matching samples are constructed, one can also measure heterogeneous effects for different vote outcomes.

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. We know that the probability of implementation after passing an anti-takeover provision is around 50% (Cuñat, Gine, Guadalupe, 2012), so our estimates can be rescaled to account for that.

There are several important advantages of this matching method. First, it allows us to determine a valid model of the interaction between shareholder votes on anti-takeover measures and the takeover probability that captures the main influences behind shareholder votes. This includes firms that would be infra-marginal in the standard regression discontinuity design. Therefore, the second advantage of this approach is that we can extrapolate the regression discontinuity results to a broader sample of firms. We can go beyond a local interpretation of the regression discontinuity design estimator, while retaining a causal interpretation. Third, 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 antitakeover provision 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.

II.3. Bounding the causal and the selection effect.

The preexisting literature focuses on the effect of ATPs or other treatments on the takeover premium conditional on a merger happening. However, the previous two sections show that 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 of bargaining, matching with bidders, competition…) and which part of the effect is due to selection (i.e. when ATPs are dropped a different population of firms experiences takeovers.

In order to correct for selection, one could have an excluded variable in a Heckman selection model, but these are virtually impossible to find since any variable that predicts takeover 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 removed), 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. 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 $p=Pr(-\gamma -X\mu_2<V<-X\mu_2)/ Pr(-\gamma -X\mu_2<V)$. These are what we'll call in what follows sharp Lee bounds (Lee, 2009).

IV. Data Description and Sample Characteristics

We construct a dataset that spans 20 years of voting data using two main data sources. For the period 1997-2013 we use Riskmetrics dataset. For the period 1994-1994 we use data form ISS tapes constructed by Ernst Maug. 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 2,809 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 the majority rules (votes caset/votes outsanting) and on the relevant majority threshold v_f* for each firm. 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 on a shareholder-sponsored proposal to remove an anti-takeover provision. We consider whether a firm is taken over within five years of the vote. Fore firms with multiple votes we treat these as separate events, but cluster standard errors by firm in our estimates. We define the merger premium for firms that are taken over as the cumulative return from four week prior to the takeover announcement up to the announcement date. We also obtain financial information from Compustat and ownership information from Thomson 13F for firms in our sample.

Table 1 presents information of the votes to remove antitakeover provision used in the paper, as well as the takeover probabilities and premium. Panel A of Table 1 shows the distribution of proposals and vote outcomes by year. While the number of proposals per year is quite robust since the beginning of the series, the percentage of proposals that passed has increased dramatically: from 5% to 7% in the mid-90s to 73% in 2013. Correspondingly the vote outcome has increased as well from 28% in 1994 to 65% in 2013. 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 to each side of the majority threshold. With the Angrist and Rokkanen (2014) method we are able to provide a causal effect on the full sample.

Panel B of Table 1 reports information about the deals in our merger sample. In the second column, we report the probability of a firm experiencing a takeover over the five years following a shareholder vote. The average probability is 13% and it peaks in 1995 and 1996 due to many successful mergers in the 1998-2000 period.[12] Columns 3 to 5 show the number of successful merger targets following a shareholder vote for the full voting sample and in the neighborhood of the majority threshold. There are a total of 135 targets within 10 percentage points and 79 targets within 5 percentage points of the majority threshold. Table 1 B also presents descriptive information on the merger premium. 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 shows basic descriptive statistics of the firms in our sample. We also present characteristics of the average S&P1500, which includes firms that were not the target of a shareholder proposal to remove an anti-takeover provision. One of the most noticeable differences is that firms in our sample are three times larger than the average SP1500 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 and capital expenditures and overheads. This suggests that while the Angrist Rokkanen method will allow us to obtain results for firms subject to an anti-takeover removal proposal, one should have caution in extrapolating the results to other firms that never had such a shareholder proposal.

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

V.1. Preliminary tests to validate the identification strategy

Before presenting results using the classic regression discontinuity design and Angrist Rokkanen (2014) identification strategies, we need to run a series of test to confirm that this is a good setting to use these methods.First, Table 3 shows that there are no pre-existing differences in firm characteristics (or trends in firm characteristics)

[12] The drop in this probability towards the end of the sample is largely driven by censoring. We deal with censoring issues in Section III.2, Table 5.

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 where 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 indicates 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 strategies.

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 a strategic behavior around the majority threshold such that the continuity assumption would be violated. Figure 1a shows a smooth overall distribution of votes and Figure 1b shows the formal continuity test proposed by McCrary (2008) that rejects the discontinuity of the density function at the majority threshold.

These tests confirm that this is a good setting to apply the classic regression discontinuity and the Angrist Rokkanen (2014) identification strategies

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 probabilities and the expected premium using the regression discontinuity design.

We begin by presenting graphical evidence using all of our data. Figure 2a 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 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. If one just looked at this evidence one 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.

In turn, Figure 2b shows the same graph with the unconditional premium in the vertical axis. We observe again 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 4 presents regression estimates of the effect at the discontinuity seen in Figures 2a and 2b using four different estimating methods. Columns 1 to 5 of Table 4 shows the non-parametric test, which consist 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 4 show the regression discontinuity estimate using polynomial controls of order two and three to each side of the discontinuity. Finally, columns 9 and 10 of Table 4 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 5 years after a shareholder vote. The results show a small positive effect of passing a proposal when all observations are included (column 1). This effect becomes bigger and more significant when the effect 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)

[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

estimates suggest that firms that pass a proposal to drop an anti-takeover provision, have an additional cumulative probability between 12% and 14% to be the target of a takeover within 5 years following the vote. And the point estimates in columns 9 and 10, are around 10%, which is around 2% below the previous ones but not statistically different.

Overall estimates range between 9.5% and 14%, which is a sizeable effect, when compared with the sample-wide average five-year probability of a takeover of 13%.

Throughout the paper we use whether the firm merged within five years of the vote as the independent variable for takeover, however, for later years, this variable is censored. Our results are no sensitive to different definitions as can be inferred from Table 5, which shows the effects year by year, and suggests the effects are fairly distributed but largest in years 1 and 5 after the vote.

In Panel B of Table 4 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). We do not explicitly discount the premiums paid, nor do we consider premiums paid beyond 5 years. The results are robust to discounting and different horizons (available upon request).

The results in Columns 1 to 5 of Table 4 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 between 5% and 7%. Columns 6 and 7 using the flexible polynomial approach, show expected premiums of about 6%. Finally, the local regression approach produces slightly smaller estimates of 4.7% (IK) and 5.36% (CCT). These are again very large effects against an average unconditional premium of 4.83% in the sample.

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, 2013)

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 6.

The goal of Table 6 is to test whether conditioning for a sufficient number of variables one 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 Conditional Independence Assumption we will 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 (ln sales, ln 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 6 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 is reflecting 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 6), the correlation becomes statistically insignificant and the point estimates get closer to zero. For example, take column 3 of Table 6. There is a highly significant -0.0018 coefficient on the vote variable that drops to an insignificant -0.0001, when one includes the variable 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 Rokkanen (2014), we complement formal CIA testing with a graphical tool. This is shown in Figure 3 that plots the residuals of regressions that include the covariates in Table 6 excluding shareholder votes. If the conditional independence assumption 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 3 shows outcomes (takeover probability in Figure 3A and unconditional premium in Figure 3B) 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 quite good job at 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 Rokkanen (2014) propose to use 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 5 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 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 7 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 2.76% increase in the unconditional premium (Column 3) that the acquiring firms pay for the acquired firms.

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 7 panel B) with a 3.5% 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 positively significant and economically large. The mean (within 5 year) 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 that probability by 2.5 percentage points. Third, these matching estimates can be interpreted as *causal.* Using the regression discontinuity setting to establish the Conditional Independence Assumption allows us to obtain the causal effects of removing anti-takeover provisions, not just around the discontinuity, but also away from it, for the full set of firms that voted to remove an antitakeover provision. While this is still not the full set of listed firms in the USA, it actually represents a substantial share of the S&P 1500 index (929 distinct firms).

In addition, once the CIA is established one can provide not only mean estimates of passing a provision (those in Table 7), 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 (Adapting Kline 2011 as in AR). We do this by fitting a linear OLS

[14] Note that, unfortunately, one cannot apply the Angrist 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.

model that uses the same variables and coefficients as the matching model 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 for firms that did not pass a provision had they passed one? This is shown in Figure 4 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 would be the takeover probability (Panel A) and the unconditional premium (Panel C) had they passed the proposal (based on our model). 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 4 answer the question what would the takeover and unconditional premium have been had the firms that passed a provision not passed it? And how 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, i.e. for firms up to around 25% from the discontinuity, they 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 tends to disappear. There are very few observations (13% of the sample) with such high vote outcomes. Although these firms represent a small part of the sample, and don't contribute a lot 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 those firms seems independent of the actual passing of the proposal.

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

VI.1. Causal Effect on the Premium β

In Section IV we obtained causal estimates for the effect of treatment on the unconditional premium ΔY and the takeover probability Δ P. However, we would like to recover as well the effect on the premium itself, β. This is the expected premium that a given firm would get if it removes the anti-takeover provision: it is the estimate of the price effect of removing the anti-takeover provision for a given firm that is taken over. Given the potentially quite strong selection in the data (our estimated Δ P is quite large) it is not possible to infer from ΔY the value for the causal β.

The value of β can be bounded using the method in Lee (2009). The proposed bounds rely only on the 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.

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).

Table 8 Column 1 estimates the bounds proposed by Lee (2009) using the same linear reweighting as in Table 7 so we can interpret the results as causal. This method yields estimates of β that bound between 0.2% and 5.3%. This means that the direct premium effect of dropping an anti-takeover provision on a given firm is positive. Although the bounds include the possibility of a very small positive premium effect (0.2%), the lower bound is still not negative. We will explore a few hypotheses behind this positive effect below.

The remaining columns in Table 8 test additional premium measures for rubustness purposes. Column 2 reports the effect on the Target Premium computed as the change in price 1 week before announcement until completion. Column (3), (4) report premiums based on abnormal returns using the FFM factors, the different windows (+/- 1 day +/- 5 days) and are relative to the Announcement date. Column (5) reports the cumulative abnormal returns from the vote date until the announcement date plus one day, using the FFM factors. All of them show unambiguously a positive causal premium.

VI.3. 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).[15] Results can be seen in Table 9.

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 (0.2), we find that the remainder of the takeover premium is driven by selection (49%) and only 1% is driven by the causal premium itself. With our upper bound estimate for β (5.3), 26% of the unconditional premium is explained by the effect on premiums holding the population constant, and 24% by selection.

This implies that half of the value implications of dropping an ATP can be attributed to an increased probability of experiencing a takeover. The causal effect on the premium is imprecisely estimated (1% - 27%) but it is unambiguously positive. This result goes against the intuition that the main causal effect of ATPs on premiums should be negative and mainly driven by worse bargaining opportunities of the management of the target firm. We explore the determinants of this positive causal effect in the next section. Combining the results in columns (2) and (3) we can also show that, fixing a given firm, 65% to 98% of the shareholder gains come from higher probabilities of being taken over while the remaining 35% to 2% can be attributed to better premiums.

Finally, the selection effect is quite high in both bounds (24% to 49% of the effect). This suggests that, even when treatment can be interpreted as randomly assigned, failing to account for the selection of targets under treatment (no ATP) would induce to substantial biases.

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[15] We take the estimates for Δ P and ΔY from Columns 1 and 3 of Table 7. 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 8.

VII. Understanding Positive Target Premiums

The analysis in the prior section shows that target premiums are higher when companies pass proposals to remove anti-takeover provisions, even after accounting for selection. This is the opposite results from what is commonly argued in the literature (and in corporate finance textbooks). A possible explanation for this positive effect is that lowering anti-takeover provisions may increase the competition among bidders. They could also attract more cash deals which are associated with higher premiums (Offenberg and Christo, 2015). Lower takeover barriers may also reduce the incentives to dilute target shareholders ex-post, increasing the manager's incentives to increase shareholder value and reducing the manager's benefits of control.[16]

We explore these hypotheses in Table 10. Given that the population of merged companies changes with the removal of an anti-takeover provision, selection is an issue for this exercise too and we also need to analyze these effects using Lee bounds. The results show that when proposals to withdraw anti-takeover provisions are passed, there is an impact on several deal characteristics that makes them more contested, inducing higher premia. First, the merger deals do not seem to have differential profits for bidders in the treatment and control groups. Column (1) reports acquirer premiums on a -40/+1 days around announcement. The treatment group seems to have lower acquirer premiums with the bounds ranging from -6% to 0. However, Column (2) shows that the bounds for the effect of ATPs on -1/+1 days around announcement clearly include zero. This evidence is suggestive of small effects of dropping an ATP on bargaining.

Next we explore whether dropping an ATP leads to more contested bids and more competition amongst bidders. The results show that lowering an ATP attracts more bidders (Column 3). Moreover, the deals are more likely to be unsolicited and are challenged with a higher probability (Columns 4 and 5). In Column 5, we find that the way in which the premium paid to targets includes a lower proportion of shares (stock percent) and, therefore, involves higher levels of cash as pointed by Offenberg and Christo (2015). In all cases, both the upper and lower bounds of the estimates are of the same sign, such that while we cannot provide the exact point estimate, we can uniquely establish the sign of the effect for all these variables.

[16] A related argument can be found in Burkhart Gromb and Panunzi (1998)

Finally in Column 7 we explore whether lower ATPs improve the matching between bidders and targets. The dependent variable is a dummy variable that takes value one if both firms belong to the same 2 digit SIC industry. The results show that unambiguously, bidder and target firms are between 17% and 23% more likely to belong to the same industry when an ATP is dropped. This result suggest that once ATPs are dropped, hard industry synergies are more determinant and probably other factors such as private benefits of control or conglomerate activity are of lower importance.

VIII- Conclusion

In spite of the attention devoted to the effects of antitakeover provisions, there is still little causal evidence on their effects on takeover probabilities and the takeover premium. Furthermore, a lot of the exiting literature fails to account for selection when computing the takeover premium. We show that this selection effect is 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.

To establish causality in all these estimates, we extend the classic regression discontinuity design using the Angrist and Rokkanen (2013) identification strategy. We show that for firms with contested votes, (the classic regression discontinuity firms) voting to remove an anti-takeover provision leads to a 10% higher probability of being taken over and a 4.7% higher expected unconditional takeover premium for shareholders. For non-contested votes (away from the majority threshold, Angrist and Rokkanen, 2013), the effect is still positive and significant for both outcomes of interest. For an average firm removing an anti-takeover provision leads to a 4.5% higher probability of being taken over and a 2.8% higher expected unconditional takeover premium. Our evidence suggests a clear positive effect of removing anti-takeover provision on both takeover probabilities and premia for all firms and not just those with close-call votes. In addition, our methodology allows us to examine heterogeneous responses for different vote outcomes. We find that *most* would have had lower takeover probability and lower expected premium if those anti-takeover provision proposals had failed to pass. However, firms with very high vote outcomes (20% higher than the majority threshold) we estimate that the effects of removing anti-takeover provisions tend to disappear. These are new result that the earlier literature was not able to establish causally.

We also show that the premium effect accounting for selection can be bound between 0.2% and 5.3%. This means that voting to remove and anti-takeover provision increases rather than lowers (as previous literature seems to suggest) the premium paid to target firms. We explore the reasons behind this increase and find evidence of higher competition for these firms. This suggests the firms with better governance structure and more attractive to bidders, synergies are likely easier to be realized for those and hence competition increases, raising premiums.

Finally, we use our estimates and decomposition to establish how much of the effect of passing an anti-takeover provision on the unconditional premium is driven by the probability, price and selection effects. Out of these three channels, we find that half of the shareholder value is coming from the increased probability of mergers. But there are substantial positive premium and selection effects as well.

Our analysis takes all anti-takeover provisions as identical, and does not consider heterogeneity of effects for different kinds of firms. We think these are important avenues to explore and are left to future research.

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Figures

Figure 1a: Distribution of Votes

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

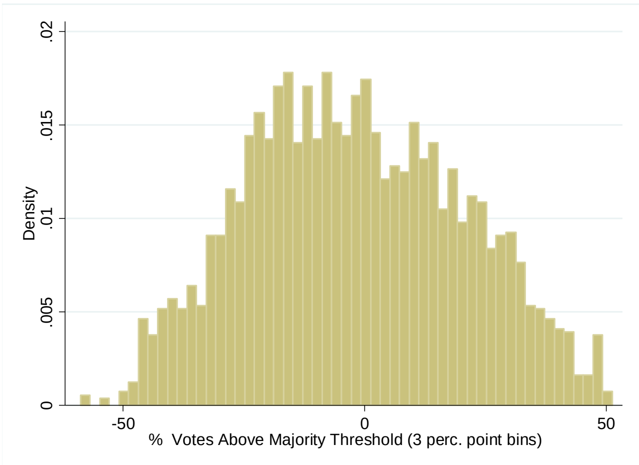


Figure 1b: Continuity of Vote, McCrary 2008

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

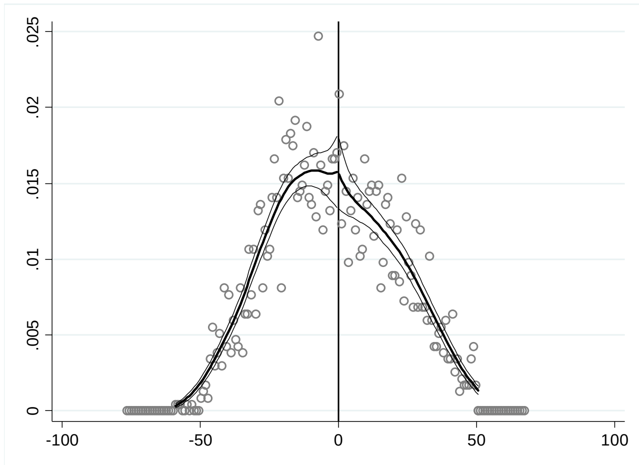


Figure 2a: Merger Probability

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.

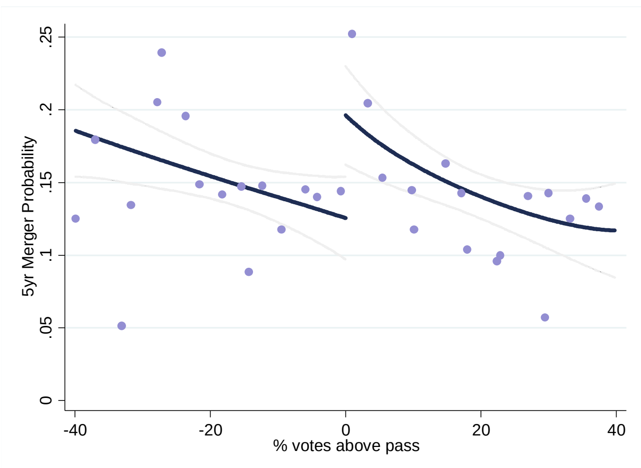


Figure 2b: Unconditional Premium

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.

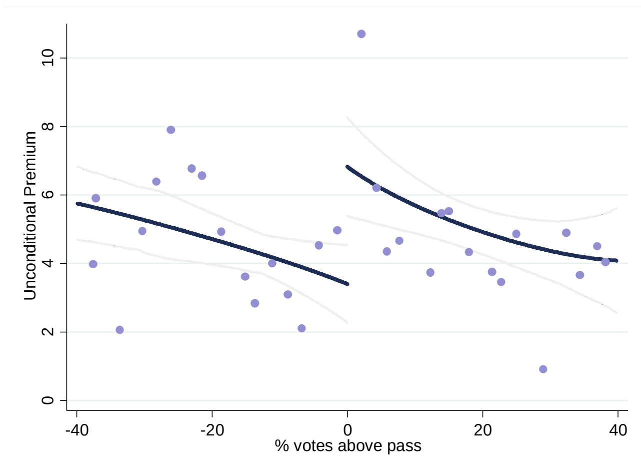


Figure 3a: Conditional Independence Test
Merger Probability

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

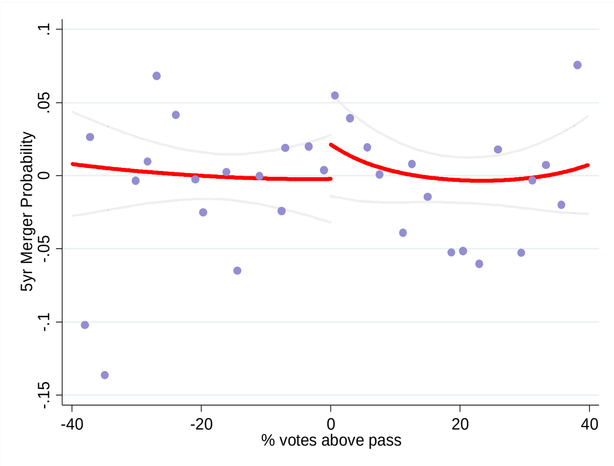


Figure 3b: Conditional Independence Test
Premiums

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

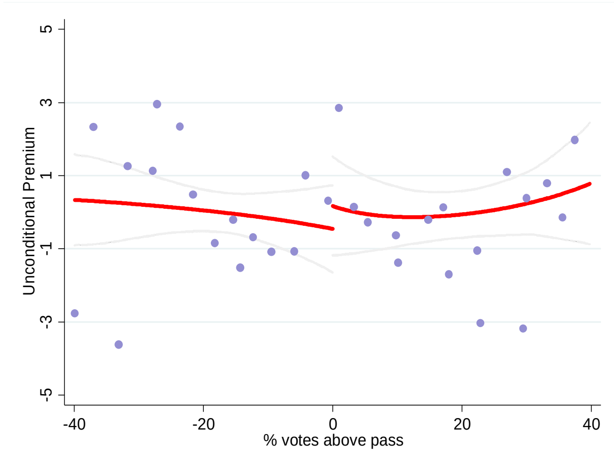


Figure 4a: Extrapolation Merger Probabilities LHS

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

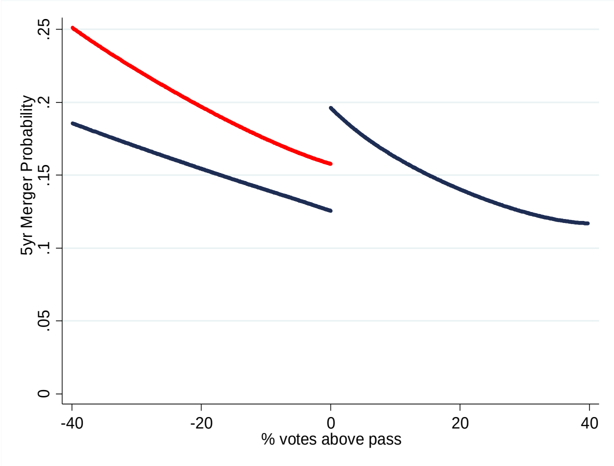


Figure 4b: Extrapolation Merger Probabilities RHS

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

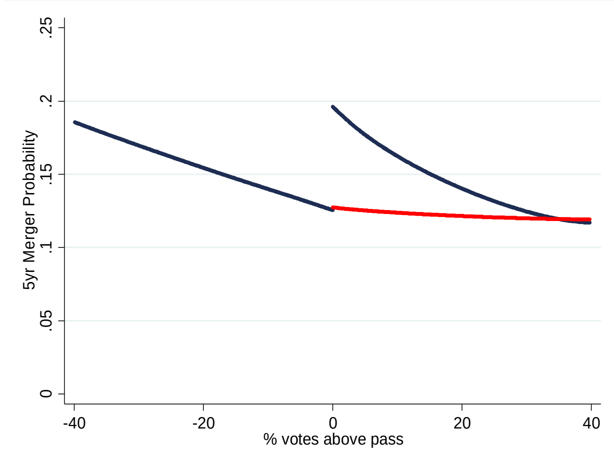


Figure 4c: Extrapolation Unconditional Premium LHS

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

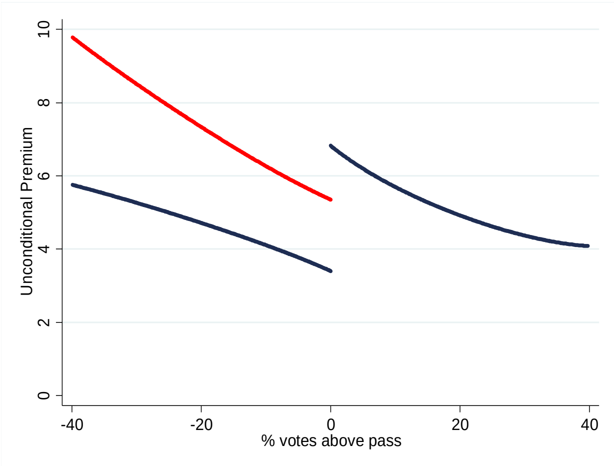
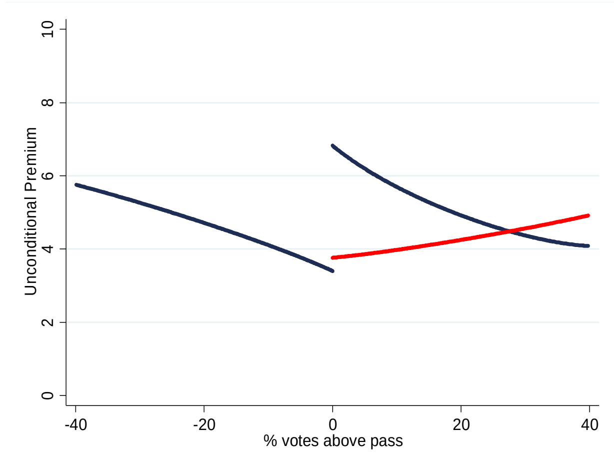


Figure 4d : Extrapolation Unconditional Premium RHS

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



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 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 2809 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	130	68	52.31%	53.8%	21	43
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	**2809**	**1270**	**45.21%**	**48.2%**	**425**	**836**

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. Columns 2, 3 and 4 display 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%	13	4	5	45.06	49.02	40.72	4.50	18.39	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%**	**420**	**79**	**135**	32.36	32.05	25.69	4.83	15.22	1490

Table 2

Descriptive Statistics

This table describes the sample of 2,809 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.

	N	**Mean**	**Median**	**Std. dev.**	**10th Per.**	**90th Per.**	**Mean SP1500**	**t-test**
Market Value ($mil)	2784	28,127	8,551	58,016	518.8	71,103	9,561	14
Tobin Q	2676	1.58	1.25	0.98	0.95	2.57	1.96	-14
Return on Equity	2788	0.139	0.105	1.711	-0.072	0.283	-0.04	1.1
Return on Assets	2786	0.030	0.031	0.089	-0.026	0.107	0.12	-7.6
OROA (Cashflow/Total Assets)	2712	0.073	0.074	0.083	0.007	0.159	0.084	-5.8
Profit Margin (EBITDA/Sales)	2730	0.157	0.168	1.693	0.054	0.384	0.13	0.3
Cash Liquidity (CHE/Sales)	2786	0.089	0.051	0.108	0.006	0.220	0.13	-18.2
Leverage (DLTT+DLC)/AT	2784	0.288	0.279	0.166	0.077	0.506	0.212	18.22
Capital Expenditures (Capx/ AT)	2665	0.054	0.043	0.048	0.003	0.109	0.052	0.5
Overheads (SGA/Op.Exp.)	2112	0.281	0.251	0.182	0.076	0.511	0.314	-5.58
Property, Plant, Equip / Assets	2478	0.653	0.644	0.396	0.146	1.16	0.52	12.4
Ownership by Instit. Shareholders	2600	0.635	0.651	0.195	0.371	0.864	0.682	-5.6
Ownership Concentration Herfindahl	2602	0.055	0.041	0.057	0.022	0.092	0.063	-6.75

Table 3

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.

	Before meeting (t-1)		**Change, from (t-2) to (t-1)**	
	(1)	(2)	(3)	(4)
A.				
Tobin Q	0.097	-0.002	-0.052**	0.007
	(0.064)	(0.132)	(0.025)	(0.078)
Return on Assets	-0.006	-0.009	-0.007**	-0.007
	(0.005)	(0.011)	(0.003)	(0.010)
Return on Equity	-0.070	-0.363	0.011	-0.297
	(0.109)	(0.367)	(0.098)	(0.381)
Profit Margin	-0.101	0.134	0.020	-0.019
	(0.091)	(0.154)	(0.019)	(0.036)
Cash Liquidity	0.003	0.015	0.001	-0.002
	(0.007)	(0.014)	(0.002)	(0.007)
Leverage/ Assets	-0.029**	0.013	0.008***	0.009
	(0.014)	(0.023)	(0.003)	(0.009)
Overheads (SGA/Op. Exp.)	-0.015	0.009	-0.003*	-0.006
	(0.014)	(0.029)	(0.002)	(0.004)
Capital Expenditures /Capx/At)	0.005	0.003	-0.003**	-0.003
	(0.004)	(0.007)	(0.001)	(0.003)
Log Total Assets	-1.029***	-0.326	0.005	-0.008
	(0.158)	(0.238)	(0.010)	(0.026)
B.				
Institutional Owners %	0.088***	0.018	0.002	-0.005
	(0.012)	(0.024)	(0.003)	(0.010)
Herfinal Index	-0.012***	-0.002	0.003*	0.003
	(0.004)	(0.007)	(0.002)	(0.004)
Number Proposals	-0.011	0.048	NA	NA
	(0.039)	(0.128)		
Polynomial in the vote share	no	yes	no	yes

Table 4
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 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 6and 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	0.0508***	0.0557	0.0853**	0.106*	0.127*	0.122***	0.141***	0.095***	0.104**
	(0.0195)	(0.0345)	(0.0412)	(0.0565)	(0.0702)	(0.0421)	(0.0514)	(0.029)	(0.04)
Obs	2,807	822	415	231	139	2,807	2,807	2,807	2,807
Rsq/Z	0.048	0.006	0.012	0.017	0.028	0.053	0.053	3.24	2.36

Panel B: Unconditional Premium

	Full	+/-10	+/-5	+/-2.5	+/-1.5	poly hl 2	poly hl 3	IK	CCT
yes	2.601***	3.235**	3.852**	5.379**	7.237**	6.571***	6.542***	4.76***	5.36**
	(0.792)	(1.266)	(1.801)	(2.641)	(3.151)	(1.758)	(2.267)	(1.43)	(2.21)
Obs	2,807	822	415	237	150	2,807	2,807	2,807	2,807
Rsq/Z	0.033	0.011	0.012	0.021	0.039	0.038	0.039	3.30	2.42

Table5
Takeover Probability Over Multiple Years and without Censoring

This table presents the effect of passing an anti-takeover proposal on the probability of becoming a target over multiple years. For Panel A, the probability of becoming a target is estimated for year 1 to 5 after the vote. For Panel B, the probability is estimated in a cumulative fashion from year 1 to 5 and we avoid the censoring effect by adjusting the years in the sample -- hence in column 5 all observations will have 5 years of data after the vote. All estimates use the local linear regression approach by Imbens Kalyanaraman (2012). 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 in Year N

	(1) In Year 1	(2) In Year 2	(3) In Year 3	(4) In Year 4	(5) In Year 5
yes	0.042**	-0.010	0,025**	0.014	0.036*
	(0.017)	(0.012)	(0.010)	(0.011)	(0.020)
Obs	2,807	2,807	2,807	2,807	2,807
Z	2.37	-0.9	2.2	1.19	1.79

Panel B: Cumulative Probability of becoming a takeover target over the next (N) years

	(1) Over next 1 year	(2) next 2 years	(3) next 3 years	(4) next 4 years	(5) next 5 years
Voting Sample	94-2013	94-2012	94-2011	94-2010	94-2009
yes	0.042**	0.032*	0.061**	0.073**	0.089**
	(0.017)	(0.019)	(0.023)	(0.026)	(0.033)
Obs	2,807	2,744	2,637	2,520	2,390
Z	2.37	1.68	2.63	2.80	2.69

Table 6

Conditional Independence Test

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.

	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	-0.00230***	-0.000157	-0.0018**	-0.00015	-0.0746**	-0.0153	-0.0355	0.0308
	(0.000874)	(0.000907)	(0.000862)	(0.000999)	(0.0321)	(0.0338)	(0.0356)	(0.0414)
ln Sales		-0.0173		-0.0309**		-1.593***		-0.723
		(0.0150)		(0.0152)		(0.558)		(0.629)
Profit Margin		0.237***		0.00359		5.191*		-1.758
		(0.0763)		(0.0835)		(2.844)		(3.457)
Ln Market Value		-0.0141		-0.0128		-0.387		-0.695
		(0.0140)		(0.0142)		(0.520)		(0.588)
Cash Liquidity		0.0798		0.173*		2.392		10.24**
		(0.108)		(0.102)		(4.042)		(4.226)
Percent Inst. Own.		0.0365		-0.182**		3.295		-9.134***
		(0.0650)		(0.0820)		(2.420)		(3.394)
Av. Ind. Tobins'Q		0.00610		0.0151		0.414		0.444
		(0.0112)		(0.0108)		(0.419)		(0.448)
Av. Ind. Market Value		0.0716***		0.0225*		1.583***		0.0712
		(0.0117)		(0.0133)		(0.434)		(0.549)
Entrenchment Index		0.0127		0.0111		-0.167		0.524
		(0.00812)		(0.0103)		(0.303)		(0.425)
Year Dummies		Y		Y		Y		Y
Obs	1,225	1,225	1,067	1,067	1,225	1,225	1,067	1,067
R-sq	0.006	0.137	0.0032	0.0590	0.004	0.110	0.001	0.076

Table 7
CIA Estimates and Propensity Score Matching

This Table reports estimates of the effect of passing a proposal to remove and anti-takeover provision on the takeover probability and the unconditional premium using the Angrist Rokkanen (2014) methodology. 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 6: Log 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.

Panel A: Propensity Score Weighting

	(1)	(2)	(3)	(4)
	Takeover Probability		Unconditional Premium	
yes	0.0451**	0.0476*	2.76***	2.806**
	(0.0208)	(0.025)	(1.036)	(1.17)
t stat	2.17	1.88	2.67	2.40
Model	Y	N	Y	N
Obs	2,051	2,052	2,052	2,053

Panel B: Nearest Neighbor Matching with clustering

	(1)	(2)
	Takeover Probability	Unconditional Premium
yes	0.0349*	2.511***
	(0.0210)	(0.913)
Obs	2,292	2,292

Table 8
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. Column (3), (4) report premiums based on abnormal returns using the FFM factors, the different windows (+/- 1 day +/- 5 days) and are relative to the Announcement date. Column (5) reports the cumulative abnormal returns from the vote date until the announcement date plus one day, using the FFM factors.

	(1)	(2)	(3)	(4)	(5)
	Premium 4weeks before Announce. to Completion	Premium 1 week before Announce. to Completion	CAR(-1,1)	CAR(-5,5)	CAR (Vote,Ann+1)
Panel A: Lower Bound Estimation					
yes	0.189	6.233*	0.085***	0.0780***	0.195
	(4.051)	(3.450)	(0.015)	(0.0158)	(0.210)
Z	0.05	1.81	5.58	4.93	0.93
Panel B: Upper Bound Estimation					
yes	5.356**	9.89***	0.112***	0.099***	0.421**
	(2.860)	(3.207)	(0.015)	(0.022)	(0.196)
R-sq/Z	1.88	3.08	5.07	4.32	2.15
Obs selected	410	408	401	401	398
Obs	2292	2292	2292	2292	2292

Table 9
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 7- column 3-. Column (2) to column (5) provide an estimate of the four different components that affect shareholder value via changes in the probability of a takeover, changes in the premium 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 7, column 1. Column (4) estimates the interaction effect and column (5) 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 = 0.2			
2.7%	0.027%	1.33%	1.34%
	(1%)	(49%)	(49%)
Panel B: Upper Bound Estimation of β = 5.3			
2.7%	0.71%	1.33%	0.65%
	(26%)	(49%)	(24%)

Table 10
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. Column (1) reports the effect on the Acquirer Premium (computed as the change in price 40 days before announcement until one day after). Column (2) shows the abnormal returns of the bidder on a +/- 1 day window around the announcement of the deal. Column (3), (4) and (5) report the effect on the Number of Bidders, the deal being Unsolicited and the deal being Challenged. Column (6) reports the effect on the percentage of Stock paid for the target. Column (7) reports the effect on the likelihood of Target and Acquiror belonging to the same 2-digit SIC code.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Bargaining		Competition				Matching
	Acquirer Premium	Acquirer CAR(-1,1)	Number of Bidders	Unsolicited	Challenged Deal	Stock Percent	Same 2Digit SIC
Panel A: Lower Bound Estimation							
yes	-0.069**	-0.0289**	0.155**	0.037	0.109***	-27.100***	0.178**
	(0.030)	(0.011)	(0.0637)	(0.030)	(0.038)	(7.262)	(0.068)
Z	-2.23	-2.50	2.44	1.23	2.85	-3.73	2.62
Panel B: Upper Bound Estimation							
yes	0.00021	0.0271**	0.26**	0.096	0.167*	-3,136	0.237***
	(0.0313)	(0.0129)	(0.1001)	(0.092)	(0.091)	(5.391)	(0.067)
R-sq/Z	0.01	2.09	2.62	1.04	1.81	-0.58	3.51
Obs selected	272	273	410	410	410	219	410
Obs	2292	2292	2292	2292	2292	2292	2292