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Which antitakeover provisions deter takeovers?

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Abstract

Antitakeover provisions play a central role in corporate governance research. But there is little agreement over which, if any, provisions actually affect takeover likelihoods. In tests that account for the endogenous use of antitakeover provisions, we find that at most 11 of the 24 G-index provisions are negatively related to takeover likelihood. Various indices used in the literature measure takeover deterrence to the extent they include this subset of provisions. In more stringent tests, only four provisions are consistently and negatively related to takeover likelihood throughout the 1995-2020 period, while one provision (golden parachutes) is positively related to takeover likelihood.

Keywords: Antitakeover provisions, takeover defenses, takeover likelihood, governance, acquisitions

JEL Classifications: G34, K22, L14

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Which antitakeover provisions deter takeovers?

1. Introduction

Many research papers use provisions from Gompers, Ishii, and Metrick's (2003) G-index to measure the strength of a firm's takeover defenses.[1] But, for such a well-developed literature, there is little agreement about which specific provisions are best suited for this purpose. Field and Karpoff (2002) use an index of 10 provisions; Cremers and Nair (2005) propose an alternative takeover index (ATI) of five provisions; Bebchuk, Cohen, and Ferrell (2009) propose an E-index of six provisions; Cremers, Masconale, and Sepe (2016) argue that only three of the E-index provisions relate to entrenchment; and Cain, Davidoff, and McKeon (2017) propose an index that emphasizes the state antitakeover laws that are part of the G-index. Some researchers, e.g. Bates, Becher, and Lemmon (2008), use an index with only one provision such as classified boards, and other unique subsets of provisions are common.[2] The widespread use of such individualized measures reflects disagreement about two important questions: Which provisions actually impact a firm's takeover likelihood? And which provisions should be used for well-specified empirical tests?

This paper seeks to answer these questions empirically using U.S. data. Later, we incorporate theoretical arguments that some provisions should be more influential than others. But our main inferences are based on data-driven, atheoretic tests that examine the relation between firms' takeover likelihoods and their uses of the specific provisions that comprise the G-index. In so doing, we deliberately adopt an inductive approach, drawing inferences from patterns in the data rather than imposing theoretical assertions about which provisions are most influential.

To clarify our objective, we note that many papers already examine whether provisions from the G-index are related to outcomes such as firm value or stock returns (e.g., Gompers et al., 2003; Bebchuk et

[1] Lucian Bebchuk's website lists more than 300 papers that use the E-index alone. As of May 1, 2022, 10,298 papers on Google Scholar cite Gompers, et al. (2003) and 4,754 papers cite Bebchuk et al. (2009).
[2] See, e.g., Cremers, Nair, and John (2009), Kadyrzhanova and Rhodes-Kropf (2011), Harford, Humphery-Jenner, and Powell (2012), Liu and Mulherin (2018), and Guernsey, Sepe, and Serfling (2022).

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al., 2009).[3] Such investigations involve tests of the joint hypotheses that (i) the selected provisions affect the firm's takeover likelihood and (ii) a change in takeover likelihood affects the outcome variable (e.g., firm value). As an example, Bebchuk et al. (2009) conclude that fair price provisions are unrelated to firm value. This can mean that fair price provisions do not affect takeover likelihoods, or, alternatively, that they do affect takeover likelihoods but in ways that have zero average net impact on firm value. Our tests focus on the first of these two joint hypotheses and not the combination. We examine whether fair price provisions – and each of the other 23 G-index provisions – affect firms' takeover likelihoods.

A challenge for our investigation is that firms' antitakeover provisions and takeover likelihoods may be endogenous. To identify causal influences of each provision on takeover likelihood, we construct two separate instruments to identify exogenous variation in firms' use of that provision. The first instrument is based on predetermined (i.e., five-year lagged) information from non-industry matched geographical peer firms. The second instrument draws from previous evidence that firms' antitakeover provisions are determined largely at the times of their IPOs and that there is a large time component to the popularity of various antitakeover provisions at IPO firms.[4] Following guidelines discussed in the literature, we test and confirm that both instruments meet the requirements for strong instruments. Section 3 discusses the instruments in detail and argues that both reasonably meet the exclusion restriction. In addition, we corroborate inferences from our two-stage least squares and limited information maximum likelihood models using recursive bivariate probit models, which can yield unbiased estimates even if the exclusion condition is not met (Wilde, 2000; Greene, 2003, pages 715-16).

As reported in Section 4, the evidence suggests that endogeneity is more important for some provisions than for others. Some empirical approaches used to account for endogeneity, such as two-stage least squares (2SLS), are well suited to estimate the marginal effects of the endogenous provisions but yield

[3] Gompers Ishii, and Metrick (2003) formulate the 24-provision G-index to measure shareholder rights and not takeover deterrence per se. Bebchuk, Cohen, and Ferrell (2009) select six of the G-index provisions to formulate the E-index as a measure of managerial entrenchment. Regardless of their original formulations, many researchers use these indices, or components of these indices, to measure takeover deterrence.

[4] Johnson et al. (2021) show that firms maintain their pre-existing antitakeover provisions in 94% of firm-years during the 15 years after their IPOs, and various researchers (e.g., Daines and Klausner 2001) show that there is a strong time component to IPO firms' use of antitakeover provisions.

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inefficient estimates for non-endogenous provisions. We therefore test for endogeneity and emphasize results from 2SLS, limited information maximum likelihood (LIML), and recursive bivariate probit (RBP) models when endogeneity is indicated, and emphasize results from OLS and probit models when endogeneity is not indicated. We also derive inferences from different specifications within each of these approaches that control for, or exclude, the other G-index provisions when estimating the marginal effect of a given provision.

Of the 24 G-index provisions, only 13 are related to takeover likelihood in the empirical tests. Eleven of these provisions are negatively related to takeover likelihood in at least some tests, including anti-greenmail provisions, blank check preferred stock, classified boards, director contracts, director indemnification, director liability provisions, directors' duties provisions, executive severance contracts, fair price provisions, supermajority vote requirements for mergers, and unequal voting rights. Two provisions are positively related to takeover likelihood – golden parachutes and restrictions on action by written consent – implying they should be given negative weight if they are included in an index to measure a firm's takeover defenses.

The empirical relations between some provisions and takeover likelihood have changed over time. Our most strict screen indicates that only four provisions (supermajority vote requirements for mergers, directors' duties provisions, fair price provisions, and unequal voting rights) are consistently and negatively related to takeover likelihood throughout the 1995-2020 period, while one provision (golden parachutes) is consistently and positively related to takeover likelihood. Notably, the initial negative relation between the use of a classified board and takeover likelihood does not continue in the later years of our sample, possibly due to activism that corresponds with a decrease in the use of classified boards among large firms over time.

These results identify the subset of provisions that best reflect a firm's empirical likelihood of remaining independent and that can serve as proxies for a firm's takeover defenses. They also inform long-running theoretical debates in the corporate governance literature. One debate is whether the actual deployment of a (clear day) poison pill provides incremental takeover protection. The G-index includes poison pills, and Bebchuk et al. (2009) include poison pills in the E-index because they view pills as offering

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a strong defense against unsolicited takeover bids. Coates (2000) and Catan (2019), however, argue that the actual deployment of a poison pill affords no incremental takeover protection because virtually all firms have shadow poison pills, which is the right to adopt a poison pill at any time without seeking shareholder approval. Our results support this latter argument, as we find that clear-day poison pills are not associated with incremental takeover deterrence.[5]

A second debate is whether shadow poison pills render most other antitakeover provisions meaningless. Citing the availability of shadow pills, Klausner (2013) and Catan and Kahan (2016) argue that most firm-level antitakeover provisions and state antitakeover laws offer negligible incremental takeover protection. In contrast, Cain, Davidoff, and McKeon (2017) and Karpoff and Wittry (2017) argue that other antitakeover provisions and court decisions also help to deter takeovers. Our results are consistent with this latter argument, as we find that several of the G-index provisions offer incremental takeover defense.

A third debate our results address is whether golden parachutes increase or decrease firms' takeover likelihood. Both the G-index and E-index include golden parachutes. Empirical research that uses either index to measure takeover deterrence therefore requires an assumption that golden parachutes deter takeovers. Some researchers, however, argue that golden parachutes are, if anything, positively related to takeover likelihood (see Sokolyk, 2011; Goktan and Kieschnick, 2012; and Bebchuk, Cohen and Wang, 2014). Our tests support this latter position, as we find that golden parachutes are not takeover deterrents and have a positive effect on takeover likelihood.

Next, we examine several commonly used sets of provisions to understand whether these various combinations are correlated with takeover deterrence and, if so, why. The sets include the G-index, E-index, Cremers and Nair's (2005) ATI, Field and Karpoff's (2002) group of ten provisions, and specific combinations of provisions suggested in the literature. We show that the ability of the G-index, E-index, or any other group of provisions to explain takeover likelihoods is attributable to the inclusion of one or more

[5] Our focus is on the direct effect of each provision on takeover likelihood, so our results do not speak to whether the deployment of a poison pill is associated with other effects, e.g., on firm value or on the managers or directors who adopt them.

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of the 11 provisions that individually are related to takeover deterrence. The inclusion of other provisions (or including golden parachutes with the wrong sign) merely adds bias or measurement noise to an index that is used to measure a firm's takeover deterrence.

This finding raises the question of whether the measurement noise in the G-index or E-index is large enough to affect inferences. We examine this question by revisiting a debate over whether a firm's takeover defenses affect its expected takeover premium. Prior evidence is mixed, as some findings indicate that G-index provisions decrease the unconditional takeover premium (e.g., see Cuñat, Gine, and Guadalupe, 2020) while other findings indicate that some G-index provisions are positively or not significantly related to the takeover premium (e.g., Comment and Schwert 1995; Becher, Bates, and Lemmon 2008; Sokolyk 2011). In our sample there is a statistically insignificant relation between a firm's unconditional premium and its takeover defenses as measured by either the G-index or E-index. In contrast, an index comprised of the 11 provisions that our analysis shows are negatively related to takeover likelihood is negatively and significantly related to the unconditional takeover premium. These findings illustrate that empirical inferences can be affected by the measurement noise in the G-index and E-index.

This paper makes three contributions to the literature. First, in tests that account for endogeneity, we provide evidence on which antitakeover provisions are empirically related to takeover deterrence. This evidence can help to improve tests that rely on measures of takeover deterrence, such as those concerning the long-running debate over whether deterrence serves to increase or decrease firm value.[6] Second, our results address theoretical debates about the impacts of many individual antitakeover provisions. For example, we show that takeover deterrence is not well measured either by the full set of 24 G-index provisions or by focusing only on classified boards. Also, clear-day poison pills do not offer incremental takeover deterrence and golden parachutes are positively, not negatively, related to takeover likelihood. Third, our results help to explain why many papers can find that the G-index and E-index are related to

[6] See Burkart and Panunzi (2006) and Straska and Waller (2014) for surveys of the literature on takeover defenses and firm value. On one side of the debate, Bebchuk et al. (2009) conclude that takeover defenses are entrenching devices that decrease firm value. On the other side, Cremers et al. (2017) conclude that defenses serve shareholder interests by increasing value. Johnson et al. (2021) argue that defenses convey both costs and benefits that tend to change with firm age such that defenses typically add value for young firms and decrease value for older firms.

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various firm outcomes, while other papers conclude that the G-index and E-index are poor measures of a firm's takeover defenses (e.g., see Core, Guay, and Rusticus 2006). This is because the G-index and E-index combine provisions that are negatively related to takeover likelihood with provisions that are not significantly related to takeover likelihood or that have positive marginal effects on takeover likelihood. We infer that the G-index and E-index are serviceable measures of takeover deterrence in some applications, but they contain measurement noise that is large enough to affect empirical inferences in other applications.[7]

This paper proceeds as follows. Section 2 describes firms' uses of the 24 provisions that are in the G-index and discusses related research. In Section 3 we describe the data and our empirical approaches to address the endogenous nature of firms' antitakeover provisions. Section 4 reports results of several tests of each individual provision's effect on takeover likelihoods. Section 5 draws inferences from these tests to identify the 11 provisions that are negatively related to takeover likelihood and two provisions that are positively related to takeover likelihood. Section 6 examines why some popular indices, including the G-index and E-index, are serviceable but noisy measures of takeover deterrence in tests that account for endogeneity, and Section 7 examines changes in the provisions' deterrence effects over time. Section 8 shows that measurement noise in the G-index and E-index can affect empirical inferences using a test of firms' unconditional takeover premiums. Section 9 concludes.

2. Firms' uses of antitakeover provisions and related research

Table 1 reports on firms' uses of the 24 G-index provisions using data from the Investor Responsibility Research Center (IRRC) from 1990 to 2006. The IRRC data were acquired by different organizations over several years starting in 2005.[8] Major changes occurred during those years in the way the provision-level data were collected and the type of information covered. For this reason, and to be consistent with the large body of literature that has focused on the earlier data, for our main tests we focus

[7] Larcker, Reiss, and Xiao (2015) and Frankenreiter, Hwang, Nili and Talley (2021) report that the G-index and E-index are noisy measures also because of errors in the counts of specific provisions.
[8] The IRRC data have been acquired and variously controlled by ISS, Riskmetrics, and MSCI, and have been listed on the WRDS platform alternatively under the Riskmetrics and ISS names. In this paper we refer to the 1990-2006 data on takeover defenses as the IRRC data.

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on the IRRC data published in 1990, 1993, 1995, 1998, 2000, 2002, 2004, and 2006. Each of these volumes provides corporate governance information for between 1,400 and 2,000 firms with a general focus on S&P 1500 firms.

We end in 2006 because this is the last year for which the data are collected and reported in a manner that is consistent with earlier years. Antitakeover provision data are available from Institutional Shareholders Services (ISS) after 2006, but as shown in the Internet Appendix Figure B.2, there is a structural break in the counts of some provisions after 2006. Nonetheless, in Section 8 we extend our analysis using the ISS data for the 2007-2020 period.

[Insert Table 1]

The data show that there is widespread use of many of the provisions, with 17 of the 24 provisions being deployed at more than 10% of firms in 2006. The most frequent provisions include blank check preferred stock, straight (not cumulative) voting, non-secret ballots, coverage by business combination laws, and golden parachutes.

Internet Appendix A describes each provision and summarizes prior research regarding its possible effect on takeover likelihood. The literature contains many theoretical conjectures about the impacts of various antitakeover provisions on takeover likelihood. The empirical evidence about these provisions, in contrast, is both limited and mixed. For example, Ryngaert (1988) and Field and Karpoff (2002) report evidence that is consistent with the inference that poison pills deter takeover bids, whereas Comment and Schwert (1995), Heron and Lie (2006), and Cremers et al. (2020) find that existing or shadow pills increase takeover premia without decreasing takeover likelihood. As another example, Bebchuk et al. (2002), Coates (2000), and Sokolyk (2011) find no significant relation between the use of a fair price provision and takeover likelihood, whereas Borokhovich, Brunarski, and Parrino (1997), Field and Karpoff (2002), and Goktan and Kieschnick (2012) find a significant negative relation. Internet Appendix A documents similarly mixed results regarding the deterrence effects of such other provisions as classified boards, limits on shareholders' abilities to amend bylaws or the corporate charter, supermajority vote requirements for mergers, directors' duties provisions, business combination laws, and blank check preferred stock.

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A key characteristic of most of these studies, and a possible reason for their mixed results, is that they do not account for the endogenous use of antitakeover provisions. As an exception, John, Kadyrzhanova, and Lee (2021) use merger waves and state poison pill endorsement laws to identify tests of the effect of classified boards on takeover likelihood. Cuñat, Gine, and Guadalupe (2020) achieve identification with a regression discontinuity approach around shareholder votes close to the majority requirement, and show that takeover probabilities increase when shareholders vote to remove antitakeover provisions. Their tests, however, group all proposals together and do not examine the marginal effect of each provision, which is our focus.[9]

Our paper is also related to Karpoff, Schonlau, and Wehrly (2017), who find that, after accounting for endogeneity, the G-index or E-index are negatively related to takeover likelihood. That paper, however, also does not examine which individual provisions matter for takeover deterrence. By focusing on the deterrence effects of each individual provision, our paper provides new evidence on which provisions yield reliable specifications in tests that seek to control for, or measure the effects of, takeover deterrence. This allows us also to address ongoing debates over whether specific provisions have significant effects on takeover likelihood (and whether the effects are positive or negative), and to demonstrate how the inclusion of irrelevant provisions in the G-index and E-index introduces noise that affects empirical inferences.

3. Data and empirical approach

3.1. Sample description

Following the existing literature, we fill in the IRRC data for missing years by projecting forward

[9] Other researchers have attempted to account for endogeneity in various ways as they examine the relation between antitakeover provisions and other outcome variables such as Tobin's *q*. For example, Bebchuk et al. (2009) and Straska and Waller (2010) employ lagged provisions to address endogeneity concerns. Cremers and Ferrell (2014) focus on differences in the relation between firm value and provisions before and after the 1985 *Moran v. Household International, Inc.* decision to achieve identification. Kadyrzhanova and Rhodes-Kropf (2011) use age-at-IPO as an instrument in a two-step estimation approach concerning the relation between provisions and takeover premiums. While these papers focus on different outcomes, they highlight researchers' previous efforts to address the problem of endogeneity as they examine the joint hypotheses that (i) their selected provisions affect takeover likelihood and (ii) the change in takeover likelihood affects their outcome variable. Again, our focus is on only the first of these two joint hypotheses.

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from the most recent IRRC data. For example, the IRRC governance data from 1993 are used in 1994 and the 1995 data are used for 1996 and 1997. We assume the 2006 data stay in effect for four years. Thus, our main governance sample includes firm-years from 1990-2010. As discussed in Sections 3.3.a and 3.3.b, we construct our instruments using IRRC data with a five-year lag. This means the takeover models are estimated starting in 1995. After imposing all the data requirements we have 19,385 firm-years in the main sample. Table 2, Panel A reports the number of firms each year and the average number of provisions at those firms.

[Insert Table 2]

We combine the provision-level data from IRRC with acquisition data from the Thomson Security Data Company (SDC) and financial and industry information from Compustat and CRSP. Target firms are identified using the SDC data on U.S. targets with deal form M, AM, or AA and with a completed status. This approach follows prior empirical research on takeover likelihoods.[10] In particular, we focus on whether an antitakeover provision is associated with the firm's continued independence as a distinct entity. Not only does this approach follow the literature, it also avoids controversy over how to classify acquisitions as friendly or hostile, and whether such distinctions are meaningful. Boone and Mulherin (2009) show that the acquisition process is more complex than frequently characterized, and Schwert (2000) points out that "hostility" reflects bidders' and targets' strategic choices. Schwert (2000) also shows that most acquisitions described as hostile are not economically distinguishable from deals described as friendly.

The control variables used to explain takeovers also are motivated by prior work on takeover likelihoods. These variables include firm size (AT), leverage (DLTT/AT), the market-to-book ratio ((CSHO*PRC + DLTT)/AT), return on assets (OIADP/AT), the property ratio (PPEGT/AT), the liquidity ratio ((ACT-LCT)/AT), average sales growth over three years (average($(SALE_t - SALE_{t-1})/SALE_{t-1}$)), the prior one-year market-adjusted return, and industry concentration as measured year-by-year using the

[10] For examples, see Palepu (1986), Ambrose and Megginson (1992), Song and Walkling (1993), Comment and Schwert (1995), Field and Karpoff (2002), and Karpoff, Schonlau, and Wehrly (2017).

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Herfindahl-Hirschman index based on sales.[11] Industry controls are based on the Fama-French 49 industries, although our main results are robust to using Global Industry Classification Standard industries in place of the Fama-French industries.

3.2. Empirical methods

Our empirical investigation faces two main complications. First, if provision use and takeover likelihood are jointly determined, ordinary least squares estimates of a provision's marginal deterrence effect will be biased. To address this issue we use several empirical methods to examine and account for endogeneity, relying on a combination of results from ordinary least squares (OLS), two-stage least squares (2SLS), limited information maximum likelihood (LIML), probit, and recursive bivariate probit (RBP) models. As described in more detail below, the LIML and RBP models rely on different underlying assumptions and provide a way to corroborate or refute the 2SLS results using alternative empirical approaches.

Second, it is not obvious how best to account for the various other provisions within the G-index when estimating the marginal effect of a single provision. One approach would be to include all other 23 provisions in every test for any single provision and then to instrument for all 24 provisions. In practice, however, including multiple first stage equations at once exacerbates the 2SLS "blow up" problem for errors in the second stage equation, particularly because our evidence shows that the instruments for some provisions are weak (Jiang 2017). To keep our tests empirically tractable, we estimate the marginal effect of each provision using: (1) models that include the other 23 provisions individually, (2) models that include the other 23 provisions as an index, and (3) models that do not include the other 23 provisions at all. We seek to err on the side of inclusivity. So, to make preliminary inferences from 2SLS and OLS models, we provisionally infer a significant relation with takeover likelihood if the results from any one (or more) of

[11] If PPEGT is missing but PPENT is not, we use PPENT to calculate the property ratio. ACT and LCT are not reported in Compustat for banks, so requiring these variables eliminates banks from our sample.

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the three models are statistically significant. We probe these preliminary inferences using the results from LIML, RBP, and Probit tests.

The five types of models (2SLS, OLS, LIML, RBP, and Probit) reflect different distributional assumptions and have different sensitivities to the effects of endogeneity, and therefore provide a robustness check for our main inferences. In addition, Section 5 explores the sensitivity of our results to various groupings of provisions employed by previous researchers, and the Internet Appendix reports alternative specifications. The empirical results are not materially affected if we include or leave out the other 23 provisions when we instrument for and test one provision at a time. This result implies that, even if it were tractable to include separate instruments for all 24 provisions in a single 2SLS model with 24 first-stage equations, the results would be similar to the tests we are able to conduct.

The 2SLS, LIML, and RBP models are each based on two equations, with the 2SLS version displayed in Equations 1 and 2. Equation 1 models takeovers (y_1) as a function of the individual provision (p_1) and a set of control variables ($x_1, \ldots, x_k$). Equation 2 is the "first-stage" equation and models the endogenous variable (p_1) as a function of instrument(s) (z) and the same set of control variables as used in the takeover model. Identification is achieved by isolating exogenous variation in the endogenous variable using variation in the instrument and, as discussed below, requires that the relevancy and exclusion conditions are met.[12]

$$y_1 = \partial p_1 + \beta_1 x_1 + \cdots \beta_k x_k + u \qquad \text{(takeover model)} \qquad (1)$$

$$p_1 = \gamma_1 z + \gamma_2 x_1 + \cdots + \gamma_k x_k + e \qquad \text{(first-stage model)} \qquad (2)$$

We also estimate the same equations using a limited-information maximum likelihood (LIML) approach. As noted by Stock and Yogo (2005), Hayashi (2000, page 542), and Cameron and Trivedi (2010, page 204), 2SLS and LIML estimators have the same asymptotic distributions. LIML estimates, however,

[12] Using 2SLS with a linear probability model (LPM) is consistent with the approach discussed in econometrics texts such as Angrist and Pischke (2009, page 198) and Cameron and Trivedi (2010, page 485). Cameron and Trivedi note that using a 2SLS approach with a LPM provides consistent estimates but that heteroskedasticity-robust standard errors must be used for inference.

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are more robust to finite sample bias and weak instruments than 2SLS estimates. For these reasons, and following Angrist and Pischke (2009, pages 209-213) we use the LIML models to corroborate our inferences from overidentified 2SLS models. We obtain similar results using either methodology.

Both 2SLS and LIML approaches rely on variation in the instrument to identify exogenous variation in p_1. For identification, an instrument must meet both relevance and exclusion conditions (e.g. Roberts and Whited 2013). The relevance condition is testable, and we report the first-stage F-statistic and the R-squared value for each of our tests. Staiger and Stock (1997) suggest a rule-of-thumb that the F-statistic be at least 10 for a strong instrument in a 2SLS setting and Stock and Yogo (2005) note that the F-statistic can be even smaller than 10 for strong instruments in comparable LIML models.

The exclusion condition requires $\text{cov}(z, u) = 0$ and can be thought of as the requirement that the instrument only affect whether a specific firm is acquired (y_1) via its relation with the endogenous provision (p_1) and not via some other pathway captured in the error term. Because the exclusion condition is not directly testable we discuss the creation of our instruments in detail in the next section and argue that the exclusion condition is plausibly met.

Additionally, we estimate takeover likelihoods using recursive bivariate probit (RBP) models. RBP models are specific to settings, like ours, in which both the endogenous variable and the final outcome variable are binary. The RBP approach allows for, and models, the correlation in errors across equations. This has two advantages: (1) correlation between the omitted variables in both equations can exist without creating potential bias in the RBP estimates, and (2) testing whether the correlation is statistically different from zero is a test of whether the provision is endogenous.[13] Also, as opposed to 2SLS models, the exclusion condition is not necessarily required for identification in RBP models (e.g., Wilde 2000; Greene 2003, pp. 714-717) although it helps (e.g., Wooldridge, 2010, pp. 598-599). Greene specifically notes that for the RBP model, "the endogenous nature of one of the variables on the right-hand side of the first equation can be ignored in formulating the log-likelihood…" and that we "can ignore the simultaneity in this model and

[13] See Wooldridge (2010, pp. 591-597) and Greene (2003, pp. 712-717). If the correlation is not statistically different from zero, then an RBP model is unnecessary and a single probit model can be used.

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we cannot in the linear regression model…" (Greene 2003, pp. 715-716). Thus, the RBP model represents a way to further probe our conclusions from the other models using an empirical approach that depends less on the (non-testable) exclusion condition for identification. A potential drawback with using an RBP model is that, without an excluded instrument, identification and inference can depend on the assumed nonlinear distribution.[14]

In an RBP model the two left-hand side variables in Equations 1 and 2 are considered latent variables (y_1^*, p_1^*). By assumption, y_1 and p_1 are observed to equal 1 when their underlying respective latent variables are above a certain threshold. Following the notation in Greene (2003), the RBP model can be written as $Prob[y_1 = 1, p_1 = 1 | \boldsymbol{x}_1, \boldsymbol{x}_2] = \Phi_2(\boldsymbol{x}_1'\boldsymbol{\beta}_1 + \delta p_1, \boldsymbol{x}_2'\boldsymbol{\beta}_2, \rho)$, where in our application y_1 is set to 1 if the firm is acquired within the next year, p_1 is set to 1 if the firm has the provision, $\boldsymbol{x}_1$ represents the set of control variables included in the takeover equation, $\boldsymbol{x}_2$ represents both this set of control variables as well as the instruments, $\boldsymbol{\beta}_1, \boldsymbol{\beta}_2$ represent the coefficients on the variables included in $\boldsymbol{x}_1$ and $\boldsymbol{x}_2$, Φ_2 represents the bivariate normal cumulative distribution function, and ρ is the correlation between the error terms in the two equations where the error terms are assumed to be distributed bivariate normal with mean zero and unit variance. The coefficients are estimated using maximum likelihood.[15]

3.3. Instrumental variables – motivation and calculation

To satisfy the relevance and exclusion requirements described above, our instruments should (1) strongly correlate with the provision at the firm, and (2) not relate to the likelihood of takeover of that firm in that year in other ways. We use two distinctly different types of instruments. The first is based on the

[14] Wilde (2000) shows that identification is possible in a recursive bivariate probit model even without an excluded instrument as long as the data matrix is full rank and sufficient variability exists in the included regressors. Others (e.g., Mondfardini and Radice 2008; Marra and Radice 2011; Mourifie and Meango 2014; and Wooldridge 2010, pages 595-599) note that while this conclusion is correct, identification can be fragile in practice if the existing variation in the included regressors is small and also that inference can be problematic if the assumed functional form is not correct. For our purposes, we note that our included regressors provide substantial variation and that we include the predetermined instruments as part of the system of equations. Hence, in our setting identification can come from both the functional form and the exclusion condition.

[15] For more information see the discussion in Greene (2003, pp. 715-716). Other finance studies employing bivariate probit models include Bates, Becher, Lemmon (2008), Ljungqvist, Marston, and Wilhelm (2009), Jiang, Li and Wang (2012), Cornelli, Kominek, and Ljungqvist (2013), and Yermack (2014).

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five-year lagged incidence of provisions at geographically-proximate firms that are not in the same industry as the focus firm. The second is based on the lagged incidence of provisions at firms that went public within one year of the firm in question but that are not in the same industry.[16] For each type of peer group, we construct the instruments using a five-year lag to better assure that the instrument is predetermined relative to the year in which we estimate takeover likelihood.

3.3.a. Geography-based instruments

For the geography-based instruments, we use zip codes to identify all firms within a 100-mile radius of the focus firm's headquarters. We then eliminate firms within this group if they (1) have the same Fama-French 49 industry classification as the focus firm or (2) are located in a different state. If fewer than three peer firms are found using this approach then a statewide net is used instead of a 100-mile radius. Geographic proximity could explain takeover defenses if shared legal or consulting services by region or a spillover of management ideas occurred in the past at the local level (e.g., via university-sponsored CEO forums or local chapters of the National Association of Corporate Directors) and if takeover defense decisions, once made, are sticky over time as shown by Hannes (2006) and Johnson et al. (2021). Thus, the correlation in defenses observed among geographically proximate but non-industry peers during our sample period exists for reasons that occurred years before and that are not related to the current takeover likelihood of the firms. As reported in Internet Appendix Figure B.1, the firm headquarters of firms in our sample are also distributed widely across the US.

To illustrate the construction of the geography-based instruments, assume that there are two possible anti-takeover provisions (provisions A and B). Assume the firm in question has four geographically-proximate non-industry peer firms; the presence of provision A five years prior to the year of analysis using binary variables at these four firms is (0,0,1,1) and the presence of provision B at these four firms is (1,1,0,1). Using these numbers, 50% of the geographically-proximate firms had provision A

[16] Financial research using similar instrumental variables focused on non-industry peer groups include Carline and Gogineni (2021), Greene and Smith (2021), Karpoff, Schonlau, and Wehrly (2017), and Johnson et al. (2021).

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and 75% had provision B five years before the year of analysis. The instrument values for the focus firm in the current year would be 0.50 for provision A and 0.75 for provision B.

As discussed in Karpoff, Schonlau, and Wehrly (2017), we construct the instruments in this manner to address possible concerns about either industry-level influences (like industry merger waves), or current but unobserved factors that might relate to takeovers. Our instruments address these concerns because (1) the information used to calculate each firm's instrument is drawn from firms outside the focus firm's own industry, and (2) the information is drawn from five years in the past, and hence dates back to a period before conditions that might affect the takeover likelihood of the focus firm in the current year. Given that we also directly control for a host of firm-level factors at the focus firm as well as year and industry effects when modeling the takeover likelihoods, the predetermined information from outside a firm's industry should have no additional ability to explain a given firm's takeover likelihood five years later.

One concern is that geographic shocks or commonalities drive both antitakeover provisions and takeover likelihoods. For example, suppose geographic economic downturns affect multiple industries at the same time, encouraging both firms' adoption of antitakeover provisions and takeover likelihoods. This concern is mitigated by the fact that our tests control for firms' current fundamentals in real time. (Geographic economic shocks also do not explain the results from our second type of instrument that is based on IPO cohorts.) Concerns about geographically-specific economic factors also are mitigated by the fact that our sample focuses on S&P 1500 firms. These firms tend to have geographically dispersed operations and their takeover likelihoods are primarily a function of their overall fundamentals (again, for which we control) and not the economic circumstances at the narrow locales of their headquarters.

Hence, the exclusion condition is plausibly satisfied for the geography-based instrument because the predetermined takeover defense decisions of non-industry firms with physically proximal headquarters are not directly related to the specific and current takeover likelihood of the focus firm years later in our sample. Also, given that the geography instrument is based on headquarter locations and not state of incorporation, the instrument does not pick up the tendency for firms to select Delaware, Nevada, or other specific states due to their laws, as examined by Bebchuk and Cohen (2003) and Dyreng, Lindsey, and

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Thornock (2014). In robustness tests we confirm that our main inferences are not sensitive to using GIC as opposed to Fama-French 49 industry classifications.

3.3.b. IPO-year-based instruments

As an alternative to the geography-based instrument we also construct an instrument based on IPO-year peer effects. To construct this instrument we identify firms that went public within one year of the focus firm but that are not in the same industry. This instrument is based on evidence that a firm's use of takeover defenses is strongly influenced by the year it went public and that, once made, the takeover defense decisions are rarely changed during our sample period. Along these lines, Daines and Klausner (2001) and Field and Karpoff (2002) show that IPO firms' use of takeover defenses varies systematically over time and Hannes (2006) and Johnson et al. (2021) report that firms' use of takeover defenses is sticky after the IPO. For example, Johnson et al. (2021) report that firms go public with an average 2.4 of the six E-index provisions and maintain their prior year's defenses in 94% of all firm-years in their sample (and remove any defenses in only 1.3% of firm-years).[17]

The fact that takeover defenses are sticky by itself helps to identify our tests, as stickiness induces arbitrary deviations from firms' potentially targeted use of defenses as firms age, an observation also noted by Bebchuk (2003) and Johnson et al. (2021). But our instruments go much further to achieve identification, as it seems extremely unlikely that the provisions adopted by a firm's non-industry IPO-year cohort would have any direct relation with the focus firm's takeover likelihood many years later. As with the geography-based instrument, we further strengthen the argument that the exclusion condition is met by lagging all measures of IPO-year non-industry peer firms' takeover provisions by five years.

Given the size and age of firms covered in the IRRC data, many of the firms in our sample went public years before our sample period (1995-2010). We define IPO-year cohorts starting in 1950 and move forward year-by-year through 2010. All firms that went public before 1950 are included as part of the 1950

[17] In Section 7 we consider changes in the use of some provisions over time. Johnson et al. (2021) present evidence indicating that defenses are removed rarely because the cost of removal, which includes overcoming a collective action problem by shareholders, typically exceeds the benefits.

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cohort.[18] Using this approach results in most years having 10 or more peer firms, with some years having more than 100 peer firms per year. Following the logic introduced with the geography-based instrument, the IPO instrument for a provision at a given firm is calculated as the incidence of that provision five years in the past at the set of non-industry IPO-year cohort firms. In the Internet Appendix, Table B.1 provides the number of IPOs in each year for our sample, and Table B.2 provides raw correlations between G-index provisions and the geography- and IPO-year based instruments.

To illustrate the overall logic used for the IPO-year-instrument, consider Ross Stores, Microsoft, and Harley-Davidson, which share the same IPO cohort in our sample but come from three different industries (e.g., discount department stores, software, and motorcycles). At the time of their IPOs these firms had been developing for at least 28, 11, and 83 years, respectively. They differed significantly in terms of size, profitability, location, and industry in the year of the IPO and those differences became more pronounced over time. It seems plausible that, some years after their IPOs (say in 1995 during our sample period), the 1990 takeover provisions at Microsoft and Ross Stores can be used to instrument Harley-Davidson's takeover provisions in 1995. Indeed, it is difficult to conceive of a reasonable pathway by which the incidence of provisions at Microsoft and Ross Stores from five years before the analysis somehow has some other direct, but unknown, effect on Harley-Davidson's takeover likelihood in 1995 – especially after controlling for: (i) Harley-Davidson's own firm fundamentals in 1995 in the takeover model, and (ii) industry and year fixed effects. This example illustrates how the IPO-year-based instrument plausibly satisfies the exclusion condition.

3.3.c. Unobserved heterogeneity in group average instruments

Gormley and Matsa (2014) identify a potential problem with using group averages of an independent variable as an instrument that arises when unobserved heterogeneity exists at the group-level that is correlated with the outcome of interest. Suppose, for example, we were to instrument for a firm's

[18] The IPO dates are based on Compustat information. If Compustat does not report the year of the IPO we use the first year the firm appears in CRSP as the year of the IPO.

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takeover defenses using information from firms in the same industry. The firms in that industry could share characteristics that are correlated with both their takeover likelihoods and their use of takeover defenses, and our instrument would lead to inconsistent estimates even with industry fixed effects in the 2SLS equations. Fortunately, such a problem is unlikely in our setting, for several reasons. First, our geographic- and IPO-based peer groups explicitly exclude firms in the same industry as the focus firm, so any group-level unobserved heterogeneity would have to exist across industries. Second, any unobserved heterogeneity would need to persist across time and across different industries (given the five-year lags and the year fixed effects). Third, the unobserved heterogeneity would have to explain the focus firm's takeover likelihood over and above the many controls already included in the takeover model that control for the firm's own fundamentals. Fourth, we get similar results using two completely different instruments, so any unobserved group heterogeneity from the geography-based instrument would have to coincidentally generate similar empirical results as the unobserved group heterogeneity from a completely different group, namely the IPO-cohort firms. Finally, we note that the RBP models generate similar results to our tests that rely on the exclusion restriction for identification, even though the RBP models are not subject in the same way to exclusion restriction concerns. For all of these reasons, it is unlikely that our results are driven by group-level unobserved heterogeneity.

4. Individual provisions and takeover likelihoods

4.1. Preliminary inferences based on 2SLS and OLS models

We derive inferences about the marginal effect of each antitakeover provision on takeover likelihood from a combination of the results from the 2SLS, OLS, LIML, RBP, and probit models. Figure 1 illustrates our decision process. OLS estimates are more efficient than 2SLS estimates but, in the presence of endogeneity, OLS estimates are inconsistent (Cameron and Trivedi 2010, pages 177-182). So we begin by testing for the presence of endogeneity and emphasize inferences from the less-efficient 2SLS results when endogeneity is indicated.

[Insert Figure 1]

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Our initial test for endogeneity uses a variation of the Durbin-Wu-Hausman approach that is robust to heteroskedastic and clustered errors, as described by Wooldridge (2010, pages 129-132) and Cameron and Trivedi (2010, page 189).[19] If endogeneity is indicated (following the left branch of the diagram in Figure 1), we derive our initial inference about each provision from the 2SLS results. For most of the provisions we have strong instruments, as described below, and hence the Durbin-Wu-Hausman test provides good information about which of these provisions are endogenous after controlling for the other variables in the model. However, given that some of the instruments are of marginal strength, and the power of this test can be affected by weak instruments, we are conservative in this step and use a cut-off p-value of 0.15.[20] This is conservative in the sense that we identify the presence of endogeneity even if it is weakly indicated. Our main inferences are not sensitive to using a lower cutoff value because the collective results from the LIML, RBP, and probit test results, described below, are consistent with the 2SLS results for the affected provisions. For the subset of provisions for which endogeneity is not indicated (the right branch in Figure 1), we draw our initial inferences from the OLS results because OLS estimators are more efficient than 2SLS estimators in the absence of endogeneity.

Table 3 reports the tests on which we base preliminary inferences. The 2SLS coefficients are from the second stage linear probability takeover equation described in Section 3.2, in which the provision is instrumented using both the lagged geography- and IPO-year-based instruments described in Section 3.3. The first-stage 2SLS regressions are reported in Table B.3 of the Internet Appendix. (The 2SLS results in Table 3 are based on overidentified models. Internet Appendix Table B.4 shows that we obtain similar inferences for most provisions from just-identified models.) Each model reported in the table includes the controls described in Section 3.1, including firm size, leverage, market-to-book, ROA, property ratio, liquidity ratio, average sales growth, prior year abnormal returns, industry concentration, as well as

[19] Cameron and Trivedi (2010, page 190) describe this test as a regression-based "robustified Durbin-Wu-Hausman" test. We implement this in Stata and the Stata documentation for the ivregress/estat endogenous command refers to this as the "regression-based test."

[20] Given that 2SLS estimates are not as efficient as OLS estimates, being conservative in this step makes it marginally less likely that we will find significance in the takeover equation for some provisions. As reported in Table 4, we also utilize a second test for endogeneity to corroborate the initial Durbin-Wu-Hausman results.

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indicators for the firm's industry and year. The inclusion of control variables raises the prospect of "bad controls" that might themselves be affected by the antitakeover provisions (Angrist and Pischke 2009). Not including controls, however, risks omitted variable bias because other researchers have found these firm characteristics to be related to takeover likelihood.

[Insert Table 3]

The results in each row of Columns 4 and 7 are from 24 different 2SLS (or OLS) models in which each provision is included in the model with the above-mentioned firm, industry, and time controls but without the other 23 provisions. The results in Columns 5 and 8 each are from a single 2SLS (or OLS) regression that includes all 24 provisions simultaneously as separate indicator variables in addition to the firm, industry, and time controls. The results in Columns 6 and 9 are from the same 24 distinct models as in Columns 4 and 7 but with the addition of a control variable for the other 23 G-index provisions in each regression. This additional control variable is a count variable like the G-index but based only on the 23 other provisions not being focused on in each row.

The results from the Durbin-Wu-Hausman test for endogeneity are reported in Columns 1 and 2 in Table 3, and indicate the presence of endogeneity for nine provisions. The 2SLS results are highlighted in green for these nine provisions, and indicate that six of the nine provisions are negatively and significantly related to takeover likelihood in at least one of the three specifications that are summarized in Models 4, 5, and 6. These provisions are antigreenmail, classified board, director indemnification, directors' duties, fair price, and supermajority vote provisions. For three provisions – cashout laws, compensation plans, and pension parachutes – the Durbin-Wu-Hausman test indicates the presence of endogeneity, but the 2SLS results indicate that the provision is not significantly related to takeover likelihood. For two of these three provisions (compensation plans and pension parachutes), the F-statistic from the first stage also indicates a

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weak instrument problem. For provisions with such weak instruments (F-statistic substantially below 10), "nm" indicates that the 2SLS coefficient is not meaningful.[21,22]

The right branch of the decision tree in Figure 1 illustrates our decision process if the Durbin-Wu-Hausman test does not indicate the presence of endogeneity in the adoption of the specific provision. As highlighted in yellow in Table 3, there is at least some support for a negative relation between takeover likelihood and the use of six of the provisions (blank check preferred stock, director contracts, director liability, executive severance, and unequal voting), and a positive relation for three provisions (golden parachutes, poison pills, and written consent).

4.2. Corroborating tests based on LIML, RBP, and probit models

To probe the robustness of the preliminary inferences from Table 3, we consider results from LIML, RPB, and probit models, as summarized in Table 4. Like 2SLS, the LIML and RBP tests attempt to control for endogeneity. As noted by Stock and Yogo (2005) and Hayashi (2000, page 542), 2SLS and LIML estimators have the same asymptotic distributions but LIML estimators are more robust to both small sample bias and to weak instruments. It is for this reason that Angrist and Pischke (2009, pages 209-213) recommend using LIML models to corroborate inference from overidentified 2SLS models. In our applications, the LIML results do just that, as the LIML and 2SLS results are similar for all nine of the provisions for which the Durbin-Wu-Hausman test indicates endogeneity. The same six provisions that are negatively and significantly related to takeover likelihood in the 2SLS tests are also negatively related to

[21] Following Staiger and Stock (1997), we infer that the instrument is strong if the first-stage F-statistic is close to 10 or higher. Stock and Yogo (2005) test statistics were derived in a setting with homoskedastic errors. We follow Cameron and Trivedi (2010, page 199) and use F-statistics using heteroskedastic robust standard errors of roughly 10 or higher as rejecting the null of weak instruments. As described in Stock and Yogo (2005) the necessary F-statistic for strong instruments for LIML models can be smaller than 10. As discussed in Cameron and Trivedi (2010, pages 194-197) and Stock and Yogo (2005) the finite sample properties of 2SLS estimators are affected by weak instruments in ways that (1) can lead to large bias in the IV estimator, and (2) can affect the detection of significant results in unpredictable ways. For these reasons we report "nm" for the 2SLS results affected by weak instruments.

[22] As shown in Table 1, six provisions appear in fewer than 4% of firms by 2006 (cashout laws, charter amendment limitations, executive severance provisions, pension parachutes, silver parachutes, and unequal voting). The F-statistics on the first-stage regressions are less than 5 for five of these six provisions. Instruments based on provisions that appear infrequently at peer firms could be noisy because the use of even a single provision can have a large effect on the instrumented variable.

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takeover likelihood in the LIML tests. Obtaining the same inference, and even the same point estimates in most cases, using both LIML and 2SLS models implies that the results are unlikely to be driven by weak instruments or small sample bias because, as noted in Stock and Yogo (2005), the 2SLS and LIML estimators have "different properties when the instruments are weak" and are both consistent when the instruments are strong.

The RBP tests provide additional robustness information for these inferences both by estimating the marginal effects of the provisions using a different set of modeling assumptions as well as by providing an alternative test for whether specific provisions are endogenous. We begin by investigating whether the data support the use of a bivariate probit system of equations compared to a simple probit, by testing whether the correlation in omitted factors across the two bivariate probit equations is statistically different from zero.[23] This test reveals whether there is evidence of endogeneity after controlling for the other variables in the model.[24] The p-values for this test are reported in Column 4 in Table 4, and the null hypothesis is that the correlation is zero. If the null is not rejected, this implies that a single probit model is adequate and the single equation estimates would be more efficient than an RBP model. As with the previous Durbin-Wu-Hausman test, we again conservatively assign a cut-off p-value of 0.15 in this test to indicate the presence of endogeneity. If the null is rejected, we infer that the provision is endogenous with takeover likelihood and that we should rely on the RBP estimates (Columns 6–8) to account for the correlation in unobservables across equations in drawing inferences about the relation between provision use and takeover likelihood. For endogenous provisions, the pertinent RBP estimates are highlighted in green. If the RBP test does not indicate the presence of endogeneity, we rely on the single probit model (Columns 9–11, yellow highlighting) to estimate the relation between takeover likelihood and the provision.

[Insert Table 4]

[23] See Wooldridge (2010, section 15.7.3), Cameron and Trivedi (2010, page 531) and Greene (2003, pages 712-716).
[24] Examples of studies employing this test in the finance literature include Bates, Becher, Lemmon (2008), Ljungqvist, Marston, and Wilhelm (2009), Jiang, Li and Wang (2012), Cornelli, Kominek, and Ljungqvist (2013), Yermack (2014), Karpoff, Schonlau, and Wehrly (2017), and Papadimitri, Staikouras, Travlos, and Tsoumas (2019).

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The results from the bivariate probit test for endogeneity largely overlap with those from the Durbin-Wu-Hausman test. The p-value in the bivariate probit test for endogeneity for anti-greenmail provisions, for example, is 0.021, whereas in the Durbin-Wu-Hausman test it is 0.035. This result implies that we should rely heavily on the RBP model results (and not the probit results) in Table 4 for anti-greenmail provisions. The antigreenmail coefficients in the RBP tests (in Columns 6–8) are all negative and statistically significant. Together with the 2SLS and LIML test results, this is strong evidence that antigreenmail provisions are negatively related to takeover likelihood. We find similarly consistent results across 2SLS, LIML, and RBP tests for the other provisions for which endogeneity is indicated. The one notable exception is for classified boards, which we discuss further below.

For 14 of the 24 provisions, the bivariate probit test does not indicate that the provision is endogenous with takeover likelihood. For these provisions we emphasize the results, highlighted in yellow, from the single equation probit models (Columns 9–11). These results are largely consistent with the OLS results in Table 3. In particular, we find some evidence that the following provisions are negatively related to takeover likelihood: blank check preferred stock, director contracts, director liability, executive severance, and unequal voting. Two provisions – golden parachutes and restrictions on action by written consent – are positively related to takeover likelihood.

The Table 4 results do not support the preliminary inference for one provision, poison pills. The results in Table 3 suggest that poison pills are not endogenous and the OLS estimates suggest a positive relation between the presence of a clear-day poison pill and takeover likelihood. The results in Table 4, however, indicate that poison pills are endogenous with takeover likelihood, and that after controlling for endogeneity, clear-day pills are negatively but not significantly related to takeover likelihood. Given the weak instrument for poison pills, and the marginal p-value from the Durbin-Wu-Hausman test in Table 3 of 0.165, we infer the initial test for endogeneity lacked power to identify endogeneity. Together, the results from Tables 3 and 4 suggest that clear-day poison pills do not have a reliably statistically significant relation with takeover likelihood in our sample. Note that this conclusion does not suggest that poison pills are not effective takeover deterrents; rather, given that all firms effectively have shadow pills, our results suggest

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that the firms in our sample that have clear-day pills are not systematically more or less likely to be acquired after accounting for endogeneity. In the right branch of Figure 1, poison pills are the one provision for which the preliminary inference from Table 3 is not supported by the results in Table 4.

The results for classified boards also are somewhat mixed. The Durbin-Wu-Hausman test indicates that classified boards and takeover likelihood are endogenously determined, and the 2SLS and LIML results indicate that classified boards are negatively related to takeover likelihood in tests that control for endogeneity. However, the bivariate probit test does not reject the null that classified boards are exogenous, and the coefficients for classified boards in the RBP and probit tests, while negative, are not statistically significant. In subsequent tests, we treat classified boards as being negatively related to takeover likelihood, especially when we incorporate theoretical arguments below in Section 5, which indicate that classified boards are strong takeover deterrents. However, our main inferences – regarding how takeover deterrence reflects the influence of only a subset of the G-index provisions – are not affected by whether we include or exclude classified boards in the set of influential provisions.

5. The subset of antitakeover provisions that affect takeover likelihood

5.1. Summary of inferences from the empirical models

Columns 1 and 2 of Table 5 summarize the main inferences from Tables 3 and 4. Column 3 of Table 5 highlights the 13 provisions for which the preliminary and corroborating inferences are consistent. Eleven of these provisions are negatively related to takeover likelihood: anti-greenmail provisions, blank check preferred stock, classified boards, director contracts, director indemnification, limitations on director liability, directors' duties provisions, executive severance, fair price provisions, supermajority vote requirements, and unequal voting rights. Two provisions are positively related to takeover likelihood: golden parachutes and restrictions on shareholders' ability to act by written consent.

Our conclusion that 11 of the 24 G-index provisions are empirically related to takeover deterrence reflects some judgment. For example, we exclude two provisions with significant results in Table 4 (limitations on charter amendments and pension parachutes) because their Table 3 results are not

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statistically significant. We include blank check preferred stock even though its coefficients are statistically insignificant in some tests. The results for most of the provisions, however, are straightforward and do not require judgment calls. It also is important to emphasize that the main inferences we make in Sections 6 and 7 are not qualitatively affected if we categorize these marginal provisions differently.

[Insert Table 5]

5.2. Theoretical arguments about provisions' deterrence effects

We have conducted our analysis thus far without considering theoretical arguments about which provisions should matter most for a firm's takeover defense. This is because we explicitly seek an empirically based inductive approach to long-running debates over which antitakeover provisions affect takeover likelihoods, debates that rely heavily on theoretical or legal assertions. An important qualification of any theoretical argument about a provision is that the provision's specific legal mechanism conceivably matters less than the mere fact that the provision is deployed. For example, even provisions that appear to offer only weak takeover deterrence can signal managers' willingness to fight unsolicited takeover bids or otherwise maintain the firm as a distinct entity. Thus, theoretical arguments about whether, say, fair price provisions or supermajority vote requirements offer strong legal protections ignore the possibility that a provision can offer takeover defense because it indicates that the management team will fight to keep the firm independent. Such limitations of strictly theoretical assertions are one of the reasons we pursue an atheoretic approach throughout most of this paper.

Nonetheless, in this section we combine our empirical results with theoretical arguments in the corporate governance literature, and identify a total of eight provisions that have both some empirical support in our tests and some theoretical support in the literature. These eight provisions are summarized in Column 4 of Table 5.

We include classified boards in this group of eight provisions because many of our results show they are negatively related to takeover likelihood and many experts argue that classified boards have a strong deterrence effect. This is because, when combined with a shadow poison pill, a classified board can

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delay even a successful bidder's ability to gain voting control of the target firm's board (e.g., Coates, 2000; Klausner, 2013; Catan and Kahan, 2016; Catan, 2019; John et al., 2021). Similarly, our results regarding blank check preferred stock are consistent with theoretical arguments that it can be used to place newly-issued stock with friendly investors or to implement a quick adoption of a poison pill, thus increasing the firm's arsenal of defenses (e.g., Danielson and Karpoff, 1998; Gompers et al., 2003).

Additional theoretical arguments support our findings regarding supermajority vote requirements to approve a merger, unequal voting rights, fair price provisions, and directors' duties provisions, all of which work to raise a bidder's acquisition costs and lower the bidder's expected return from making a bid (e.g., see Stulz 1988). Supermajority vote requirements and unequal voting rights can allow a minority voting block to defeat a merger; fair price provisions constrain two-tier offers and assure that bidders pay relatively high prices for all the target shares acquired; and directors' duties provisions provide target firm managers latitude to reject takeover bids for a wide range of reasons and facilitate a "Just say no" takeover defense (Daines and Klausner 2001).

This group of eight provisions also includes anti-greenmail provisions, which can offer takeover protection by denying one potential source of gain for a bidder and decreasing the expected profitability of a bid (Eckbo 1990; Bhagat and Jefferis 1994). Finally, the positive effect of a golden parachute on takeover likelihood is consistent with arguments that it aligns target managers' and shareholders' incentives during a takeover bid.[25]

The theoretical arguments for some of these provisions are ambiguous. For example, court rulings in the 1980s validated defensive actions against bidders' attempts to receive greenmail payments, possibly decreasing the importance of anti-greenmail provisions. Consistent with this observation, most of the anti-greenmail provisions in our sample were deployed early in our sample period. One could argue that there is little theoretical support for including anti-greenmail provisions, even though they are negatively and significantly related to takeover likelihood even after the 1980s and in tests that control for firm age. We

[25] See Agrawal and Knoeber (1998), Machlin, Choe, and Miles (1993), Sokolyk (2011), Goktan and Kieschick (2012), Mansi, Wald, and Zhang (2016), Bebchuk, Cohen and Wang (2014), and Goktan, Kieschnick, and Moussawi (2018).

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include them here, and note that none of the results in the following sections are substantially affected if we exclude anti-greenmail provisions from the group of eight.

Another example of an ambiguous theoretical argument regards fair price provisions, which we find are negatively related to takeover likelihood. This finding is consistent with a finding by Cain et al. (2017) that coverage by a fair price law is negatively related to hostile acquisition likelihood. In contrast, coverage by a business combination law is not significantly related to takeover likelihood in our tests (although in Tables 3 and 4, all of the OLS, 2SLS, LIML, probit, and RBP coefficients for business combination laws are negative). These findings may seem anomalous, as business combination laws effectively impose fair price provisions after a forced delay of 2-5 years and thus appear to offer stronger takeover defense than fair price provisions by themselves. Again, the results in the following sections are not substantially affected if we exclude fair price provisions from the group of eight.

For five of the 13 provisions highlighted in Column 3 of Table 5, the theoretical support is particularly weak and we exclude them from the list of eight provisions with both theoretical and empirical support. For example, executive severance agreements compensate high-level executives upon removal. But, unlike golden parachutes, these agreements do not depend on a change in control, so the theoretical argument that they serve as takeover deterrents is weak. Similarly, Bebchuk et al. (2009) argue that director indemnification provisions, director contracts, and limits on director liability are related to issues of liability and indemnification in the event of shareholder lawsuits and are unrelated to corporate control transactions. Gompers et al. (2003) treat each of these as a restriction on shareholders' rights and a measure of the quality of firm governance rather than as a takeover protection per se (see also Core, 1997, 2000), implying that, theoretically, there is not a strong argument that they deter takeovers. Finally, we can find no theoretical arguments in the literature that are consistent with a positive marginal effect on takeover likelihood of restrictions on action by written consent, although we note that Sokolyk (2011) reports a similar result.

Column 4 of Table 5 identifies the eight provisions that remain after removing the five provisions with weak theoretical support. This subset includes seven provisions that our empirical results indicate are negatively related to takeover likelihood, plus golden parachutes, which our empirical results and some

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theoretical arguments indicate are positively related to takeover likelihood. In Sections 6 and 7 we estimate results using both the larger empirically-based group of 13 provisions and this smaller subgroup of eight provisions.

6. Indices and interactions of antitakeover provisions

In this section we use our results to examine whether takeover defense indices are related to takeover deterrence, and why. We also consider whether provisions interact as complements or substitutes, and whether they work primarily to deter takeover bids or facilitate active bid resistence.

6.1. How well do indices of antitakeover provisions work, and why?

Karpoff et al. (2017) show that, accounting for endogeneity, both the G-index and E-index capture some aspect of takeover deterrence. In this section we examine why these indices "work," and conclude that they work only to the extent they include some of the 11 provisions that are significantly and negatively related to takeover likelihood. We also show that the same is true of the O-index (the name Straska and Waller (2014) assign to the set of G-index provisions not included in the E-index) and the FK-index used by Field and Karpoff (2002), Chemmanur et al. (2011), and Johnson et al. (2015).

[Insert Table 6]

These results are reported in Panel A of Table 6. Column 1 reports on the relation between each set of provisions and takeover likelihood using a linear probability model without correcting for the endogeneity of the provisions.[26] Neither the G-index nor the E-index correlates with takeover likelihoods in tests that do not account for endogeneity, consistent with results in Core et al. (2006), Bates et al. (2008), Sokolyk (2011), Goktan and Kieschnick (2012), and Karpoff et al. (2017). The O-index and FK-index also

[26] Untabulated tests confirm that the qualitative results in Table 6 are generally not sensitive to whether or not we include a control variable in the model for the set of G-index provisions not included in each row's subset.

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are not significantly related to takeover likelihood, and the ATI is weakly related to an increase in takeover likelihood.[27]

Column 2 reports the marginal effect of each index as estimated from the takeover equation after accounting for endogeneity using a 2SLS approach. We calculate index-specific geography-based and IPO-year-based instruments for each set of provisions by summing the lagged provision-level instruments for the specific subset of provisions included in the index. In these tests, after accounting for endogeneity, the G-index, E-index, O-index, and FK-index all are negatively related to takeover likelihood. Columns 3 and 4 report results from the first-stage regressions indicating the presence of strong instruments for each group using the guidelines in the literature where instruments are considered strong if the F-statistic is above 10.

The results in Column 2 extend the results from Karpoff et al. (2017), and show that the G-index, E-index, O-index, and FK-index do capture some aspect of takeover deterrence, i.e., they "work." Our results also show, however, why these indices work. In each case, the index-level results are driven by the inclusion of one or more of the 11 provisions highlighted in Table 5 as being related to takeover deterrence. None of the subsets of provisions that are included in an established index, but that fall outside of these 11 deterrence provisions, are statistically related to takeover likelihoods, whether or not we account for endogeneity and whether or not we examine their relationship with takeover likelihood individually or as a group.

These results are illustrated in Figure 2. Figure 2 presents a Venn diagram that displays the provisions that appear in the various indices, and how each index overlaps with the 11 key provisions that we identify as being significantly related to takeover deterrence. The ability of any one index to measure takeover likelihood depends on the extent to which it includes one or more of these 11 provisions.

[Insert Figure 2]

[27] In the original ATI some of the provisions were considered jointly. For example, a value of 1 was added to the index if the firm had either limitations on calling special meetings or limitations on acting by written consent. Similarly a value of 1 was added to the index if the firm had either a blank check provision or a poison pill. Because some indices treat the provisions jointly whereas others treat them separately, and to ensure we can compare the various indices and subsets of provisions in Table 6, we code the indices in Table 6 Panel A as though each provision is considered separately. Hence a value of 2 (rather than 1) is added to the ATI index in Table 6 if the firm in question has both a blank check and a poison pill.

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6.2. Interaction effects among different provisions

Our analysis implies that 11 of the 24 G-index provisions are significantly related to takeover deterrence, and two are positively related to takeover likelihood. It is possible, however, that provisions work as complements such that the marginal impact of any one provision depends on whether the firm also deploys another provision. For example, Bebchuk et al. (2002) argue that classified boards are particularly impactful for firms that also have poison pills.

To examine possible interaction effects, we examine four specific combinations of provisions that previous findings suggest might work in concert to provide strong takeover protection. These include: (1) classified boards combined with poison pills (Coates 2000; Daines and Klausner 2001; Bebchuk et al. 2002); (2) classified boards combined with supermajority vote requirements (for either mergers or to amend the charter), shareholder meeting requirements, and fair price provisions (Danielson and Karpoff, 1998); (3) poison pills combined with limits on shareholder meetings or on the ability of shareholders to act by written consent (Bebchuk and Hamdani, 2002; Daines and Klausner, 2001); and (4) classified boards combined with non-cumulative voting rules (Coates 2001; Bebchuk et al., 2002; Daines and Klausner, 2001). Finally, we examine the joint influence of the eight provisions highlighted in Column 4 of Table 5 that are supported by both theoretical arguments and our empirical results. This group consists of seven provisions that negatively relate to takeover likelihood, plus golden parachutes, which are positively related to takeover likelihood. In computing an index based on these eight provisions, we add one if the firm does not have a golden parachute.

Panel B of Table 6 reports the results. After accounting for endogeneity, the point estimates in Column 2 for all five of these groups of provisions are negative. The first two groupings in Rows 16 and 17 – suggested by previous work – are statistically significant. These results support arguments that classified boards are strong takeover deterrents, combined with either poison pills or a combination of supermajority vote and shareholder meeting requirements. These results likely understate the marginal

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effects of these two groups given that we are only able to capture the effects of clear-day poison pills relative to shadow pills for the reasons discussed earlier.

The results in Rows 18 and 19 show that classified boards are not significantly related to takeover deterrence when considered in combination with non-cumulative voting rules, and clear-day poison pills are not incrementally related to deterrence when considered in combination with limits to meetings or written consent. We infer that there is no evidence in tests of established ad hoc subsets of provisions that implies strong interaction effects among antitakeover provisions beyond our main finding that 11 of the G-index provisions are associated with takeover deterrence. The results in Row 20, however, indicate that an index based on the eight provisions that are backed by both theory and our empirical results is also significantly related to takeover deterrence.

The Internet Appendix Table B.5 reports results for additional groupings of provisions. For example, we examine the marginal effect on takeover likelihood of the group of six negatively related provisions that are identified via the left branch of the decision tree in Figure 1, an index of 13 provisions in which one is added when firms do not have a golden parachute or restrictions on written consent, and groups of provisions that omit blank check preferred stock, for which the evidence of takeover deterrence is marginal. The results all support our main finding in this section, which is that any index of provisions helps to explain a firm's takeover likelihood only to the extent that it includes one or more of the 11 negatively related provisions.

6.3. Do antitakeover provisions deter or defeat takeover bids?

Antitakeover provisions can work by helping incumbent managers defeat unsolicited bids that have already occurred, or by deterring takeover bids from being made. To distinguish between these possible channels, we examine the provisions used by firms that become takeover targets but that have their takeover bids withdrawn. Bids can be withdrawn for many reasons, but the presence of an antitakeover provision suggests that the provision helped to fend off the bid.

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Using data on firms in our sample that received takeover bids, we estimate probit regressions in which the dependent variable equals one if the bid subsequently was withdrawn. The results are tabulated in the Internet Appendix Table B.6, and show that five provisions are positively and significantly related to the likelihood of a withdrawn bid: director contracts, director indemnification provisions, director liability provisions, fair price provisions, and unequal voting rights. These provisions also are among our group of 11 provisions that, empirically, appear to offer takeover protection. The finding that these five provisions are associated with withdrawn bids suggests that they facilitate bid resistance by the target firms' managers. The other six provisions that are negatively related to takeover likelihood – anti-greenmail provisions, blank check preferred stock, classified boards, directors' duties provisions, executive severance contracts, and supermajority vote requirements for mergers – appear to work primarily by deterring takeover bids in the first place.

7. Changes in provisions' uses and deterrence effects over time

Our inferences are based on the assumption that the marginal deterrence of an antitakeover provision, conditional on observable firm characteristics, is time invariant and does not depend on the provision's frequency in the population of firms. This assumption could be violated, however. For example, Field and Lowry (2021) report that the percentage of firms with classified boards has declined over time. Such a change could reflect a change in managers' perceptions of the net benefits of classified boards, including a change in classified boards' ability to defend against takeover bids.

We conduct several tests to examine whether the provisions' deterrence effects are stable over time. First, we examine whether each provision's association with takeover likelihood changes within our sample period by re-estimating our tests for the first half of our sample (1995-2002) and then again for the second half (2003-2010). Table 7 reports a summary, while the full results are reported in Table B.7 in the Internet Appendix. For most provisions, the deterrence effects are similar in the first and second halves of the sample period. For a few provisions, however, the deterrence effects differ. Most notably, antigreenmail, director liability, and director indemnification provisions are more strongly associated with takeover deterrence in

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the second half of the sample period than in the first half, whereas director contract provisions are more strongly associated with takeover deterrence in the first half of the sample.

[Insert Table 7]

Next, we use 2007-2020 data from Institutional Shareholders Services, Inc. (ISS) to examine whether there are significant changes in firms' uses of the provisions after our sample period. These data are available via the ISS-Governance library accessed via WRDS. The comparisons beyond 2006 are limited because the later ISS data report directly on only 13 of the original 24 provisions that are in the IRRC data and that constitute the G-index. Internet Appendix Figure B.2 reports the proportion of firms with each provision when we splice the two databases together. The counts of several provisions show discrete jumps, including fair price provisions, restrictions on bylaw amendments, and restrictions on charter amendments. The discrete jumps appear to reflect differences in how these provisions are counted rather than large changes in the numbers of firms using them.

Nonetheless, a comparison of the two databases provides some information about changes in firms' uses of provisions after 2006. Consistent with Field and Lowry (2021), we find that the percentage of firms with classified boards decreases, from 60% in 2004 to 30% in 2020. The use of poison pills also declines over time, while the use of supermajority vote provisions, restrictions on special meetings, and restrictions on the use of written consent all increase.

To examine whether the relation between takeover likelihood and the specific antitakeover provisions has changed over time, we replicated our tests for takeover likelihood for the 13 provisions that are covered by both the 1990-2006 IRRC data and the newer 2007-2020 ISS data. Summary results are tabulated in Table 8, with more detail in Internet Appendix Table B.8. For most provisions, the results are similar to the results in Table 3 of the paper. Provisions that are consistently and robustly negatively related to takeover likelihood in both our main sample period and in the later time period include supermajority vote requirements for mergers, unequal voting rules, and fair price provisions.

[Insert Table 8]

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The results for a few provisions, however, are different. Blank check preferred stock, for example, is not significantly related to takeover likelihood in the 2007-2020 data. Blank check preferred stock can be used to implement a poison pill, so this change could reflect firms' decreased use of poison pills and the increased use of different securities to have the option to implement a pill. Another notable change is that restrictions on charter amendments are negatively related to takeover likelihood in the 2007-2020 data. This could reflect an increase in the use of such restrictions to deter hedge fund activists from exercising influence via charter amendments.

The most important change over time regards classified boards. Classified boards are negatively related to takeover likelihood in some specifications using the 1995-2006 data, and also using the 2007-2020 data in tests that do not control for endogeneity. In tests that control for endogeneity, however, classified boards are positively related to takeover likelihood in the 2007-2020 tests. We believe the most likely explanation for this change is that, during the 2007-2020 period, the instrumented variables for classified boards reflect selection effects and fail to meet the exclusion requirement. Guernsey, Feng, Liu, and Serfling (2022) report that large firms tended to drop classified boards while smaller firms tended to adopt them during this period. Cremers, Litov, and Sepe (2017) and Field and Lowry (2021) argue that classified boards convey various costs and benefits – including takeover deterrence and board continuity – that have changed over time, and Johnson et al. (2021) find that the entrenchment costs of classified boards tend to be large for seasoned firms and small for younger and smaller firms. Together, these results suggest that firms declassifying boards during the 2007-2020 period were more likely to use classified boards to deter takeovers, while firms adopting classified boards were more likely to use them for other purposes.[28]

If we incorporate the results in Tables 7 and 8, even fewer than the 13 (or 8) provisions summarized in Table 5 are consistently related to takeover likelihood over time. Three provisions – supermajority vote requirements for mergers, fair price provisions, unequal voting rights – are negatively related to takeover

[28] Relatedly, many larger firms declassified their boards in response to pressure from the Harvard Shareholder Rights Project (see Cremers and Sepe, 2017; Bebchuk and Cohen 2017). Such selection effects by themselves could violate the exclusion restriction in our instrumental variable tests during the 2007-2020 period.

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likelihood in both periods, 1995-2006 and 2007-2020. In addition, directors' duties provisions are consistently and negatively related to takeover likelihood during the only period for which we have data (1995-2006), and one provision – golden parachutes – is positively related to takeover likelihood in both the 1995-2006 and 2007-2020 periods. So, whereas our main tests provide some evidence that 11 provisions are negatively related to takeover likelihood and two provisions are positively related, a more strict screen that incorporates the 2007-2020 data indicates that only four provisions are consistently and negatively related to takeover likelihood while one provision (golden parachutes) is consistently and positively related to takeover likelihood.

8. An illustrative test of unconditional takeover premiums

Our finding that the G-index and E-index contain provisions that are not empirically related to takeover deterrence indicates that these indices are noisy measures of a firm's takeover defenses. In this section, we show that this measurement noise can be large enough to affect empirical inferences by examining whether and how a firm's takeover defenses affect its unconditional takeover premium. Prior evidence on this question is mixed.[29] We also find mixed results, but show that the results depend on whether we use the G-index or E-index to measure takeover deterrence, or indices based on the subset of provisions that are empirically related to takeover likelihoods. In these tests, we use indices based on the 11 provisions that are empirically related to takeover deterrence in our main tests and as summarized in Table 5, plus golden parachutes, which are positively related to takeover likelihood.

Following Comment and Schwert (1995), we set the unconditional takeover premium equal to zero in nontakeover firm-years, and equal to the takeover premium in any year in which the firm is acquired. The premium is measured using SDC's ratio of the offer price to the target firm's stock price four weeks

[29] Comment and Schwert (1995), Heron and Lie (2006), and Becher, Bates, and Lemmon (2008) conclude that unconditional takeover premiums are positively related to a firm's use of antitakeover provisions, and Guernsey, Sepe, and Serfling (2022) find that during market shocks, firms covered by state antitakeover laws realize higher premiums without a reduction in takeover likelihood. Kadyrzhanova and Rhodes-Kropf (2011) conclude that some provisions increase unconditional takeover premiums while others work to decrease the premium. Sokolyk (2011) concludes that some defenses are related to unconditional takeover premiums (most often negatively), although the G-index is not. Bebchuk, Coates, and Subramanian (2002) and Cuñat et al. (2020) conclude that the relation is negative.

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before the takeover is announced. When the SDC's four-week takeover premium is not reported, we use the ratio of the target firm's closing stock price the day after the announced deal to the stock price one month before. In addition to an index based on the firm's antitakeover provisions, we include the control variables described in Section 3.1.

Table 9 reports the results for the period from 1995-2010. In OLS regressions that do not control for endogeneity, neither the G-index nor the E-index is significantly related to the unconditional takeover premium (see Models 1 and 2). In contrast, an index based on the 11 deterrence provisions is negatively and significantly related to the unconditional takeover premium, as the coefficient in Model 3 is -0.001 and is significant at the 1% level. Model 4 reports results based on the subset of eight provisions that have both empirical and theoretical support, as summarized in Table 5. In calculating this index, we add one for firms that do not have golden parachutes. The coefficient for this eight-provision index is -0.001 and is significant at the 5% level. These coefficients reflect both selection and treatment effects, but they are consistent with the joint hypothesis that the relation is negative and that focusing on these subsets of provisions yields better-specified tests of the relation between the unconditional takeover premium and takeover defenses than the G-index and E-index do.

[Insert Table 9]

Models 5-8 report on tests that control for endogeneity using the same approach for constructing instrumental variables at the index level as described in Section 6. Here, the coefficient on the instrumented G-index is negative and statistically significant, although the coefficient on the instrumented E-index is not significant. Both the instrumented indices based on 11 and 8 provisions are strongly and negatively related to the unconditional takeover premium (at the 1% significance level) after accounting for endogeneity.

Together, these results indicate that a firm's unconditional takeover premium is negatively related to its takeover defenses, implying that the deterrence effect on takeover likelihood more than offsets any positive effect that the defenses might have on the conditional takeover premium, on average. This relation is apparent using indices based on (11 or 8) provisions that actually are related to takeover likelihoods, both in simple OLS tests and in tests that control for endogeneity. Using the G-index or E-index, however, this

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relation is not apparent in tests that do not control for endogeneity. Even in tests that control for endogeneity, the relation between the E-index and unconditional takeover premium is not statistically significant – possibly due, in part, to the inclusion of golden parachutes in the E-index in a way that presumes a negative, rather than a positive, relation with takeover likelihood.

9. Conclusions

Among the many provisions researchers use to measure takeover deterrence, which ones actually deter takeovers? We conjecture that there is little prior research on this basic yet fundamental question because of concerns about noisy data, endogeneity, and omitted variables. Our tests are not immune to these concerns. Nonetheless, we use multiple instruments, empirical methods, and model specifications to gain insight about the subset of G-index provisions that plausibly provide takeover deterrence. This explicitly inductive approach provides evidence for a negative relation with takeover likelihood for, at most, 11 of the 24 provisions in the G-index: anti-greenmail, blank check, classified boards, director contracts, director indemnification, limitations on director liability, directors' duties provisions, executive severance, fair price provisions, supermajority vote requirements for mergers, and unequal voting rights. Golden parachutes and limitations to act by written consent, which are included in some indices that are used to measure takeover deterrence, are positively related to takeover likelihood.

In addition to this group of 13 provisions that are related to takeover likelihood (11 negatively, two positively), we identify a subset of eight provisions for which there also are theoretical arguments that support an effect on takeover likelihood. And in our most stringent robustness tests, only five of these provisions are consistently related to takeover deterrence: directors' duties provisions, fair price provisions, supermajority vote requirements for mergers, unequal voting rights, and (with a negative sign indicating that they are positively related to takeover likelihood) golden parachutes. We propose that future research seeking to measure a firm's takeover defenses should focus on the antitakeover provisions that, empirically, are associated with takeover deterrence. Researchers seeking measures of takeover deterrence can use our broad group of 13 provisions, the group of 11 provisions that are negatively related to takeover likelihood,

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the subgroup of eight provisions that also have some theoretical support, or the smaller subgroup of five provisions that are most robustly and consistently related to takeover likelihood. Our tests show that these alternatives measure takeover deterrence with less bias and noise than the G-index or E-index.

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Table 1: Firms' uses of antitakeover provisions

This table reports the percentage of firms in the IRRC data using each of the 24 G-index provisions from 1990 through 2006. The years in the table correspond to the years in which IRRC provides data. The percentages are based on the entire IRRC data set without imposing the data requirements used in subsequent tables. Section 3 describes the IRRC sample and Internet Appendix A describes each provision.

	1990	1993	1995	1998	2000	2002	2004	2006
Anti-greenmail	18.8	19.6	19.0	16.4	15.4	14.5	14.2	14.6
Blank check preferred stock	75.8	79.4	85.0	87.6	89.3	90.5	90.8	91.3
Business combination law	84.0	87.2	87.5	88.8	89.1	88.3	86.4	86.0
Bylaw amendment limitations	14.0	15.7	15.7	17.7	19.6	22.3	22.7	22.4
Cashout law	4.1	3.7	3.5	2.9	2.5	2.4	2.6	2.9
Charter amendment limitations	3.1	3.2	3.1	2.9	3.2	2.4	2.6	3.0
Classified board	57.2	58.4	59.7	57.8	57.9	59.4	59.7	56.6
Compensation plan	43.8	64.0	71.3	61.2	71.0	72.9	73.9	72.7
Director contract	16.3	15.1	12.6	10.7	8.8	7.8	6.8	6.5
Director indemnification	40.9	39.4	38.4	24.3	22.9	18.4	16.4	16.6
Director liability	72.4	69.1	65.6	46.3	41.7	32.6	28.8	27.7
Directors' duties	10.4	11.0	10.9	9.9	9.9	10.5	11.0	11.6
Executive severance	14.2	5.8	10.7	11.6	9.9	6.8	6.1	3.1
Fair price	56.6	57.6	56.1	47.9	47.3	42.6	43.0	43.8
Golden parachute	50.4	53.3	53.6	55.4	64.9	67.7	73.4	77.6
Not cumulative voting	82.2	84.1	85.6	88.4	89.5	90.7	91.4	91.9
Not secret ballot	97.1	90.6	88.3	90.7	89.9	89.6	89.0	88.0
Pension parachutes	3.8	5.0	3.8	2.1	1.5	1.0	0.8	0.7
Poison pill	50.9	53.7	53.2	51.6	55.8	55.1	55.1	51.2
Silver parachutes	3.9	4.5	3.3	2.2	1.8	1.6	1.1	1.4
Special meeting limitations	23.9	28.8	31.0	33.5	36.4	47.9	49.5	49.8
Supermajority vote requirement	38.2	39.0	38.0	33.6	33.7	32.0	32.0	32.5
Unequal voting	2.3	2.1	2.0	1.8	1.5	1.5	1.6	1.4
Written consent limitations	23.9	28.4	31.2	32.0	34.4	43.9	46.8	48.3

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Table 2: Summary information about the sample

Panel A reports the number of firms in the sample, the number of those firms acquired each year, and the mean, median, minimum, maximum, and standard deviation of the number of provisions at those firms each year. The sample consists of the intersection of CRSP, Compustat, and IRRC firms as described in Section 3. Panel B provides information about the mean and median values of control variables used in subsequent tests.

Panel A:

Year	Number of Firms	Number of Takeovers	Mean number of provisions	Median number of provisions	Minimum number of provisions	Maximum number of provisions	Standard deviation
1995	1,056	23	9.37	9.00	2.00	17.00	2.80
1996	1,046	49	9.34	9.00	2.00	17.00	2.81
1997	998	48	9.33	9.00	2.00	17.00	2.80
1998	1,267	97	8.92	9.00	2.00	18.00	2.83
1999	1,199	89	8.88	9.00	2.00	18.00	2.81
2000	1,191	59	9.05	9.00	3.00	19.00	2.68
2001	1,157	28	9.04	9.00	3.00	19.00	2.69
2002	1,353	33	9.17	9.00	2.00	18.00	2.61
2003	1,344	41	9.15	9.00	2.00	18.00	2.60
2004	1,408	62	9.15	9.00	2.00	18.00	2.51
2005	1,379	71	9.17	9.00	2.00	18.00	2.50
2006	1,296	82	9.13	9.00	2.00	18.00	2.47
2007	1,260	58	9.11	9.00	2.00	18.00	2.47
2008	1,182	27	9.12	9.00	2.00	18.00	2.46
2009	1,148	36	9.11	9.00	2.00	18.00	2.46
2010	1,101	64	9.15	9.00	2.00	18.00	2.45

Panel B:	Mean	Median	Observations
Number of provisions (G-index)	9.13	9.00	19,385
Firm size ($ millions)	5,593.95	1,438.15	19,385
Market value of equity ($ millions)	6,742.85	1,437.55	19,385
Leverage	0.21	0.19	19,385
Market to book	1.57	1.18	19,385
ROA	0.09	0.09	19,385
Property ratio	0.58	0.51	19,385
Liquidity ratio	0.20	0.18	19,385
Sales growth	0.09	0.07	19,385
3-year sales growth	0.03	0.02	19,385
Market-adjusted return	0.03	-0.03	19,385
Industry concentration	6.27	5.03	19,385

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Table 3: Individual provisions' impact on takeover likelihood estimated using OLS and after correcting for endogeneity using 2SLS models

This table presents estimates from 2SLS and OLS models that employ the following equations:

$$y_1 = \propto_0 + p_i\partial_i + (\sum_{q=1}^{23} p_q\varphi_j \quad or\ Index_{23}\vartheta_{23}) + \sum_{j=1}^{74} x_j\beta_j + e \quad \text{(takeover equation)}$$

$$p_i = \alpha_0 + z_p_{geo,i}\pi_i + z_p_{ipo,i}\theta_i + (\sum_{q=1}^{23} p_q\omega_j \quad or\ Index_{23}\alpha_{23}) + \sum_{j=1}^{74} x_j\gamma_j + u \quad \text{(first stage equation 2SLS)}$$

In all specifications the dependent variable (y_1) in the takeover equation is set to 1 in year t if the firm in question is acquired within the next year. p_i is an indicator variable for each of the 24 takeover provisions considered in the paper. In all specifications the same 74 firm, industry, and year control variables described in Section 3.1 are included, but not tabulated. These control variables include firm size, leverage, market-to-book, ROA, property ratio, liquidity ratio, average sales growth, prior year abnormal returns, and industry concentration as well as Fama-French 49 industry and year effects. The column headings indicate whether only the takeover equation (OLS specifications) or both equations (2SLS specifications) were used in calculating the marginal effects.

Columns 1-3 provide diagnostic information generated by 2SLS specifications for individual provisions while controlling for the presence of the 23 other provisions. Column 1 presents the p-value from a test for whether the provision variable is endogenous. This test is discussed as the regression-based "robustified Durbin-Wu-Hausman" test in Cameron and Trivedi (2010, page 187-188). Column 2 indicates whether the p-value from this test is less than 15%. For this test the null hypothesis is that the provision is exogenous, so a p-value < 15% provides evidence against exogeneity. We use a 15% cutoff to be conservative in the sense that we identify the presence of endogeneity even if it is weakly indicated. Column 3 reports the F-statistic from the first-stage equations and provides a sense for the strength of the instruments. (The first-stage results are presented in Internet Appendix Table B.3.)

Columns 4-9 report each provision's marginal effect (∂_i) from the takeover equation. Parentheses are included in the takeover and first stage equations above to highlight differences between specifications. In Columns 4 and 7 the parenthetical terms are not included in the specification(s); the 24 marginal effects in each of these two columns are estimated using 24 separate regressions where only that row's provision is included in each regression (without the other 23 provisions) along with the control variables described above. In Columns 5 and 8, all 24 provisions are simultaneously included as separate indicators. In Column 5 each provision is instrumented in turn (one at a time) while simultaneously including the other 23 provisions as indicator variables. In Columns 6 and 9 each provision is included (and for Column 6, instrumented) in turn while simultaneously controlling collectively for the other 23 provisions using an index ($Index_{23}$). The results in Columns 4-6 are from overidentified 2SLS models that include both geography- and IPO-year-based instruments (z_p_{geo} and z_p_{ipo}) in the first stage. The instruments are described in Section 3.3. If the instruments are weak then "nm" appears in Columns 4-6 indicating that the 2SLS coefficient is not meaningful in those cases. In determining which instruments are weak, we loosely follow Staiger and Stock's (1997) suggestion that the F-statistic should be around 10 or larger.

Column 10 provides a summary of our preliminary inferences from the results. As indicated by green highlighting, the inference is based on the 2SLS results if there is evidence of endogeneity in the use of the provision. Otherwise, as indicated by yellow highlighting, the inference is based on the OLS results. The data consist of 19,385 firm-year observations. Standard errors are corrected for heteroskedasticity and clustered by firm. Significance at the 10%, 5% and 1% levels are shown using *, **, and ***, respectively.

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Table 3, continued

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Tests for endogeneity			2SLS models			OLS models			
	Endogeneity test p-value (H_0: exogenous)	Evidence of endogeneity	1st-stage F-statistic	Single indicator for provision of interest	Single equation with 24 indicators	Single indicator with index of other 23 provisions	Single indicator for provision of interest	Single equation with 24 indicators	Single indicator with index of other 23 provisions	Preliminary inference
Anti-greenmail	0.035	Yes	148.2	-0.025***	-0.025**	-0.025***	-0.009**	-0.008*	-0.009**	–
Blank check pref. stock	0.908	No	24.8	-0.011	-0.068	-0.011	-0.007	-0.011**	-0.007	–
Business combination	0.502	No	60.3	-0.012	-0.009	-0.012	-0.001	-0.002	-0.001	
Bylaw amendments	0.756	No	21.3	-0.006	-0.072	-0.007	0.005	0.002	0.006	
Cashout laws	0.111	Yes	26.8	-0.027	-0.027	-0.027	-0.004	0.003	-0.004	
Charter amendments	0.219	No	2.7	nm	nm	nm	0.015	0.011	0.015	
Classified board	0.123	Yes	9.0	-0.080*	-0.153*	-0.066	-0.002	-0.005	-0.002	–
Compensation plans	0.001	Yes	3.7	nm	nm	nm	0.007*	0.006	0.007**	
Director contracts	0.755	No	14.6	-0.023	-0.049	-0.027	-0.012***	-0.012**	-0.013***	–
Director indemnification	0.100	Yes	117.3	-0.023**	-0.037	-0.027**	-0.008**	-0.004	-0.009***	–
Director liability	0.341	No	375.4	-0.012*	-0.008	-0.013*	-0.007**	-0.005	-0.007**	–
Directors' duties	0.006	Yes	39.1	-0.045***	-0.078***	-0.046***	-0.003	0.000	-0.003	–
Executive severance	0.945	No	1.6	nm	nm	nm	-0.011**	-0.002	-0.011**	–
Fair price	0.000	Yes	112.9	-0.037***	-0.042**	-0.043***	-0.004	0.001	-0.004	–
Golden parachutes	0.438	No	1.1	nm	nm	nm	0.017***	0.016***	0.018***	+
Not cumulative voting	0.371	No	35.0	0.021	0.018	0.020	0.002	-0.002	0.002	
Not secret ballot	0.582	No	19.6	0.006	-0.010	0.006	-0.007	-0.008	-0.007	
Pension parachutes	0.130	Yes	4.5	nm	nm	nm	0.005	-0.001	0.005	
Poison pill	0.165	No	1.2	nm	nm	nm	0.005*	0.003	0.006**	+
Silver parachutes	0.422	No	0.8	nm	nm	nm	0.010	0.011	0.010	
Special meeting	0.896	No	44.5	0.001	-0.025	0.001	0.003	-0.003	0.004	
Supermajority vote	0.009	Yes	132.9	-0.031***	-0.031**	-0.033***	-0.010***	-0.008**	-0.010***	–
Unequal voting	0.388	No	1.4	nm	nm	nm	-0.035***	-0.030***	-0.035***	–
Written consent	0.687	No	73.9	0.006	-0.024	0.005	0.011***	0.012***	0.013***	+

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Table 4: Individual provisions' estimated impact on takeover likelihood using limited information maximum likelihood, recursive bivariate probit models, and probit models

This table reports marginal effects for provisions on takeover likelihood using limited information maximum likelihood (LIML, Columns 1-3), recursive bivariate probit (RBP, Columns 6-8), and probit (Columns 9-11) models. The underlying equations (shown in the Table 3 heading) are similar for the 2SLS, LIML, and RBP models but different underlying key assumptions are made in each approach. In all specifications the same 74 firm, industry, and time control variables described in Section 3.1 are included but not tabulated. These control variables include firm size, leverage, market-to-book, ROA, property ratio, liquidity ratio, average sales growth, prior year abnormal returns, and industry concentration as well as Fama-French 49 industry and year effects. For the RBP model, the two left-hand side variables in the equations are considered latent variables (y_1^*, p_i^*). By assumption, y_1 and p_i are observed to equal 1 when their underlying respective latent variables are above a certain threshold. In the RBP model the errors are assumed to have a bivariate normal distribution with a modeled correlation of ρ: $\binom{e}{u} \sim N\left\{\binom{0}{0}, \binom{1\ \rho}{\rho\ 1}\right\}$. The RBP marginal effects are estimated as the difference in the predicted probability of observing a takeover conditional on having or not having the provision while holding all other characteristics at the firm constant following Greene (5 ed., page 716). For both the RBP and LIML approaches, maximum likelihood estimation techniques are used.

Column 4 reports the p-values from tests of whether the $\rho's$ are equal to zero. Following Wooldridge (2010, section 15.7.3) this type of test is a test of whether endogeneity, as captured by the correlation of omitted factors in both equations, is present; if it is not present, then this suggests a single equation probit model can be used. To be conservative we use a 15% cutoff in Column 5 when interpreting the p-value from Column 4.

As noted in the column headers, Columns 1, 6, and 9 report the marginal effects from 24 separate regressions (24 in each column) estimated using regressions where only that row's provision is included in the regression (without the other 23 provisions) along with the control variables described above. In Columns 2, 7, and 10, all 24 provisions are simultaneously included as separate indicators in the same regression. In Columns 2 and 7 each provision is instrumented in turn (one at a time) while simultaneously including the other 23 provisions as indicator variables. In Columns 3 and 8 each row's provision is instrumented in turn while simultaneously controlling collectively for the other 23 provisions using an index (Index_{23}). The results in Columns 1-3 (6-8) are from LIML (RBP) models that include both geography- and IPO-year-based instruments (z_p_{geo} and z_p_{ipo}). The instruments are described in Section 3.3. If the instruments are weak then "nm" appears in Columns 1-3 indicating that the LIML coefficient is not meaningful in those cases. In determining which instruments are weak, we loosely follow Staiger and Stock's (1997) suggestion that the F-statistic should be around 10 or larger but allow for slightly smaller F-statistics for LIML models consistent with Stock and Yogo (2005).

Column 12 summarizes the inferences from the results. If the results are clear and the inference strong the summary in Column 12 appears without parentheses. If the results are mixed or weak the inference appears within parentheses. The summary is based on a combination of results. Inference is based on the LIML results if the corresponding F-statistics (Table 3, Column 3) are above ~10. Inference is based on the RBP results (green highlighting) if there is evidence of endogeneity (Columns 4 and 5) or on the probit results (yellow highlighting) if there is not evidence of endogeneity. As in previous tests, the data consist of 19,385 firm-year observations. Standard errors are corrected for heteroskedasticity and clustered by firm. Significance at the 10%, 5% and 1% levels are shown using *, **, and ***, respectively.

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Table 4, continued

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	LIML models			Tests for endogeneity		RBP models			Probit models			
	Single indicator for provision of interest	Single equation with 24 indicators	Single indicator with index of other 23 provisions	Bivariate test p-value	Evidence of endogeneity	Single indicator for provision of interest	Single equation with 24 indicators	Single indicator with index of other 23 provisions	Single indicator for provision of interest	Single equation with 24 indicators	Single indicator with index of other 23 provisions	Inference
Anti-greenmail	-0.025***	-0.025**	-0.025***	0.021	Yes	-0.023***	-0.020***	-0.023***	-0.009**	-0.007*	-0.009**	–
Blank check preferred	-0.011	-0.068	-0.011	0.628	No	0.008	-0.013	0.008	-0.006	-0.010**	-0.007	–
Business combination	-0.012	-0.009	-0.012	0.985	No	-0.001	-0.002	-0.001	-0.001	-0.002	-0.001	
Bylaw amendments	-0.006	-0.072	-0.007	0.500	No	-0.010	-0.028	-0.011	0.005	0.002	0.005	
Cashout laws	-0.027	-0.027	-0.027	0.142	Yes	-0.018	-0.013	-0.018	-0.004	0.003	-0.004	
Charter amendments	nm	nm	nm	0.035	Yes	-0.052***	-0.051***	-0.052***	0.011	0.008	0.011	–
Classified board	-0.080*	-0.153*	-0.068	0.300	No	-0.021	-0.031	-0.021	-0.001	-0.004	-0.002	(–)
Compensation plans	nm	nm	nm	0.088	Yes	-0.036	-0.040	-0.035	0.006**	0.005*	0.007**	
Director contracts	-0.023	-0.050	-0.027	0.397	No	-0.030*	-0.025	-0.031*	-0.013***	-0.013**	-0.014***	–
Director indemnification	-0.023**	-0.037	-0.027**	0.041	Yes	-0.023***	-0.019*	-0.025***	-0.008**	-0.005	-0.009***	–
Director liability	-0.012*	-0.008	-0.013*	0.237	No	-0.012**	-0.007	-0.013**	-0.006**	-0.003	-0.006**	–
Directors' duties	-0.045***	-0.078***	-0.046***	0.010	Yes	-0.031***	-0.032***	-0.031***	-0.002	0.000	-0.003	–
Executive severance	nm	nm	nm	0.639	No	-0.027	-0.036	-0.027	-0.010*	-0.001	-0.010*	–
Fair price	-0.037***	-0.042**	-0.043***	0.002	Yes	-0.028***	-0.014	-0.029***	-0.004	0.001	-0.004	–
Golden parachutes	nm	nm	nm	0.884	No	0.019	0.025	0.025	0.017***	0.016***	0.018***	+
No cumulative voting	0.021	0.018	0.020	0.524	No	0.013	0.005	0.013	0.003	-0.002	0.003	
Not secret ballot	0.006	-0.010	0.006	0.289	No	0.011	0.003	0.011	-0.006	-0.007	-0.006	
Pension parachutes	nm	nm	nm	0.033	Yes	-0.043***	-0.045***	-0.044***	0.005	0.001	0.005	–
Poison pill	nm	nm	nm	0.055	Yes	-0.048	-0.048	-0.045	0.006**	0.003	0.006**	
Silver parachutes	nm	nm	nm	0.348	No	-0.021	-0.020	-0.021	0.009	0.010	0.009	
Special meeting	0.001	-0.025	0.001	0.855	No	0.000	-0.017	0.000	0.003	-0.003	0.003	
Supermajority vote	-0.031***	-0.031**	-0.033***	0.007	Yes	-0.029***	-0.024***	-0.031***	-0.009***	-0.007**	-0.010***	–
Unequal voting	nm	nm	nm	0.422	No	0.030	0.031	0.030	-0.044**	-0.039**	-0.044**	–
Written consent	0.006	-0.025	0.005	0.464	No	0.005	-0.006	0.000	0.011***	0.011***	0.012***	+

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Table 5: Summary of results from all five sets of empirical models

Column 1 summarizes the preliminary results regarding takeover deterrence from Table 3 based on 2SLS and OLS models. Column 2 summarizes the results regarding takeover deterrence from Table 4 based on LIML, RBP, and probit models. Column 3 combines these results to identify the subset of 13 provisions for which the evidence from both Tables 3 and 4 are consistent. These 13 provisions include 11 provisions that are negatively related to takeover likelihood and two provisions that are positively related to takeover likelihood. Column 4 identifies a subset of eight of these 13 provisions for which previous researchers have advanced theoretical arguments that are consistent with the empirical findings summarized in Column 3, as discussed in Section 5 of the paper.

	(1) Preliminary inference from Table 3 (OLS and 2SLS tests)	(2) Inference from Table 4 (LIML,RBP, and probit tests)	(3) 13 provisions with consistent preliminary and corroborating results regarding takeover likelihood	(4) 8 provisions with both empirical and theoretical support regarding takeover likelihood
Anti-greenmail	negative	negative	negative	negative
Blank check pref. stock	weakly negative	negative	negative	negative
Business combination				
Bylaw amendments				
Cashout laws				
Charter amendments		negative		
Classified board	negative	negative	negative	negative
Compensation plans				
Director contracts	negative	negative	negative	
Director indemnification	negative	negative	negative	
Director liability	negative	negative	negative	
Directors' duties	negative	negative	negative	negative
Executive severance	negative	negative	negative	
Fair price	negative	negative	negative	negative
Golden parachutes	positive	positive	positive	positive
Not cumulative voting				
Not secret ballot				
Pension parachutes		negative		
Poison pill	positive			
Silver parachutes				
Special meeting				
Supermajority vote	negative	negative	negative	negative
Unequal voting	negative	negative	negative	negative
Written consent	positive	positive	positive	

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Table 6: How different combinations of antitakeover provisions relate to takeover deterrence

This table reports results from takeover likelihood models based on 20 different indices of antitakeover provisions. Each row reports the regression coefficient ∂ from the takeover equation below using limited probability (LPM) and 2SLS models with each row focusing, in turn, on a different set of provisions described in the table. All tests include the firm, industry and year controls discussed in Section 3.1. In each test, the dependent variable (y_1) is set to 1 in year *t* if the firm was acquired within the next year:

$$y_1 = \propto_0 + subset_i \partial_i + \sum_{j=1}^{74} x_j \beta_j + e \qquad \text{(takeover equation)}$$

The results reported in Column (1) are estimated without correcting for endogeneity. The results reported in Column (2) are from the second stage of a 2SLS model in which an instrument for $subset_i$ is estimated from the following first-stage equation, using the geography- and IPO-year-based instruments ($set_i geo_IV_i$, $set_i ipo_IV_i$) described in Sections 3.3 and 6:

$$subset_i = \delta_0 + subset_i geo_IV_i \pi_i + set_i ipo_IV_i \vartheta_i + \sum_{j=1}^{74} x_j \omega_j + \mu \qquad (1^{st} \text{ stage})$$

Panel A reports results when $subset_i$ is based on one of five common antitakeover indices used in the literature. To illustrate, Row 1 reports results using Gompers, Ishii, and Metrick's (2003) G-index. Row 2 reports results using the subset of provisions in the G-index that are identified in Table 5 as meaningfully associated with takeover deterrence, and Row 3 reports results using the G-index provisions that our tests indicate do not significantly relate to takeover deterrence. Panel B reports results when $subset_i$ is based on one of four combinations implied by previous arguments in the literature, plus one developed in this paper, as discussed in Section 6.2 and summarized in Table 5. Columns 3 and 4 report the F-statistic and R-square values from the first stage equations when estimating the marginal effects in Column 2. As in previous tests, the data consist of 19,385 firm-year observations from 1995-2010. Standard errors are corrected for heteroskedasticity and clustered by firm. *, **, *** report statistical significance at the 10%, 5%, and 1% levels, respectively.

		(1) LPM marginal effect (∂)	(2) 2SLS LPM marginal effect (∂)	(3) 1st Stage F-statistic	(4) 1st Stage R-square
Panel A: Five common indices					
1	G-index – All 24 provisions	0.000	-0.010***	74.127	0.165
2	Subset of 11 G-index provisions associated with deterrence	-0.004***	-0.009***	214.200	0.318
3	Subset of 13 G-index provisions not associated with deterrence	0.003***	-0.035	4.611	0.099
4	E-index – All six provisions	0.001	-0.036***	20.768	0.108
5	Subset of two E-index provisions associated with deterrence	-0.005**	-0.032***	67.772	0.146
6	Subset of four E-index provisions not associated with deterrence	0.007***	0.005	11.735	0.098
7	O-index – All 18 provisions	-0.001	-0.010***	116.168	0.184
8	Subset of nine O-index provisions associated with deterrence	-0.005***	-0.009***	254.695	0.333
9	Subset of nine O-index provisions not associated with deterrence	0.004***	-0.046*	4.553	0.078
10	FK-index – All ten provisions	-0.001	-0.020***	42.121	0.122
11	Subset of seven FK-index provisions associated with deterrence	-0.004***	-0.016***	111.468	0.211
12	Subset of three FK-index provisions not associated with deterrence	0.004*	-0.049	4.918	0.083
13	ATI – All five provisions	0.002*	-0.009	23.207	0.087
14	Subset of two ATI provisions associated with deterrence	-0.003	-0.011	20.630	0.051
15	Subset of three ATI provisions not associated with deterrence	0.004*	-0.049	4.918	0.083

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Table 6, continued

Panel B: Five other unique combinations of provisions					
16	Classified board and poison pill	-0.001	-0.181*	3.052	0.059
17	Classified board, supermajority vote requirement for a merger, fair price provision, and shareholder meeting restrictions	-0.011**	-0.171***	22.516	0.058
18	Poison pill and (limits to meetings *or* to action by written consent)	0.004	-0.025	26.172	0.074
19	Non-cumulative voting and classified board	0.000	-0.029	17.227	0.050
20	8 provisions with both empirical and theoretical support (Table 5)	-0.005***	-0.016***	104.625	0.185

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Table 7: Individual provisions' impact on takeover likelihood by period (1995-2002, 2003-2010)

Table 7 reports on regressions similar to those shown in Columns 6 and 9 of Table 3, but for subperiods from 1995 – 2002 (Columns 1-3) and 2003 – 2010 (Columns 4-6). The *2SLS Results Emphasized* columns indicate provisions that show evidence of endogeneity using a 10% cutoff with the regression-based "robustified Durbin-Wu Hausman" test (Cameron and Trivedi 2010, page 187-188), and have a strong instrument. As indicated in Figure 1, we base inferences on the 2SLS results for these provisions. Internet Appendix Table B.7 reports additional information about these subperiod tests. Standard errors are corrected for heteroskedasticity and clustered by firm. Significance at the 10%, 5% and 1% levels are shown using *, **, and ***, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
		1995 - 2002			2003 - 2010	
	2SLS Results Emphasized	2SLS Single indicator with index of other 23 provisions	OLS Single indicator with index of other 23 provisions	2SLS Results Emphasized	2SLS Single indicator with index of other 23 provisions	OLS Single indicator with index of other 23 provisions
Anti-greenmail		-0.015	-0.006	Yes	-0.035***	-0.013**
Blank check preferred stock		-0.116	-0.011		0.044	-0.003
Business combination law		-0.002	0.006		-0.022	-0.007
Bylaw amendment limitations		-0.097	0.003		0.010	0.007
Cashout law	Yes	-0.017	0.015		-0.045*	-0.027***
Charter amendments		-0.216	0.014		0.007	0.017
Classified board		-0.057	-0.007		-0.068	0.002
Compensation plan		0.032	0.016***		-0.170**	-0.003
Director contract		-0.037	-0.015**		-0.062	-0.007
Director indemnification		-0.007	-0.007	Yes	-0.037**	-0.010**
Director liability		0.003	-0.005		-0.021**	-0.009**
Directors' duties	Yes	-0.042*	-0.001	Yes	-0.052**	-0.005
Executive severance		-0.149	-0.013*		-0.119	-0.010
Fair price	Yes	-0.047**	-0.003	Yes	-0.039***	-0.006
Golden parachute		-0.046	0.021***		-0.010	0.014***
Not cumulative voting		0.011	0.004		0.043	0.001
Not secret ballot		-0.04	-0.008		0.025	-0.008
Pension parachutes		-0.228	0.009		-0.127	-0.012
Poison pill		-0.067	0.010**		-0.195	0.003
Silver parachutes		0.205	0.008		-1.224	0.009
Special meeting limitations		-0.060	-0.001		0.021	0.006
Supermajority vote requirement		-0.025*	-0.007	Yes	-0.042***	-0.014***
Unequal voting		-0.030	-0.034***		2.001	-0.037***
Written consent limitations		-0.028	0.006		0.018	0.018***

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Table 8: Individual provisions' impact on takeover likelihood using ISS data from 2007-2020

Table 8 reports on regressions similar to those shown in Columns 6 and 9 of Table 3, but using ISS data from 2007-2020 that cover 13 of the original 24 G-index provisions. The five-year lagged instruments in years 2002-2006 were created using IRRC data. The *2SLS Results Emphasized* column indicates provisions that show evidence of endogeneity using a 10% cutoff with the regression-based "robustified Durbin-Wu Hausman" test (Cameron and Trivedi 2010, pages 187-188), and have a strong instrument. As indicated in Figure 1, we base inferences on the 2SLS results for these provisions. The Internet Appendix Table B.8 reports additional information about these tests. Standard errors are corrected for heteroskedasticity and clustered by firm. Significance at the 10%, 5% and 1% levels are shown using *, **, and ***, respectively.

		2SLS	OLS
	2SLS Results Emphasized	Single indicator with index of other 12 provisions	Single indicator with index of other 12 provisions
Blank check preferred	Yes	0.091	-0.003
Classified board	Yes	0.075*	-0.009**
Special meeting limitations		-0.047	-0.001
Written consent limitations		-0.019	-0.001
Golden parachutes		-0.013	0.016***
Bylaw amendment limitations		0.09	-0.003
Charter amendment limitations		0.757	-0.021***
Not cumulative voting		0.052	0.004
Not secret ballot	Yes	0.065*	0.007
Supermajority vote requirement	Yes	-0.087**	-0.003
Unequal voting		0.239	-0.024***
Poison pill		-0.147	0.005
Fair price	Yes	-0.088***	0.002

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Table 9: Illustration of result sensitivity in tests for unconditional takeover premiums

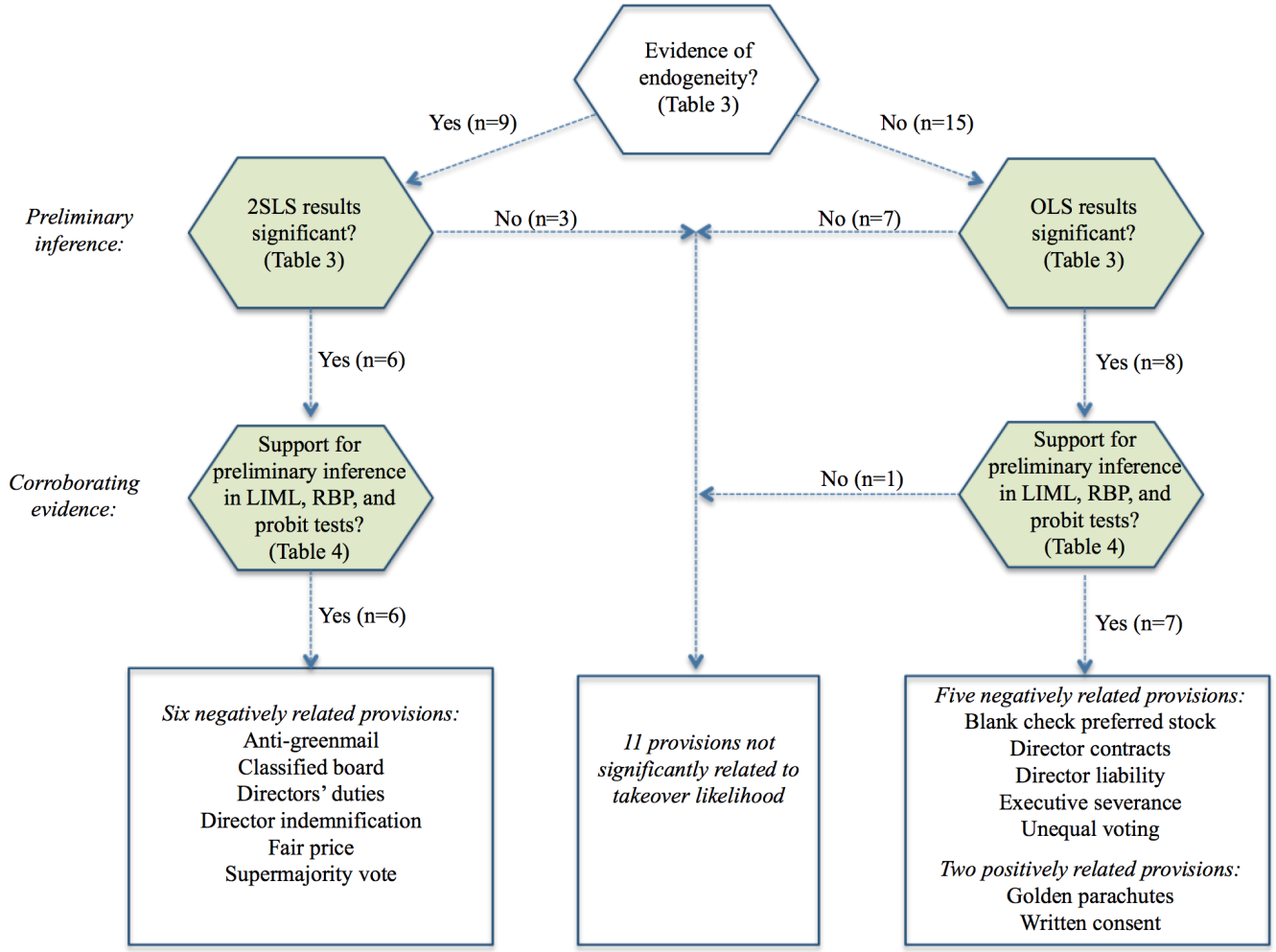
This table reports results from tests of firms' unconditional takeover premiums, as in Comment and Schwert (2005), using four different indices of antitakeover provisions: the G-index, the E-index, an index based on the 11 provisions that are significantly related to takeover deterrence, and an index of eight provisions that are supported by both our empirical results and previous theoretical arguments made in the literature about provisions that, a priori, are considered to have strong impacts on takeover deterrence. Table 5 lists the specific provisions in these indices of 11 and 8 provisions. All tests include the control variables described in Section 3.1. Columns 1-4 report coefficients from simple regressions that do not account for the potential endogeneity of the antitakeover provisions. Columns 5-8 report coefficients from the second stage of 2SLS models in which instruments for the indices are estimated in first stage regressions using the geography- and IPO-year-based instruments described in Sections 3.3 and 6. As in previous tests, the data consist of 19,385 firm-year observations from 1995-2010. The F-statistics and R-square values from the first stage models are shown in the column footers. Standard errors are robust to heteroskedasticity and clustered at the firm-level. *p*-values are reported below the coefficients with significance shown at the 10%, 5%, and 1% levels using *, **, ***, respectively.

	OLS (1)	OLS (2)	OLS (3)	OLS (4)	2SLS (5)	2SLS (6)	2SLS (7)	2SLS (8)
G-index	-0.000				-0.003*			
	(0.976)				(0.075)			
E-index		0.001				-0.002		
		(0.344)				(0.739)		
Eleven deterrence provisions			-0.001***				-0.003***	
			(0.003)				(0.010)	
Subset of eight provisions				-0.001**				-0.005***
				(0.017)				(0.000)
Firm size	-0.005***	-0.005***	-0.004***	-0.005***	-0.004***	-0.005***	-0.004***	-0.004***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Leverage	0.008	0.008	0.008	0.008	0.008*	0.009*	0.007	0.006
	(0.107)	(0.119)	(0.129)	(0.128)	(0.095)	(0.077)	(0.189)	(0.245)
Property ratio	-0.003***	-0.003***	-0.003***	-0.003***	-0.003***	-0.003***	-0.003***	-0.003***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Liquidity ratio	-0.004	-0.004	-0.003	-0.003	-0.002	-0.004	-0.003	-0.003
	(0.107)	(0.100)	(0.142)	(0.129)	(0.270)	(0.106)	(0.210)	(0.234)
Sales growth	-0.007	-0.007	-0.008	-0.007	-0.010	-0.008	-0.008	-0.008
	(0.342)	(0.362)	(0.334)	(0.347)	(0.269)	(0.396)	(0.321)	(0.340)
ROA	-0.003	-0.003	-0.003	-0.003	-0.004	-0.003	-0.004	-0.004
	(0.351)	(0.365)	(0.294)	(0.337)	(0.263)	(0.376)	(0.246)	(0.288)
Market-adjusted return	0.002	0.001	0.003	0.003	0.004	0.002	0.005	0.006
	(0.906)	(0.920)	(0.828)	(0.852)	(0.772)	(0.870)	(0.698)	(0.676)
Industry concentration	0.005	0.005	0.005	0.005	0.005*	0.005	0.005*	0.005
	(0.102)	(0.103)	(0.100)	(0.103)	(0.096)	(0.120)	(0.098)	(0.102)
Constant	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.584)	(0.627)	(0.563)	(0.544)	(0.483)	(0.528)	(0.531)	(0.424)
Year controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	19,385	19,385	19,385	19,385	19,385	19,385	19,385	19,385
R-square	0.013	0.013	0.014	0.014	0.007	0.004	0.013	0.010
1st stage F-statistic					595.7	152.3	3490.6	1764.4
1st stage R-square					0.164	0.105	0.395	0.236

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Figure 1: Decision process for identifying significant provisions

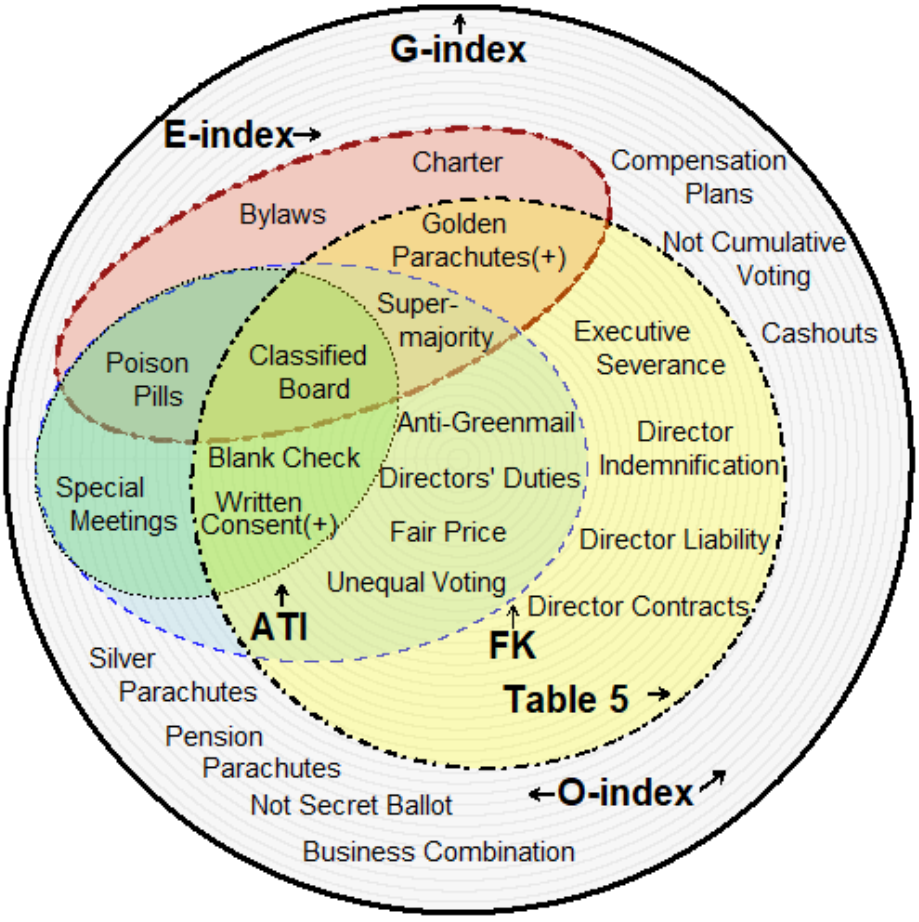
The figure displays the decision process applied to the results from 2SLS, OLS, LIML, RBP, and probit tests to identify which of the G-index antitakeover provisions are significantly related to takeover likelihood.



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Figure 2: Venn diagram of different takeover indices

In the figure below, the 24 G-index provisions are represented by the largest circle. Each of the shapes within the circle represents other indices used in the literature to proxy for takeover vulnerability. The shaded regions identify provisions that are shared between indices. The figure visually illustrates the differences in opinion that exist in the finance literature about which provisions relate to takeover likelihood. The G-index, E-index, FK-index, and ATI are described in Gompers, Ishii, and Metrick (2003), Bebchuk, Cohen, and Ferrell (2009), Field and Karpoff (2002), and Cremers, Nair, and John (2009), respectively. The O-index includes the G-index provisions minus the E-index provisions. The 13 provisions that are statistically related to takeover likelihood based on the analysis in Table 5 are identified by the large yellow circle in the center of the figure. The two provisions shown with positive signs indicate that the presence of these provisions is empirically linked to higher, not lower, takeover likelihood.



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