Why Does Corporate Governance Affect Firm Value: Evidence on a Self-Dealing Channel from a Natural Experiment in Korea

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Abstract

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Keywords: corporate governance, related-party transactions, tunneling, self-dealing, Chaebols, Korea

JEL Classifications: G30, G32, G34

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How Corporate Governance Affects Firm Value:

Evidence on a Self-Dealing Channel from a Natural Experiment in Korea[+]

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Abstract: Prior work in emerging markets provides evidence that better corporate governance predicts higher market value, but very little on the specific channels through which governance increases value. We provide evidence, from a natural experiment in Korea, that reduced tunneling can be an important channel. Korean legal reform in 1999 improved board independence of "large" firms (assets > 2 trillion won) relative to smaller firms. This shock to governance allows us to assess the effects of reform using a regression discontinuity design. In event studies of the reform events, we show that large firms whose controllers have incentive to tunnel (positive Expropriation Risk Index firms) earn strong positive returns, relative to other large firms. In panel regressions over 1998-2004, we also show that better governance (higher Korea Corporate Governance Index) moderates the negative effect of related-party transactions on value and increases the sensitivity of firm profitability to industry profitability (consistent with less tunneling).

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1. Introduction

There is evidence that firm level corporate governance affects firm market values in emerging markets, but very limited evidence on why. Through what "channels" does governance affect firm behavior, and thus market value? We study here one important channel: control of self-dealing by the controllers of business groups. Self-dealing is an important way that controllers can "tunnel" value out of firms, and one which governance can plausibly affect. We provide evidence that firm-level governance reduces "cash flow" tunneling by Korean firms through "ordinary" related-party transactions (RPTs) – purchases and sales of goods and services from affiliated firms. That is, we provide evidence for the causal channel (better governance → lower cash flow tunneling → higher Tobin's q). The effect of governance on cash flow tunneling involves apparently fairer pricing of RPTs, rather than a lower volume of RPTs.

A number of studies, in Korea and elsewhere, show that share prices are adversely affected by major RPTs such as mergers and equity issuances to insiders, but do not study whether governance can reduce the apparent harm to minority shareholders.[1] We focus here on routine RPTs – purchases and sales of goods and services from affiliated firms. These transactions are common within business groups in many countries. They may be efficient compared to transactions with unrelated firms, but can also be used to engage in "cash-flow tunneling" which extracts value from some firms in a business group, while benefiting other firms and the group controllers.[2]

We find evidence in Korea of an adverse effect of routine RPTs on firm market value in poorly governed firms. This evidence is consistent with a widespread view in Korea that intra-group transactions are an important problem. We also find evidence that investors expect governance to moderate the harm to minority shareholders from RPTs, and that governance in fact does so.

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[1] See Bae, Kang, and Kim (2002) (mergers in Korea); Baek, Kang, and Lee (2006) (equity issuance in Korea); Cheung, Rau, and Stouraitis (2006) (large, publicly announced RPTs in Hong Kong); Jiang, Lee and Yue (2010) (intercorporate loans in China).

[2] We use here the tunneling terminology suggested by Atanasov, Black, and Ciccotello (2011).

We study Korea because Korea allows identification of a causal link between governance and firm market value, based on a shock to the governance of large firms: a 1999 law requires firms with assets over 2 trillion won (about $2 billion) to have at least 50% outside directors, an audit committee with an outside director as chair and at least two-thirds outside members; and an outside director nominating committee. We exploit this legal shock to board structure at 2 trillion won by generally limiting our sample to large and mid-sized firms with assets from 0.5-4 trillion won, close to the threshold. In our event study, we use an explicit regression discontinuity (RD) research design, in which we investigate the impact of the reform on large firms, close to but above the 2 trillion won threshold, relative to mid-sized firms that are close to but below the threshold. In prior work, we find strong evidence that the reforms predict higher market value for large firms (Black and Kim, 2012). In this study, we ask the "channels" question: can reduced risk of RPTs help to explain this rise in market value, and for which firms.

We do not directly observe tunneling. Instead, we look for indirect evidence consistent with tunneling. To do so, we develop a measure of the incentives and propensity of a firm's controlling family to tunnel value out of the firm, which we term an "Expropriation Risk Index (ERI)." ERI is positive for a given firm-year if the controlling family holds, on average, lower cash-flow rights in the firm than its RPT counterparties. If so, the controller has an incentive to use the pricing of RPTs as a low-visibility way to transfer value to these counterparties. We have the data to measure ERI only for *chaebol* (Korean family-controlled business group) firms, but *chaebol* groups are a likely place to look for evidence of tunneling.

We first conduct an event study of the key reform events in 1999, and investigate whether investors expect the 1999 reforms to limit tunneling. We use a combined event study/RD design, in which we study event period returns to "large-plus" firms, just above the then-expected size threshold for the reforms of 1 trillion won, relative to mid-sized firms just below this threshold.[3]

[3] At the time of the initial legislative events, captured in the event study, the threshold for the reforms was 1 trillion won; it was raised to 2 trillion won later in the legislative process. We refer to firms with assets > 1 trillion won as "large-plus" and firms with assets > 2 trillion won as "large."

If investors expect the reforms to limit tunneling, then large-plus, positive-ERI firms should realize positive abnormal returns when the reforms are adopted. These firms in fact realize roughly 30% cumulative market adjusted returns (CMARs) during our event period. Large-plus, negative-ERI firms earn positive but smaller CMARs of 5-10%. Thus, investors appear to view the governance reforms as strongly beneficial for positive-ERI firms, and only mildly so for other large firms.

We next investigate the effect of the governance reforms on cash-flow tunneling. We find no evidence that governance affects the *volume* of RPTs. However, we do find an effect of tunneling on firm value, which is mediated by governance. More specifically, for positive-ERI firms, we find a positive interaction between a broad Korea Corporate Governance Index (KCGI) and RPT volume for positive-ERI firms. This is consistent with governance leading to improved RPT pricing for these firms. This interaction term is insignificant for negative-ERI firms.

We also assess whether RPTs are adverse to profitability and whether governance mediates that relationship. For *chaebol* firms, we find moderate evidence that RPTs predict lower profitability for poorly governed firms (with below-median *KCGI*), but no significant relationship between RPTs and profitability for better-governed firms (with above-median KCGI). The mediating impact of governance on the relationship between RPTs and profitability is stronger for positive-ERI firms than for negative-ERI firms. We also apply the Bertrand, Mehta, and Mullainathan (2002) approach to search for indirect evidence of tunneling. They assess the responsiveness of firm profitability to industry profitability. Lower firm responsiveness to industry profitability provides evidence of cash-flow tunneling. We find evidence consistent with cash-flow tunneling by positive-ERI firms, and evidence that better governance reduces cash-flow tunneling.

This paper is organized as follows. Section 2 provides a literature review, describes how we construct our governance index and Expropriation Risk Index (ERI), and discusses our data sources and some methodology issues. Section 3 presents our event study results, Section 4 our RPT-to-value regression results, and Section 5 our profitability results. Section 6 concludes.

2. Background

2.1. Literature Review

In this paper, we begin with a reasonably identified link between governance and firm market value (proxied by Tobin's *q*), based on our prior work. We then assess the evidence on a particular channel through which governance might affect value: Does governance affect tunneling? Thus, we need to situate this paper within the literature on the relationship between corporate governance and firm value ("governance to value studies"), the literature on tunneling, and the smaller literature that links governance to tunneling. Our review focuses on emerging markets.

With regard to corporate governance, we write within the literature on firm-level governance; there is a separate large literature on the impact of country-level governance. Prior research provides evidence that corporate governance is associated with firm market value (usually proxied by Tobin's *q*), but only a few studies have a basis for identification and thus causal inference.[4] A smaller set of papers addresses whether governance predicts firm outcomes other than market value (Tobin's *q*), but do not also link governance to market value.[5] Two studies link governance to both market value and other firm outcomes.[6] However, these papers are purely cross-sectional and lack identification. Our results are less directly relevant for developed markets, but there too, studies with credible identification for the effect of governance on firm behavior are scarce (Dahya and McConnell, 2007, is a notable exception).

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[4] Claessens and Yurtoglu (2013) provide a recent survey. The principal multi-country studies are Klapper and Love (2004), Durnev and Kim (2005), and Black et al. (2014). La Porta, Lopez-de-Silanes, Shleifer and Vishny (2002) and others study the association between firm market value and the "wedge" between insiders' voting and economic ownership.

[5] In Korea, Joh (2003) finds that Korean *chaebol* firms with high control-ownership disparity have lower profitability during the pre-crisis period. Hwang, Park, and Park (2004) find that better-governed *chaebol* firms pay higher dividends. Mitton (2004) finds that the Credit Lyonnais Securities Asia (CLSA) governance index predicts higher profitability and higher dividends. In China, Liu and Lu (2007) find that better governance predicts less earnings management, and (less clearly) lower tunneling.

[6] Klapper and Love (2004) report that the CLSA index predicts both firm market value and profitability. However, the *CLSA* index depends significantly on analysts' subjective views and mixes measures of management quality with measures of governance. Dahya, Dimitrov and McConnell (2008) find that proportion of independent directors predicts higher Tobin's *q* and fewer RPTs. Black et al. (2014) discuss the fragility of the estimates in these studies.

Within Korea, Bae et al., 2002 find negative share price reactions to the announcement of acquisitions by *chaebol* firms, but do not link tunneling to governance.

Several papers study the 1997-1998 Asian financial crisis. Mitton (2002) finds better share price performance for better-disclosing firms in crisis-affected countries. Lemmon and Lins (2003) find higher crisis-period returns for firms with low control-ownership disparity. Baek, Kang, and Park (2004) find both effects for Korean firms. Bae et al. (2012) report that firms with high disparity between the controller's voting and cash flow rights suffer larger share price drops during the East Asian financial crisis (plausibly due to higher tunneling), and recover faster when the crisis abates. These studies do not directly examine tunneling, but their results plausibly reflect investor response to tunneling risk.

The papers closest to ours in spirit are Bertrand, Mehta, and Mullainathan (2002), Cheung, Rau, and Stouraitis (2006), and Atanasov et al. (2010). Bertrand, Mehta, and Mullainathan develop an approach to inferring the existence of tunneling by studying the sensitivity of firms' reported profitability to changes in industry profitability. They find evidence for tunneling, principally through non-operating cash flows, and that higher director ownership predicts less tunneling. We adapt their approach to Korea below. Cheung, Rau, and Stouraitis find negative share price reactions to the announcement of RPTs which require a filing with the Hong Kong Stock Exchange. A filing is required for larger transactions, involving at least 3% of firm assets or at least HK$10 million. They assess whether inside ownership predicts share price reaction for a sub sample for which they have ownership data, and find no relationship. Atanasov et al. (2010) study the impact of Bulgarian reforms that limit "equity tunneling" on firm market values at the time of the reforms and on equity tunneling after the reforms.

Finally, it may be useful to clarify how this paper relates to our own prior work. In Black, Jang and Kim (2006a) (*BJK*), we use cross-sectional data from 2001 and report evidence that governance predicts higher market value, using the 1999 reforms as an instrument for governance. In Black and Kim (2012), we extend our governance index back to 1998 and forward to 2004, and tighten the causal link between the 1999 reforms and higher firm market values, using several

identification strategies. Black, Jang and Kim (2006b) study the factors that predict firm governance. None of these papers studies the channels that might explain the governance-to-value effect.

2.2. Korean Corporate Governance Index (*KCGI*)

What matters in corporate governance is likely to vary across countries, so a governance index should reflect local rules and practices (e.g., Black et al., 2014; Bruno and Claessens, 2010; Claessens and Yurtoglu, 2013). We therefore use a governance index tailored to Korea.

We construct a broad Korean corporate governance index (*KCGI*) from 1998 to 2004, covering almost all public companies listed on the Korea Stock Exchange. We rely for governance data on a combination of hand-collection and annual surveys by the Korea Corporate Governance Service (KCGS). Board composition data comes from annual books published by the Korea Listed Companies Association (*KLCA*). *KCGI* (0 ~ 100) consists of five equally weighted subindices, for Board Structure, Disclosure, Shareholders Rights, and Board Procedure, and Ownership Parity (a measure of whether the control group holds similar control rights and cash flow rights). We have data on *KCGI* at mid-year 2001, and year-ends 1998-2004 – a total of eight time points.

The elements and subindices cover aspects of governance which we judged to be potentially important in Korea. We made many judgment calls on which elements to include in the index, how to define them, and how to construct the subindices. For example, at the time of our study, almost all Korean firms have a controlling shareholder or group. Thus, takeover defenses were unimportant and rarely used. As a result, our index is quite different from U.S.-centric indices, which focus on takeover defenses, such as the G index developed by Gompers, Ishii and Metrick (2004) or the E index developed by Bebchuk, Cohen, and Ferrell (2009).

KCGS often changes its survey questions from year to year.[7] We reduce loss of governance elements due to these changes by hand-collecting data from company public reports,

[7] English translations of the KCGS surveys are available from the authors on request.

charters, and websites. To reduce the cost of hand-collection, we assume that firms comply with legal requirements and that firms which lacked a governance element in year *t* also lacked this element in prior years. For the elements of each subindex and how we construct them, please refer to Black and Kim (2012).

Board Structure Index is of special interest because the board structure of large firms is directly affected by 1999 legal reforms. These reforms provide the exogenous shock to governance that we rely on to identify a causal effect of governance on Tobin's *q*. Board Structure Index is composed of two subindices, Board Independence Subindex (2 elements, 0 ~ 10), and Board Committee Subindex (3 elements, 0 ~ 10). These subindices are defined as:

Board Independence Subindex = 10*(b1 + b2)/2

b1 = 1 if firm has 50% outside directors; 0 otherwise

b2 = 1 if firm has > 50% outside directors; 0 otherwise

Board Committee Subindex = 10*(b3 + b4 + b5)/3

b3 = 1 if firm has outside director nominating committee, 0 otherwise

b4 = 1 if firm has audit committee, 0 otherwise

b5 = 1 if firm has compensation committee, 0 otherwise

The 1999 law requires large firms to have elements b1, b3, and b4. For a firm which previously had none of these elements, Board Structure Index will rise from 0 to 11.67, out of 20 possible points. The large firm mean in fact rises from 0.31 in 1998 (one large firm had 50% outside directors, none had audit or other committees) to 12.78 in spring 2001.

For the other subindices, we weight all elements of each subindex equally. If data on a subindex element is missing for a particular firm, we compute the subindex using the average of the nonmissing elements. Table 1, Panel A, provides summary statistics for *KCGI* and each subindex. Table 1, Panel B, provides correlation coefficients. All subindices are strongly correlated with each other, except for Ownership Parity, which is only weakly correlated with other subindices.

2.3. Data on RPTs and Expropriation Risk Index (ERI)

Routine RPTs can be seen as similar to partial vertical integration. They can increase efficiency by reducing transaction costs and the risk of opportunism. But they can also reduce efficiency, if the firm would do better to transact with an unrelated party. If firms engage in RPTs principally when it is efficient to do so, governance might have little impact on RPT volume.

Even if RPTs are efficient, they can be priced to benefit controllers and harm minority shareholders. The controllers' incentives to engage in mispriced RPTs depend on their relative ownership of the transacting firms. If the controllers own a larger percentage of Firm B than of Firm A, the controllers have an incentive to use "transfer pricing" to transfer value to B at A's expense. For example, the controllers can cause A to overpay B for services, or cause A to sell its output to B for less than fair value.

We do not directly observe the fairness of RPT pricing. We seek instead to capture controllers' incentives to transfer value between related firms through our "Expropriation Risk Index (ERI)." ERI captures the extent to which, when a firm engages in transactions with related parties, the controlling family or group owns a larger fraction of the cash flow rights in the related firms than it owns in the subject firm. For each firm *i*, related counterparty *j*, and year *t*, we compute a cash flow rights differential as:

(Cash Flow Differential)$_{ijt}$ = (controller's fractional cash flow rights in firm *j*) – (controller's fractional cash flow rights in firm *i*).[8]

We then define an Expropriation Risk Index (ERI) to capture the intuition that RPTs will tend to move value to firms in which the controller has higher cash flow rights:

$$\text{ERI}_{it} = \sum_{j \neq i} \left(\frac{RPTs}{Sales} \right)_{ijt} \times \left(\text{Cash Flow Differential} \right)_{ijt}$$

[8] If the counterparty is an individual in the controlling family, we treat this as 100% ownership of the counterparty. The KFTC treats transactions with wholly owned subsidiaries as RPTs; we exclude these transactions in computing ERI.

This index will be positive if the firm's RPTs are primarily with counterparties in which the controllers have larger fractional cash flow rights and thus incentives to use RPTs to extract value from the firm. The magnitude of ERI depends on both the level of RPTs and on the degree of cash flow differential between the counterparties.

All public firms must disclose in the footnotes to their annual financial statements amounts owed to the firm by affiliated firms (including receivables), debts owed to affiliated firms (including payables), purchases (sales) of goods and services from (to) each affiliate, and purchases (sales) of assets from (to) all affiliates together. However, these disclosures do not include data on the pricing of these transactions. Our measure of RPTs is RPTs/sales, defined as (sum of sales to and purchases from related-parties)/ total sales (winsorized at 99%). In some analyses, we consider separately purchases from related parties (RPPs/sales) and sales to related parties (RPSs/sales).

The disclosures provided by all public firms allow us to compute the volume of RPTs, but not ERI. We know the counterparty for each transaction. This lets us determine the control group's ownership of the counterparty if the counterparty is publicly traded, but not if the counterparty is privately held. Many RPTs – and often the more troublesome ones -- are with private firms. We can, however, compute ERI for firms which are part of major *chaebol* groups, because the KFTC requires these firms to disclose the controlling family's ownership of all group members, including private firms.[9]

Our ERI measure for firm *i* is related to but distinct from measures of the controller's cash flow rights in firm *i*, or the "wedge" between the controller's voting rights and cash flow rights. ERI takes into account not only the controller's cash flow rights in firm *i*, but also its cash flow rights in other group firms, and the volume of related-party transactions between them. It thus

[9] For 1998 through mid-2000, KFTC listed the 30 largest *chaebol* groups in each year. Beginning with year-end 2001, KFTC used a group size threshold, and reported information on roughly 50 groups in each year. We treat a firm as a *chaebol* firm if it is listed as such in any of the KFTC annual reports. In prior work, we defined *chaebol* dummy as equal to 1 if a firm belonged to one of the top 30 *chaebol*. In regressions, we use membership in the top-30 *chaebol* as a control variable, unless specifically stated otherwise.

can capture the incentive to tunnel in a way that is more sensitive to the behavior of controllers of specific firms than a simple measure of cash flow rights. We confirm below that *ERI* has predictive value for our sample, while a simpler measure of cash flow rights does not.

2.4. Identification for Large Firms

The endogeneity of firm governance, other firm characteristics, and outcome variables of interest is a core issue for corporate governance research (e.g., Lehn, Patro and Zhao, 2007). In this study, we begin with reasonable identification for the causal effect of the board structure changes that were required for large firms. We summarize the basis for identification here. Black and Kim (2012) provide more details. We lack a similar exogenous shock for the rest of *KCGI*. However, firms which are required to change their board structure may make followon changes in other aspects of governance. Nasev, Black and Kim (2014) find evidence that large firms improve their disclosure after the 1999 reforms, but no evidence for significant followon changes in other aspects of governance.

Before the 1997-1998 East Asian financial crisis, most Korean firms had no outside directors and only a few banks and "state-owned enterprises (SOEs)," in which the government held a majority stake, had 50% outside directors. Legal reforms in 1998 required all public firms to have at least 25% outside directors. Further reforms in 1999 (i) made it possible for the first time for firms to have committees of the board of directors, such as an audit committee; and (ii) required large firms (assets > 2 trillion won, about $2 billion) to have at least 50% outside directors, an audit committee, and an outside director nominating committee. The large firm rules came into force partly in 2000 and fully in 2001.

Figure 1 provides evidence on the exogeneity of this shock to large firm governance. It shows Board Structure Index for each year for mid-sized firms (assets from 0.5-2 trillion won) and large firms at year-ends from 1998 to 2004. In each figure, the vertical line indicates 2 trillion won; the horizontal line indicates the minimum Board Structure Index score which large firms must have by 2001. In 1998, only one firm has a nonzero score. By 2001, large firms fully

comply with the new rules; some mid-sized firms also voluntarily comply.[10] Figure 2 shows the change in *KCGI* and its component indices over time, separately for large and mid-sized firms.

This shock to the board structure of large firms provides a basis for credible causal inference with regard to how this change affected firm value (Tobin's *q*). An event study of key legislative events should capture investor views, at the time of the reforms, on how the reforms should affect firm market values. A difference-in-differences (DiD) analysis of changes in Tobin's *q*, from just before to just after the reforms, should also capture the predicted effect of the reforms. An instrumental variables (IV) analysis, with "Large Firm *IV 1999*" (=1 if large firm dummy =1 and year is 1999 or later, 0 otherwise) as an instrument for Board Structure Subindex is mathematically very similar to DiD. Black and Kim (2012) find strong evidence from all three approaches that the market values of large firms increased, relative to a control group of mid-sized firms.

Below, we initially present event study analyses of how the 1999 reforms affected three subgroups of *large-plus* firms (assets from 1-4 trillion won): positive-ERI *chaebol* firms, negative-ERI *chaebol* firms, and non-*chaebol* firms (for which we cannot measure ERI). At the time of the key legislative events, the expected size threshold was 1 trillion won; this threshold was later raised to 2 trillion won. The event study design has greater statistical power than DiD or IV to capture treatment effects in a small sample. In unreported DiD regressions, we obtain consistent but weaker results (summarized below).

2.5. Sample, Control Variables, and Regression Specification

An important limitation of this study is the modest number of large firms. This both limits statistical power and increases the risk that a few "odd" observations could drive our results. Our main results use a sample of mid-sized and large Korean public firms, within a size band from 0.5-4 trillion won in assets. Our choice of this size band reflects a compromise between desire for a

[10] We check for and find no evidence that firms shrink to avoid complying with the large firm rules. See Black and Kim (2012) for details.

narrow band, to make treated and control firms more similar, and need for a reasonable sample size. The tradeoff between bandwidth and sample size is often faced in regression discontinuity designs, but is especially acute for us because of the limited number of large firms.

In our event study (Section 3), we have a treatment group of 54 large-plus firms (assets from 1-4 trillion won) and a control group of 43 mid-sized firms (assets from 0.5-1 trillion won). The treatment group includes 19 positive-ERI *chaebol* firms, 16 negative-ERI *chaebol* firms, and 19 non-*chaebol* firms. In our regressions (Sections 4 and 5), the treatment group, after excluding outliers, is 52 large firms (assets from 2-4 trillion won) and the control group is 120 mid-sized firms (assets from 0.5-2 trillion won). For regressions which rely on *ERI*, we further limit the panel to *chaebol* firms (41 large firms; 63 midsized firms). In unreported results, we vary the size band, with consistent results.[11] To reduce the risk that one or several influential observations could drive our results, we exclude outliers in our regressions (see details below).

In regressions, we use firm fixed effects to control for unobserved, time-invariant firm characteristics, year dummies to address variation over time that is common to all firms, and firm clusters to address correlated within-firm observations.

We use an extensive set of control variables to limit omitted variable bias. The control variables capture factors that are likely to affect Tobin's *q*, including growth opportunities, profitability, existence of intangible, off-balance-sheet assets, and capital intensity, See Black, Jang and Kim (2006) for a fuller discussion of our controls. Table 2, Panel A, defines the principal variables used in this study. Table 2, Panel B, provides summary statistics for these variables.

Data comes from various sources. We take balance sheet, income, cash flow statement data, foreign ownership data, related-party transactions, and original listing year from the *TS2000* database maintained by the KLCA; adjusted return data from the Korea Securities Research

[11] If we increase the upper limit on size from 4 trillion won to 8 trillion won, we gain 12 large firms (11 of these are *chaebol* firms), and obtain results (both coefficients and *t*-statistics) quite similar to those we report. If we further expand the size band at both the low and high ends, our results generally strengthen.

Institute database; information on *chaebol* groups from annual reports by the Korea Fair Trade Commission (KFTC); other stock market data from the KSE; information on *ADRs* from JP Morgan and Citibank websites; and industry classification from the Korea Statistics Office (KSO). Share ownership comes from the KSE for financial institutions and from a hand-collected database for other firms.

3. Event Study Results

If investors expect positive-*ERI* firms to engage in more tunneling than negative-*ERI* firms, *and* expect that the 1999 reforms will reduce tunneling of value at the firms affected by the reforms, then large-plus, positive-*ERI* firms should realize higher event-period returns than large-plus, negative-ERI firms when the reforms are adopted, relative to a control group of mid-sized firms.[12] We test this prediction by studying returns over the period from June 1, 1999 (just before the first significant reform event, on June 2-3, 1999) through August 30, 1999 (shortly after the last significant, clean event, on August 25, 1999). The expected size threshold for the reforms during this period was 1 trillion won, so we consider large-plus firms (assets from 1-4 trillion won) to be treated firms, and mid-sized firms (assets from 0.5-1 trillion won), to be control firms.

The first important reform announcements, a general announcement on June 2 and a more specific announcement on June 3, 1999, indicated that the government would focus on *chaebol* reform, and on audit committees and on outside directors. Many but not all *chaebol* firms are also large-plus firms. On July 2, the government announced that the reforms would apply to "large" firms (rather than *chaebol* firms) but did not specify a size threshold. The size threshold was first disclosed on August 25, 1999, when a draft law specified a 1 trillion won threshold. These events provide an event period running from June 1, 1999 (the day before the June 2 announcement) through Aug. 30, 1999 (3 trading days after the Aug. 25 announcement). We are not aware of other regulatory changes during this period that differentially affected large and small firms. The size threshold was raised to 2 trillion won during September and October, 1999; there

[12] In our event study, positive-ERI firms are firms for which the average of 1998 and 1999 ERIs is positive.

is no single clean event for this change. We summarize the key events in Table 3; Black and Kim (2012) provide further details.

We compute cumulative market adjusted returns (*CMARs*) to large-plus firms during the event period, relative to a "Mid-sized Index" – an equally weighted index of the mid-sized firms in our sample. The *CMARs* are the sum of daily MARs over the event period. Size is measured at year-end 1998.

3.1. Graphical Results

We begin with graphical results. Figure 3, Panel A, shows CMARs, over May 20 – September 9, 1999 (10 trading days before Event 1 through 10 trading days after Event 3), for three groups of large-plus firms: (i) positive-ERI *chaebol* firms; (ii) negative-ERI *chaebol* firms; and (iii) non-*chaebol* firms, for whom we cannot compute ERI. Vertical lines indicate the principal event dates (June 3, July 2, and August 25); dotted vertical lines indicate the full event period. During the event period, the returns to the positive-ERI subsample are strongly positive, while the returns to the other two groups are not. Figure 3 thus provides evidence that investors see the reforms as very good news for large-plus, positive-ERI firms, but only mildly good news for other large-plus firms. Total event period returns to the positive-ERI firms are around 30%, compared to under 5% for the other two treatment groups. This is consistent with investors anticipating that the governance reforms will benefit minority shareholders by limiting tunneling at positive-ERI firms.[13]

Figure 3, Panel B extends the plot in Panel A to cover a longer window (March 31 – October 31, 1999) and confirms this finding. The gap between CMARs for positive versus negative-ERI firms persists after the event period. In the pre-reform period, there is a relative rise in prices for large-plus non-*chaebol* firms in April 1999 (well before the start of the reform period), but no relative trend for large-plus, positive-ERI versus negative-ERI firms.

[13] The divergence between large-plus and mid-sized firms during the event period does not appear to be related to overall market movements. During the event period, overall share prices are flat to slightly declining. There is also little divergence between large-plus and mid-sized firms earlier in 1999, when overall share prices were rising strongly.

3.2. Regression Results

We turn next to regression analysis of the event period returns. We regress the *CMARs* on treatment group dummy variables and control variables. A typical regression is:

$$CMAR_i = \alpha + \sum_k (\gamma_k * D_{treat,k}) + \sum_j (\lambda_j * X_j) + \varepsilon_i \qquad (2)$$

Here k indexes the three treatment groups, $D_{treat,k}$ is a dummy variable for treatment group k (=1 if firm i belongs to group k, 0 otherwise) and $\mathbf{X}_j$ is a vector of control variables. The coefficients γ_k capture the average CMARs for the treatment groups over the event period.

In Table 4 Panel A, we report CMARs for the full event period, from June 1 – August 30, 1999, separately for large-plus positive-ERI *chaebol* firms; large-plus negative-ERI *chaebol* firms, and large-plus non-*chaebol* firms (which have missing ERI); the sample also includes the control group of mid-sized firms. The event period is common to all firms, so returns are likely to violate the usual regression assumption of independent observations. In particular, firms in the same industry could move together. We allow for this possibility by computing standard errors using industry-group clusters, with industries based on 4-digit Korea industry codes.[14] We also drop outlier observations for which a studentized residual obtained by regressing CMAR on large-plus positive ERI dummy, large-plus negative ERI dummy, and large-plus missing ERI dummy is greater than ±1.96, because outlier observations are likely to reflect firm-specific news, rather than a response to the governance reforms.

Regression (1) includes only the treatment dummies and a constant term. The return to large-plus positive ERI firms is strongly positive at 28% (t = 3.51). The returns to the other two groups are positive, but small and not significant.

The treated and control firms differ in size, which might separately predict event period returns. In regression (2), we therefore also control for *ln*(market cap). In regression (3), we

[14] This approach allows for cross-sectional correlation within industry, but assumes independence across industries. This is a compromise between the potential for cross-sectional correlation to produce biased standard errors, and the need to preserve statistical power. Brown and Warner (1985, p. 22) suggest that there can be "gains from procedures assuming independence [across industries] . . . even when . . . all [firms] have the same event date." Bernard (1987, at 11 and Table 1) similarly finds that while *intra*industry correlation can be important, "interindustry cross-sectional correlation is small relative to intraindustry correlation."

use a flexible size control – the first six powers of *ln*(market cap). Regression (4) further adds a battery of control variables, listed in Table 2. The results for large-plus positive-ERI firms are similar across specifications. These firms realize large statistically significant CMARs of 28-32% depending on specification.

The CMARs for the other two treatment groups are higher in regression (4), at 11% for large-plus, negative ERI firms and 14% for non-*chaebol* firms, but remain individually and jointly statistically insignificant. Although insignificant, the positive returns for these two groups suggest that investors believed the reforms had value for these other two groups as well, but less value than for large-plus positive-ERI firms.

In regression (5), we use two treatment groups: large-plus *chaebol* firms and large-plus non-*chaebol* firms, and also interact the large-plus *chaebol* dummy with a positive ERI dummy; the control group is again mid-sized firms. The coefficient on the non-interacted *chaebol* dummy provides an estimate of the CMAR for negative-ERI *chaebol* firms. The coefficient on the interaction term measures the difference in returns between positive- and negative-ERI *chaebol* firms. The interaction term is large (0.25) and statistically significant (t = 4.79), indicating greater event-period returns for positive-ERI *chaebol* firms than for negative-ERI *chaebol* firms.

Finally, in column (6), we switch to a classic event study methodology, and do not exclude outliers. We combine all firms in each treatment group into a single portfolio. Thus we fully allow for cross-sectional correlation within each group, at the cost of lower statistical power, but assumes independence across groups. We estimate the cumulative abnormal return to this portfolio over the event period, relative to the Mid-sized Index.[15] The coefficients in column (6) come from separate event studies for each treatment group. The coefficient for large-plus, positive-ERI firms is 0.315 and remains statistically strong (t = 3.64). The coefficients for other groups are statistically insignificant.

[15] Firms are equally weighted. We estimate the market model over January-May and September-December 1999.

In Table 4, Panels B and C, we show the results over narrower event windows around events 2 and 3. These are the principal events for which investors had reason to expect the reforms to apply to firms above a size threshold. The results are consistent with those for the full event window. During a (-2, +3) window around Event 2, large-plus, positive ERI *chaebol* firms earn positive, significant returns of 8-11%. During a similar window around Event 3, these firms earn positive, significant returns of 4-8%. During the periods around both events, large-plus, positive-ERI *chaebol* firms realize significantly higher returns than their negative-ERI counterparts.

Although the full-window returns are positive and significant only for positive-ERI firms, we find significant, positive returns to large-plus non-*chaebol* firms for Event 2 and significant, positive returns to negative-ERI firms for Event 3. This is consistent with investors seeing the governance reforms as likely to limit tunneling, or have other benefits, for all firms, not only positive-ERI firms. The short-window results suggest that investors see the board structure reforms are good news for all treated firms, but much stronger good news for positive-ERI firms.

Our definition of ERI implies that positive-ERI firms will tend to have lower inside ownership than negative-ERI firms. Among large-plus firms, the mean "cash flow rights" of the control group (defined as direct and indirect ownership of the firm's shares by the control group) is 10.6% for positive ERI firms versus 20.2% for negative ERI firms. We therefore assess, in unreported regressions, whether ERI or cash-flow rights are a better predictor of event period returns. If we add cash flow rights to the event study regressions in Table 4, Panel A, this variable takes a small and insignificant coefficient and the coefficients on large-plus, positive ERI dummy change only slightly. If we add cash flow rights plus its interactions with large-plus, positive-ERI dummy and large-plus negative ERI dummy to regressions (3) and (4) in Panel A, the coefficients on the interaction terms are insignificant, while the coefficients on the large-plus, positive-ERI dummies remain strong (generally higher coefficients and *t*-values).

In unreported analyses, we use a DiD approach with consistent but weaker results. Following Black and Kim (2012), we regress the change in *ln*(Tobin's *q*) for large and mid-sized

firms from just before the reform period (May 31, 1999) to December 31, 1999, on two treatment dummies (large, positive- ERI and large, negative- ERI). We use mid-sized firms (assets from 0.5–2 trillion won) as the control group and large firms (assets from 2-8 trillion won) as the treated group. We use 8 trillion as the upper bound on size, versus the 4 trillion won limit in the event study, to enhance sample size. This provides a treated sample of 33 large firms (22 with positive ERI, 11 with negative ERI; we drop large firms with missing ERI). The coefficient on large, positive ERI dummy is statistically significant (coefficient = 0.127, t = 1.96); the coefficient for large, negative ERI firms is insignificant (coefficient = 0.035, t = 0.76).[16]

The event study results provide evidence that investors expect the governance reforms to increase value for positive-ERI firms, presumably by reducing tunneling. They suggest, but less strongly, that investors expect the reforms to specifically reduce *cash-flow* tunneling. Positive ERI could also proxy for tunneling risk more broadly, including the risk of extraordinary related-party transactions, such as the mergers within *chaebol* groups studied by Bae, Kang and Kim (2002) or the intragroup loans to weaker members of Indian business groups documented by Gopalan, Nanda and Seru (2007).

4. RPT-to-Value Regression Results

We next assess the relationship between RPTs and firm value, and whether governance mediates this relationship. We first assess whether governance affects the level of RPTs that firms engage in, and find that it does not. We then study whether governance appears to affect the value of RPTs, and find evidence that it does.

[16] We find similar DiD results if we use a narrower upper size bound of 4 trillion won. With this upper bound, we have only 12 large positive-ERI firms and 7 negative ERI firms. The large-positive-ERI firms show an 0.11 (t = 1.23) rise in ln(q) relative to mid-sized firms, and a .077 (t = 1.34) rise in ln(Tobin's q), relative to large, negative-ERI firms. Lack of statistical significance is not surprising due to the small sample sizes. For all 28 large firms with assets from 2-4 trillion won, Black and Kim (2012) find a positive and statistically significant jump in *ln*(Tobin's q) from May-December 1999, relative to mid-sized firms (coefficient on large-firm dummy of 0.17, t = 2.31)).

4.1. Regression Model

In Table 5 and later regression tables, we use a standard firm effects model, with firm fixed effects (FE) or firm random effects (RE) (Wooldridge, 2010, § 10.2). The model for Table 6 is:

$$(\text{RPTs/sales})_{i,t} = \beta_0 + \beta_1 \times KCGI_{i,t} + \boldsymbol{\beta}_2 \times \mathbf{x}_{i,t} + g_t + f_i + \varepsilon_{i,t} \qquad (3)$$

Here $\mathbf{x}_{i,t}$ is a vector of covariates, which we assume to be exogenous, g_t are year dummies and f_i are firm effects. Exogeneity requires that current *KCGI* does not influence future **x**'s. The random effects model also assumes "strict exogeneity" of the firm effects; one form of this assumption is that the firm effects are uncorrelated with the covariates in all time periods: $\text{Cov}(f_i, \mathbf{x}_{i,t}) = 0 \ \forall \ t$. These exogeneity requirements are unlikely to be strictly true, but may be a reasonable approximation. First, firm characteristics only weakly predict corporate governance (Black, Jang and Kim, 2006b). Bhargava and Sargan (1983) suggest that exogeneity is more reasonable if one uses RE or FE to address unobserved heterogeneity, has a relatively "short" time dimension, and a time-persistent variable of interest. Both FE and RE will be inconsistent if there are omitted time-varying firm covariates that correlate with both *KCGI* and the outcome variable. The FE estimator is consistent even if the firm effects are correlated with *KCGI* and other covariates, but relies only on within-firm variation, and thus has less power.

Strict exogeneity is unlikely to be satisfied in governance studies. Thus, given sufficient time variation in governance, FE is ordinarily preferred. However, RE has greater power, due to ability to exploit both within-firm and across-firm variation. Also, the RE estimator converges to the FE estimator as the number of time periods grows. In Table 6, where we study whether KCGI predicts RPT volume, we use both RE and FE specifications, because RE provides a check on the null results we obtain with FE. Note that the RE estimator converges to the FE estimator as the "random effects λ" approaches 1. Let σ_ε and σ_f be the standard deviations of $\varepsilon_{i,t}$ and f_i, T be the number of periods. Then λ is defined as:

$$\lambda = 1 - \frac{\sigma_\varepsilon}{\sqrt{\sigma_\varepsilon^2 + T * \sigma_f^2}}$$

The median λ's in the RE specifications in Table 5 are around 0.7. Thus, the additional bias of RE estimates relative to FE, due to violation of the strict exogeneity assumption, should be limited. In later tables, we use the stronger FE specification.

Unlike our event study, we no longer have an explicit RD design. We do, however, continue to limit the sample to mid-sized firms with assets from 0.5-2 trillion won and large firms with assets from 2-4 trillion won. All regressions include extensive control variables (listed in Table 2), but generally suppress the coefficients on control variables to save space. We use standard errors with firm clusters to address the correlation between observations of each firm across time. To reduce the influence of outliers, we (i) use *ln*(Tobin's *q*) as the dependent variable because it is closer to normally distributed than non-logged *q*; and (ii) identify firm-year observations as outliers and drop them if a studentized residual from regressing *ln*(Tobin's *q*) on *KCGI* > ±1.96. In unreported results, we obtain similar results if we do not exclude outliers.

4.2. Evidence on Volume of RPTs

Governance can potentially affect RPTs in two distinct ways. First, better-governed firms could engage in fewer RPTs. Second, governance could affect the pricing of the RPTs that firms engage in. We consider first whether governance affects RPT volume. We begin by observing that RPTs are common, especially for *chaebol* firms. The mean (median) ratio of RPTs/sales for *chaebol* firms is 0.27 (0.20); compared to 0.10 (0.03) for non-*chaebol* firms (see Table 2, Panel B). In Table 5, regressions (1)-(2), which cover all mid-sized and large firms, we find no relationship between KCGI and RPT volume. The coefficients on KCGI are small, statistically insignificant, and positive. We obtain similar, null results in regressions (3)-(4), which are limited to *chaebol* firms.[17]

[17] Compare the cross-country study by Dahya, Dimitrov and McConnell (2007), who find a barely significant negative coefficient on proportion of independent directors in predicting an existence-of-RPTs dummy variable.

The lack of effect of governance on RPT volume could mean that governance is ineffective in controlling RPTs. But it could also mean that transacting with related parties is efficient, and that the harm to minority investors comes from RPT pricing, not from transactions with related parties as such. Note that improved pricing implies lower private benefits for insiders, but not higher overall firm value.

In Table 6, Panel A, regression (1), we assess whether RPT volume predicts Tobin's *q*, for mid-sized and large *chaebol* firms taken together. It does not; the coefficient on RPT/sales is insignificant but positive; where we would expect a negative sign if investors were generally averse to RPTs. In regression (2), we add *KCGI* as an additional independent variable; the coefficient on RPTs/sales remains positive and the coefficient on *KCGI* is positive but only marginally significant. These results are consistent with RPTs being efficient for firms, on the whole, so that the principal concern for these transactions is fairness of pricing, rather than the choice to deal with a related party rather than an unrelated party.

4.3. Evidence on RPTs, Governance, and Firm Value: *Chaebol* Firms

In the remaining columns of Table 6, Panel A, we provide indirect evidence that KCGI influences RPT pricing. In the remaining regressions, we add an interaction between *KCGI* and RPTs/sales. A representative specification, for regression (3), is:

$$\ln(\text{Tobin's } q)_{i,t} = \beta_0 + \beta_1 \times (RPTs/sales)_{i,t} + \beta_2 \times KCGI_{i,t} + \beta_3 \times (RPTs/sales)_{i,t} \times KCGI_{i,t} + \boldsymbol{\beta}_4 \times \mathbf{x}_{i,t} + g_t + f_i + \varepsilon_{i,t} \quad (4)$$

Our principal interest is in the coefficient β_3 on the interaction term.

Regressions (3)-(6) use an interaction between *KCGI* and RPTs/sales. Regressions (7)-(10) are similar except we replace *KCGI* with a *KCGI* dummy variable (=1 if *KCGI* > 40, the mean value for mid-sized and large *chaebol* firms). In both regressions (3) and (7), the coefficient on the interaction between *KCGI* (or *KCGI* dummy) and RPTs/sales is positive and marginally significant, while the coefficient on RPTs/sales (in regression (3), the estimate for a hypothetical firm with *KCGI* = 0; in regression (7), the estimate for firms with *KCGI* < 40) is negative, although

insignificant. These results provide initial, albeit mild evidence of a negative relationship between RPTs and Tobin's *q* for poorly governed firms, which is moderated by *KCGI*.

In regressions (4)-(5) and (8)-(9), we seek to sharpen these results, by limiting the sample to *chaebol* firms with positive mean ERI (over all sample years). Regressions (4) and (8) include all 65 positive-ERI firms; regressions (5) and (9) are further limited to the 41 positive-ERI firms which belong to the 18 largest *chaebol* groups (14 groups designated as such by Korea Fair Trade Commission throughout the sample period and four groups spun off from the original 14). The major *chaebol* groups are generally believed, with supporting anecdotal evidence, to engage in RPTs that benefit their controlling families. The sample for regressions (6) and (10) is *chaebol* firms with negative mean ERI.

In regression (4), for which the sample is all *chaebol* firms with positive mean ERI, the results from regression (3) strengthen slightly. The coefficient on the RPTs/sales * *KCGI* interaction term increases from 0.0046 to 0.0050 ($t = 1.69$)), the coefficient on RPTs/sales increases in magnitude from -0.133 to -0.175. The effect of limiting the sample to positive-*ERI* firms shows up more strongly in regression (8), where we replace continuous *KCGI* with *KCGI* dummy. The coefficient on *KCGI* dummy is positive and statistically significant, at 0.176 ($t = 2.45$).

In regressions (5) and (9), which are limited to positive ERI firms within the top-18 *chaebol* groups, our results strengthen further. In regression (5), the coefficient on RPTs/sales * *KCGI* increases again, to 0.0068, and becomes statistically significant ($t = 2.41$); the negative coefficient on RPTs/sales also increases in magnitude to -0.194. In regression (9), the coefficient on *KCGI* dummy * RPTs/sales rises to 0.302 ($t = 4.06$). In regression (5), the -0.194 coefficient on RPTs/sales implies that a 10% increase in RPTs/sales predicts a 1.94% drop in Tobin's *q* for a hypothetical firm with *KCGI* = 0. This negative coefficient on RPTs/sales, together with the +0.0068 coefficient on RPTs/sales interacted with *KCGI* together imply that the predicted effect of RPTs/sales is neutral at about *KCGI* = 29, and positive for higher levels of *KCGI*.[18]

[18] In unreported results, we find consistent results if we use the full sample of mid-sized and large *chaebol* firms and interact *KCGI* dummy, RPTs/Sales, and ERI dummy (= 1 if ERI > 0, 0 otherwise). The coefficient on this

For negative-ERI firms, in contrast, if we take regressions (6) and (10) together, there is no significant evidence that RPTs are adverse to firm value or that *KCGI* moderates any impact of RPTs on firm value. This is clearer in regression (10). Here, the coefficient on RPTs/sales is positive, while the coefficient on *KCGI* dummy * RPTs/sales is small and insignificant

Taken together, the results for *chaebol* firms from Table 6, Panel A provide evidence consistent with our event study results: Investors – when they have the information on RPTs and RPT counterparties – appear to assess the impact of RPTs on firm market value taking into account the specific identities of the firm's RPT counterparties, which predict whether the pricing of the RPTs is likely to be adverse to the firm. For positive-ERI firms, which are at risk of cash flow tunneling through RPTs, investors treat RPTs as harmful to value and expect better governance to mitigate transfer pricing risk.

In Table 6, Panel B, we replace related-party transactions (RPTs)/sales with separate variables for related-party purchases (RPPs/sales) and sales (RPSs/sales). The structure of Panel B is otherwise similar to Panel A. We provide the *p*-value from an *F*-test for the combined significance of *KCGI* (or *KCGI* dummy) interacted with each of RPPs/sales and RPSs/sales. In big picture, the interaction between *KCGI* (or *KCGI* dummy) and RPTs is stronger for purchases from related parties than for sales to related parties. The coefficients on the interaction term for RPPs/sales are significant or marginally significant in regressions (3)-(4) and (7)-(8). In contrast, the coefficients on the interactions of KCGI and KCGI dummy with RPSs/sales are always insignificant and occasionally negative. For example, in regression (4), the coefficient on RPPs/sales is negative at -0.391 and the *KCGI* * RPPs/sales interaction term is positive and significant at +0.0149 ($t = 2.12$), while the corresponding coefficients for RPSs/sales are small and insignificant.

The stronger evidence for related-party purchases is consistent with anecdotal evidence in Korea. Public *chaebol* firms, with small family economic ownership, often purchase goods and

triple interaction is positive and marginally significant ($t = 1.94$), indicating that investors expect governance to have a stronger moderating effect on RPT pricing for firms with positive ERI.

services from private firms with larger, often 100% family ownership. These transactions provide an incentive for cash-flow tunneling and opportunity for the public firm to pay above-market prices, perhaps especially for services, since a fair price is often hard to determine, or provide other favorable terms to the private firm. For example, Hyundai Motors gives exclusive car delivery (including exporting) rights to an affiliate called Glovis, wholly owned by the controlling family of Hyundai Group, and pays a logistics fee to Glovis.[19] In contrast, the products of the public firms may have a more readily ascertainable market value, which reduces the opportunity to use the price term to benefit the controllers. However, the greater risk of unfair pricing in purchases is a tendency, not a hard rule.

In Panel C, we investigate which subindices contribute to the overall effect of *KCGI* on the value of RPTs found in Panels A and B. We focus on positive-ERI *chaebol* firms, and replace *KCGI* and *KCGI* dummy, and their interactions with RPTs/sales, with separate variables for Board Structure Subindex, Disclosure Subindex, and the other subindices of *KCGI* combined (or, in regressions (7)-(10), above-mean dummy variables) and the interactions of these three variables with RPTs/sales.[20] The principal driver of the overall interaction between *KCGI* and RPTs/sales is Board Structure Subindex. For this subindex, the interaction coefficient is positive and statistically significant for positive-ERI firms within the top-18 *chaebol* in regression (2), using the full Subindex. The interaction coefficient is again positive and significant in regression (4), where we use above-mean Board Structure dummy. However, in regressions (1) and (3), for which the sample is all positive-ERI *chaebol* firms we do not find significant interactions.

Our results can be compared to Bertrand, Mehta, and Mullainathan (2002), who find evidence for Indian firms of tunneling through non-operating cash flows, but no evidence that tunneling affects operating profits. In contrast, we find evidence consistent with controllers

[19] See A Scratch on Hyundai's Paint Job*, Bloomberg BusinessWeek*, April 17, 2006.

[20] We use means rather than medians to create subindex dummy variables because the median value of Board Structure Subindex is 0. The mean values are 4.2, 4.8, and 31.8 respectively for Board Structure Subindex, Disclosure Subindex, and the sum of other subindices. We obtain similar results if we use breakpoints for the subindex dummy variables at the 67th percentile for each subindex.

engaging in cash-flow tunneling through routine RPTs, which affect operating profits. Unlike Bertrand, Mehta, and Mullainathan, we find no evidence of tunneling through non-operating cash flows (see section 5.2 below). See also Atanasov et al. (2010), who find evidence that when controllers of Bulgarian firms face limits on equity tunneling, operating profits drop, consistent with these controllers engaging in more cash-flow tunneling.

4.4. RPTs and Firm Value: Evidence from both *Chaebol* and *Non-Chaebol* Firms

In the previous section, we studied whether governance mediates the effect of RPTs on value for *chaebol* firms, for which we could compute ERI. In this section, we extend this analysis to all mid-sized and large firms within our size band (0.5 to 4 trillion won in assets), both *chaebol* and non-*chaebol*. We assess whether *KCGI* mediates the relationship between RPTs and Tobin's *q* for this broader sample. On the whole, our results are consistent with, but somewhat weaker than, the results for *chaebol* firms in Table 6. Weaker results are expected because we can no longer focus on a subset of positive ERI firms, for whom investors have reason to expect tunneling through RPTs.

In Table 7, Regressions (1)-(3) are similar to the corresponding regressions in Table 6, Panel A. Table 7 also includes the full set of control variables that we use in other regressions, but suppress elsewhere for space reasons. In regression (1), the coefficient on RPTs/sales is negative but insignificant; the coefficient is similar when we add *KCGI* in regression (2). In regression (3) we add the core variable of interest -- an interaction between *KCGI* and RPTs/sales. The interaction term is positive and marginally significant, while the coefficient on RPTs/ sales (which now indicates the predicted impact of RPTs/sales for a hypothetical firm with *KCGI* = 0) becomes negative. These results -- a negative relationship between RPTs/sales and Tobin's *q* for low-*KCGI* firms, and mediation of this negative impact by *KCGI* -- are consistent with, although weaker than, those for *chaebol* firms in Table 6, Panel A, regression (3). In regression (4), we replace *KCGI* with dummy (=1 if *KCGI* > 40). The interaction between RPTs/sales and KCGI dummy turns positive and significant. In unreported regressions using subindices instead of

KCGI, the interactions between RPTs/sales and Board Structure and Disclosure Subindex dummies are positive, but, in most specifications, not statistically significant.

4.5. Endogeneity Concerns

The specifications in Tables 6 and 7 are not true causal specifications. They rely instead on firm fixed effects and extensive control variables. But they are strong specifications nonetheless. In particular, one factor that investors are likely to consider, in assessing the risk of tunneling through RPTs is the identity of the controller of a *chaebol* group. Our FE specification holds that identity constant, because Korean firms almost never move from one group to another. Our narrow firm size band, with a legal shock to governance in the middle of the band, ensures that the sample firms are similar, in ways other than governance.

For Table 6, we can say a bit more. In Appendix Table 1, we assess covariate balance – do the mean values for positive and negative ERI firms differ for observed control variables? The two groups are very similar for most control variables. Sole ownership is the principal exception. Positive-ERI firms have lower sole ownership, but this is expected, due to how we define ERI. Positive ERI firms also have higher foreign ownership, which is likely related to lower sole ownership, since the shares not held by the controller have to be held by someone; and faster sales growth. To explain our results, an unobserved covariate would need to both (i) differ substantially between positive-ERI and negative-ERI firms, and also (ii) predict the outcome variable (in Table 6, Tobin's *q*). The existence of reasonable balance for observed covariates makes it less likely that an unobserved covariate will be substantially different between positive-ERI and negative-ERI firms.[21]

5. RPTs, Governance, and Profitability

The RPT results in Tables 5 and 6 provide evidence that investors *believe* governance will moderate tunneling through RPTs, but not direct evidence that governance does so. We assess in

[21] One possibility that we cannot effectively address: *KCGI* has a strong time trend. If an unobserved variable predicts Tobin's q and also has a strong time trend, we could wrongly attribute that effect to *KCGI.*

this part the relationship between RPTs and profitability, and whether governance mediates that relationship.

5.1. RPTs and Firm Profitability

In Table 8, we investigate whether firms with higher levels of RPTs are less profitable, and whether governance mediates that relationship. We present results with KCGI dummy as our governance measure, and two measures of profitability: EBIT/sales and net income/sales. Regressions (1)-(5) use EBIT/sales as the measure of profitability; regressions (6)-(10) use similar specifications with net income/sales as the dependent variable. Results with continuous KCGI, or with assets instead of sales in the denominator, are consistent but weaker.

In regressions (1) and (6), we present a simple regression of the profitability measure on RPTs/sales plus control variables, for a sample of all chaebol firms within our 0.5-4 trillion won size bounds. The RPTs/sales variable takes a negative coefficient in both regressions, statistically significant with net income/sales as the measure of profitability and marginally significant with EBIT/sales as the measure of profitability. However, one should be cautious treating this relationship as causal. Firms that engage in more RPTs may do so for business reasons, and could be less profitable even if they conducted similar transactions with unrelated firms.

In the remaining regressions, we add the key variable of interest, the interaction between RPTs/sales and KCGI dummy. A representative specification, for regression (2), is:

$$\begin{aligned}(\text{EBIT/sales})_{i,t} = {} & \beta_0 + \beta_1 \times (RPTs/sales)_{i,t} + \beta_2 \times (KCGI\ \text{dum})_{i,t} \\ & + \beta_3 \times (RPTs/sales)_{i,t} \times (KCGI\ \text{dum})_{i,t} + \boldsymbol{\beta}_4 \times \mathbf{x}_{i,t} + g_t + f_i + \varepsilon_{i,t}\end{aligned} \tag{5}$$

The interaction term is positive and marginally significant in both regressions (2) and (7). In the remaining regressions, we investigate the coefficient on this interaction term for different subsamples. In regressions (3) and (8), we limit the sample to *chaebol* firms with mean ERI > 0. The coefficients on the interaction terms become positive and statistically significant, although not strongly so. In regressions (4) and (9), we limit the sample to positive ERI firms within the top 18 *chaebol* groups. The coefficients are similar but statistically a bit weaker, due to the smaller

sample size. In both sets of regressions, the negative direct coefficient on RPTs/sales and the positive coefficient on the interaction term are similar in magnitude. Thus, RPTs/sales predict lower profitability for firms with *KCGI* dummy = 0, , but are roughly neutral for better-governed positive-ERI firms, with *KCGI* dummy = 1. Finally, in regressions (5) and (10), we limit the sample to *chaebol* firms with mean ERI < 0. For these firms, the interaction term is still positive, but is much smaller in magnitude and is not close to being statistically significant.

These results for profitability are consistent with the Tobin's *q* regressions in Table 6. They provide modest evidence that governance in fact improves the profitability of RPTs.

5.2. Sensitivity of Firm Profitability to Industry Profitability

We turn next to a second source of evidence on whether RPTs affect profitability, and whether governance mediates that effect. We adapt, for our dataset, the approach of Bertrand, Mehta, and Mullainathan (2002), who study Indian business groups. Their idea is to measure the responsiveness of *firm* profitability to shocks to *industry* profitability. Low responsiveness suggests that insiders extract more (fewer) pre-tunneling profits as the firm does better (worse). Bertrand et al. report evidence that firm responsiveness to industry shocks is associated with measures of both opportunity to tunnel and of incentives to tunnel (their proxy for incentives is the cash flow rights of the control group). Siegel and Choudhury (2012) fail to replicate their results for India, but the approach is useful even if the Siegel and Choudhury reanalysis is correct for Indian firms.[22] Bae, Cheon and Kang (2008) find a positive correlation between share returns of firms within Korean *chaebol* groups, suggesting that investors expect a combination of tunneling of profits away from more profitable firms and propping of less profitable firms.

[22] Siegel and Choudhury raise four methodological concerns with the Bertrand, Mehta and Mullainathan approach. Two involve missing data, which is not a concern for us. The other two concerns involve heteroskedasticity and correlated observations within firm over time; we address these concerns by using standard errors clustered on firm. Siegel and Choudhury also argue that different firms will respond differently to industry shocks. These differences in response will add noise to an effort to use a shock to industry profitability to predict firm profitability, and thus make it harder to find a significant effect of governance. They do not imply that the results we find are spurious. We have our own concerns with their methods. One involves their use of EBITDA, not scaled to firm size, as the key outcome variable. We instead use EBITDA/assets, which is a more conventional measure of profitability, less prone to outliers. A second involves their use of only minimal control variables. We use a much more extensive set of controls.

Our RPT results above suggest that: (i) RPTs tend to be adverse to value and profitability; (ii) governance reduces these adverse effects; and (iii) the mediating effect of *KCGI* is important principally for firms with mean ERI > 0. By analogy, we hypothesize that: (i) the sensitivity of firm profitability to industry profitability should rise with governance; and (ii) among *chaebol* firms, where we can measure ERI, this effect should appear principally, or more strongly, for firms with mean ERI > 0.

Table 9 presents our results. We present results for all Korean public firms. If we limit the sample to firms with assets from 0.5-4 trillion won, as in prior regressions, coefficients are similar but standard errors increase.[23] In regression (1), we confirm that industry profitability, measured by EBITDA/assets, predicts firm profitability. We estimate industry profitability for firm *k* in 4-digit industry *i* as [(EBITDA summed across all other firms in industry *i*)/(assets summed across these firms)]. The coefficient on industry EBITDA/assets is 0.203 ($t = 6.53$). If we more closely track the Bertrand et al. specification by using unscaled firm EBITDA, and defining industry EBITDA as industry EBITDA/assets * (firm *k*'s assets)), the coefficient on industry EBITDA is 0.67. A coefficient of less than 1 suggests that firms on average face higher tunneling as profits rise, and reduced tunneling or even propping as profits fall.

In Regression (2), we add *KCGI* and its interaction with industry EBITDA/assets. The coefficient on the interaction term is positive but not statistically significant. A positive coefficient implies that firm profitability responds more strongly to shocks to industry profitability for better-governed firms. This is consistent with governance reducing cash-flow tunneling.

In Regression (3), we replace *KCGI* and the *KCGI* * industry profitability interaction term with each subindex included both separately and interacted with industry profitability. Board Structure Subindex has a positive and significant interaction with industry profitability. The 0.013 coefficient on the interaction term implies that a worst-to-best change in Board Structure (20

[23] With the size-limited sample, the coefficients on the interaction terms in regressions (2), (3), (5), and (6) are: 0.0024 ($t = 0.87$) in regression (2) vs. 0.0022 ($t = 1.36$) reported in Table 9; 0.0107 (t = 1.03) for Board Structure * Industry EBITDA/assets vs. 0.0127 ($t = 2.12$) reported; 0.0066 ($t = 2.16$) in regression (5) vs. 0.0065 (t = 2.38) reported; 0.0038 ($t = 1.43$) in regression (6), vs. 0.0039 ($t = 2.55$) reported.

points) increases the sensitivity of firm profitability to industry profitability by 0.26, which is economically large relative to the overall sensitivity of 0.20 shown in regression (1). The interactions with other subindices are insignificant.

If we more closely replicate Bertrand et al. by using unscaled EBITDA or EBIT as dependent variables, with corresponding industry measures, results are stronger but we have less confidence in this specification because it is prone to outliers and gives greater weight to larger firms. We also obtain stronger results with the minimal control variables used by Bertrand, Mehta, and Mullainathan (firm age and *ln*(assets)). In unreported robustness checks, we obtain similar but weaker results if we use EBIT (rather than EBITDA)/assets as the dependent variable.

In regressions (4) and (5), we limit the sample to *chaebol* firms. In regression (4), the sample is firms with mean ERI < 0, for whom tunneling is a smaller concern. The coefficient on the interaction between *KCGI* and industry profitability is positive but not significant. In regression (5), for *chaebol* firms with mean ERI > 0, the coefficient on *KCGI* * industry profitability is 0.0065 and is statistically significant (t = 2.38), although the difference in coefficients between the two groups is not significant. The higher coefficients on KCGI * industry EBITDA/assets for *chaebol* firms in regressions (4)-(5), compared to the full sample estimate in regression (2), is consistent with Table 6, in which *chaebol* firms drive the relationship between RPTs and firm value.

In regressions (6) and (7), we assess whether governance affects both tunneling of value out of profitable firms, and reverse tunneling (propping) for unprofitable firms, by dividing the sample into profitable firms (EBITDA > 0) and unprofitable firms (EBITDA < 0). In regression (6), for profitable firms, the interaction between *KCGI* and industry profitability is positive and significant, consistent with better governance limiting tunneling out of these firms. In contrast, in regression (7), for unprofitable firms, the coefficient on *KCGI* * industry profitability is insignificant and indeed negative. A non-result for unprofitable firms could arise if either (i) governance affects tunneling but not propping; or (ii) there is limited propping of unprofitable

firms to begin with, because then there is nothing for *KCGI* to mediate. However, any conclusions are tentative, because the small number of unprofitable firms limits statistical power.

Tunneling through routine RPTs should affect operating profits, and we so find. Bertrand, Mehta, and Mullainathan (2004) report that controllers of Indian firms engage in tunneling through *non-operating* cash flows – non-operating profit *falls* as industry profitability rises. We find no evidence of this. In regressions similar to Table 9, regression (1), with non-operating profit/assets as the dependent variable, the coefficient on industry EBITDA/assets is positive and marginally significant for all firms, and similar in magnitude but insignificant for *chaebol* firms. In regressions similar to Table 9, regression (2), we find no evidence that governance mediates the already weak relationship between industry profitability and non-operating profit.

5.3. Anecdotal Evidence

We have provided a variety of sources of evidence that investors expect governance to add value for positive ERI firms, for whom tunneling is more likely, that governance improvements in fact do so, and that board structure, in particular, mediates the relationship between RPTs and firm value. We turn here to anecdotal evidence: Is it plausible that outside directors, either on the full board or the audit committee, can know enough to police the fairness of RPTs? Our judgment is a cautious yes. First, in some cases, we expect that controllers will offer fairer terms to controlled firms, in order to avoid the need to obtain approval for a suspect transaction from outside directors,. Second, we believe that in some cases, outside directors will have both the will and the ability to scrutinize RPTs. We offer here some anecdotal evidence.

First, one of us (Black) was a director of Kookmin Bank during 2003-2005, during our sample period. He can report that during that period, the Kookmin board, which contained a majority of independent directors, closely scrutinized RPTs.

Second, the Korean coauthors of this paper have extensive experience with challenging and reporting on self-dealing by *chaebol* controllers. One of us (Jang) was a core member of the leading Korean shareholder activist group, People's Solidarity for Participatory Democracy (PSPD), during much of the period we study and chair of PSPD's Participatory Economy

Committee. Profs. Jang and Kim are founding members of the Korean Center for Good Corporate Governance (created 2001), the Solidarity for Economic Reform non-governmental organization (SER, created 2006), and an SER-affiliated think tank, the Economic Reform Research Institute (ERRI, created in 2009). ERRI publishes reports on, among other things, tunneling by Korea firms; Prof. Kim is the current head of ERRI. Rho (2007) provides extensive information on shareholder activism in Korea, including Prof. Jang's activities. Prof. Park is a founding member and the current director of the Korean Corporate Governance Service. The results we report are consistent with their personal experience.

Third, we offer one anecdote for which good public sources exist, involving SK Telecom. SK Telecom was the first Korean company to grant outside directors approval power for RPTs. A newspaper article reports that in 1998, of 12 RPTs brought to the board, 4 were approved as is, 6 were approved after the terms were revised, and two were disapproved.[24] Of the firm's outside directors, one, Sang-Koo Nam, was jointly nominated by PSPD and an activist foreign fund which held a large stake in SK Telecom (Tiger Management); a second, Dae-Sik Kim, was nominated by SK's outside director nominating committee, but approved by PSPD and Tiger. There was also public disclosure of a 2005 transaction, in which the outside directors disapproved a service contract between SK Telecom and another SK-group company, 55% owned by SK's controlling family. The board later approved the transaction on revised terms, over the objections of directors Nam and Kim.[25]

6. Conclusion

In prior work, we develop a broad Korean corporate governance index (*KCGI*), and report evidence that 1999 legal reforms to the board structure of large firms causally predict higher firm market value (Black and Kim, 2012). We study here the "channels" question: *Why* does

[24] Kee-Dong Lee, Five-years after the introduction of outside directors: the success story of SK Telecom, Maeil Business Newspaper (March 28, 2002) (in Korean).

[25] Jin-Woo Lee, "Lively discussions at board meetings and increased influence of outside directors, eDaily, Oct. 25, 2006 (in Korean)

governance increases the market value of Korea firms, and for which firms? We find evidence for a "tunneling channel." More specifically, we find evidence consistent with a reduction in ″cash-flow tunneling." The effect of governance on tunneling is concentrated in *chaebol* firms with positive scores on an Expropriation Risk Index (ERI), which measures controllers' incentive to tunnel.

We find that positive-ERI firms subject to the reform earn larger abnormal returns than the negative-ERI firms during the period covering the main reform events. We then show that better governance, measured by *KCGI* and in particular by Board Structure Subindex, reduces value loss from RPTs in positive-ERI firms, but not in negative-ERI firms. We also find evidence that (i) *KCGI* mediates the impact of RPTs on firm profitability; and (ii) *KCGI* increases the sensitivity of firm profitability to industry profitability, which suggests reduced tunneling and propping, and does so more strongly for positive-ERI firms.

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Figure 1: Board Structure Index and Asset Size

The scatter plots show the relationship between *ln*(assets in billion won) and Board Structure Index (0~20) from 1998–2004 for mid-sized firms (assets from 0.5-2 trillion won) and large firms (assets > 2 trillion won). The 1999 reforms require large firms to have a minimum Board Structure Index value ≥ 11.7 (5 points for 50% outside directors; 6.7 points for audit and outside director nomination committees). Audit committee is required in 2000; 50% outside directors and outside director nominating committee in 2001. Sample excludes banks and SOEs. Vertical line indicates 2 trillion won; horizontal line indicates minimum Index value for large firms. Firm size is measured separately for each year. Number of firms with assets from 0.5-2 trillion won varies by year from 84 to 107; number of firms with assets > 2 trillion won varies from 53-61.

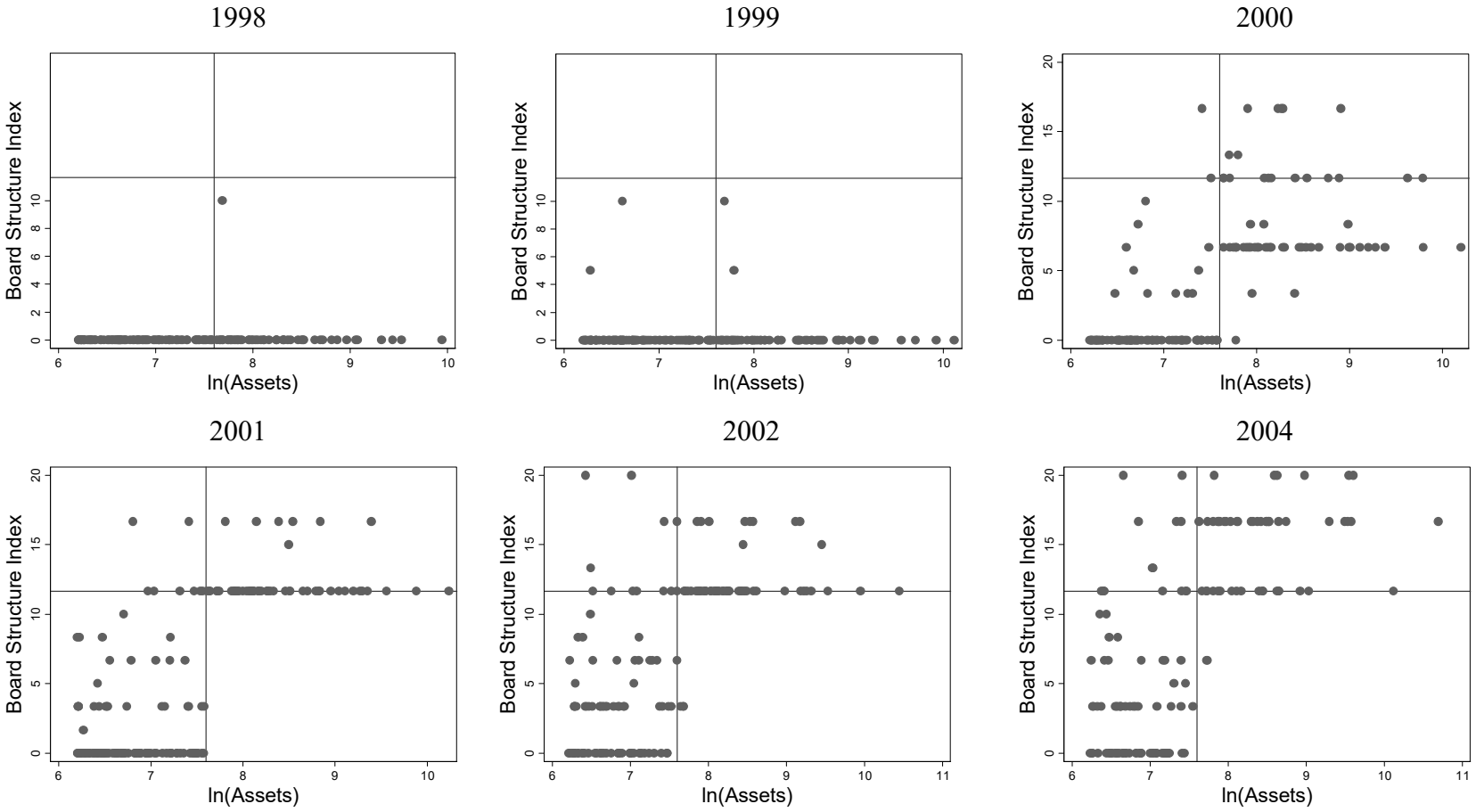
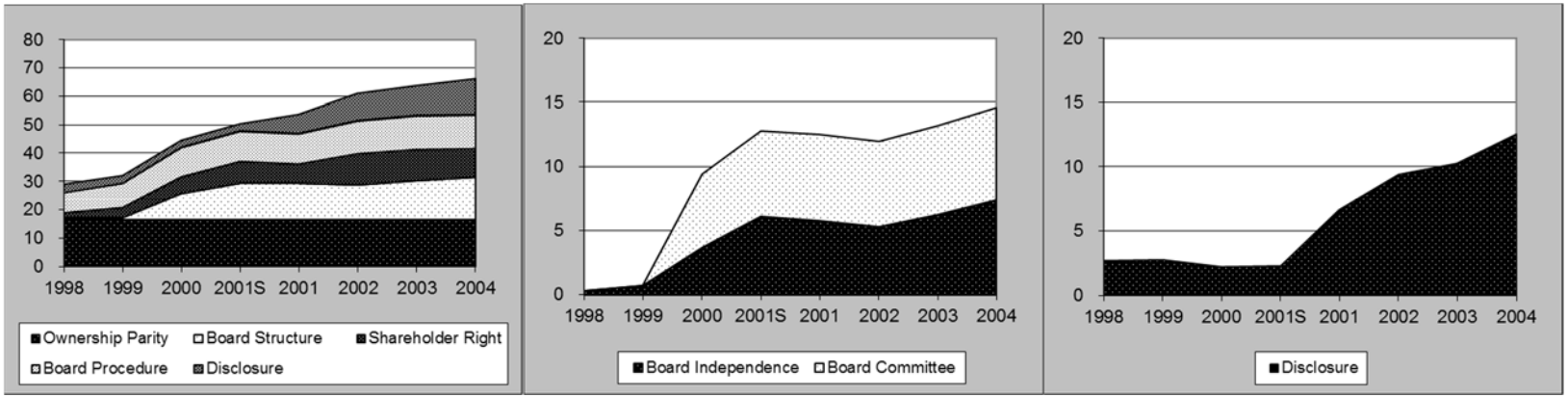


Figure 2: Change in *KCGI* and its Subindices over Time

The left set of charts show mean values of *KCGI* and each component index from 1998 to 2004, separately for large firms (2 trillion won < assets ≤ 4 trillion won as of year-end 2000) and mid-sized firms (0.5 trillion won < assets ≤ 2 trillion won as of year-end 2000). The middle set of charts provide an expanded view of the Board Independence and Board Committee sub-subindices, which together comprise Board Structure Subindex. The right set of charts provide an expanded view of the Disclosure subindex. Sample includes banks and SOEs. Number of mid-sized firms varies by year from 85 to 111; number of large firms varies from 25 to 34.

Large Firms (2 < assets ≤ 4 trillion won)



Mid-sized Firms (0.5 < assets ≤ 2 trillion won)

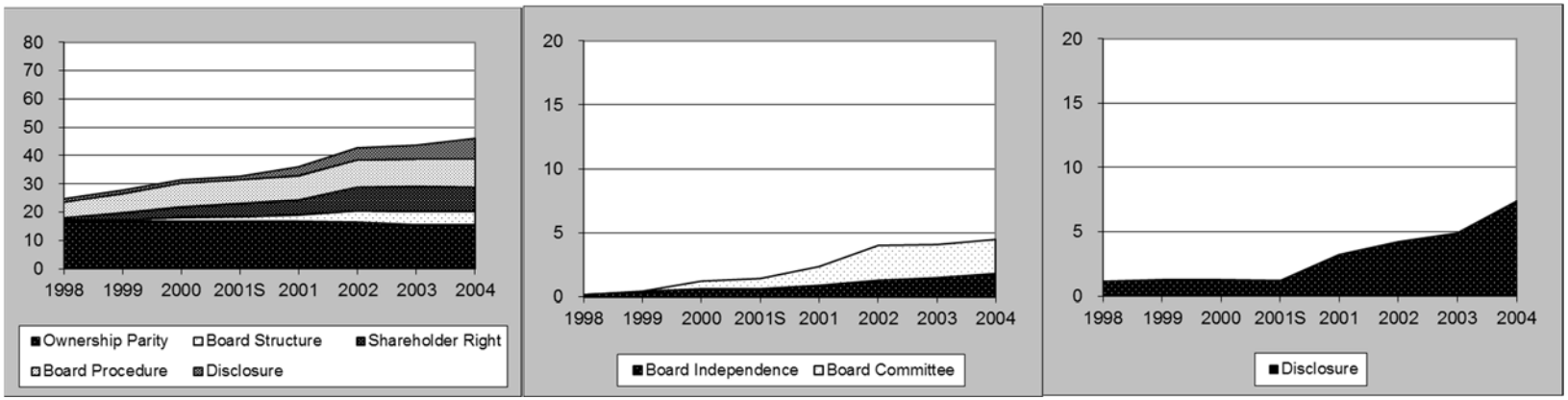
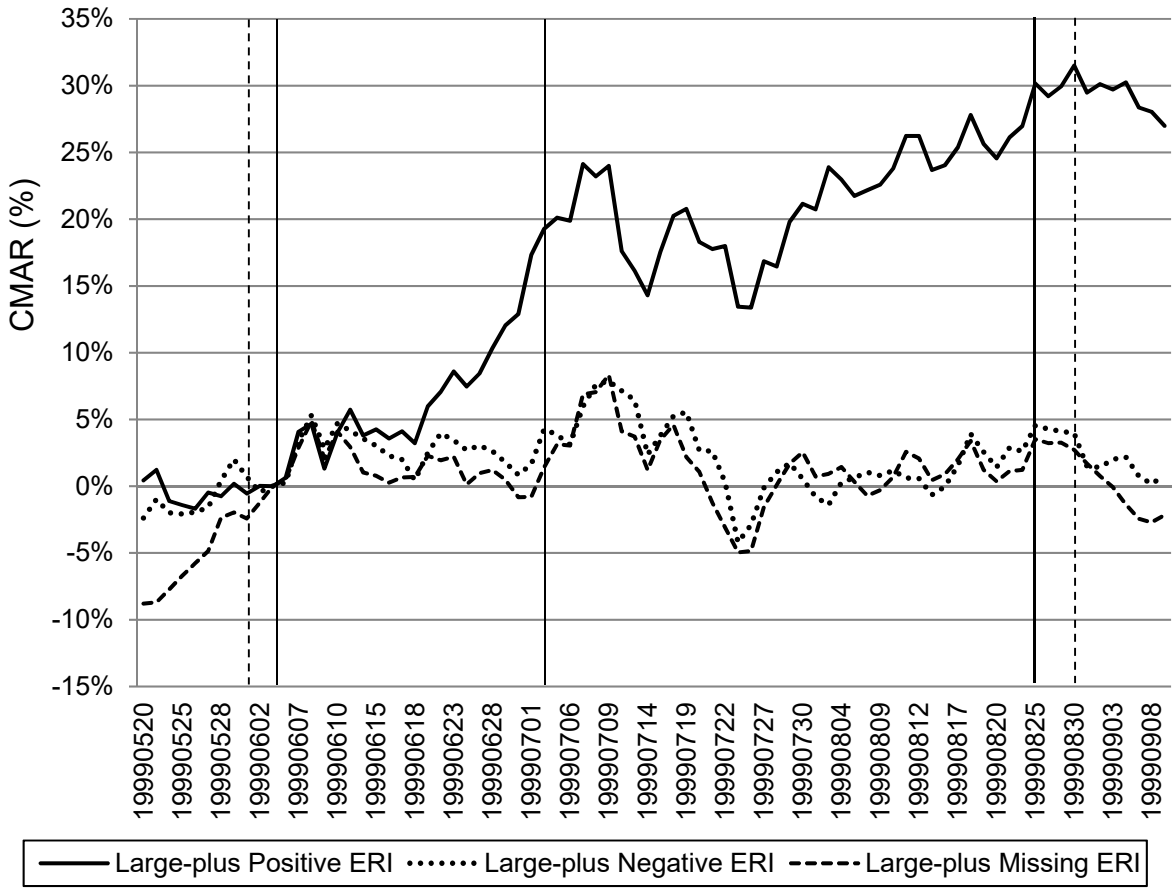


Figure 3: Event Period Returns to Large Plus Firms

Panel A. Short Window (May 20 − September 9, 1999)

Figure shows cumulative market adjusted returns (CMARs) over May 20 - September 9, 1999 (ten trading days before and after *Events* 1 and 3), to three treatment groups of large-plus firms (1 trillion won < assets < 4 trillion won): (1) *chaebol* firms with mean Expropriation Risk Index (ERI, defined in text, over 1998-1999) > 0; (2) *chaebol* firms with mean ERI < 0; and (3) firms with missing ERI (mostly non-*chaebol*), relative to equally weighted index of mid-sized firms (0.5 trillion won < assets < 1 trillion won). Principal event period is June 1 – August 30, 1999. We drop outlier observations for which a studentized residual from regressing CMAR on large-plus positive ERI firm dummy, large-plus negative ERI dummy, and large-plus missing ERI dummy > ±1.96. Solid Vertical lines indicate principal event dates (June 3, July 2, August 25); dotted vertical lines indicate start and end of event period (running from Event 1 *minus* 2 day to Event 3 *plus* 3 days). Assets are measured at year-end 1998. Sample excludes banks and SOEs. Sample of large-plus firms includes 19 positive ERI firms, 16 negative-ERI firms, and 19 non-*chaebol* firms with missing ERI. Control group is 47 mid-sized firms.



Panel B. Longer Window (March 31 to Oct. 31, 1999)

Figure shows cumulative market adjusted returns (CMARs) over March 31 – October 31, 1999 (two months before and after *Events* 1 and 3), to three treatment groups of large-plus firms (1 trillion won < asserts < 4 trillion won): (1) *chaebol* firms with mean Expropriation Risk Index (ERI, defined in text, over 1998-1999) > 0; (2*) chaebol* firms with mean ERI < 0; and (3) firms with missing ERI (mostly non-*chaebol*), relative to equally weighted index of mid-sized firms (0.5 trillion won < assets < 1 trillion won). Principal event period is June 1 – August 30, 1999. We drop outlier observations for which studentized residual obtained by regressing CMAR on large-plus positive ERI firm dummy, large-plus negative ERI dummy, and large-plus missing ERI dummy > ±1.96. Solid Vertical lines indicate principal event dates (June 3, July 2, August 25). Assets are measured at year-end 1998. Sample is same as panel A.

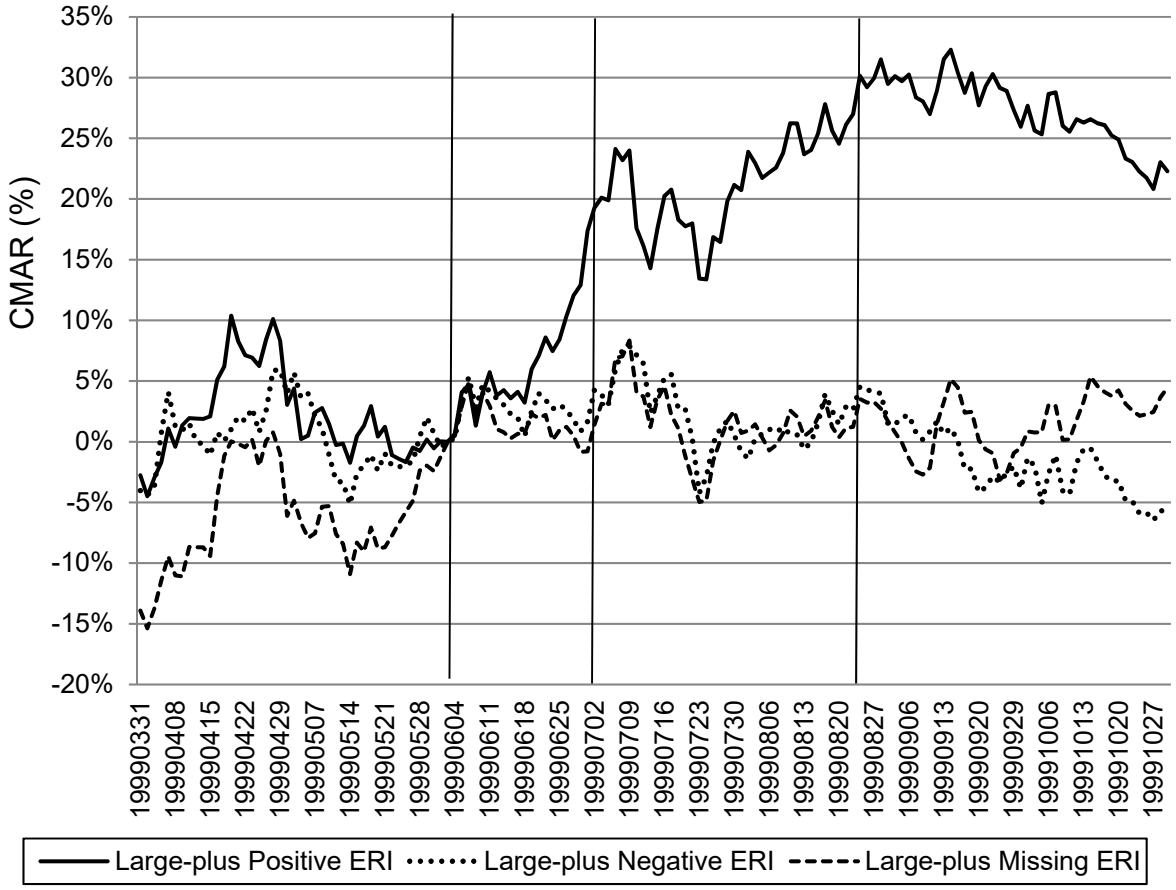


Table 1: Summary Statistics for *KCGI* and its Subindices

Panel A

This table presents number of observations, sample mean, and other statistics for *KCGI* and its subindices, by year, for an unbalanced panel. Large firms have book asset value from 2-4 trillion won as of year-end 2000. Mid-sized firms range from 0.5-2 trillion won as of year-end 2000. Sample includes banks and SOEs.

Index	Year	Obs.	Mean			Median	Std. Dev.	Min	Max
			All	Large	Mid				
KCGI	1998	484	24.23	28.22	24.44	23.33	6.72	10.62	64.10
	2000	535	31.54	44.28	31.37	29.18	10.47	7.76	84.80
	2002	466	43.05	60.80	42.68	39.73	13.64	14.00	97.14
	2004	512	44.89	66.66	45.94	42.03	13.74	20.10	98.82
Board Structure	1998	511	0.25	0.31	0.18	0.00	1.54	0.00	10.00
	2004	513	3.81	14.33	3.33	0.00	5.83	0.00	20.00
Ownership Parity	1998	516	17.63	16.93	16.83	18.89	2.97	3.63	20.00
	2004	520	17.03	17.18	15.77	18.69	3.60	4.20	20.00
Disclosure	1998	523	4.56	2.73	1.10	4.44	2.82	0.00	17.50
	2004	521	9.10	12.56	7.36	9.09	2.99	1.43	18.82
Board Procedure	1998	535	1.17	7.19	5.55	0.00	3.15	0.00	20.00
	2004	521	6.30	11.76	9.89	6.67	5.87	0.00	20.00
Shareholder Rights	1998	516	0.82	1.62	1.04	0.00	2.89	0.00	20.00
	2004	521	8.65	10.26	8.60	6.67	3.23	5.00	20.00

Panel B

This table presents Pearson correlation coefficients for *KCGI* and its subindices for firms with book asset value between 0.5-4 trillion won. *, **, and *** indicate significance at 10%, 5%, and 1% levels

	KCGI	Board Structure	Ownership Parity	Disclosure	Board Procedure	Shareholder Rights
KCGI	1.00					
Board Structure	0.76***	1.00				
Ownership Parity	0.21***	0.01	1.00			
Disclosure	0.70***	0.30***	-0.02	1.00		
Board Procedure	0.66***	0.45***	-0.05	0.34***	1.00	
Shareholder Rights	0.72***	0.40***	-0.08*	0.42***	0.42***	1.00

Table 2: Other Variables

Definition and summary statistics for other variables used in this paper. *Panel A* defines each variable and *Panel B* provides summary statistics for firms with book asset value between 0.5-4 trillion won, including banks and SOEs. Book and market values are measured at year end, except that market values for mid-2001 are measured on the last day of June. Firms with missing data for R&D/sales, advertising/sales, or exports/sales are assumed to have 0 values.

Panel A: Variable Definitions

Variables	Descriptions
Principal Variables	
Tobin's *q*	Estimated as [(book value of debt + preferred stock) + market value of common stock]/[book value of assets]. Book values are measured at year-end.
RPTs/sales	Sum of sales to and purchases from related-parties divided by total sales; winsorized at 99%
RPSs/sales	Sales to related-parties divided by total sales; winsorized at 99%
RPPs/sales	Purchases from related-parties divided by total sales; winsorized at 99%
Cash flow rights	Direct plus indirect fractional ownership of firm's shares by members of control group. Available only for *chaebol* firms.
ERI (Expropriation Risk Index)	Defined in the text. Available only for *chaebol* firms.
Industry EBITDA/assets	(EBITDA summed across all other firms in the same 4-digit industry)/(assets summed across all other firms in the same 4-digit industry).
Control Variables	
Assets	Book asset value in billion won. Use *ln*(assets) in regressions
Years Listed	Number of years since original listing on Korea Stock Exchange. Use *ln*(Years Listed) in regressions
Leverage	(Book value of debt)/ (Market value of common stock), winsorized at 99%
Sales Growth (5 yrs)	Geometric average sales growth rate during the past 5 fiscal years (or available period if less), winsorized at 1%/99%. If fiscal year changes, we keep only years which cover full 12 months
R&D/Sales	Ratio of research and development (*R&D*) expense to sales.
Advertising/Sales	Ratio of advertising expense to sales.
Exports/Sales	Ratio of export revenue to sales
PPE/Sales	Ratio of property, plant, and equipment to sales.
PPE/Sales Squared	Squared term of PPE/Sales
Capex/PPE	Ratio of capital expenditures to PPE
EBIT/Sales	Ratio of earnings before interest and taxes to sales, winsorized at 1%/99%.
EBIT/Assets	Ratio of earnings before interest and taxes to assets, winsorized at 1%/99%
Market Share	Firm's share of total sales by all firms in the same 4-digit industry listed on KSE.
Share Turnover	[Common shares traded during the year / publicly held shares, winsorized at 99%. The denominator is defined as [common shares outstanding x (1 – total affiliated ownership)]
Foreign Ownership	[common shares held by foreign investors / common shares outstanding]
Sole Ownership	[Number of common shares held by group controlling shareholder and family members / Number of common shares outstanding]
Sole Ownership Squared	Squared term of sole ownership
Top 30 c*haebol* Dummy	1 if a member of one of the top-30 business groups (based on total group assets) as of April of each year as identified by Korea Fair Trade Commission; 0 otherwise. We treat former state-owned enterprises as non-*chaebol* firms.
Level 1 (2/3) ADR Dummy	1 if firm has level 1 (level 2 or 3) American Depository Receipts (ADRs); 0 otherwise.
MSCI Index Dummy	1 if firm is in Morgan Stanley Capital International Index; 0 otherwise.

Panel B: Summary Statistics for Selected Variables (for firms with assets between 0.5-4 trillion won)

Variable	No. of Obs.	No. of "1" values	Mean	Median	Standard Deviation	Minimum	Maximum
Tobin's *q*	981	-	0.88	0.84	0.32	0.22	6.05
ln(Tobin's *q*)	981	-	-0.18	-0.18	0.29	-1.53	1.80
RPTs/sales	835	-	0.21	0.13	0.25	0.00	1.15
for *chaebol* firms	548	-	0.27	0.20	0.26	0.00	1.15
for non-*chaebol* firms	287	-	0.10	0.03	0.17	0.00	1.15
RPSs/sales	835	-	0.12	0.05	0.18	0.00	0.92
RPPs/sales	835	-	0.09	0.04	0.12	0.00	0.66
Industry EBITDA/assets	979	-	0.05	0.05	0.06	-0.30	0.76
Cash flow rights	424	-	0.17	0.13	0.15	0.00	0.74
ERI (Expropriation Risk Index)	391	-	0.02	0.01	0.04	-0.09	0.13
Book value of assets	982	-	1480.26	1137.41	935.16	500.91	3997.77
Years listed	982	-	19.12	23.00	10.46	0.00	48.00
Leverage	981	-	9.97	3.91	16.76	0.06	102.09
Sales growth (5 *yr*)	974	-	0.12	0.09	0.20	-0.30	1.30
R&D/sales	982	-	0.01	0.00	0.06	0.00	1.64
Advertising/sales	982	-	0.01	0.00	0.02	0.00	0.13
Exports/sales	982	-	0.24	0.07	0.29	0.00	1.00
PPE/Sales	982	-	0.57	0.46	0.54	0.00	6.84
Capex/PPE	982	-	0.12	0.08	0.13	0.00	1.00
EBIT/sales	982	-	0.06	0.07	0.13	-0.63	0.36
EBIT/assets	982	-	0.05	0.05	0.06	-0.26	0.24
Market Share	982	-	0.10	0.03	0.19	0.00	1.00
Share Turnover	980	-	4.66	3.28	5.27	0.03	44.47
Foreign Ownership	973	-	12.76	5.21	16.38	0.00	92.97
Sole Ownership	982	-	13.08	8.60	13.77	0.00	74.61
Top 30 Chaebol Dummy	982	448	0.46	0.00	0.50	0.00	1.00
Level 1 ADR Dummy	982	54	0.06	0.00	0.23	0.00	1.00
MSCI Index Dummy	982	268	0.27	0.00	0.45	0.00	1.00

Table 3: Key announcement dates for 1999 Korean governance reforms.

Key announcements for 1999 reforms to rules governing outside directors, audit committees, and nominating committees for listed Korean firms, from search of KINDS (Korean Integrated News Database System) database, which includes all major Korean newspapers. Announcements used in event study are in **boldface**.

Event	Dates	Information
	1998: various	1998 reforms, effective starting with 1999 annual meetings, require all listed firms to have a minimum of 25% outside directors.
	March 18, 1999	Corporate Governance Reform Committee created to recommend reforms.
	May 24–26, 1999	President appoints new Minister of Finance and Economy and other economic ministers; instructs them to focus on *chaebol* reform, they so report to the press.
1	**June 2, 1999**	**News articles: government economic policy will shift from "lower leverage" to "corporate governance reform" (understood to include independent directors and audit committees).**
	June 3, 1999	**Speech by new Minister of Finance and Economy: *chaebol* reform will focus on corporate governance reform.**
	June 25, 1999	Ministry of Finance and Economy says some provisions in the Korean Corporate Governance Code will be mandated by law, mentions higher outside director ratio, audit committees, and minority shareholders' rights.
2	**July 2, 1999**	**Government announces that audit committee, dominated by outside directors, will be mandated for large firms (size threshold is not specified).**
3	**Aug. 25, 1999**	**Government announces plans to require large firms (news stories speculate that threshold will be 1 trillion won) to have 50% outside directors and a director nomination committee, dominated by outside directors. Ministry of Justice announces a reform bill to allow companies to adopt board committees, including an audit committee with at least three members, including at least 2/3 outside directors, instead of an internal auditor. Proposal also includes more details on previously announced *chaebol* reforms, of which the most significant were limits on investments by one *chaebol* member in another and board approval and disclosure of large related party transactions.**
	Aug. 26, 1999	**Corporate Governance Reform Committee releases first draft of proposed Corporate Governance Code. For large firms (over 1 trillion won), the Code recommends 50% outside directors. For all firms, it recommends (i) an audit committee, with at least one member having expertise in auditing; (ii) an outside director nominating committee; (iii) a board with at least eight directors; (iv) cumulative voting for directors.**
	Sept. 21–29, 1999	Government announces that it is considering raising the size threshold to 2 trillion won.
	Nov. 22, 1999	Government submits a bill to require large firms to have: (i) at least 50% outside directors; (ii) at least three outside directors; (iii) an audit committee composed of at least 2/3 outside directors; (iv) an outside director nomination committee composed of at least 50% outside directors.
	Dec. 16, 1999	National Assembly passes a bill to revise the Securities Transaction Act to require large firms to have 50% outside directors, an audit committee, and an outside director nomination committee. The supplementary provisions clarify effective dates. Audit committee, outside director nomination committee, and a minimum of three outside directors are required as of the first annual general meeting of shareholders (AGM) after January 21, 2000. The 50% outside director ratio should be met on the first AGM after fiscal year 2000.

Table 4: Event Period Abnormal Returns

Regressions (1)-(5): Cumulative market adjusted returns (*CMARs*) for large-plus firms (excluding banks and SOEs) relative to equally weighted index of mid-sized firms (0.5 trillion won < assets < 1 trillion won), over various event periods. Regressions (1)-(4) show CMARs for firms in three treatment groups of large-plus firms (1 trillion won < asserts < 4 trillion won): (1) *chaebol* firms with positive Expropriation Risk Index (mean ERI, defined in text, over 1998-1999) > 0; (2) *chaebol* firms with negative ERI (also over 1998-1999); and (3) firms with missing ERI (mostly non-*chaebol*); control group is mid-sized firms (0.5 trillion won < assets < 1 trillion won). Regression (5) show CMARs for firms in two treatment groups of large-plus firms, ERI missing and ERI non-missing, and interacts ERI non-missing with a positive ERI dummy. For each regression, we drop outlier observations for which a studentized residual obtained by regressing CMAR on large-plus positive ERI firm dummy, large-plus negative ERI dummy, and large-plus missing ERI dummy is greater than ±1.96. Column (6) shows cumulative abnormal returns (CARs) over event period, relative to Mid-sized Index, from separate classic event studies (one for each treatment group) in which we combine all firms in each treatment group into a single equally-weighted portfolio. Market model is estimated over Jan-May and Sept-Dec 1999. Assets are measured at year-end 1998. *, **, and *** indicate significance at 10%, 5%, and 1% levels. *t*-statistics, using industry-group clusters, are in parentheses. Significant results (at 5% level or better) are in **boldface** (suppressed for constant term).

Panel A: Events 1~3 (June 1 ~ August 30)

Dependent variable	CMAR					Event study
Methodology	Regression, with industry-group clusters					CARs
Treatment group	(1)	(2)	(3)	(4)	(5)	(6)
1. Large-plus, positive ERI	**0.2821*** **	**0.3166*** **	**0.3022*** **	**0.3162*** **		**0.3150*** **
	(3.51)	**(3.64)**	**(3.16)**	**(3.04)**		**(3.64)**
2. Large-plus, negative ERI0	0.0361	0.0652	0.0395	0.1089		-0.051
	(0.61)	(1.00)	(0.63)	(1.24)		(0.49)
3. Large-plus, ERI missing	0.0440	0.0774	0.0474	0.1388	0.0774	0.0688
	(0.48)	(0.83)	(0.50)	(1.22)	(0.83)	(0.88)
Large-plus, ERI non-missing					0.0652	
					(1.00)	
x Large-plus, positive ERI					**0.2514*** **	
					(4.79)	
ln(market cap)		-0.0375	344.36	**498.57** **	-0.0375	
		(-1.59)	(1.69)	**(2.16)**	(-1.59)	
Constant	-0.0332	0.1510	-287.34*	**-412.77** **	0.1510	
	(-0.63)	(1.37)	(-1.73)	**(-2.17)**	(1.37)	
6 powers of *ln*(market cap)	N	N	Y	Y	N	
Other controls	N	N	N	Y	N	
Observations						
group 1/2/3	17/13/18	17/13/18	17/13/18	16/11/18	17/13/18	19/16/19
Midsized firms	43	43	43	41	43	47
Adjusted R^2	0.1587	0.1653	0.1671	0.1974	0.1653	

Panel B: Event 2 (June 30 ~ July 7)

Dependent variable	CMAR					Event
Methodology	Regression, with industry-group clusters					study
Treatment group	(1)	(2)	(3)	(4)	(5)	(6)
1. Large-plus, positive ERI	**0.0845*** **	**0.1070*** **	**0.1076*** **	**0.0955*** **		**0.0992*** **
	(3.74)	**(4.00)**	**(4.43)**	**(2.96)**		**(3.77)**
2. Large-plus, negative ERI	0.0084	0.0248	0.0357*	0.0650		0.0074
	(0.45)	(1.17)	(1.84)	(1.68)		(0.23)
3. Large-plus, ERI missing	0.0291*	**0.0506** **	**0.0525*** **	**0.0926*** **	**0.0506** **	**0.0536** **
	(1.98)	**(2.58)**	**(2.83)**	**(3.62)**	**(2.58)**	**(2.24)**
Large-plus, ERI non-missing					0.0248	
					(1.17)	
x Large-plus, positive ERI					**0.0822*** **	
					(3.73)	
ln(market cap)		-0.0208*	100.67	**136.55** **	-0.0208*	
		(-1.87)	(1.69)	**(2.29)**	(-1.87)	
Constant	-0.0043	0.0978*	-86.11*	**-114.16** **	0.0978*	
	(-0.41)	(1.74)	(-1.77)	**(-2.34)**	(1.74)	
6 powers of *ln*(market cap)	N	N	Y	Y	N	
Other controls	N	N	N	Y	N	
Observations						
group 1/2/3	17/15/16	17/15/16	17/15/16	17/12/16	17/15/16	19/16/19
Midsized firms	46	46	46	43	46	47
Adjusted R^2	0.1118	0.1418	0.2077	0.2753	0.1418	

Panel C: Event 3 (August 23 ~ August 30)

Dependent variable	CMAR					Event
Methodology	Regression, with industry-group clusters					study
Treatment group	(1)	(2)	(3)	(4)	(5)	(6)
1. Large-plus, positive ERI	**0.0662*** **	**0.0747*** **	**0.0723*** **	0.0456*		**0.0609** **
	(3.50)	**(3.74)**	**(3.99)**	(1.85)		**(2.31)**
2. Large-plus, negative ERI	**0.0358** **	**0.0421** **	**0.0491*** **	0.0387*		0.0083
	(2.10)	**(2.50)**	**(3.22)**	(1.89)		(0.27)
3. Large-plus, ERI missing	0.0114	0.0189	0.0157	0.0257	0.0189	0.019
	(0.66)	(0.99)	(0.93)	(1.27)	(0.99)	(0.78)
Large-plus, ERI non-missing					**0.0421** **	
					(2.50)	
x Large-plus, positive ERI					**0.0326*** **	
					(2.84)	
ln(market cap)		-0.0080	-47.05	-103.12	-0.0080	
		(-1.02)	(-1.01)	(-1.56)	(-1.02)	
Constant	-0.0042	0.0346	39.35	86.43	0.0346	
	(-0.32)	(0.81)	(1.00)	(1.59)	(0.81)	
6 powers of *ln*(market cap)	N	N	Y	Y	N	
Other controls	N	N	N	Y	N	
Observations						
group 1/2/3	19/14/18	19/14/18	19/14/18	18/11/18	19/14/18	19/16/19
Midsized firms	44	44	44	41	44	47
Adjusted R^2	0.1333	0.1339	0.1382	0.1940	0.1339	

Table 5: Corporate Governance and Volume of Related-Party Transactions

Firm random effects (RE) and fixed effects (FE) regressions of RPTs/sales on *KCGI*, control variables, year dummies, industry dummies (for RE), and constant term, for firms (excluding banks and SOEs) with 0.5 trillion won < assets < 4 trillion won. The odd-numbered regressions use RE, the even-numbered regressions use FE. Sample for regressions (3)-(4) is limited to *chaebol* firms. Observations are identified as outliers each year if a studentized residual from regressing RPTs/sales on *KCGI* > ±1.96. All regressions use unbalanced panels and year dummies. Control variables are the same as in Table 7, except we drop top-30 chaebol dummy in regressions (3)-(4). *t*-statistics, with standard errors clustered on firm, are in parentheses. R^2 is overall for RE, within for FE.

Dependent variable: RPTs/sales	(1)	(2)	(3)	(4)
Model	RE	FE	RE	FE
Sample	All		*Chaebol*	
KCGI	0.0011	0.0007	0.0011	0.0005
	(1.34)	(0.87)	(1.09)	(0.47)
controls, constant term	yes	yes	yes	yes
4-digit industry dummies	yes	-	yes	-
No. of Observations	747		478	
No. of firms/large firms	167/48		100/39	
R^2	0.389	0.129	0.436	0.195
Random effects λ	0.703		0.620	

Table 6: Corporate Governance, Related-Party Transactions, and Firm Value

(*Chaebol* Firms)

Firm fixed effects regressions for *chaebol* firms (excluding banks and SOEs) with 0.5 trillion won < assets < 4 trillion won. Panel A show regressions of *ln*(Tobin's q) on *KCGI* or *KCGI* Dummy (=1 if KCGI > *chaebol* median of 40), (RPTs)/sales, interaction term, and control variables. Panel B replaces RPTs with purchases from (RPPs) and sales to (RPSs) related parties. Panel C replaces *KCGI* (or *KCGI* Dummy) with Board Structure Subindex, Disclosure Subindex, and Sum of Other Subindices (or related dummies, which = 1 if index > *chaebol* means of 4.2, 4.8, and 31.8, respectively). Expropriation Risk Index (ERI) is defined in the text. Control variables are the same as in Table 7, except we drop top 30 *chaebol* dummy. Sample for columns (4) and (8) is firms with mean ERI (over all sample years) > 0; columns (5) and (9) are further limited to top-18 *chaebol* groups (14 groups designated as such by Korean Fair Trade Commission throughout the sample period and four groups spun off from the original 14); sample for columns (6) and (10) is firms with mean ERI ≤ 0. Observations are identified as outliers each year if a studentized residual from regressing *ln*(Tobin's *q*) on *KCGI* (or *KCGI* dummy) > ±1.96. All regressions use unbalanced panels and year dummies. *t*-statistics, with standard errors clustered on firm, are in parentheses. *F*-tests in Panel B are for joint significance of *KCGI* (or *KCGI* Dummy) * RPSs/sales and * RPPs/sales. Significant results (at 5% level or better) are shown in **boldface.**

Panel A: Interact *KCGI* (or *KCGI* Dummy) with Related-Party Transactions

Dependent variable: *ln*(Tobin's *q*)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Chaebol	All	All	All	All	Top 18	All	All	All	Top 18	All
Mean ERI	All	All	All	> 0	> 0	≤ 0	All	> 0	> 0	≤ 0
RPTs/sales	0.0589	0.0629	-0.1328	-0.1753	-0.1938	-0.1323	-0.0070	-0.0615	-0.0416	0.0740
	(1.05)	(1.12)	(-1.04)	(-1.32)	(-1.56)	(-0.48)	(-0.11)	(-0.93)	(-0.77)	(0.52)
x *KCGI*			0.0046*	0.0050*	**0.0068****	0.0054				
			(1.72)	(1.69)	**(2.41)**	(1.15)				
x *KCGI* Dummy (>40)							0.0983	**0.1758****	**0.3023*****	-0.0376
							(1.60)	(2.45)	(4.06)	(-0.30)
KCGI		0.0024*	0.0012	-0.0007	-0.0007	0.0044				
		(1.67)	(0.75)	(-0.39)	(-0.43)	(1.31)				
KCGI Dummy (>40)							0.0195	-0.0495	-0.0659*	0.1178*
							(0.57)	(-1.17)	(-1.84)	(1.93)
controls, constant term	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
No. of Observations	509	509	509	318	189	191	509	318	189	191
No. of firms/large firms	104/41	104/41	104/41	65/25	41/20	39/16	104/41	65/25	41/20	39/16
Within R^2	0.207	0.215	0.223	0.216	0.314	0.436	0.223	0.225	0.353	0.436

Panel B: Interact *KCGI* (or *KCGI* Dummy) Separately with Related-Party Sales and Purchases

Dependent variable: *ln*(Tobin's *q*)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Chaebol	All	All	All	All	Top 18	All	All	All	Top 18	All
Mean ERI	All	All	All	> 0	> 0	≤ 0	All	> 0	> 0	≤ 0
RPSs/Sales	-0.0462	-0.0380	-0.0289	-0.0057	-0.2938	-0.3905	-0.0435	-0.1383	-0.1896	0.1303
	(-0.54)	(-0.45)	(-0.17)	(-0.03)	(-1.08)	(-0.97)	(-0.49)	(-1.30)	(-1.61)	(0.86)
x *KCGI*			-0.0004	-0.0035	0.0027	0.0118				
			(-0.10)	(-0.79)	(0.39)	(1.18)				
x *KCGI* Dummy (>40)							0.0071	0.0411	0.1564	-0.0807
							(0.09)	(0.38)	(0.78)	(-0.44)
RPPs/Sales	0.1974*	0.1949*	-0.2761	-0.3906	-0.0287	0.1814	0.0390	0.0167	0.1350	0.0580
	(1.76)	(1.73)	(-0.89)	(-1.23)	(-0.08)	(0.43)	(0.25)	(0.10)	(1.07)	(0.23)
x *KCGI*			0.0114*	**0.0149****	0.0085	-0.0036				
			(1.69)	**(2.12)**	(0.99)	(-0.31)				
x *KCGI* Dummy (>40)							0.2727*	**0.3227****	0.3098	0.1431
							(1.71)	**(1.97)**	(1.67)	(0.59)
KCGI		0.0023	0.0012	-0.0004	0.0001	0.0043				
		(1.63)	(0.74)	(-0.19)	(0.06)	(1.33)				
KCGI Dummy (>40)							0.0086	-0.0503	-0.0431	0.0982
							(0.25)	(-1.19)	(-1.05)	(1.59)
controls, constant term	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
No. of Observations	509	509	509	318	189	191	509	318	189	191
No. of firms/large firms	104/41	104/41	104/41	65/25	41/20	39/16	104/41	65/25	41/20	39/16
F-test (*p*-values)			0.1759	0.1119	0.1863	0.4260	0.1682	0.0590*	**0.0329****	0.8070
Within R^2	0.214	0.222	0.235	0.246	0.360	0.439	0.234	0.244	0.380	0.437

Panel C: Interact *KCGI* Subindices (or Subindex Dummies) with RPTs/Sales

Dependent variable: *ln*(Tobin's *q*)	(1)	(2)	(3)	(4)
Chaebol	All	Top 18	All	Top 18
Mean ERI	> 0	> 0	> 0	> 0
RPTs/sales	-0.1888	0.1420	-0.0516	-0.0134
	(-0.84)	(0.49)	(-0.61)	(-0.20)
x Board Structure	0.0074	**0.0289*** **		
	(0.82)	**(3.22)**		
x Disclosure	0.0008	0.0008		
	(0.07)	(0.07)		
x Sum of Other Sub-indices	0.0051	-0.0059		
	(0.66)	(-0.59)		
x Board Structure Dummy			-0.0311	**0.2835** **
(> 4.2)			(-0.25)	**(2.63)**
x Disclosure Dummy			-0.0013	-0.0014
(> 4.8)			(-0.14)	(-0.15)
x Sum of Other Sub-indices Dummy			0.1389	-0.0423
(> 31.8)			(1.38)	(-0.42)
Board Structure	-0.0000	-0.0032		
	(-0.00)	(-0.83)		
Disclosure	-0.0026	-0.0007		
	(-0.66)	(-0.20)		
Sum of Other Sub-indices	0.0002	0.0024		
	(0.07)	(0.60)		
Board Structure Dummy			-0.0018	-0.0240
(> 4.2)			(-0.04)	(-0.44)
Disclosure Dummy			0.0051	0.0001
(> 4.8)			(0.15)	(0.00)
Sum of Other Sub-indices Dummy			-0.0140	0.0061
(> 31.8)			(-0.36)	(0.16)
controls, constant term	Y	Y	Y	Y
No. of Observations	312	187	312	187
No. of firms/large firms	65/25	41/20	65/25	41/20
Within R^2	0.223	0.341	0.223	0.329

Table 7: Corporate Governance, Related-Party Transactions, and Firm Value

(All Firms with assets from 0.5-4 Trillion Won)

Firm fixed effects regressions of *ln*(Tobin's *q*) on indicated governance variables (*KCGI*, KCGI dummy, subindices, subindex dummies), (RPTs)/sales, interaction between RPTs/sales and governance variable(s), and control variables, for sample of all firms (excluding banks) with 0.5 trillion won < assets < 4 trillion won. Observations are identified as outliers and excluded if a studentized residual from regressing *ln*(Tobin's *q*) on *KCGI* > ±1.96. *, **, and *** respectively indicate significance levels at 10%, 5%, and 1% levels. All regressions use unbalanced panels and year dummies. *t*-statistics, with standard errors clustered on firm, are in parentheses. Sample after excluding outliers is 768 observations of 52 large and 120 mid-sized firms. *F*-test in Regressions (5) and (6) is for joint significance of RPTs/Sales interacted with Board Structure Index and Disclosure Index or with corresponding subindex dummies. Significant results (at 5% level or better) are shown in **boldface (**suppressed for constant term)**.**

Dep. variable: *ln*(Tobin's *q*)	(1)	(2)	(3)	(4)
RPTs/sales	0.0550	0.0589	-0.1388	-0.0024
	(0.98)	(1.07)	(-1.14)	(-0.04)
KCGI		**0.0028****	0.0017	
		(2.42)	(1.33)	
KCGI dummy				0.0175
				(0.83)
KCGI or KCGI dummy x RPTs/sales			0.0046*	**0.1160****
			(1.85)	**(2.26)**
ln(assets)	**0.0961****	**0.0916****	**0.0993****	**0.1072****
	(2.10)	**(2.05)**	**(2.25)**	**(2.36)**
ln(years listed)	-0.0360	-0.0296	-0.0270	-0.0228
	(-0.64)	(-0.55)	(-0.53)	(-0.42)
Leverage	0.0019*	0.0018*	0.0018*	0.0017*
	(1.89)	(1.85)	(1.88)	(1.77)
Sales growth	-0.0309	-0.0279	-0.0089	-0.0080
	(-0.52)	(-0.47)	(-0.15)	(-0.14)
R&D/sales	-0.0365	**-0.0592****	-0.0474*	-0.0325
	(-1.48)	**(-2.30)**	(-1.88)	(-1.31)
Advertising/sales	0.4103	0.3779	0.7737	0.7290
	(0.18)	(0.16)	(0.35)	(0.33)
Export/sales	-0.0013	0.0151	0.0182	-0.0115
	(-0.02)	(0.19)	(0.25)	(-0.15)
PP&E/sales	**-0.1686*****	**-0.1672*****	**-0.1500****	**-0.1507****
	(-2.89)	**(-2.90)**	**(-2.49)**	**(-2.47)**
PP&E/sales^2	**0.0216*****	**0.0218*****	**0.0191*****	**0.0188****
	(3.03)	**(3.10)**	**(2.63)**	**(2.51)**
CAPEX/PP&E	-0.0251	-0.0336	-0.0296	-0.0277
	(-0.46)	(-0.61)	(-0.52)	(-0.50)
EBIT/sales	**-0.3942*****	**-0.3903*****	**-0.3822*****	**-0.3742****
	(-2.71)	**(-2.66)**	**(-2.62)**	**(-2.43)**
EBIT/assets	**1.0227*****	**1.0649*****	**1.0396*****	**0.9932****
	(2.82)	**(2.88)**	**(2.83)**	**(2.55)**
Market share	-0.0953	-0.1052	-0.1129	-0.0711
	(-0.76)	(-0.87)	(-0.98)	(-0.61)
Turnover	0.0019	0.0015	0.0014	0.0017
	(0.87)	(0.71)	(0.66)	(0.81)
Foreign ownership	**0.0030*****	**0.0027*****	**0.0026*****	**0.0029*****
	(3.48)	**(3.15)**	**(3.14)**	**(3.50)**
Chaebol dummy	0.0382	0.0403	0.0379	0.0395

Dep. variable: *ln*(Tobin's *q*)	(1)	(2)	(3)	(4)
	(1.38)	(1.46)	(1.38)	(1.45)
Sole ownership	-0.0027	-0.0037	-0.0039	-0.0030
	(-0.95)	(-1.30)	(-1.37)	(-1.07)
Sole ownership^2	0.0001	0.0001	0.0001	0.0001
	(0.86)	(1.06)	(1.14)	(0.99)
ADR Level 1	0.0125	-0.0069	-0.0049	0.0033
	(0.23)	(-0.13)	(-0.08)	(0.06)
MSCI	0.0286	0.0245	0.0255	0.0307
	(1.12)	(0.99)	(1.05)	(1.23)
Constant	-0.7325	-0.7799	-0.8070	-0.8449
	(-2.04)	(-2.21)	(-2.33)	(-2.38)
F-test (*p*-value)				
Within R^2	0.1837	0.1957	0.2037	0.2012

Table 8: KCGI and Profitability of RPTs

(*Chaebol* Firms)

Firm fixed effects regressions for *chaebol* firms (excluding banks and SOEs) with 0.5 trillion won < assets < 4 trillion won. Regressions (1)-(5): Regressions of EBIT/sales *KCGI* Dummy (=1 if KCGI > 40), (RPTs)/sales, interaction term, and control variables. Regressions (6)-(10) are similar except dependent variable is net income/sales. Expropriation Risk Index (ERI) is defined in the text. Control variables are the same as in Table 7, except we drop top 30 *chaebol* dummy, EBIT/sales, and EBIT/assets. Sample for columns (3) and (8) is firms with mean ERI (over all sample years) > 0; columns (4) and (9) are further limited to top-18 *chaebol* groups; sample for columns (5) and (10) is firms with mean ERI ≤ 0. Observations are identified as outliers each year if a studentized residual from regressing dependent variable on *KCGI* dummy > ±1.96. All regressions use unbalanced panels and year dummies. *t*-statistics, with standard errors clustered on firm, are in parentheses. Significant results (at 5% level or better) are shown in **boldface.**

Dependent variable	**EBIT/sales**					**Net income/sales**				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Chaebol	All	All	All	Top 18	All	All	All	All	Top 18	All
Mean ERI	All	All	> 0	> 0	≤ 0	All	All	> 0	> 0	≤ 0
RPTs/sales	-0.0269*	**-0.0425****	-0.0360*	**-0.0522****	-0.0567	**-0.0439*****	**-0.0777*****	**-0.0802*****	-0.0351	-0.0615
	(-1.74)	**(-2.44)**	(-1.80)	**(-2.22)**	(-1.59)	**(-2.84)**	**(-3.01)**	**(-2.72)**	(-1.40)	(-1.45)
x *KCGI* Dummy		0.0299*	**0.0343****	0.0408	0.0170		0.0551*	**0.0850****	0.0800*	0.0192
		(1.93)	**(2.07)**	(1.46)	(0.40)		(1.70)	**(2.05)**	(1.81)	(0.48)
KCGI Dummy		-0.0114	-0.0119	-0.0173	-0.0103		**-0.0218****	-0.0288*	-0.0278	-0.0126
		(-1.30)	(-0.96)	(-0.95)	(-0.70)		**(-2.02)**	(-1.76)	(-0.99)	(-0.67)
Constant, other controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
No. of Observations	510	509	319	188	190	509	510	324	193	186
No. of firms/large firms	104/41	104/41	65/25	41/20	39/16	104/41	104/41	65/25	41/20	39/16
Within R^2	0.136	0.140	0.098	0.200	0.287	0.236	0.240	0.249	0.261	0.454

Table 9: Corporate Governance, Industry Profitability and Tunneling

Firm fixed effects regressions of EBITDA/assets on industry EBITDA/assets, *KCGI* (or it subindices), *ln*(assets), their interaction terms, and other control variables. Regression design is adapted from Bertrand, Mehta and Mullainathan (2002). Industry EBITDA/assets = (EBITDA summed across all other firms in the same 4-digit industry)/(assets summed across all other firms in the same 4-digit industry). Sample for regressions (1)-(3) and (6)-(7) is all firms, for regressions (4)-(5) is limited to *chaebol* firms. Other control variables are the same as in Table 8. Observations are identified as outliers if a studentized residual from regressing the dependent variable on *KCGI* is greater than ±1.96. All regressions use unbalanced panels, year dummies, and firm clusters. *t*-values are in parentheses. F-test in regression (3) is for combined significance of interactions of Board Structure Subindex and Disclosure Subindex with Industry EBITDA/Assets. Significant results (at 5% level or better) are shown in **boldface.**

Dependent var.: EBITDA/Assets	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Sample	all	all	all	mean ERI < 0	mean ERI > 0	EBITDA > 0	EBITDA < 0
KCGI		-0.0000		-0.0003	-0.0004	-0.0002	-0.0001
		(-0.15)		(-0.80)	(-1.39)	(-1.30)	(-0.28)
x Industry EBITDA/assets		0.0022		0.0055	**0.0065****	**0.0039****	-0.0052
		(1.36)		(1.25)	**(2.38)**	**(2.55)**	(-0.78)
Board Structure Subindex			-0.0002				
			(-0.35)				
x Industry EBITDA/assets			**0.0127****				
			(2.12)				
Disclosure Subindex			0.0000				
			(0.03)				
x Industry EBITDA/assets			0.0018				
			(0.38)				
Industry EBITDA/assets	**0.2027*****	0.1231*	0.1241	-0.0662	-0.0550	0.0223	0.2378
	(6.53)	(1.81)	(0.79)	(-0.32)	(-0.42)	(0.36)	(1.08)
Other subindices of *KCGI*	N	N	Y	N	N	N	N
Other subindices x Industry EBITDA/assets	N	N	Y	N	N	N	N
ln(assets)	Y	Y	Y	Y	Y	Y	Y
Controls, constant term	Y	Y	Y	Y	Y	Y	Y
No. of Observations	3,822	3,822	3,822	382	780	3,401	421
No. of firms	679	679	679	67	128	661	219
Within R^2	0.1425	0.1435	0.1483	0.3560	0.2024	0.2005	0.2089

Appendix Table 1: Covariate Balance for Positive-ERI and Negative ERI Firms

Mean values for control variables, for positive-ERI and negative ERI Firms (sample based on Table 4). Standard errors are clustered on firm. Observations = 499 (318 with ERI > 0; 191 with ERI < 0). *, **, *** indicates significance at the 10%, 5%, and 1% level, based on two-sample *t*-test. Lower sole ownership for positive ERI firms is expected, due to the definition of ERI.

Variables	ERI > 0	ERI <0	Difference	p-value
	Mean	Mean	in means	
ln(Tobin's *q*)	-0.1575	-0.1771	0.0196	0.595
RPTs/sales	0.2737	0.2604	0.0133	0.778
KCGI	40.1117	41.7539	-1.6421	0.405
ln(asasets)	7.2167	7.3213	-0.1046	0.362
ln(years listed)	2.8581	2.9026	-0.0445	0.749
Leverage	5.3641	7.1417	-1.7776	0.173
Sales growth (5 yr)	0.1363	0.0845	0.0518	**0.017****
R&D/sales	0.0070	0.0116	-0.0046	0.135
Advertising/sales	0.0073	0.0057	0.0016	0.504
Exports/sales	0.2942	0.2831	0.0111	0.846
PPE/sales	0.5833	0.5508	0.0325	0.734
Capex/PPE	0.1229	0.1202	0.0027	0.864
EBIT/sales	0.0614	0.0612	0.0002	0.983
EBIT/assets	0.0554	0.0488	0.0066	0.340
Market Share	0.1188	0.1237	-0.0049	0.901
Share Turnover	5.1608	4.3451	0.8157	0.168
Foreign Ownership	15.2540	10.8678	4.3862	0.087*
Sole Ownership	9.1948	19.3548	-10.1600	**0.000*****

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