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ABSTRACT: Outside directors and audit committees are widely considered to be central elements of good corporate governance. Yet evidence supporting this conventional wisdom is limited. A core problem is that board structure is endogenous to other firm characteristics. Good identification strategies are rarely available. Korea provides a unique laboratory for assessing whether there is a causal connection between board structure and firm value in an emerging market. We rely on a 1999 Korean law which mandates 50% outside directors, an audit committee, and a director nominating committee for large public firms, but not for smaller public firms. We use a combination of event study, difference-in-difference, firm fixed effects, instrumental variable, and regression discontinuity methods, and exploit the synergies between different methods to improve identification. We find consistent evidence across methods of economically important share price increases for large firms. Large firms' share prices jump relative to small firms when the reforms are announced, and thereafter remain higher than those of small firms. Several years after the reforms, large firms' profitability rises relative to small firms and their asset sales to related parties decline, suggesting channels through which board structure affects firm value. In a firm fixed effects framework, we find (i) similar share price gains for large firms, which are legally required to change board structure, and smaller firms which do so voluntarily; and (ii) evidence for the separate value of board independence and board committee structure (largely audit committee).

Key words: Korea, outside directors, audit committees, corporate governance, board of directors

JEL classification: G32, G34

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1. Introduction

A minimum number of outside directors (perhaps a majority), and an audit committee staffed principally or solely by outside directors, are standard corporate governance prescriptions. Both are prescribed by law in many countries, and are central components of most voluntary, "comply or explain" corporate governance codes. Yet empirical support for the value of these governance elements is limited. In developed countries, there is mixed evidence on whether board structure predicts share price or overall corporate performance. In emerging markets, there is cross-sectional evidence supporting the value of outside directors, but little evidence on audit committees.

Moreover, in all markets, there is a core identification problem: Board structure is usually chosen voluntarily and is likely endogenous to other firm characteristics. Firm value or performance could predict board structure, rather than vice-versa, or optimal board structure could be endogenous to other omitted firm characteristics (e.g., Hermalin and Weisbach, 1998, 2003, Lehn, Patro and Zhao, 2003). Convincing identification strategies are rarely available.

Korea provides a unique laboratory for assessing whether there is a causal connection between board structure and firm value in an emerging market. In response to the 1997-1998 East Asian financial crisis, Korea adopted governance rules in 1999, effective partly in 2000 and partly in 2001, which require "large" firms (assets > 2 trillion won, around $2 billion) to have 50% outside directors, an audit committee with an outside chair and at least 2/3 outside members, and an outside director nominating committee. Smaller firms must have 25% outside directors. Our identification strategy relies on this law. We conduct event study and difference-in-difference (DiD) estimation of the effect of adoption of these rules, with large firms serving as the treatment group and small firms as the control group. We support the event study and DiD analyses with instrumental variable (IV) analysis of the effect of these governance elements on firm value, using a "large firm dummy" (equals 1 for firms with assets > 2 trillion won, 0 otherwise) to instrument for board structure. We embed our analysis in a regression

discontinuity (RD) framework, in which we separately control for a possible smooth effect of firm size on firm value or performance.

We measure the governance of Korean public firms over 1998-2004, relying largely on hand-collected data. We construct an overall corporate governance index which lets us control for other attributes of corporate governance that correlate with board structure and might separately predict firm value. We employ extensive control variables to (imperfectly) address omitted variable bias.

We report consistent evidence, across all methods, for a connection between board structure (outside directors and audit committees) and firm market value. The share prices (and thus Tobin's q's) of large firms jump relative to small firms when the reforms are announced. The large-minus-small difference in Tobin's q is stable both before and after the legal change. Several years after the reforms, large firms' profitability rises relative to unreformed small firms, and their asset sales to related parties decline, suggesting possible channels through which board structure may affect firm value.

Each of these approaches has well understood econometric advantages and limitations. Each alone can do only so much to establish a likely causal link between board structure and firm market value. But taken together, we believe that they offer robust evidence of such a link, in an important emerging market. There are important synergies between methods, with the event study permitting tighter identification in the DiD analysis, the firm fixed effect results permitting tighter identification in the IV analysis, and the event study and DiD analyses supporting the IV analysis by providing identification in time as well as in firm size. To our knowledge, the "multiple identification strategies" approach has no direct precursors in the economics or finance literatures. Yet it has substantial advantages over any single approach, and is likely to be adaptable to other studies which rely on legal change for identification.

The estimated effects are economically important. In our event study, large firms' share prices rise by an average of 21% relative to small firms during a roughly 6-week event window which captures the principal early legislative events (June 1 - July 8, 1999). Our *DiD* results

suggest a roughly 0.15-0.16 increase in *ln*(Tobin's *q*) from June 1, 1999 (just before the first public reports on the new rules), through the end of 1999. *IV* estimates are similar. For a firm which had none of the three board structure elements required by the 1999 law, the predicted increase in *ln*(Tobin's *q*) is 0.28-0.40; predicted increases in share price are substantially higher.

The event study, *IV* and *DiD* results provide evidence that investors assigned positive value to the board structure reforms for large Korean firms; the ex post results on profitability and related party transactions provide evidence on possible channels. These results cannot, however, tell us (i) whether similar changes would be valuable for "small" firms (assets < 2 trillion won), or (ii) how much of the value increase is due to each of the reforms (50% outside directors, audit committees, and nominating committees). To address these issues, we study all Korean firms, both large and small, using firm fixed effects to control for unobserved, time-invariant firm-level heterogeneity, and year dummies and extensive control variables to control for time-varying heterogeneity. We find evidence supporting the separate value of (i) 50% outside directors; (ii) more than 50% outside directors, and (iii) an audit committee (principally for financial firms). The results for director independence are robust; the audit committee results , although the audit committee results are not always significant in robustness checks. The predicted effect on Tobin's *q* of having 50% outside directors is similar for large firms, which are legally required to change their board structure, and smaller firms which do so voluntarily. For firms with less than 50% outside directors, the proportion of outside directors does not predict Tobin's *q* over the available range (from the 25% minimum established by the 1999 law to 49%.). The evidence on small firms is consistent with the investment strategy of the Korea Corporate Governance Fund, managed by Lazard Freres. This fund relies on the value effect of governance reforms, by investing in unreformed small firms, pressuring them to add outside directors and an audit committee, and selling after the reforms are adopted.[1]

We also find evidence of endogeneity for smaller firms, as well as important differences

[1] Hasung Jang, Dean of Korea Business School and our coauthor in related work, is a consultant to this fund. The Korean Center for Good Corporate Governance is also a consultant to this fund. Woochan Kim is affiliated with this Center..

between pooled *OLS* and firm fixed effects estimates. These differences support doubts about the reliability of cross-sectional estimates in studies of board composition, as well as studies of corporate governance more generally (Chidambaran, Palia and Zheng, 2006, Lehn, Patro and Zhao, 2006; Listokin, 2007; Wintoki, Netter and Linck, 2007).

Some limitations of this research: First, we cannot know the extent to which our results may generalize beyond Korea. Second, our results imply that the governance of large Korean firms was *not* in equilibrium in 1999, when the board structure rules were adopted. This is plausible, given Korea's history, but suggests caution in reaching policy conclusions for other countries based on our results.

Third, some market value gains realized by outside investors may have come at the expense of reduced private benefits of control for insiders. Thus, market value gains do not directly imply gains in overall firm value. However, there are two indirect sources of evidence on gains in firm value. One is our evidence on two possible channels though which the reforms could affect value -- higher profitability and reduced self-dealing channel. Another is the voluntary adoption of these reforms, especially audit committees, by a substantial number of small firms, suggests a value effect.

Fourth, our identification strategy does not let us study separately the effects on firm value of board independence and audit committees. We can do so for a pooled sample which includes small firms, but for these firms, we cannot rule out endogeneity.

This paper is organized as follows. Section 2 reviews prior literature on the connection between board composition, or the presence of an audit committee, and overall firm value or performance. Section 3 discusses the principal empirical challenges and our identification strategy. Section 4 describes our data sources and how we construct our governance indices. Section 5 presents event study results. Section 6 presents difference-in-difference results. Section 7 presents firm fixed effects results for our full sample. Section 8 presents instrumental variable results. Section 9 concludes.

2. Literature Review

We provide here a brief literature review. We focus on research in emerging markets and on research that focuses specifically on boards of directors and audit committees. Section 2.1 addresses research on board independence and section 2.2 discusses research on audit committees. We discuss empirical research; but note that there are several models of optimal board structure that explicitly account for potential endogeneity (for example, Harris and Raviv, 2007; Hermalin and Weisbach, 1998).

We do not discuss in detail research on overall corporate governance which does not specifically focus on boards of directors or audit committees. Cross-country research of this type includes Durnev and Kim (2005); Doidge, Karolyi and Stulz (2004a); Klapper and Love (2004); Aggarwal, Isil, Stulz and Williamson (2006); and Bruno and Claessens (2007). Single-country research on overall corporate governance in emerging markets which includes time-series data includes Black, Kim, Jang and Park (2007) (Korea multiyear); and Black, Love and Rachinsky (2006) (Russia). There are also some single-year, single-country studies, for example, Cheung, Connelly, Limpaphayon and Zhou (2007) (Hong Kong).

This paper builds on our prior research on Korea, principally Black, Jang and Kim (2006). In section 2.4, we summarize the identification strategies we employ here, and discuss how this paper builds on and differs from this prior work. Section 2.5 discusses the econometric value of applying multiple identification strategies together.

2.1. Board Independence

2.1.1. Board Independence in Developed Markets

Board independence predicts firm behavior in a variety of ways: For example, more independent boards make better acquisition decisions, are more likely to choose an outsider as CEO, are more likely to resist a takeover bid, and are more likely to fire the CEO following poor performance. For reviews, see Bhagat and Black (1999), Hermalin and Weisbach (2003). However, evidence on the association between board independence and overall firm value or

performance is mixed, and the direction of causation is unclear (Lehn, Patro, and Zhao, 2003). Good identification strategies thus far do not exist.

Many studies (e.g., Morck, Shleifer, and Vishny (1988), Baysinger and Hoskisson (1990), Hermalin and Weisbach (1991), Mehran (1995), and Klein (1998)) find no significant *OLS* relationship between the two in the United States. Yermack (1996), Agrawal and Knoeber (1996) and Bhagat and Black (2002) find a negative relationship in the U.S., as do Erickson, Park, Reising, and Shin (2005) in Canada. Bhagat and Black (2002) and Erickson et al. (2005) report evidence that the negative relationship reflects reverse causation, in which firms which experience poor performance increase the independence of their boards. Wintoki, Linck & Netter (2007) report a negative relationship between independence and performance using *OLS*, which flips sign with firm fixed effects and disappears with his preferred GMM approach with U.S. data. On the theoretical side, there is no reason to expect a monotonic relationship between board independence and measure of firm performance or value (e.g., Hermalin and Weisbach, 2003; Adams and Ferreira, 2007).

The only study with plausible identification is Dahya & McConnell (2007). They find improved operating performance for U.K. firms which previously had only one or two outside directors, but increase this number to three to comply with the Cadbury Committee "comply or explain" recommendation to have at least 3 outside directors. However, identification is imperfect, because firms can still choose to have fewer than three outside directors.

2.1.2. Board Independence in Emerging Markets

In contrast to the mixed findings in developed markets, several emerging market studies find a positive cross-sectional relationship between board independence and firm performance. Dahya, Dimitrov, and McConnell (2007) report cross-country evidence for a 22-country sample, with independent directors having a stronger effect in countries with weaker governance. Positive effects of director independence have been found in several individual countries, including Korea (Black, Jang and Kim, 2006a and Choi, Park and Yoo, 2007); Taiwan (Yeh and Woidtke, 2005), and Ukraine (Zheka, 2006).

Choi, Park and Yoo (2007) also study Korean boards of directors, and report that board independence correlates with firm value for Korean firms. However, they rely on *OLS* for a pooled sample of all firms. They attempt but do not report firm fixed effects, which apparently wash away their results. They report two-stage least squares results using lagged firm variables as instruments, but this is not appropriate if firm governance and financial characteristics both persist over time, or both correlate with omitted variables.

2.2. Audit Committees

Research on the connection between audit committees and overall firm value is scarce, and does not offer an identification strategy. Klein (1998) finds a correlation between the presence of an audit committee and a variety of accounting and market performance measures. Vafaes and Theodorou (1998) and Weir, Laing, and McKnight (2003) find similar results in the U.K. There are no comparable studies in emerging markets.

Most of the remaining literature on audit committees focuses on an association between audit committees and financial fraud or financial reporting decisions (e.g., Anderson, Mansi, and Reeb, 2004; Xie, Davidson and DaDalt, 2002; Defond, Hann and Hu, 2004) and is not directly relevant.

3. Empirical Issues

In this section, we first discuss the identification and other empirical challenges that this research faces (Section 3.1). Addressing these is the principal goal of this paper. We then discuss our multiple identification strategy approach, and how this paper

3.1. Empirical Challenges to Identification

Most work on the connection between board structure or other aspects of corporate governance and firm value or performance face a set of empirical challenges, which make identification difficult. Several recent articles contend that because of these challenges, we still know very little about the effects of board structure, or corporate governance more generally, on firm value or performance (Chidambaran, Palia and Zheng, 2006; Lehn, Patro and Zhao, 2006;

Listokin, 2007). We summarize these issues here and refer readers to these papers for more details.

One problem is the potential for the reverse causation flavor of endogeneity, in which firm performance predicts board structure, rather than vice versa. This is a real concern for studies of board structure. In the U.S., Bhagat and Black (2002) report evidence of reverse causation in the determination of board composition; Erickson, Park, Reising and Shin (2005) report similar evidence for Canada. In the U.K., Arcot and Bruno (2006) and MacNeil (2006) report that well-performing firms are more likely to depart from the "comply or explain" U.K. Combined Code of Corporate Governance.

A second likely form of endogeneity involves optimal governance varying across firms, based on firm characteristics, so that even if a governance attribute correlates with firm value in cross-section, this does not imply that this attribute would be valuable at other firms (Demsetz and Lehn, 1985). For evidence on the factors that influence board composition, see, e.g., Boone, Field, Karpoff and Raheja (2007), Gillan, Hartzell and Starks (2006); Agrawal and Knoeber (2001). A third possibility is that firms may use governance to signal good underlying attributes, but governance has no separate effect on value or performance.

A further problem in many emerging markets is omitted variable bias. Different aspects of governance are often positively correlated. Moreover, a wide range of firm characteristics could plausibly predict both board structure and firm value or performance. Yet most studies control for a limited set of governance attributes and other firm characteristics. This concern is especially acute for cross-country studies, due to data limitations in the principal international financial databases. At a minimum, to solidly establish association (even without identification), one would want to use time series data and a firm fixed effects specification to address whether unobserved time-invariant firm characteristics explain an observed correlation between governance and market value. Yet most research relies on cross-sectional results, either because time series data is not available or because there is too little variation across time to make firm fixed effects feasible.

The principal goal of this paper is to exploit Korea's 1999 legal reforms, plus the rich data available on Korean firms, to address these empirical issues. Most countries, when they regulate governance, apply the same rules to all public firms. The regulated aspects of firm governance are then not endogenous, but there is no direct way to separate the effects of governance on value from other unobserved country characteristics. Korea, however, applied stricter board structure rules to large firms than to small firms. This exogenous change lets us identify the effect of the difference between large and small firm governance on Tobin's q or another dependent variable. We do so both in *size* through our instrumental variable approach (large firms have higher Tobin's q than smaller firms) and in *time* (the extra value of large firms appears precisely when it should, if governance is the underlying cause -- during the time period in 1999 when the reforms were announced and then adopted.

3.2 Multiple Identification Strategy; Comparison to Our Prior Work

In this section, we discuss our multiple identification strategy approach, and also how this paper differs from our other work on Korea. This section assumes some familiarity with Black, Jang and Kim (2006a). Readers can skip the discussion of our prior work without loss of continuity.

This paper is part of a series of papers on Korean corporate governance. Our other papers study primarily overall corporate governance. In Black, Jang and Kim (2006a) (BJK), we use cross-sectional data from 2001, and exploit the same instrument used here (a large firm dummy, which equals 1 if a firm's assets are over 2 trillion won) to estimate the relationship between an overall Korean corporate governance index on firm value. That overall index is composed of five subindices, for board structure, board procedure, shareholder rights, disclosure and ownership parity. BJK find that the large firm dummy instrument predicts higher Tobin's *q* (and market/book ratio) in 2001, shortly after the 1999 reforms became effective. They use a regression discontinuity approach, in which we control for a smooth parametric function of ln(assets), and find a discrete jump in Tobin's *q* at 2 trillion won.

In Black, Jang, and Kim (2006b), we ask, again using 2001 data, what firm characteristics predict firms' governance choices. In Black, Kim, Jang and Park (2007), we extend our governance index to cover the period from 1998-2004, study the effect of overall governance on value in a firm fixed effects framework, and examine possible channels through which governance may affect value.

This paper builds on Black, Jang and Kim (2006a) (below BJK). We focus here on whether we can use the 1999 legal changes in large firms' board structures to identify the effect of these changes on firm value. We use the remainder of our overall Korea Corporate Governance Index (KCGI) as a control variable. In BJK, we have what can be called identification in size: Tobin's *q* jumps at 2 trillion won. However, large firm dummy correlates significantly not only with board structure, but also with board procedure, shareholder rights, and disclosure. Thus, it is unclear exactly what we are instrumenting for -- a change in board structure alone, or a change in overall governance. We write there (paragraph structure not shown and defined terms consistent with this paper):

> Large firm dummy correlates strongly with Board Structure Index (r = 0.87), and positively but less strongly with the rest of *KCGI* (r = 0.51). We address here some issues that arise because of this difference. [Above], we use large firm dummy to instrument for all of *KCGI*. An alternative approach is to instrument only for Board Structure Index. This approach offers a clear link between the instrument and the instrumented variable. However, large firm dummy may predict changes in the rest of *KCGI* . . . . [and thus] likely does not predict Tobin's *q only* through Board Structure Index, as required for a valid instrument. [If we run *2SLS* and replace all of *KCGI* with Board Structure Index], the .0133 coefficient on instrumented-Board Structure Index is an upward biased estimate of the true coefficient, [because this estimate] captures the effect of large firm dummy on both Board Structure Index and the rest of *KCGI* . . . . [If] we add (*KCGI* - Board Structure Index) as an additional control variable . . ., [t]his reduces the overestimation but may still produce a biased coefficient, because *2SLS* assumes that (*KCGI* - Board Structure Index) is independent of large firm dummy, when it is not.

Thus in BJK, the large firm dummy instrument was useful, but provided only partial identification. It was therefore unclear to what extent board structure change, as opposed to overall governance change, was the source of large firms' higher Tobin's *q*.

In this paper, we use a multiyear framework to move beyond BJK and tighten the link between board structure and firm value, in a number of ways. First and most centrally, we use event study and *DiD* estimation to connect the higher value of large firms to the 1999 change in

time, as well as in firm size. The value jump appears precisely when it should -- when the law is adopted in 1999, even though it does not come into force until 2001. This makes it much more unlikely that an unobserved factor is driving the higher value of larger firms. The timing of the jump also makes it unlikely that a large-minus-small firm difference in other aspects of governance can explain the jump.

Second, we show that the value increase persists over time. Korean investors did not become enamored of better board structure at first, and then learn later that board structure really makes little difference. Over time, an increasing number of small firms get this market message, voluntarily reform their own boards, and obtain similar value increases.

Third, a value increase of the magnitude we observe should leave *some* traces in firm performance. These traces, however, should appear *not* in 1999, when the law is adopted, nor in 2001, when the 50% outside director requirement becomes effective, but thereafter, as board structure impacts firm behavior. This too, is what we find. The profitability of large firms rises, relative to small firms, beginning in 2002. Asset sales to related parties also decline after the reforms become effective. This evidence on the channels through which governance affects firm value is tentative, due to the time gaps involved and our inability to rule out other explanations. Still, perhaps as important as that profitability increased beginning in 2002, is that nothing happens before then. The 1999 value increase in large firms is not explained by contemporaneous changes in performance, nor by investors anticipating performance changes which occur at times that cannot be explained by the governance reforms.

Fourth, we return to *IV* estimation, and tighten the link between our instrument and board structure. While large firms score higher than small firms on aspects of governance other than board structure, this difference does not explain the value jump at 2 trillion won. The large-versus-small difference in other aspects of governance does not increase at the time when the law is adopted. Moreover, in a firm fixed effects framework, Board Structure Index strongly predicts Tobin's *q*, while the rest of *KCGI* is insignificant. Thus, although large firm dummy correlates with the rest of *KCGI*, it appears to predict the dependent variable, Tobin's q, *through*

board structure, not through the rest of *KCGI*. It is therefore appropriate in our IV analysis -- as it was not in BJK -- to treat large firm dummy as instrumenting for Board Structure Index, while controlling for the rest of KCGI. Any remaining bias in the coefficient on instrumented-Board Structure Index seems likely to be small. As we discuss below, any remaining bias is likely to cause our IV estimates to understate the full effect of board structure on firm value. We also strengthen the IV analysis by using the panel data framework to move from the OLS-based estimation in BJK to firm random effects estimation.

Our identification depends on a difference between large firms and small firms. However, firm size may directly predict the dependent variable (share returns in the event study, Tobin's *q* in the *DiD* and *IV* approaches), We therefore embed all three approaches in a regression discontinuity framework, adapted from labor economics (e.g., Angrist and Lavy, 1999), in which we separately control for the continuous effect of size on the dependent variable. This procedure hopefully separates the discontinuous impact of the governance change at 2 trillion won for large firms from the direct link of firm size.

Taking these steps together, we believe that this paper moves from the partial identification in BJK, where it was unclear whether large firm dummy should be seen as instrumenting for board structure or for all of *KCGI*, to much tighter identification of an effect of board structure on firm value:

- in size (at 2 trillion won);
- in time (when the legal reforms are adopted);
- in the aspects of governance for which we identify a value effect (board structure in particular, rather than governance in general k);
- in persistence of the value effect over time;
- through evidence on possible channels, with the channel effects appearing in time roughly when they should; and
- through consistent results across multiple approaches: event study, *DiD*, firm fixed effects, and *IV* results are statistically strong and consistent in magnitude.

Identification comes from the cumulative power of these different approaches in rule out competing explanations.

3.3 Econometric Advances

Each of the econometric techniques we use has well-known strengths and weaknesses. Each has been used before in corporate governance research. For example, BJK use IV estimation; Karpoff and Malatesta (1995) and Black and Khanna (2007) use event study methods to study a legal change that applies differently to different firms; Dahya, Dimitrov and McConnell (2007) use DiD analysis; Black, Love and Rachinsky (2006) use a firm fixed effects framework; Litvak (2007a, 2007b) uses both event study and DiD approaches, but in separate papers. We are not aware, however, of efforts to combine multiple approaches in a single paper, and exploit the synergy between them.

The combination of approaches has significant value. The identification in time offered by the event study and DiD approaches strengthens the identification in size offered by the IV analysis, and vice versa. Identification in time, combined with firm fixed effects analysis of whether other aspects of governance predict Tobin's *q*, permit us to identify the large firm dummy instrument with board structure specifically, rather than corporate governance generally. The persistence over time of the large-versus-small difference strengthens the event study results, by showing that investors' initial reaction to the proposed reforms was not reversed once investors had experience with the actual reforms. The firm fixed effects results for voluntary reforms by small firms strengthen the governance explanation for the large firm results. And so on. In the end, we can tell a story for what might be wrong with each individual identification strategy. But we can't invent a consistent story that explains the full family of results without reference to the value of board structure reforms.

The data requirements for applying several identification strategies are not trivial. Still, our results suggests that other studies, especially studies which use legal change as the basis for identification, might also benefit from applying multiple identification strategies together.

4. Data and Index Construction

4.1 Post-Crisis Legal Reform and Event Dates

Prior to 1998, few Korean firms had outside directors and almost none had 50% outside directors, except for a few banks and majority state-owned enterprises (SOEs). The corporate

law did not provide for an audit committee, or indeed for any separate committees of the board of directors. Following the 1997-1998 East Asian financial crisis (which was acute in the second half of 1997 and the first half of 1998), Korean firms began to introduce outside directors and other governance reforms, partly voluntarily and partly due to legal changes, including the 1999 reforms we focus on here.

Reforms adopted during 1998 required all public firms to have at least 25% outside directors. The corporate law was amended in 1999 to permit audit and other committees of the board. The 50% outside director, audit committee, and nominating committee rules for large firms were adopted in 1999, with the first important legislative event dates in early June, and legislative action in December, and the rules coming into force in 2000 and 2001. We discuss potential usable event dates in Part 4.

4.2 Sample, Governance Index, and Variables

We study Korean companies listed on the Korea Stock Exchange, excluding banks and SOEs (our sample would otherwise include 14 banks and 6 SOEs). We determine board composition at 6-month intervals from 1998-2004, relying on books published annually by the Korea Listed Companies Association (KLCA), containing information on each director of each Korean public company.[2]

To control for other attributes of firm governance, we also construct a detailed corporate governance index (*KCGI*) from 1998-2004. Observations of *KCGI* are at year-end, except for 2001, when we also have mid-year data. *KCGI* (0 ~ 100) consists of five equally weighted indices: Board Structure; Board Procedure; Shareholder Rights; Disclosure; and Ownership Parity. For details on *KCGI* and the underlying data sources, see Table 2 and Black, Kim, Jang, and Park (2007).

[2] We determine board composition at 6-month intervals by combining this year-end information with data on annual meeting dates for each firm.

Board Structure Index, which is the focus of this study, is composed of Board Independence Subindex (2 elements, 0 ~ 10), and Board Committee Subindex (3 elements, 0 ~ 10). Board Structure Index and its 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 director 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. In fact, the large firm mean rises from 0.20 in 1998 (one out of the 51 large firms had 50% outside directors, no firms had audit or other committees) to 12.47 in spring 2001.

Data comes from various sources. We take balance sheet, income, cash flow statement data, foreign ownership data, and original listing year from the *TS2000* database maintained by the KLCA; a list of companies affiliated with the top-30 *chaebol* from press releases by the Korean Fair Trade Commission; stock market data from the KSE; information on *ADRs* from JP Morgan and Citibank websites; and industry classification from the Korea Statistics Office.

Figure 1 shows the mean values of Board Independence Subindex and Board Committee Subindex over time for balanced panels of large (assets > 2 trillion won) and small Korean public firms, respectively. *Table 3* provides summary statistics for large firms, small firms, and the large-minus-small difference, for KCGI, Board Structure Index, its components, and the other indices within *KCGI*. *Table 4, Panel A* defines the principal variables we study in this paper; *Panel B* provides summary statistics for the principal independent variables. *Table 5* provides selected correlation coefficients.

5. Event Study Analysis

If the 1999 rules for large firm governance affect market value, investors anticipate this effect when the legislation is proposed, and legislative events can be identified with sufficient precision, then an event study can help to identify a causal impact of the reforms on market value.

5.1 Event Dates

Table 1 provides a full list of news announcements related to the 1999 legal reforms. The four event dates we judged to be most likely to be significant are in bold. Announcements on June 2-3, 1999 (event 1) indicated that the government would amend Korea's corporate governance rules, focusing on *chaebol* reform. It provided few details, but prior news stories make it clear that the reforms would focus on audit committees and on outside directors. Followup announcements on June 25 (event 2) and July 2 (event 3) provide details. A draft law circulated by the Ministry of Finance on August 25, 1999 (event 4) required large firms to have 50% outside directors and an audit committee with at least 2/3 outside directors.

Early statements stressed *chaebol* reform, rather than large firm reform as such. The first explicit statement that the reforms will be mandatory for "large" but not small firms comes on July 2 (event 3). However, as a practical matter, reform directed at *chaebol* firms was likely to involve a size threshold, since there is large overlap between large firms and *chaebol* firms and no general definition of which firms are members of a *chaebol* group.[3] Since the early announcements did not specify the size threshold for a firms to be considered large, we need to choose a size cutoff for treating firms as "large" in our event study. Later new stories and government announcements initially placed the size threshold at 1 trillion won. The threshold was later gradually raised to 2 trillion won. Because the first mention of the 2 trillion won level is in Sept. 1999, and our event dates are in June-August, we use 1 trillion won in assets at year-end 1998 as the dividing line between large and small firms. In robustness checks, we obtain

[3] The best available definition comes from an annual report by the Korea Fair Trade Commission (KFTC) which identifies firms which are members of one of the top-30 business groups (based on group assets), based on the previous year's financial statements. This definition is not suitable as a regulatory threshold since the composition of the top-30 groups changes over time. Of the 64 large plus firms in our event study, 50 are chaebol firms based on the 1999 KFTC report.

similar results if we use a 2 trillion won threshold. To distinguish this over-1-trillion-won group from the "large" firms (assets > 2 trillion won) we study below in our *DiD* and *IV* approaches, we refer to the over-1-trillion firms as "large plus" firms.

Later events seem less likely to be important, since the key elements of the reforms were already in place. There was doubt early in the process about what reforms the government would propose and which firms the reforms would apply to, but little doubt that the Korean legislature would adopt the government's proposal -- and indeed the legislature did so, without significant change, within a few weeks after the bill was submitted. The effective date for most firms to comply was the spring 2000 shareholder meeting for the audit committee and outside director nominating committee rules, and spring 2001 for the 50% outside directors rules.[4]

5.2 Event Study Methodology

We use two principal event study methods. First, we use a regression approach to estimate the returns to large plus firms over each event period. We compute cumulative market adjusted returns (*CMARs*) to all firms during the event period, relative to a "Small Firm Index" -- an equally weighted index of the returns to firms with assets < 1 trillion won which did not adopt 50% outside directors by the end of our sample period. Our hope is that the Small Firm Index will capture events occurring in Korea during the event period that affect all firms. We regress the *CMARs* on a "large plus" dummy variable (equal to 1 if a firm has assets over 1 trillion, 0 otherwise), and other variables of interest. The *CMARs* are the simple sum of daily market adjusted returns over a k-day event period from day τ to day (τ+k-1) is:

$$CMAR_i = \sum_{t=\tau}^{\tau+k-1} (r_{i,t} - r_{m,t})$$

Here $r_{i,t}$ ($r_{m,t}$) is the return to firm *i* (the Small Firm Index) on day *t*.

[4] The 50% outside director requirement was effective for annual shareholder meetings after April 2000. Since most Korean firms hold annual meetings in February or March, the effective date for most firms was spring 2001. Large firms were also required to have at least 3 outside directors by their 2000 annual meeting. We do not separately study the 3-outside-directors rule. This rule had two principal effects. First, it was a transition rule: Large firms needed to get to three outside directors in 2000 and to 50% outside directors in 2001. Second, the rule acts as a minimum board size requirement: It prevents prevent firms from meeting the 50% outside director rule largely by removing insiders from their boards.

A typical regression is:

$$R_i = \alpha + \gamma * D_{\text{large-plus}} + \sum_j (\lambda_j * X_j) + \varepsilon_i$$

Here $D_{\text{large-plus}}$ is a dummy variable for large-plus firms and $\mathbf{X}_j$ is a vector of control variables. The predicted *CMAR* to small firms is captured by the coefficient α on the constant term and should be close to zero for regression which do not include control variables. The predicted extra return to large-plus firms over a *k*-day event period is k*γ.

The event period is common to all firms in our sample. This makes it likely that individual firm returns violate the usual regression assumption that each observation is independent of other observations. Two potential source of dependence are that firms in the same industry could move together, or large (small) firms could move with other large (small) firms. We therefore use industry-group clusters, with industries computed based on 4-digit Korea Standard Industrial Classification codes in all regressions. We have 32 industries and 47 clusters (not every group-industry pair includes one or more firms), which is enough clusters to make the cluster procedure appropriate. We return to the problem of cross-sectional correlation of returns below.

We identify and drop outlier observations of the dependent variable (market-adjusted or abnormal return, cumulated over the event window) if a studentized residual obtained by regressing the dependent variable on a large plus firm dummy (assets > 1 trillion won at year-end 1998) is greater than ±1.96. This is because very high or low returns probably reflect investor reaction to firm-specific events rather than to governance rules.

Second, we conduct a standard event study of daily abnormal returns over each event period. For details on the event study methodology, see Brown & Warner (1985); MacKinlay (1997). For each firm, we compute cumulative abnormal returns (*CARs*) based on the usual market model, using the Small Firm Index as the market index. We estimate the market model during Jan.-May and Aug.-Dec. 1999. We exclude June and July, 1999, which include our core

event period:[5]

$$r_{i,t} = \alpha_i + \beta_i * r_{m,t} + \varepsilon_{i,t}$$

The cumulative abnormal return over a *k*-day event period from τ to (τ+k-1) is:

$$CAR_i = \sum_{t=\tau}^{\tau+k-1} \varepsilon_{i,t}$$

The customary test-statistic for the CARs for a single firm is a standardized abnormal return:

$$SCAR_i = \frac{CAR_i}{\sigma_i * \sqrt{k}}$$

Here σ_i is the standard deviation of firm i's daily abnormal returns $\varepsilon_{i,t}$ during the estimation period. The *SCARs* should be distributed approximately unit normal if the variance of abnormal returns is constant over the estimation period and the event window, and the number of days in the estimation period is large. A roughly one-year estimation period, such as the one we adopt here, is a common choice in event studies. Let *L* be the number of large plus firms. The corresponding test statistic for a portfolio of *L* firms, which is also distributed unit normal, is (MacKinlay, 1997, equation (20)):

$$z = \frac{\sum_{i=1}^{L} SCAR_i}{\sqrt{L}}$$

5.3 Graphical Overview of Event Study Results

Figure 2 provides a graphical overview of returns to an equally weighted index of large plus versus a similar index of small firms during 1999. Each index is set to 100 at year-end 1998. The two indices move together, through the first five months of 1999. They diverge beginning in June, at the time of event 1, and remain separated thereafter. This is broadly consistent with our story -- large plus firms gain relative to small firms *when* they should, if

[5] In robustness checks; we obtain similar results but larger standard errors if we estimate the market model over July 1, 1988 through May 31, 1999; these are due to three outlier returns during August and Sept. 1998 which involve news releases related to the East Asia financial crisis.

governance changes are driving share price changes. The divergence is not tied to overall share price changes. There is little divergence earlier in 1999, during a period of strongly rising share prices. The divergence appears instead later in 1999, when an equally weighted index of share prices (which is numerically dominated by small firms) is flat or even declining.

Figure 3 narrows the time period and shows the cumulative difference between the large and small firm indices from April 30, 1999 (roughly one month before event 1) to the end of 1999. Each index is renormalized to 100 at April 30, 1999. There is an overall rise, consistent with gradual release of information, or gradual investor assessment of the implications of the governance reform, during the period from event 1 through event 3, and no significant trend thereafter. If one focuses more narrowly on event dates 1-4, shown with vertical lines in the figure, there is a visually noticeable rise around and especially in the few days after events 1 and 3, a gradual rise around event 2 (consistent with the general upward trend during this period), and not much activity around event 4 (a rise in the few days before the event, consistent with anticipation, but this rise is gradually reversed over the next couple of weeks).

5.4 Event Study Results

5.4.1. CMAR Regression Results

Table 6, Panel A reports regression results for market-adjusted returns. Some notation: (X: -y, +z) means an event window centered around event X which runs from day -y in event time to day +z, with the event date set at 0. For event 1, there are announcements on both day 0 (June 3) and day +1 (June 4). We report results for two short windows -- (1: -1, +2) and (1: -2, +3). We also report results for a "medium window" (1: -2, +15) and a "long window" (1:-2, +25). The medium window is as long as we can go after event 1 without hitting event 2. The long window includes events 2 and 3 -- it runs through day +4 following event 3. In unreported robustness checks, we obtain similar results for other intermediate windows, and for event windows which begin earlier than -2 before event 1. There is no evidence of leakage or abnormal returns during the period preceding event 1. One can see this visually in Figure 3 -- abnormal returns are small prior to day 0 of event 1. For events 2-4, we report results only for

short (-1, +2) and (-2, +3) windows. Longer windows for these events would overlap with other events.

The short window returns for each event are positive and both economically and statistically significant, for events 1, 3, and 4. The cumulative return over the long window is 21.21% (t = 10.41). The pattern of returns is consistent with gradual release of information, or gradual investor assessment of the implications of the proposed reforms. We thus give the greatest weight to this long-window result.

Overall, the event study results provides evidence that investors reacted strongly and positively to expected regulation of large firms, centered on board independence and audit committees.

5.4.2. CAR (Event Study) Results

In *Table 6, Panel B,* we switch to an alternate methodology. Instead of regressing event period returns on the large plus dummy, we use a classic event study approach, and measure *CARs* relative to the Small Firm Index. We report results based on two sets of firm groupings: First, we combine firms into industry portfolios. This allows for cross-sectional correlation within industry, but assumes independence across industries (Brown & Warner, 1980, 1985).. These results are the most comparable with the *CMAR* results in *Panel A*, where we use industry-group clusters to address intra-industry and intra-group correlation. We also compute, but do not report, firm-level *CARs*, these are similar to the industry results. We view the industry results are more reliable because the firm-level results assume cross-sectional independence across firms. This is unlikely with firms in the same industry which share a common event date, and could result in upward biased z-statistics.

The classic event study results are consistent with the CMAR results in *Panel A* for all windows. The long window CAR is +22.2% (z = 11.44). For the (-2,+3) windows around each event, the *CARs* are positive and significant for all four events, including event 2. Thus, the regression-based *CMAR* and classic event study *CAR* results tell a consistent story; if anything, the *CAR* results are stronger.

A further response to the risk of cross-sectional correlation in event period returns is to combine all large firms into a single portfolio before running the event study. This fully controls for cross sectional dependence but reduces statistical power because, compared to the industry-portfolio approach, the test statistic no longer allows for differences across industries in the variance of daily returns. In the second set of results in *Panel B*, we implement this single portfolio approach, by computing the returns to an equally weighted portfolio of large firms.

The CARs weaken, as expected, but remain reasonably strong. For the long window, the CAR increases to 23.6% (z = 3.39); the short-window results for events 1 and 3 also remain significant. For the remaining two short windows, the coefficients are economically meaningful, and indeed higher with than with industry portfolios, but we lose significance due to higher standard errors.

As to the choice between industry-based portfolios and a single portfolio of large firms, the event study literature suggests that industry portfolios are a reasonable compromise between test power and the potential for cross-sectional correlation to produce biased standard errors. Brown and Warner (1985, p. 22) find significant misspecification if all sample firms come from the same industry, but suggest that if firms come from different industries, there can be "gains from procedures assuming independence . . . even when . . . all securities of a given sample have the same event date." Bernard (1987, at 11 and Table 1) concurs that *intra*industry correlation is important when firms have a common event date, but finds that "*inter*industry cross-sectional correlation is small relative to intraindustry correlation" (emphasis added). He concludes that interindustry correlation usually should not produce serious bias in event study standard errors.

5.5. Robustness Tests

We apply a battery of additional robustness checks to our results. We obtain similar results if we (i) do not use industry-group clusters in the *Panel A* regressions; (ii) use log returns instead of fractional returns, (iii) use "jump" (buy-and-hold) returns for the entire window instead of summing daily returns; (iv) vary the estimation period for the *CAR* results; (v) do not exclude outliers in the CMAR results (coefficients are similar and remain significant for events 1,

3, and 4; coefficient increases for event 2 and becomes significant; note that the CAR results already do not exclude outliers); and (vi) winsorize extreme firm-level returns at 1%/99%, on the grounds that these returns likely reflect firm-specific news rather than a reaction to governance. We constructed the Small Firm Index using "unreformed" small firms, which did not adopt 50% outside directors during or sample period, for consistency with the *DiD* analysis below, but obtain similar results with a small firm index that includes all firms with assets < 1 trillion won.

Early announcements, especially for events 1 and 2, indicated that governance reform would focus on *chaebol* firms, rather than large firms as such. We have limited power to determine whether are results are driven by the expectation of governance reforms for chaebol firms, rather than large firms, because our sample includes only 14 "large plus" non-chaebol firms, and only 19 small chaebol firms. However, we find no evidence that investors reacted differently based on a firm's chaebol status. In unreported regressions using the long window, the returns to large plus (small) chaebol firms are similar to those to large plus (small) non-chaebol firms.[6]

5.6 Regression Discontinuity Results

An important issue for an identification strategy based on firm size is whether we might be observing a size-based effect, which is correlated with but unrelated to the regulatory size threshold. For the event study, the question would be: Perhaps large firm shares outperformed small firms during the event windows for reasons unrelated to regulation?

We address this concern in a number of ways throughout this paper, but summarize our overall approach here. First, for the event study, the narrower the event window, the less likely this alternate explanation will be. Yet we obtain positive returns over narrow event windows around events 1, 3, and 4 and for event 2 in some specifications . Second, we search and do not find news announcements during the period of legislative reform suggesting that this is an unusually good time for large firms. Third, our *DiD* results for Tobin's *q* show that the average

[6] We find evidence of a positive reaction by small *chaebol* firms to event 1, but this dissipates over a longer window period. This pattern is sensible. The event 1 announcement focused on *chaebol* reform, but by event 3, it was clear that reform would focus on large firms, rather than *chaebol* firms as such.

Tobin's *q* of large firms jumps relative to small firms during the legal reform period, but there are no similar jumps in other periods, either before and after the legal change. Fourth, our *DiD* results for firm performance show no near-term jump in the financial performance of large firms, following the 1999 share price jump. Large firm profitability improves relative to small firms, but several years later, after the board reforms become effective. This timing is consistent with governance contributing to the performance gains.

Fifth and most centrally, for each of our approaches, we apply regression discontinuity techniques, in which we control for a smooth parametric effect of firm size on the dependent variable of interest. We show the results of the regression discontinuity approach in *Table 7* for the event study; later tables apply it to our other methodologies.

5.6.1. Basic Regression Discontinuity Results for Event Study

In *Table 7, Panel A*, *Regression Set 1*, we rerun the *CMAR* regressions from *Table 6*, adding ln(market capitalization) at May 31, 1999 as an additional control variable. We show results for the short (-2,+3) windows around events 1, 3, and 4, plus the long window. We omit event 2 because we already know from *Table 6* that the CMARs for this window are insignificant. Standard errors increases, as is common in regression discontinuity analysis, but the coefficients on large plus dummy remain significant. For the long window, the CMAR is 16.7% (t = 4.28), compared to 21.2% (t = 10.41) without the firm size control.

Moreover, the coefficient on ln(market cap) is positive but modest at .0191 and not statistically significant. This is evidence that we are not simply capturing a size effect. They thus support the hypothesis that governance, rather than something else connected to firm size, is driving share price returns during the event period.

The results with ln(market cap) as a control variable are subject to the following concern: If there is a relationship between firm size and share returns over the event window, we do not know the functional form for that relationship. We controlled for a connection that is linear in ln(market cap), but that might not be the correct functional form for the underlying relationship. We respond to this concern in *Table 7, Panel A, Regression Set 2*, by using a more flexible

functional form for firm size. We control for the first six powers of ln(market cap), in order to provide a flexible functional form for the possible relationship between firm size and event period returns. The coefficients for all windows remain similar in magnitude and significant or marginally significant. The overall return over the long window gets *stronger* -- the coefficient (t-statistic) rises to 17.5% (t = 4.72). This is evidence that the event period returns to large plus firms do not reflect a general trend for larger firms to earn high returns during this period.

5.6.2. Regression Discontinuity: Extensions

The question of whether we might be capturing a general size effect, which loosely coincides with the regulatory threshold, is central to our event study, *DiD*, and *IV* results. In *Table 7, Panel B*, we therefore explore variations on the regression discontinuity theme, focusing on the long event window. Regression (1) reproduces the 6-powers result for this window from *Panel A*. Regression (2) switches from the large plus dummy (= 1 for firms with assets > 1 trillion won) to a large firm dummy (= 1 for firms with assets > 2 trillion won, which was the eventual regulatory threshold). We obtain similar results with the 2 trillion won threshold.

Regressions (3-4) add a battery of 19 additional firm-level control variables (the same control variables we use in Table 10, except for ln(assets). The control variables include firm age, leverage, growth, profitability, ownership concentration, capital asset intensity, R&D intensity, and so on. The coefficients and t-statistics on the large firm dummy variables increase. In regression (3) the coefficient on the large-plus dummy rises to 28.1% (t = 5.43).

Overall, after controlling for 6 powers of ln(market cap) plus a wide array of other firm characteristics, large plus firms gained 28.1%, relative to small firms, during the roughly 6-week window period around events 1-3. This is a dramatic difference in returns, and yet it appears unlikely that the event study is picking up a size-based difference in share returns over the event period, which is correlated with but unrelated to the governance rules.

Finally, the 28.1% gain to large firms, with full controls, relies on *CMARs*, which are a sum of daily returns. As is often the case when returns are positive over a period of time, buy-

and-hold returns for the entire window period are larger. In the buy-and-hold equivalent to *Table 7, Panel B, regression (3)*, long window returns to large-plus firms are 33.7% (t = 5.31).

5.7 Other Countries

If the period of June and July 1999 was good for large firms, for reasons unrelated to governance, it may have been good for large firms in other similar countries. We therefore study the returns to large firms in another nearby country which was also affected by the East Asian financial crisis, and also trades extensively with Mainland China -- Taiwan. We conduct an event study of the monthly returns to xxx Taiwanese firms over 1998-1999, using different size thresholds, including those corresponding to the large plus dummy (1 trillion won) used in our event study and the large firm dummy (2 trillion won) used in the rest of this paper. Large Taiwanese firms somewhat underperform small ones during 1998, catch up during Jan-Feb 1999, and have similar returns for the rest of 1999, including the period later in 1999 in which large Korean firms do well. The Taiwanese evidence is consistent with governance being the driver of the returns to large Korean firms.

6. Difference-in-Differences Analysis

We turn next to an alternative empirical procedure for identifying the governance reforms with a value increase for large firms, *at the right time* (when the reforms are adopted). If investors assign higher value to firms with 50% outside directors and an audit committee, then large firms should experience an increase in Tobin's q between May 1999 (just before the legislative reforms began) and the end of 1999, when the legal rules requiring these governance elements are adopted, controlling for other facts that affect Tobin's q. This should be the case even though the reforms come into force only later, during 2000 and 2001. One would not expect similar gains for large firms, relative to small firms, at other times.

We employ difference-in-difference analysis, with large firms (as of May 31, 1999) as the treatment group, "nonreformed" small firms as the control group, and May 31, 1999, as the base period. We compute Tobin's q at 6-month intervals from June 30, 1996 through Dec. 31, 2004, with one exception. June 30, 1999, would fall in the middle of the legal reform period, so we

move the measurement date by one month, to May 31, 1999, which precedes the reforms.[7] We exclude large firms that have 50% outside directors at May 31, 1999 from the treatment group, and "reformed" small firms, which later voluntarily adopt 50% outside directors, from the control group. We use Tobin's *q* as our principal measure of firm value, but in robustness checks, we obtain similar results for market/book. In the text, we discuss primarily the Tobin's *q* results.

In our *DiD*, firm fixed effects, and *IV* regressions, whenever we use Tobin's q or market/book as dependent variables, we take logs to address skewness in the non-logged values. We also identify and drop outliers for each year if a studentized residual, obtained from a regression of *ln*(Tobin's *q*) (or market/book) on the principal independent variable, is greater than ±1.96. In robustness checks, we obtain similar results if we do not exclude outliers and if we use non-logged dependent variables (though weaker results for market/book, which has some extreme outliers for firms with low book values of equity).

6.1. Basic *DiD* Results

In our main DiD specification, we use *ln*(Tobin's *q*) as the dependent variable. All regressions use firm fixed effects, period dummy variables, firm clusters, and Rogers' (1993) heteroskedasticity-consistent standard errors.[8] The specification is:

$$\ln(\text{Tobin's } q)_{it} = \alpha + \lambda L_{i,t} + \sum_{t=06/1996}^{12/2004} \beta_t L_{i,t} D_t + \sum_{t=06/1996}^{12/2004} \gamma_t D_t + \sum_{i=1}^{N} \mu_i F_i + \varepsilon_{it} \quad \text{---------- (1)}$$

Here:

- $L_{i,t}$ is a large firm dummy variable, which equals 1 if firm *i* is large at date *t* and 0 otherwise. This variable is time-varying only for firms whose size changes between small and large.

[7] We use 6-month periods because we have financial data available every six months. In measuring Tobin's *q* at May 31, 1999, we estimate book value by interpolating between Dec. 31, 1998, and June 30, 1999. In robustness checks, we obtain similar results if we do leave the use June 30, 1999, as the base date instead of May 31, 1999.

[8] We use both firm fixed effects and clusters because simulation studies by Petersen (2007) and Kezdi (2004) provide evidence that using firm fixed effects without clusters in a typical finance panel data set (large N, moderate T) can produce downward biased standard errors. Peterson (2007); Kezdi (2004). Bertrand, Duflo and Mullainathan (2004) recommend state clusters (analogous to firm clusters) in a *DiD* framework, but do not directly evaluate the combination of firm clusters and firm fixed effects.

- D_t is a period dummy variable which equals 1 for period t, and 0 otherwise. We omit the period dummy for the base date of May 31, 1999.
- F_i is a firm dummy variable, which equals 1 for firm i and 0 otherwise.
- t takes values, at 6-month intervals, from Dec. 31, 1996 through Dec. 31, 2004 (with May 31, 1999 substituted for June 30, 1999).

The coefficient on the large firm dummy largely captures the average difference in Tobin's q between large and small firms at the base date. The coefficients of interest are β_t, the coefficients on the interaction term between the large firm dummy and the period dummies. If the board structure rules positively affected Tobin's q, these coefficients should be insignificant for periods ending prior to and including May 31, 1999, and positive and significant for periods beginning Dec. 31, 1999. This is what we find. *Table 8* reports regression results. The regressions cover June 1996, through Dec. 2004; we report results only for Dec. 1997 through Dec. 2001, but obtain similar results for earlier and later dates.[9]

In *Table 8*, *regression (1)*, the mean *ln*(Tobin's q) for large firms jumps by 0.16 in the second half of 1999 (t = 3.58), and is stable both before and after this period. This effect is economically important. For a large firm with median Tobin's q (0.97) and leverage (0.68), the 0.16 increase in ln(Tobin's q) implies an 57% increase in share price.[10]

Figure 4 provides a similar graphical picture. It shows mean Tobin's q and mean market/book for a balanced sample of large firms, relative to a balanced sample of nonreformed small firms, from December 1997 (four semiannual periods before the treatment) to December 2001 (four semiannual periods after the treatment). The gap between the two groups is set to zero at May 31, 1999. There is a visually obvious jump from May 1999 to December 1999, and no trend before or after.

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[9] The insignificant results prior to the base date are inconsistent with one possible competing explanation for our results -- that large firms suffered more than small firms in the East Asian financial crisis, which was concentrated in the second half of 1997 and the first half of 1998, and then rebounded with a lag in the second half of 1999, for reasons unrelated to governance.

[10] Since Tobin's q is (debt/assets) + (market equity/assets), New Tobin's q = Old (debt/assets) + Old (market equity/assets) * (1+R), where R is the fractional share price increase. This equation can be solved for R if we know old q, new q, and debt/assets.

The DiD results thus provide evidence consistent with the event study. They strengthen the identification of governance with firm value in both time and size -- large firms gain, relative to small firms, when they should if the value increase responds to the legal changes. They do not gain or lose relative to small firms not at other times.

6.2. Regression Discontinuity and Other Robustness Checks

6.2.1. Regression Discontinuity Analysis

In *Table 8*, *regression (2)*, we implement the regression discontinuity approach, by also controlling for the first 6 powers of ln(assets). The coefficients and t-statistics decline only slightly. The estimated change in ln(Tobin's *q*) for large firms, relative to small firms, is 0.15 (*t* = 3.36).

In *regression (3)*, we add the full set of 19 additional firm-level control variables, to control for other time varying factors which might affect Tobin's *q*. Results are almost identical to *regression (2)*. As for the event study, the regression discontinuity regressions provide evidence that we are not simply capturing a size effect.[11]

6.2.2. Event-Like Specification

In the *DiD* approach in *Table 8*, we study changes in large firm Tobin's *q*'s, relative to a single base date (May 31, 1999). In unreported robustness checks, we also implement an "event-study-like" specification, in which we study changes in the Tobin's *q* of large firms from one period to the next, controlling for changes in the Tobin's *q* of unreformed small firms. The specification is similar to a standard event study of buy-and-hold returns over consecutive 6-month periods, except that the dependent variable of interest is ln(Tobin's *q*), instead of share price returns. More specifically, we estimate:

$$\ln(\text{Tobin's } q)_{i,t} = \alpha + \beta * Index_t + \sum_{t=12/1997}^{12/2001} (\gamma_t * E_t) + \varepsilon_{it} \text{ ---------- (2)}$$

[11] In the regression discontinuity regressions for our event study, we used ln(market capitalization) as a size measure. In the DiD analysis, we use Tobin's *q* as dependent variable. We therefore need a different size measure on the right hand side, because Tobin's *q* is a scaled version of market capitalization. We therefore use ln(assets) as a size measure. In robustness checks, we obtain (i) similar *DiD* results with ln(sales) as a size measure, (ii) similar *DiD* results if we control only for ln(assets) instead of 6 powers of ln(assets), and (iii) similar event study results if we use one power, or six powers, of ln(assets) or ln(sales) as a size measure.

Here:

- $Index_t$ is the mean *ln*(Tobin's *q*) of unreformed small firms at date *t*.
- E_t is an "event" dummy, which equals 1 for time *t* and after, and 0 for earlier periods.

The time periods are the same as in our *DiD* specification; we again use firm fixed effects and firm clusters. If legal reform affects the share prices of large firms in the second half of 1999, then $\gamma_{12/1999}$ should be positive and significant. The mean *ln*(Tobin's *q*) of large firms in fact jumps by 0.14 in the second half of 1999 (*t* = 3.43). Coefficients for other time periods are small, insignificant, and have varying sign.

6.2.3. Additional robustness checks.

To further address whether there was an economic shift in the second half of 1999, which benefited larger firms but was unrelated to governance, we rerun the regressions in *Table 8* using unreformed *mid-sized* firms as the control group, with similar (unreported) results. For example, in *Table 8*, *regression (1)*, if the control group is the 117 unreformed mid-sized firms with assets from 250 billion to 1.5 trillion won at June 30, 1999, the coefficient on (Dec. 1999 dummy * large firm dummy) declines only slightly to 0.1550 (*t* = 3.17), compared to 0.1608 (*t* = 3.58) in *Table 8*, *regression (1)*.

In additional robustness checks, we obtain similar results with a variety of alternate specifications: (i) without firm fixed effects, (ii) with fixed effects but without firm clusters (standard errors are somewhat smaller without firm clusters, as expected); (iii) with ln(market/book) instead of ln(Tobin's *q*) as a dependent variable, (iv) with the difference in ln(Tobin's *q*) from the base date as the dependent variable instead of ln(Tobin's *q*); and (v) for the post-reform period as a whole (we use monthly values of ln(Tobin's *q*) as the dependent variable and find a positive coefficient on the interaction between a post-reform dummy variable and the large firm dummy). *Table 8*, *regression (4)* shows results similar to *regression (1)* but with ln(market/book) as dependent variable.

6.3. DiD Results for Profitability and Dividends

If investors are correct in valuing large firms at a premium after the 1999 legal changes, this should leave some traces in their subsequent performance. However, any effect should be delayed, because the board structure rules come into force in part in 2000, but in part only after the Spring 2001 annual shareholder meetings. Also, a change in board structure is likely to affect firm behavior with a lag of uncertain duration. We investigate several measures of firm financial performance:

- Capital expenditures, scaled by firm size or prior level of capital expenditures
- Sales growth
- Firm profitability, measured as EBIT/assets, ordinary income/book value of equity, and net income/book value of equity, winsorized at the 5^{th} and 95^{th} percentiles. Ordinary income is defined as net income before taxes and extraordinary items (but after interest payments). This measure has been used in other work as an alternative measure of the profitability of Korean firms (e.g., Joh, 2003).
- ln(dividends, scaled by firm size (assets or sales) and by measures of profits).

We use the *DiD* specification in equation (1) above, with different dependent variables, and annual instead of semiannual data. We continue to use firm fixed effects and firm clusters. In robustness checks, we obtain similar results with without firm fixed effects, with firm fixed effects but without firm clusters (standard errors are somewhat smaller without firm clusters), and with the difference in the dependent variable from the base date as the dependent variable.

In unreported regressions, we obtain similar results for logged measures of profitability or if we exclude outliers instead of winsorizing, and similar but weaker results if we neither take logs nor winsorize. For dividends, we use ln(dividends/sales) as the dependent variable; the logarithmic transform for dividends limits the sample to dividend paying firms. In unreported regressions, we find similar results for ln(dividends/assets), but do not find a significant after-minus-before change in the likelihood that a firm will pay dividends, or the ratio of dividends to profits. We also find no significant change in capital expenditures or growth rates.

Table 9, Panel A reports results with year dummies and interactions between the year dummies and large firm dummy. Consider profitability first. The year dummies control for overall changes in profitability; the coefficient on the interaction term shows large firm profitability relative to nonreformed small firms. Large and small firms show similar profitability through 2001. Large firm profitability ticks up in 2002, and becomes significantly higher than small firm profitability in 2003 and 2004. The higher profitability is economically meaningful. For example, for EBIT/assets, the extra profitability of large firms is 3.46% in 2004.

In *Panel B*, we replace the year dummies with a single reform period dummy (= 1 for year-end 2002 and after). Large firms gain in profitability during the post reform period taken as a whole. The average post-reform increase in EBIT/assets is 2.47% (t = 4.90).

The story on dividends is generally similar. Prior to and during the 1997-1998 economic crisis, large firms paid quite small dividends, as suggested by the large negative coefficients for 1996 and 1997, interacted with the large firm dummy. Dividend rates are similar during 1999-2001, and large firm dividends rise beginning in 2002, in tandem with profitability. In unreported regressions, we do not find evidence that large firms raise their payout rates as a fraction of profits.

The profitability and dividends results in *Table 10* provide evidence on a possible channel through which board structure may affect firm value. However, because of the time gap between the share price gains in 1999 and the profitability gains beginning in 2002, they are only suggestive. Economic conditions in Korea may have changed generally to favor large firms during 2002-2004, and this change happened to follow the legally required board structure changes. However, our results are consistent with Dahya and McConnell (2007), who report that UK companies which add outside directors to comply with the Cadbury Committee recommendation for a minimum of 3 outside directors subsequently show higher profitability. Perhaps as important as the results we find for profitability and dividends is what we do *not* find: We find no evidence of a change in profitability, dividends, growth, or any other measure of

firm-level outcomes, which occurs subsequent to the 1999 share price rise and hence could explain the share price rise, yet occurs *too soon* to be consistent with legal reform contributing to the change in firm-level outcomes.

6.4. Profitability and Dividends: Regression Discontinuity Results

We next evaluate whether our results for profitability and dividends are robust to application of the regression discontinuity approach. We report results for *EBIT/assets* in *Table 9, regressions (5-6)*. In *regression (5)*, we add *ln*(assets) as a control variable. In *regression (6)*, we add the first 6 powers of *ln*(assets). The coefficients on large firm dummy interacted with the year-end 2003 and 2004 dummies decline slightly; but the results remain strong. For example the coefficient on the 2004 interaction term drops from .0346 (t = 3.75) in regression (1) to .0309 (t = 3.16) in regression (6).

We conclude that the increase in profitability, following implementation of the reforms, is not simply a size effect. Over the time period from 1996-2004 as a whole, there *is* a strong association between firm size and profitability, as shown by the economically important and statistically strong coefficient on ln(assets) in regression (5). But this overall association does not drive our *DiD* results. We caution that because higher profitability is not simply a size effect does not mean it is a governance effect -- we claim only that profitability is a possible channel through which governance can affect value, and that our results are consistent with that interpretation.

In unreported regressions, we obtain similar regression discontinuity results for (i) the other dependent variables in *Table 9, Panel A*; and (ii) for all dependent variables, for the post-reform period as a whole, in regressions similar to *Table 9, Panel B*.

6.5. Control of Related Party Transactions

We also investigate the possibility that outside directors and audit committees might increase firm value by controlling related party transactions, which are known to be problematic in Korea (e.g., Baek, Kang, and Lee, 2006). As is often the case when studying related party

transactions, data availability is a major concern. Korean disclosure rules require firms to report related party transactions, but fairness of price is not observable.

Table 9, Panel C summarizes our results for four types of related party transactions: loans to related parties, borrowing from related parties, sales of goods and services to related parties, and sales of assets to related parties. We find no evidence that the volume of loans or borrowings changes after the legal reforms take effect (regressions (1) and (2)). We similarly find no evidence that the volume of sales (or, in unreported regressions, purchases) of goods and services from related parties changes (regression (3)). In unreported probit regressions, we also find no evidence that firms with independent boards are less likely to engage in these classes of transactions at all. Of course, review of these transactions by outside directors could still affect value by ensuring that the prices for these transactions are on market terms, or by giving investors comfort that the transactions will be fair.

We do find evidence, in *regression (4)*, that the board structure reforms predict lower sales of assets to related parties. Thus, control of asset sales to related parties provides another possible channel through which outside directors may contribute to firm value. In unreported regression discontinuity regressions, the coefficient and *t*-statistic in this regression are almost unchanged if we add ln(assets) or its first 6 powers as additional control variables.

7. Firm Fixed Effects Regressions

The event study and *DiD* results presented above provide evidence that investors assigned positive value to board structure reforms for large Korean firms. They cannot, however, tell us (i) whether similar changes would have similar value at "small" firms (assets < 2 trillion won), or (ii) how much of the value increase is due to each of the three new requirements -- 50% outside directors, audit committees, or nominating committee. Our regression discontinuity results make it less likely, but do not fully exclude the possibility, that a *non-governance* shock to the expected future profitability of large firms explains both the jump in market value of large firms in the second half of 1999 and their higher profitability several years later.

To address these issues, we study all Korean firms, both large and small, by pooling observations of all firms for all years from 1998-2004 and estimating the effect of board structure on firm market value while controlling for time trends with year dummies, for time-invariant firm characteristics with firm fixed effects, and for time-varying firm characteristics with an extensive set of control variables, which are described below.[12] In computing standard errors, we use Rogers' heteroskedasticity-consistent standard errors, together with firm clusters to control for the possibility that residuals are correlated within firm across years (Kezdi, 2004; Petersen, 2007).

The firm fixed effects approach achieves several goals. First, small firms can adopt only some of the governance measures that are required for large firms, and can adopt different measures at different times. We use this variation to estimate the separate effects on firm value of different aspects of board structure. Second, we obtain similar results for small firms as in the *DiD* analysis for large firms. This strengthens the inference that governance, rather than a non-governance shock affecting large firms, explains our large firm results. Third, the fixed effects framework lets us assess the value of aspects of board structure which are not legally required -- having more than 50% independent directors or a compensation committee. Fourth, the differences between pooled OLS and firm fixed effects are sometimes large, which supports the criticisms of *OLS* methodology in corporate governance research.

For small firms, and for non-mandated elements for large firms, we lack an exogenous shock or a good instrumental variable. We thus cannot rule out the possibility that omitted time-varying firm characteristics explain both the change in governance and the observed change in firm market value. Still, firm fixed effects are a significant advance over prior studies of board structure in emerging markets, which rely on cross-sectional evidence only (e.g., Dahya, Dimitrov and McConnell, 2007; Choi, Park, and Yoo, 2007).[13] In particular, we can assess the

[12] For the *DiD* results we can go back to 1996 because we required information only on whether a firm had 50% outside directors. For firm fixed effects results, we lose 1996 and 1997 because we require full information on governance. We judged that the extensive work needed to hand-collect this information was not justified prior to 1998, because almost no firms in our sample had adopted any board structure reforms we study before this date.

[13] Choi, Park and Yoo (2007) have time series data, so they can potentially use fixed effects, but they report that

sensitivity of cross-sectional results to inclusion of firm fixed effects -- and we indeed find reason to doubt the reliability of cross-sectional estimates. Also, the similar results for large and small firms strengthen the inference for each group that governance explains our results.

7.1. Firm Fixed Effects Methodology

In Korea, outside directors were rare prior to the East Asian financial crisis, but were rapidly adopted thereafter, partly by large firms due to the legal rules discussed above, but partly voluntarily. Audit committees, too, were rare prior to the crisis, but have since been voluntarily adopted by a significant number of small firms. By the end of our sample period, 44 small firms (10% of our sample) had voluntarily adopted 50% outside directors, 59 firms (9% of our sample) had more than 50% outside directors, 67 small firms (15% of our sample) had voluntarily adopted audit committees, and 90 small firms (21% of our sample) had voluntarily adopted an outside director nominating committee. See *Table 3*. These changes provide enough time variation to make it feasible to implement a firm fixed effects specification.

We conduct firm fixed effects regressions for the full 1998-2004 period. We report selected results for pooled *OLS* (with firm clusters) and firm random effects specifications, partly to compare with fixed effects results, and partly because our two stages and three-stage least squares analysis above is limited to pooled *OLS* and firm random effects specifications. All regressions use year dummies and Rogers' (1993) robust standard errors. OLS and firm fixed effects specifications include firm clusters; clustering is not available with random effects.

A Breusch-Pagan (1980) Lagrange multiplier test rejects the pooled *OLS* model compared to the alternative of firm random effects, with a *p*-value close to zero. Thus, pooled *OLS* results may be biased. We present selected *OLS* results only for comparison with other work which relies on cross-sectional results, and to highlight the extent to which *OLS* specifications may produce biased coefficients and standard errors.

The choice between random and fixed effects specifications is a closer one. The fixed effects model has the advantage of not requiring that the firm effect be uncorrelated with the

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their results disappear if they try.

independent variables. It has the disadvantage of using many more degrees of freedom and using only information from within-firm variation, while random effects can also use information from between-firm differences. Hausman's (1987) test for whether random effects are acceptable against the alternative of fixed effects usually, but not always, rejects random effects, depending on the specification. Thus, random effects coefficients may be biased relative to fixed effects coefficients, but hopefully not severely so.

In addition to biased coefficients, the random effects specification is likely to have somewhat downward biased standard errors (hence upward biased t-statistics). With firm fixed effects, we use firm clusters; this increases standard errors by a factor of 1.25 - 1.4 (hence decreases *t*-statistics by a factor of 0.7 - 0.8) depending on the regression specification. For example, in our base unbalanced panel results in *Table 10*, regression (4), we report a t-statistic of 6.58. Without firm clusters, this would increase to 8.35 (an increase by a factor of 1.27). With random effects, clustering is not available; yet a similar inflation of t-statistics is likely if we fail to cluster. As an ad hoc approach, it seems reasonable to multiple the *t*-statistics we report below for the random effects specification by a factor of 0.75.[14]

7.2. Control Variables

Firm fixed effects can address bias due to time-invariant omitted variables that predict both board structure and Tobin's *q*, but not bias due to time varying factors that might predict both board structure and Tobin's *q*, We employ an extensive list of control variables to limit the extent of omitted variable bias. The rationale for each control variable is described below.

Different aspects of governance often correlate with each other. For example, firms that change their board structure may also change board procedures; firms that adopt independent boards may be more likely to adopt good disclosure practices; and so on. We address this possibility by controlling in all regressions for the portion of *KCGI* that is not captured by the board structure variables. Thus, in regressions with Board Structure Index as the principal

[14] The setups for random effects and fixed effects are similar, so it seems reasonable to expect a similar degree of reduction of standard errors due to failure to cluster. However, we can offer no citations, because we found no discussion of this issue in the literature.

board structure variable, we control either for (KCGI - Board Structure Index) or sometimes separately for Ownership Parity Index and for (*KCGI* - Board Structure Index - Ownership Parity Index).[15] In regressions with Board Independence Subindex as the principal board structure variable, we control for (*KCGI* - Board Independence Subindex). And so on.

We use *ln*(assets) to control for the effect of firm size on Tobin's *q*. In unreported robustness checks, we obtain similar results if we use the regression discontinuity specification, with the first 6 powers of ln(assets), and if we substitute ln(sales) or its first six powers for ln(assets).

We include *ln*(years listed) as a proxy for firm age, because younger firms are likely to be faster-growing and perhaps more intangible asset-intensive, which can lead to higher Tobin's *q*. We include leverage (measured as debt/market value of common equity) because it can influence Tobin's *q* by providing tax benefits and reducing free cash flow problems.

We control for firms' growth prospects using geometric average sales growth over the past five years and capital expenditures relative to the historical capital stock (capex/PPE). We control for intangible assets using (R&D expense)/sales and (advertising expense)/sales. To control for capital intensity, we include PPE/sales and $(\text{PPE/sales})^2$. We control for profitability measured by *EBIT*/sales. As measures possibly related to profitability or product market constraints, we include exports/sales and market share. Korean policy, especially prior to the East Asian financial crisis, favored export industries; this could affect profitability and Tobin's *q*.

We include share turnover (traded shares as a percentage of public float) as a measure of liquidity, since share prices may be higher for firms with more easily traded shares. We measure ownership as ownership by the largest single shareholder, and include ownership^2 to allow for possible nonlinearity in the relationship between inside ownership and share prices. We include fraction of foreign ownership because foreign investors are diversified and may be

[15] We obtain almost identical results for Board Structure Index and its components, whether we control for (*KCGI* - Board Structure Index), or control separately for Ownership Parity Index and (*KCGI* - Board Structure Index - Ownership Parity Index). We control separately for Ownership Parity Index in *Table 10* to highlight the differences between pooled *OLS* and firm fixed effects, and because this breakout is useful in the discussion of instrument validity in Section 8.1.

willing to pay higher prices than domestic investors, thus affecting Tobin's *q*. They may also pressure firms to improve their governance, or invest in better governed firms.

For *OLS* and random effects specifications, we include several additional variables which we omit with firm fixed effects because they have no or minimal time variation. Since both board structure and Tobin's *q* may reflect industry factors, we include industry dummies based on 4-digit Korea Standard Industrial Classification (KSIC) codes. We include a *chaebol* dummy because firms that belong to a *chaebol* group may have stronger political connections, access to financing, or be more diversified, which could affect Tobin's *q*. We include *ADR* dummies, which can proxy for foreign investor interest, liquidity, and compliance with U.S. disclosure standards. Firms with level 1 *ADR*s are traded on *NASDAQ* but are not subject to U.S. disclosure rules. Firms with level 2 or 3 *ADR*s must comply with U.S. accounting and disclosure rules and tend to have higher Tobin's *q* (Doidge, Karolyi, and Stulz, 2004). We include a dummy variable for a firm's inclusion in the Morgan Stanley Capital International Index for East Asia (*MSCI* dummy), which may proxy for price pressure due to purchases by index funds, greater liquidity, and foreign investor interest.

7.3. Results for Board Structure and Its Components

In *Table 10*, *regressions (1-4)*, we show the results of pooled *OLS*, firm random effects, and firm fixed effects (using unbalanced and balanced panels) models with *ln*(Tobin's *q*) is the dependent variable, and Board Structure Index as the principal right-hand side variable. *Regression (5)* reports fixed effects, unbalanced panel results with *ln*(market/book) is the dependent variable. Our textual discussion focuses on the firm fixed effects, unbalanced panel results for Tobin's *q*. Fixed effects results are similar for balanced panels, although we lose roughly 45% of observations and 55% of firms with a balanced panel. Later tables report only the unbalanced panel specification, but we find similar (unreported) results with balanced panels.

The coefficient on Board Structure Index is statistically strong (t = 5.81) economically meaningful. A worst-to-best improvement in Board Structure Index (from 0 to 20) increases

ln(Tobin's *q*) by 20 x .0104 = 0.208, which implies a share price increase of 56% for a firm with median values of Tobin's *q* (0.804) and leverage (0.47).

In *Table 11*, we examine the components of Board Structure Index. In the regressions in *Panel 1*, we use Board Independence and Board Committee Subindices as separate independent variables. Both are separately significant.

The next two panels in *Table 11* break down each subindex into its individual elements. In *Panel (2)*, both components of Board Independence Subindex -- element b1 (50% outside directors) and element b2 (> 50% outside directors) -- are separately positive and significant. Getting to 50% outside directors predicts 8% higher Tobin's *q*, while going beyond 50% predicts an additional 6% increase. There is thus apparent value both in getting to 50% outside directors, and exceeding this threshold. In *Panel (3)*, we study the components of Board Committee Index. Audit committee is significant and predicts about a 4% increase in ln(Tobin's *q*), and director nominating committee is marginally significant.

The results for Board Independence Subindex and 50% outside directors survive a battery of robustness checks, including using ln(market/book) as dependent variable, and putting each element of Board Structure Index separately into the same regression, despite colinearity between these elements. Having more than 50% outside directors is a bit weaker -- it is only marginally significant with ln(market/book) as dependent variable (last column of *Table 11*).

The results for Board Committee Subindex and for audit committees are less robust. With ln(market/book) as a dependent variable, Board Committee Subindex loses significance, and audit committee is only marginally significant. Audit committee also loses significance if we separately control for Board Independence Subindex and (KCGI - Board Structure Index), instead of having a single control for (KCGI - Board Committee Subindex); [and if we include each component of Board Structure Index in the same regression].

In sum, we find strong evidence that 50% outside directors predicts higher market value; reasonably strong evidence that having more than 50% outside directors also does so; and some evidence that having an audit committee separately predicts higher market value.

7.4. Subsample Results

In *Table 12*, we investigate the robustness of our results for various subsamples: (i) small firms versus large firms; (ii) financial firms vs. non-financial firms (recall that our sample already excludes banks), (iii) *chaebol* firms vs. non-*chaebol* firms, and (iv) manufacturing firms vs. non-manufacturing firms. For large firms, there is a mismatch between when investors know that board structure will change (at the time of the 1999 legal reforms) and the actual changes in board structure, which occur primarily in 2000 and 2001. In the regression with the large firm subsample, we therefore treat large firms as complying with the 1999 rules as of year-end 1999. With this adjustment, voluntary adoption of board structure reforms by small firms, and mandatory adoption by large firms, have similar predicted effects on firm value. However, *t*-statistics are weaker for large firms than for the other subsamples. This likely reflects a combination of small sample size and the lesser statistical power of fixed effects, as compared to *DiD*, to capture the effects of a one-time change in board structure.

The weaker results for large firms aside, Board Structure Index, Board Independence Subindex, and 50% outside directors are positive and significant across all subsamples. Having more than 50% outside directors is positive for all subsamples and significant for non-financial firms and non-manufacturing firms; we do not have an explanation for this pattern.

Board Committee Subindex presents a more complex picture. It is positive for all subsamples, significant for financial firms, and marginally significant for small firms and manufacturing firms, but insignificant for other subsamples. In unreported regressions, we obtain similar results across subsamples for audit committees, in a setup similar to *Table 11, Panel (3)*. It appears, not implausibly, that audit committees are especially important for financial firms; their importance for other firms is less clear.

7.5. Comparing *OLS* to Firm Fixed Effects

We noted above that a Breusch-Pagan test strongly rejects the pooled *OLS* model in favor of firm random effects, and a Hausman test usually rejects firm random effects in favor of firm fixed effects. Thus, *OLS* results may be biased, relative to firm fixed effects. We in fact find

important differences between different specifications, suggesting that the cross-sectional OLS results in most prior work may be unreliable. *Table 10* offers some clear examples. (*KCGI* - Ownership Parity - Board Structure Index) is significant in *OLS*. With random effects, it weakens, remains significant, but would lose significance if we apply the crude adjustment for lack of clustering suggested in Section 7.1, and multiply the reported *t*-statistics by 0.75. (*KCGI* - Ownership Parity - Board Structure Index) weakens further and is insignificant with firm fixed effects.

Ownership Parity Index shows an even stronger tendency to weaken as we move from pooled OLS to random effects to fixed effects. However, there is little variation in this variable across time, so the weaker results may merely reflect lack of variation, rather than underlying lack of importance.

These results support doubts about the reliability of *OLS* estimates in corporate governance research and underscore the importance of more robust estimation procedures. Time series data are not always available. Good instruments are rarely available. As Chidambaran et al. (2006), observe, "Identification of instrumental variables [for aspects of corporate governance] is extremely hard econometrically, because each potential candidate is likely to be related to another corporate governance mechanism and/or firm value." Korea is an exception in this regard for large firms, but only for large firms. If, as in most countries, a good instrument is not available, then time series data with fixed effects or at least random effects, viewed with skepticism, may be the best we can do. But our results are consistent with the skepticism voiced by Chidambaran, Palia and Zheng (2006), Wintoki, Linck and Netter (2007), and others about the value of the cross-sectional data which dominates the empirical corporate governance literature. If random and fixed effects specifications producing insignificant results, one reason could be limited time variation in the independent variable. But this outcome may also deepen one's suspicion about whether *OLS* results are causal.

7.6. Foreign Directors; Proportion of Outside Directors

Choi, Park and Yoo (2007) use a pooled *OLS* framework to study the association between board independence and the market value of Korea firms. They report two principal results: Fraction of outside directors positively predicts firm value; and presence of a foreign director positively predicts firm value. In unreported regressions, we confirm that fraction of outside directors predicts firm value in our sample, with firm fixed effects. However, this effect is entirely driven by the value of having 50% or more outside directors. We find no evidence that variation in the fraction of outside directors, between the legal minimum for small firms of 25% and 49%, predicts Tobin's *q*.

With regard to foreign directors, with firm fixed effects, the presence of a foreign director has a significant *negative* predicted effect on firm value (coefficient = -5.25%, $t = 2.11$). This is the *opposite* of the result in Choi, Park and Yoo and in two other studies (see also Oxelheim and Randoy, 2003; Choi and Hasan, 2005), and suggests the importance of using firm fixed effects instead of *OLS*, as well as the need to control for other aspects of governance. There is, however, a positive interaction between Board Independence Subindex and foreign director dummy (coefficient = .0177, $t = 2.49$). Having a foreign director has a roughly neutral predicted effect on firm value for a firm with 50% outside directors; and a predicted effect of roughly +9% for firms with more than 50% outside directors, and -9% for firms with fewer than 50% outside directors. The inference is that foreign directors can be valuable, but only if placed in a board which is already reasonably independent of management.

8. Instrumental Variable Analysis

We turn to our final methodological approach, instrumental variable analysis, using large firm dummy as an instrument for governance. This instrument relies on the legal rules discussed above, which require large firms, to have 50% outside directors, an audit committee, and an outside director nominating committee.

We place the *IV* analysis last not because it is least important, but because it is least new. We conducted a similar analysis using the same instrument, with only cross-sectional data from

2001, in BJK. The principal extensions and differences in this paper, besides confirming with a multiyear panel data set our prior single-year results:

(i) Panel data allows us to use both pooled *OLS* and random effects specifications for our two-stage least squares (*2SLS*) results.

(ii) Panel data permits an improved three-stage least squares (3SLS) analysis. We find some evidence for reverse causation, with firm value predicting Board Structure Index.

(iii) We treat Board Structure Index as the instrumented variable, rather than all of *KCGI*. In our prior work, we were agnostic on which was the better choice; we discuss below why we now prefer Board Structure Index.

Below, we summarize the *IV*, its strengths and limits, and our prior results, and then present the extensions and differences in this paper.

8.1. Methodological Issues for *IV* Analysis

We limit the instrumental variable analysis to 1999-2004 because large firm dummy is not an appropriate instrument in 1998, prior to adoption of these rules. We confirm in unreported regressions that large firm dummy predicts Tobin's *q* beginning at year-end 1999, but not before. This is consistent with the identification in time provided by the event study and *DiD* results above.

A valid instrument must be exogenous, correlated (ideally strongly) with the instrumented variable (Board Structure Index), and should predict the dependent variable (*ln*(Tobin's *q*) only indirectly through the instrumented variable, and not directly. We address each requirement in turn.

8.1.1. Exogeneity

Large firm dummy is likely to be exogenous. The governance rules that apply to large firms are mandatory and not subject to firm choice. There is no evidence that the size threshold corresponds to voluntary firm behavior prior to the 1999 adoption of the rules. For the 51 large firms in our sample with data on Board Structure Index at year-end 1998, one firm had 50% outside directors, and no firms had an audit committee or an outside director nominating committee. Indeed, corporate law rules making these committees *possible* were adopted only in

1999, at the same time as the rules making them mandatory for large firms. The mean score on Board Structure Index in 1998 is a trivial 0.20 out of 20.[16]

There is also no evidence that firms reduce or limit their size to avoid compliance with the rules. First, we have heard no anecdotal evidence of this response. Second, there is no clustering of firms just below the regulatory threshold. Pooling annual observations over 1999-2004, 62 firms have assets from 1.5-2 trillion won, and 54 firms have assets from 2-2.5 trillion won; there is no significant difference between these numbers, as proportions of the whole sample. Third, if firms shrink below 2 trillion won in assets to avoid compliance with governance rules, rather than because of business reversals, one would expect them to cease compliance. Instead, of 7 firms that were large during 2001 or 2002 but then fell below the 2 trillion won threshold, 4 retain an outside director nominating committee, 5 retain 50% outside directors, and all 7 retain an audit committee as of year-end 2004.

8.1.2. Correlation between instrument and instrumented variable

Second, large firm dummy correlates strongly with Board Structure Index, as expected since three of the five elements of Board Structure Index (50% outside directors, audit committee, and outside director nominating committee) are required for large firms. The overall correlation over 2000-2004 is $r = 0.69$; annual correlations are at least 0.70 in each year during this period 2000.[17]

8.1.3. Direct prediction of dependent variable

The harder question for instrument validity is whether large firm dummy predicts Tobin's *q* directly, or only indirectly and only through Board Structure Index. There are two principal concerns. First, firm size could (and indeed does) directly predict Tobin's *q*. Second, large

[16] See Table 3. At year-end 1999, just before the rules came into force, one additional firm had 50% outside directors; no firms as yet had audit or nominating committees.

[17] At year-end 1999, the correlation between large firm dummy and Board Structure Index is only 0.08 (not significant). One might then ask why we treat large firm dummy as an appropriate instrument beginning in 1999, rather than beginning in 2000. At year-end 1999, share values are anticipating a future change in board structure, even though board structures have not yet changed. Thus, large firm dummy predicts the dependent variable (Tobin's *q*) through its *future* effect on the instrumented variable (Board Structure Index), which has a *current* effect on firm value. This is not the usual *IV* setup, but we see no reason why it should not cause problems with instrument validity. In robustness checks, we obtain similar results if we limit the sample period to 2000-2004.

firm dummy, also predicts the remainder of *KCGI*, and the remainder of *KCGI* could predict Tobin's *q*. We address direct prediction of Tobin's *q* in this subsection, and indirect prediction through the rest of *KCGI* in the next subsection.

We address the implications for instrument validity of the direct correlation between firm size and Tobin's *q* in Black, Jang and Kim (2006). The same analysis largely applies here, so we will be fairly brief. First, in all regressions, we employ a partial regression discontinuity approach, in which we separately control for the continuous effect of *ln*(assets) on Tobin's *q*.

Second, the direct association between *ln*(assets) and Tobin's *q* is negative and significant, for all firms and for subsamples of large and small firms, while the effect of large firm dummy is positive and significant. Economically, a negative coefficient on *ln*(assets) implies that the larger a firm is, the worse it does at turning asset dollars into market value dollars. It would be a remarkable coincidence if this measure of efficiency were to steadily decline with size below 2 trillion won, jump at precisely the 2 trillion won point where governance rules kick in, for reasons other than the effect of governance on value, and then decline again with firm size above 2 trillion won. It would stretch coincidence rather implausibly for the positive effect of firm size on *Tobin's q* at 2 trillion won to be unrelated to governance, yet appear at precisely the time (second half of 1999) when the governance rules were adopted.

Third, in unreported regressions, we regress ln(Tobin's *q*) in on all control variables from *Table 10* plus large firm dummy, using pooled *OLS* and firm random effects specifications (fixed effects is inappropriate because large firm dummy in nearly invariant for each firm). The coefficient on large firm dummy is small and insignificant, consistent with large firm dummy predicting Tobin's *q* indirectly through Board Structure Index, rather than directly.

Still, there is one test that large firm dummy cannot pass. Asset size dummy could proxy for higher-order terms in the functional form of a direct relationship between size and Tobin's *q*.. We address this "functional form" concern throughout this paper by using a six powers functional form to control for firm size. Our event study, DiD, and firm fixed effects results survive this control, but our *IV* results become insignificant, if we control for 6 powers \of

ln(assets) or ln(sales). This contrasts with BJK, where the *IV* results using cross-sectional data from spring 2001 survived a similar test. The difference, compared to BJK and our event study and DiD results, appears to be that over time, the larger among the small firms voluntarily adopt some of the large firm reforms, so the jump in Board Structure Index at 2 trillion won is less sharp, and the 6-powers functional form captures much of the combined direct effect of firm size and the indirect effect of large firm dummy through Board Structure Index. For example, for "almost large" firms (with 6.6 < ln(assets) < 7.6, or 736 billion won < assets < 2 trillion won), the mean score on Board Structure Index rises from 1.29 in spring 2001 to 4.36 in 2004.

8.1.4. Prediction of dependent variable through rest of KCGI.

(This section can be skipped without loss of continuity.) In BJK, it was not clear whether we should treat large firm dummy as an instrument for Board Structure Index, while controlling for the rest of *KCGI*; or treat it as an instrument for all of *KCGI*. We reported results both ways, but relied principally on the second approach. In an *OLS* framework, neither approach is perfect. Large firm dummy correlates strongly with Board Structure Index, but also correlates with the rest of *KCGI*; and both Board Structure Index and the rest of KCGI separately predict Tobin's *q* (see BJK and *Table 10*). Thus, we could obtain biased coefficients and standard errors if we instrument for Board Structure Index while controlling for the rest of KCGI. On the other hand, given that large firm dummy predicts ln(Tobin's q) *primarily* through Board Structure Index, treating large firm dummy as instrumenting for all of *KCGI* can be misleading. In effect, the IV results would treat *KCGI* as an undifferentiated lump, when in fact we are largely instrumenting for only one part of *KCGI*.

The choice is clearer using the panel data in this study. We first separate *KCGI* into Board Structure Index(BS); Ownership Parity Index (OP), which is not correlated with large firm dummy and hence is not of concern; and (KCGI - OP - BS), which is correlated with large firm dummy (r = 0.43). With firm fixed effects (see *Table 10*), (KCGI - OP-BS) takes an economically small and statistically insignificant coefficient. If (KCGI - OP - BS) does not

predict Tobin's *q*, the issue of whether to instrument separately for Board Structure Index or instead for (KCGI - OP) goes away.

To be sure, (KCGI - OP - BS) is significant and positive with random effects in Table 10, regression (2). However, the coefficient is only 0.0014, compared to a coefficient of .0109 on Board Structure Index. Moreover, the *t*-statistic of 2.02 would lose significance if we apply the crude adjustment for lack of clustering suggested in Section 7.1, and multiply the *t*-statistic by 0.75. (KCGI - OP - BS) is also insignificant in (i) our 2SLS regressions (*Table 13, Panel B*); and (ii) unreported *2SLS* regressions in which we use large firm dummy to instrument for (KCGI - OP - BS) while controlling separately for Board Structure and Ownership Parity.

Moreover, large firm dummy predicts an 8.5 point change in Board Structure Index over 2000-2004, but only about a 2.6 point change in (KCGI -OP - BS) (regressions not reported). We can combine the predicted effects of large firm dummy on Board Structure Index and (KCGI - OP - BS) with the firm fixed effects coefficients on these variables from *Table 10*, to obtain the predicted change in Tobin's *q* due to the change in each of these governance variables at 2 trillion won. For Board Structure Index, the predicted change in ln(Tobin's *q*) is (8.5 point change in Board Structure Index) x (0.0109 coefficient on Board Structure Index) = 0.093. For (KCGI - OP - BS), the predicted change is a far smaller (2.6 points x .0014) = 0.004 -- and half that if we instead use the 0.0007 coefficient on (KCGI - OP - BS) from our *2SLS* regressions.

Taking these results together, it seems reasonable to treat large firm dummy as instrumenting for Board Structure Index, and treat Ownership Parity and (KCGI - OP - BS) as control variables, assumed to be exogenous. Any bias in the 2SLS coefficient on Board Structure Index should be small.

8.2. Durbin-Wu-Hausman Test for Endogeneity

Following the discussion above we use large firm dummy to instrument for Board Structure Index, while controlling for (KCGI - BS - OP), for Ownership Parity, for ln(assets), and for most of the other control variables we used in Table 10. We drop MSCI Index and ADR dummy due to high correlation with large firm dummy. We first assess whether there is

apparent endogeneity, using the Durbin-Wu-Hausman test (Davidson and MacKinnon, 1993, Wooldridge, 2006). See *Table 13, Panel A*. The Durbin-Wu-Hausman test is similar to *2SLS*. It assumes that asset size dummy is a valid instrument for Board Structure Index. In the first stage, we regress Board Structure Index on large firm dummy and other control variables, which are assumed to be exogenous. In the second stage, we regress ln(Tobin's *q*) on Board Structure Index, control variables, and the residual from the first-stage regression. A significant coefficient on the first-stage residual is evidence of endogeneity. The coefficient on Board Structure Subindex is identical to the *2SLS* or the *3SLS* coefficients.

The Durbin-Wu-Hausman test does not reject the null of no endogeneity when we use *ln*(Tobin's *q*) as a measure of firm value, but does reject the null when we use *ln*(market/book) (market value of common stock/book value of common stock) as an alternate measure of firm value. We thus have mild evidence of endogeneity. We return to this potential endogeneity below, with our *3SLS* results.

8.3. 2SLS Results

Table 13, Panel B reports *2SLS* results, using both *OLS* and firm random effects specifications, with *ln*(Tobin's *q*) and *ln*(market/book) as alternative measures of firm market value. Our *IV* analysis is limited to pooled OLS and firm random effects specifications. Large firm dummy is almost time-invariant for each firm, so it cannot be used as an instrument in combination with firm fixed effects. We use firm clusters with the pooled *OLS* specification, but are not able to combine clustering with random effects. Despite the lack of clustering, we prefer the random effects specification. We show *OLS* principally as a robustness check, and for comparison with the 3SLS results in Panel C (for which random effects are not available).

Instrumented Board Structure Index strongly predicts both variables, in both specifications. The predicted effects are economically large and statistically significant, even after we crudely multiply the random effects *t*-statistics by 0.8 to adjust for lack of clustering (see discussion above of this adjustment). Coefficients are larger than in the corresponding non-instrumental-variable regressions in *Table 10*. For example, a firm that goes from the 1998

mean score of 2.11 on Board Structure Index to the 2004 mean of 14.75 has 30% higher predicted Tobin's *q* using the firm random effects coefficient.[18]

8.3. 3SLS Results

Table 14, presents *3SLS* regressions, using an OLS specification. Unfortunately, firm random effects are not available with *3SLS*; neither are firm clusters or Rogers' robust standard errors. We again instrument for Board Structure Index using large firm dummy.

The empirical challenge in *3SLS* is how best to instrument for ln(Tobin's *q*). We have no perfect instrument, but have several respectable ones. In Table 14, we use three instruments: -- R&D/sales, advertising/sales, and EBIT/sales. These variables are expected on theoretical grounds to predict Tobin's *q*; in fact predict Tobin's *q* in the *OLS*, random effects and fixed effects regressions in *Table 10*; have no obvious theoretical connection to board structure; and do not predict board structure in unreported regressions similar to *Table 10* with Board Structure Index as dependent variable. In robustness checks, we obtain similar results with other combinations of these instrumental variables.. The *3SLS* equations are as follows:

$$\ln(\text{Tobin's } q) = f(\text{Board Structure Index, instruments for Tobin's } q\text{, other variables}) + \varepsilon$$

$$\text{Board Structure Index} = g(\ln(\text{Tobin's } q)\text{, large firm dummy, other variables}) + \eta$$

The *t*-statistics for instrumented Board Structure Index are substantially larger in *3SLS* than in *2SLS*; the coefficients are (necessarily) identical to 2SLS. A Hansen overidentification test does not suggest that the instruments are endogenous (p = 0.43 for Tobin's *q*).

We also find mild evidence of reverse causation, with instrumented *ln*(Tobin's *q*) and *ln*(market/book) predicting Board Structure Index. However, statistical significant would likely disappear if we were able to use clusters. Moreover, the dominant effect runs from Board Structure Index to *ln*(Tobin's *q*). For example, a 10-point increase in Board Structure Index predicts 0.15 higher ln(Tobin's q), while this predicted change in *ln*(Tobin's *q*) predicts only an 0.5 point (increase in Board Structure Index. Thus, even a large change in ln(Tobin's *q*) has only a modest predicted effect on Board Structure Index.

[18] (2SLS coefficient of .0207 x 12.64 point difference in Board Structure Index = 0.262 increase in ln(Tobin's *q*). Then exp(0.262) = 1.299, implying a 30% increase in Tobin's *q*.

9. Conclusion

Outside directors and audit committees are widely considered to be central elements of good corporate governance. Yet compelling evidence to support this conventional wisdom is limited. Prior work in emerging markets on the connection between board composition and committee structure and overall firm value or performance relies principally on cross-sectional data, which may be unreliable.

Korea provides a unique laboratory for addressing the empirical issues in identifying the relationship between board structure and firm value. We rely on Korea's 1999 adoption of legal rules which apply to large but not small Korean firms, as a legal shock which provides the potential for identification. We then adopt a multiple identification strategies approach, and seek to identify this legal change with an increase in firm market values. We use event study and *DiD* approaches to provide evidence of identification in *time*. An increase in large firm value, relative to small firms, happens *when* it should -- when the reforms are adopted -- and there is no comparable jump in the relative value of large firms at other times. We use instrumental variable analysis, using large firm dummy to instrument for Board Structure Index, to also provide identification in *size* -- the premium for large firms appears where it should, at the 2 trillion won threshold for the large firm rules, and not an another size. We confirm, in a firm fixed effects approach, that small firms which voluntarily reform experience similar value increase to large firms, and provide evidence that the value comes primarily from board independence, but likely also from establishment of an audit committee.

We embed our analysis in a regression discontinuity framework -- we control throughout for a continuous measure of firm size. In our event study, DiD, and firm fixed effects results, we control for firm size in robustness checks using a flexible 6-powers functional form; our results survive. The IV results become insignificant, but survived a similar test in our prior work with cross-sectional data, and there are other reasons to believe that large firm dummy is an appropriate instrument for Board Structure Index.

Overall, we report evidence consistent with a positive share price impact of boards with

50% or greater outside directors, and likely audit committees. The effect of the legal reforms is economically large -- a roughly 20% increase in Tobin's *q*, or about a 50% increase in share price. We also find that large firms enjoy higher profitability with a lag after changing their board structures, suggesting that profitability may be one channel through which board independence affects firm value. Finally, differences between *OLS* and firm fixed effects estimates are sometimes large, which supports the unreliability of *OLS* estimates.

We also innovate in methodology. No one of our approaches is new, but the combined use of several identification techniques to address the same issue is new, has more power than any one method alone, and potentially can be applied to other studies which rely on legal shocks for identification.

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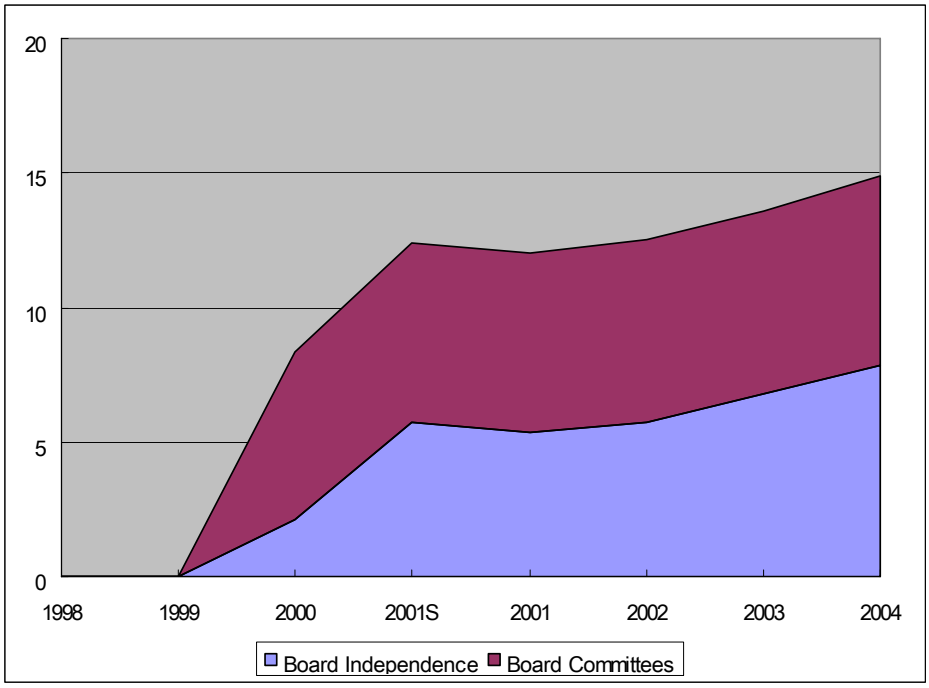
Figure 1: Change in Board Independence and Board Committees Over Time

Figures show mean values of Board Independence Subindex (0~10) and Board Committees Subindex (0~10) from year-end 1998 through year-end 2004, for balanced panels of large Korean public firms (assets > 2 trillion won) and small Korean public firms, respectively.

Large Firms (balanced panel, 28 firms)



Small Firms (balanced panel, 220 firms)

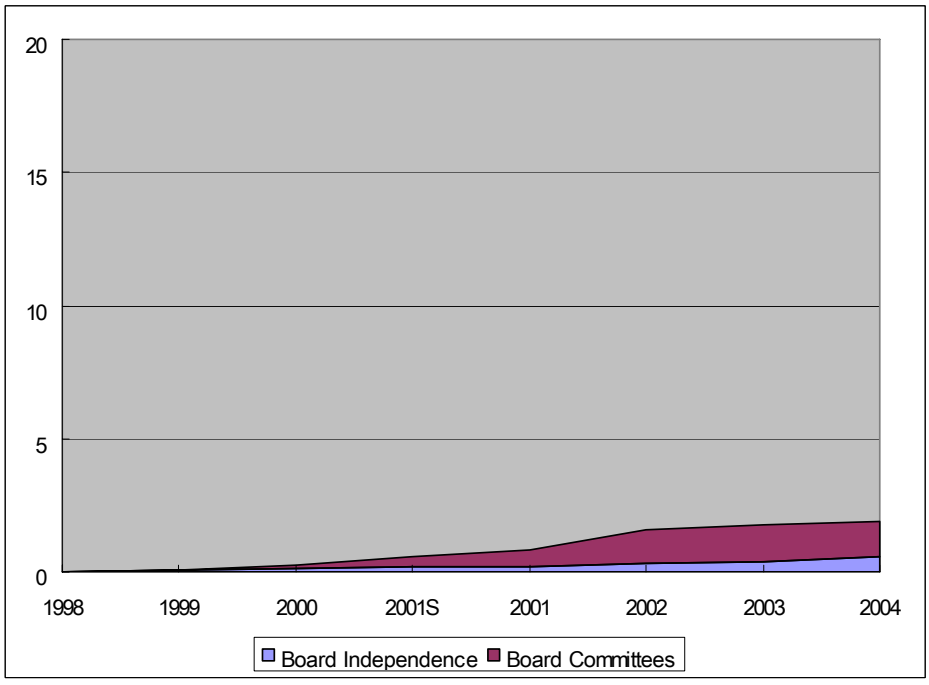


Figure 2: Cumulative Returns to Small and Large Plus Firms for 1998-99

Cumulative returns to "large plus" Korean firms (assets > 1 trillion won at year-end 1998, and small firms (assets < 1 trillion won) during 1998 and 1999. Base level for each group is set to 100 at Dec. 31, 1997.

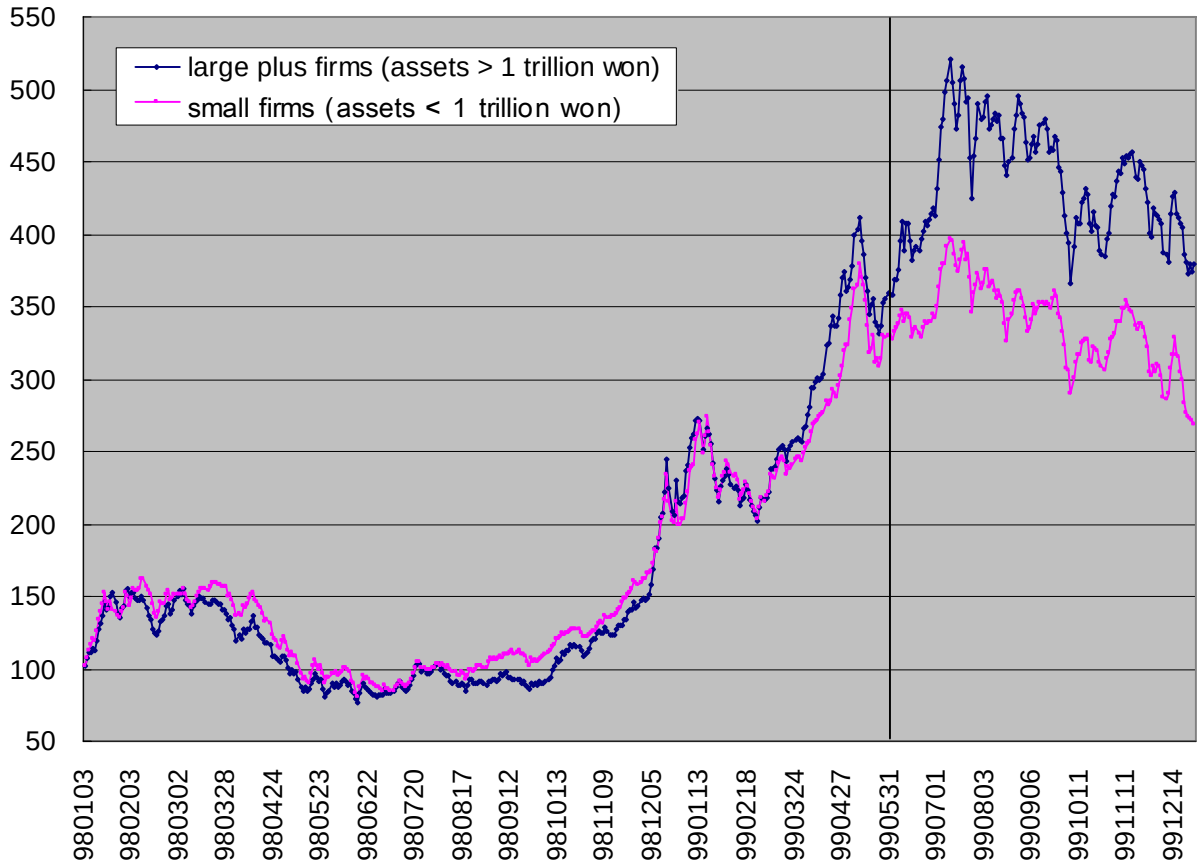


Figure 3. Cumulative Market-Adjusted Returns for Large Plus Firms

Cumulative market-adjusted returns (*CMAR*) to "large plus" Korean firms (assets > 1 trillion won at year-end 1998), relative to Small Firm Index (equally weighted index of firms with assets < 1 trillion won), from April 30-Dec. 31, 1999. CMAR is set to zero at April 30, 1999. Vertical lines show day 0 for event 1 (June 3) , event 2 (June 25), event 3 (July 2)and event 4 (Aug. 25).

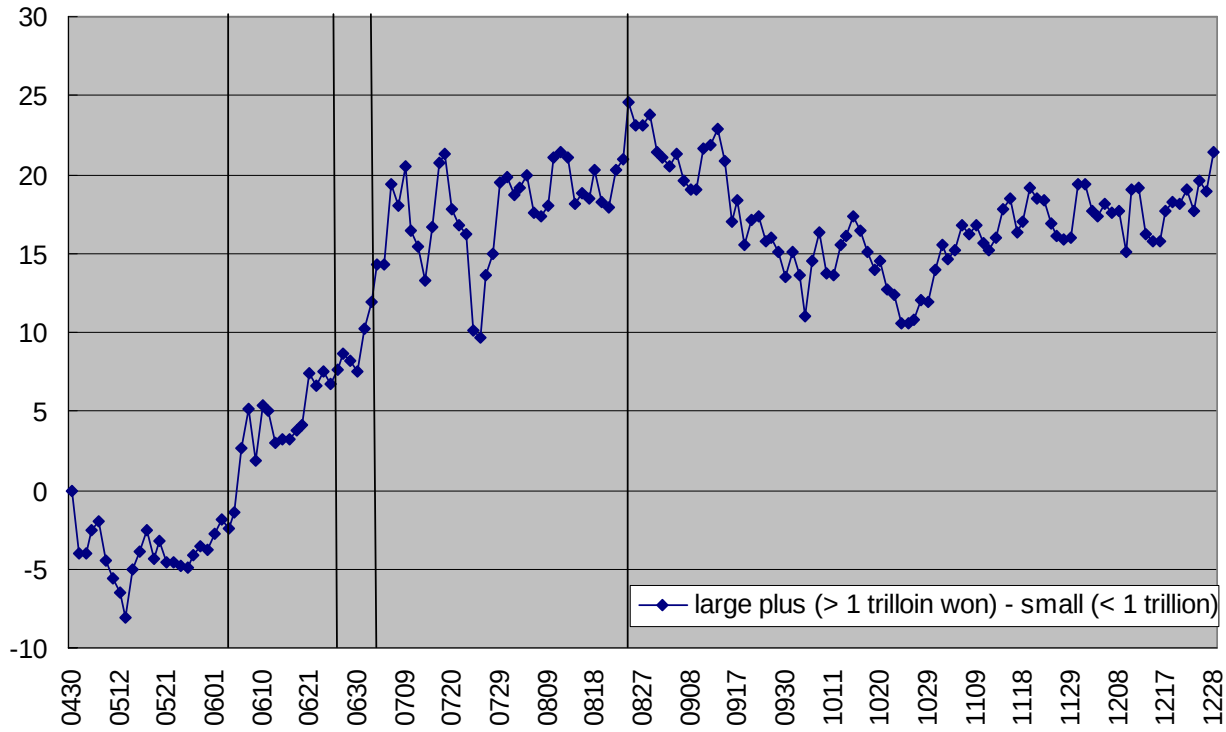


Figure 4: Difference in Tobin's *q*: Firms with 50% Outside Directors versus Other Firms

Solid line: (mean Tobin's *q* for large firms) - (mean Tobin's *q* for nonreformed small firms), at indicated dates. Difference is set to zero at the base date of May 31, 1999. Dates are every 6 months, except we replace June 30, 1999 with May 31, 1999, because principal event period begins in early June 1999. Dashed line shows market/book ratio. Sample is a balanced panel of 46 large firms and 159 small firms (size measured at June 30, 1999). Sample excludes large firms which had 50% outside directors at May 31, 1999, and small firms which voluntarily adopted 50% outside directors as of Dec. 31, 2001.

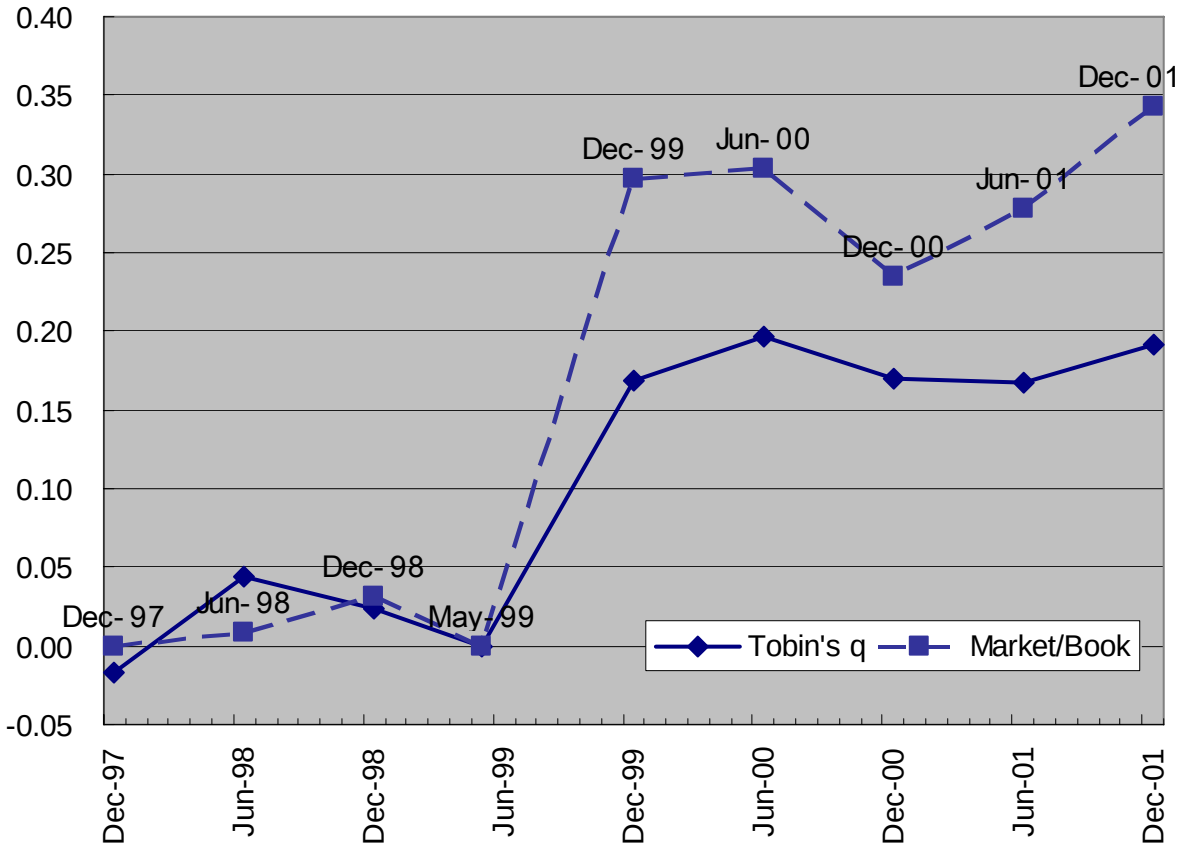


Table 1. Announcement Dates for 1999 Korean Governance Reforms

This table lists the principal news announcements related to the 1999 legal reforms to the rules governing outside directors, audit committees, and nominating committees, based on a search of the KINDS (Korean Integrated News Database System) database, which includes all major Korean newspapers. The table focuses on announcements involving all firms. Announcements, contemporaneous to those listed below, indicated that reforms similar to those for large firms, would also apply to banks, or to financial institutions generally. Announcements in March-April 1999 discussed mandating audit committees, but either applied to all firms or had a much lower threshold (0.1 trillion won) than later announcements. The principal announcements for our event study are in **boldface**.

Event No.	Dates	Information
	1998: various	*1998 reforms, effective starting with 1999 annual meetings, require all listed firms to have a minimum of 25% outside directors*
	Mar. 18, 1999	Corporate Governance Reform Committee is formed to prepare a Korean Corporate Governance Code, patterned on the voluntary UK and OECD codes.
	Mar. 23, 1999	Ministry of Justice announces that it will require listed companies (over 0.1 trillion won) to have audit committees composed of at least three members with at least 2/3 outside directors.
	April 9, 1999	Ministry of Finance and Economy announces that audit committees will be required for firms with assets > 0.1 trillion won
	May 24, 1999	President replaces Minister of Finance and Economy and other economy-related ministers
	May 26, 1999	President tells new ministers at cabinet meeting that he will focus on chaebol reform. Ministers so report in press interviews.
1	**June 2, 1999**	**News articles: government economic policy will shift from "lower leverage" to "corporate governance reform" (which obviously includes independent directors and audit committees).**
	June 3, 1999	**Speech by new Minister of Finance and Economy Bong-Kyun Kang: chaebol reform will focus on corporate governance reform**
	June 7, 1999	News articles: Economy-related ministers have agreed on broad direction of reforms. For corporate reform, they will support the governance guidelines being drafted by the Corporate Governance Reform Committee, and will require audit committees.
2	**June 25, 1999**	**Ministry of Finance and Economy announces that some provisions in the Korean Corporate Governance Code will be mandated by law, and mentions higher outside director ratio, audit committees, and minority shareholders' rights as examples.**
3	**July 2, 1999**	**Government announced that audit committee, dominated by outside directors, will be mandated by law for large firms (the size threshold was not specified). Voting rights of chaebol-affiliated financial institutions for shares of industrial affiliates will be restricted.**
	Aug. 15, 1999	Independence Day speech by President Kim on chaebol reform. Previously announced principles include improving transparency, banning cross-guarantees, lowering corporate leverage, enhancing specialization, and strengthening managers' accountability; new principles include banning industrial firms' control over financial firms and restricting circular shareholding.
4	**Aug. 25, 1999**	**Government announces plans to implement President Kim's August 15 speech. Measures include: (i) for large listed firms *(news articles speculate that the threshold will be 1 trillion won)*, increase the outside director ratio to 50% and introduce director nomination committee, dominated by outside directors; and (ii) replace internal auditor with an audit committee, dominated by outside directors. Chaebol reforms include limiting investments and loans by chaebol firms to affiliated firms; (iv) consolidated financial statements; board approval and disclosure for related-party transactions. Ministry of Justice announces a reform bill to: (i) allow companies to adopt an audit committee with at least three members, including at least 2/3 outside directors, instead of an internal auditor, and to**

Event No.	Dates	Information
		establish other board committees (e.g., compensation and outside director nominating committees); (ii) allow voting by mail and electronic means; and (iii) limit to 10% of outstanding shares the number of shares that can be acquired by exercising stock options.
	Aug. 26, 1999	**Corporate Governance Reform Committee releases first draft of "Korean Code of Best Practice for Corporate Governance." 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 8 directors; (iv) cumulative voting for directors.**
	Sept. 15, 1999	Government will postpone the effective date for plan to require 50% outside directors for firms with assets > 1 trillion won until 2001 shareholder meeting.
	Sept. 21-28, 1999	Several announcements during this period concerned the size threshold for various rules. On Sept. 21, the Government announced that the new rules would apply to listed firms with assets above *2 trillion won* (instead of 1 trillion), and that large firms must establish an "outside director nominating committee" instead of "director nominating committee." On Sept. 22, the Committee on Corporate Governance announced its final Code of Best Practice for Corporate Governance, which retained a 1 trillion won threshold. On Sept. 23, an announcement by the Ministry of Finance and Economy retained the 1 trillion won threshold, but on Sept. 28, the Ministry raised the size threshold to 2 trillion won.
	Oct. 20, 1999	Ministry of Finance and Economy announces that it has finalized its reform bill. The bill requires firms with assets above 2 trillion won to have 50% outside directors, at least 3 outside directors, an audit committee with at least 2/3 outside directors, and an outside director nomination committee with at least 50% outside directors. Other listed firms will be required to have at least 25% outside directors ratio. Audit and outside director nomination committees will be required from 2000; 50% outside directors from 2001.
	Nov. 22, 1999	Government submits a bill to require firms over 2 trillion won 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. 7, 1999	National Assembly passes a bill to revise the Commercial Act to allow firms to have an audit committee instead of an internal auditor; the committee must include at least 3 members, of which at least 2/3 must be outside directors.
	Dec. 16, 1999	National Assembly passes a bill to revise the Securities Transaction Act to require large firms (assets over 2 trillion won) to have 50% outside directors, an audit committee, and an outside director nomination committee.
	Jan. 23, 2000	Ministry of Finance and Economy announces a Presidential Degree that clarifies the effective dates of each reform measure. A minimum of 3 outside directors, audit committee, and outside director nomination committee will be effective as of 2000. The 50% outside director ratio will be effective as of 2001.

Table 2: Construction of *KCGI*, 1998-2004

This table shows (i) the governance elements used to construct *KCGI*. (ii) data sources; and (iii) the rules we use to fill in missing information. Element labels are consistent with Black, Jang, and Kim (2006) (shown in mid-2001 column). Data sources are: director database, ownership database, annual surveys by the Korea Corporate Governance Service (KCGS) beginning spring 2001, and hand-collection. KCGS surveys are in spring of each year and provide end-of-prior-year information, except as shown. We ***extrapolate*** for ***missing elements*** **as** follows: (i) if an element is available in year X, but not in year X+1 (X-1), we extrapolate year X value to year X+1 (X-1). We ***interpolate*** for ***missing firms*** and ***missing elements*** using the following rules applied sequentially: (i) if a firm answers the KCGS survey in years X and X+2, but not year X+1, we use in year X+1 the average of the X and X+2 values; and (ii) if an element is available in years X and X+2, but not year X+1, we use in year X+1 the average of the X and X+2 values. We assume elements are present if they are legally required. *Italics* indicate legally required elements.

For hand-collection, we generally collect values in year X only for firms which had this governance element in year X+1. Thus, for compensation committee, we have KCGS data starting in 2002. We hand collect data for 2001 for firms which had this committee in 2002, collect data for 2000 for firms which had this committee in 2001, etc. For some elements, a change in KCGS methodology led to inconsistency between responses for different years. For these questions, we either replace a 1 value in year X with 0 if the X+1 value is 0, or replace a 0 value in year X with 1 if the X+1 value was 1, as seemed appropriate given the nature of the element. Details on these and other adjustments to the KCGS raw data are available from the authors on request.

Date	**1998-2000**	**mid-2001**	**2001**	**2002**	**2003**	**2004**
Shareholder Rights Index (A)						
Firm permits cumulative voting for election of directors.	hand-collect	A1	I-3-①	1-(16)	1-A-(4)	1-A-(4)
Firm permits voting by mail.	hand-collect	A2	I-3-②	1-(17)	1-A-(5)	1-A-(5)
Firm discloses director candidates to shareholders in advance of shareholder meeting.	hand-collect	A4	I-9-③	*required*	*required*	*required*
Board approval required for related party transactions (*required 2000 for top 10 chaebol, mid-2001 for all chaebol, 2001 on for large and chaebol firms*)	hand-collect	A5	II-2-6-①	same as 2001	same as 2001	same as 2001
Board Structure Index (B)						
Firm has at least 50% outside directors *(rule adopted 1999 required beginning mid-2001 for large firms)*	director database	*B1*	*I-2-③, II-2-1*	*director database*	*2-A-(1)*	*2-A-(1)*
Firm has more than 50% outside directors (director database except as indicated)	director database	B2	I-2-③, II-2-1	1 for large firms if 1 in 2003 or 2-A-(1) ≥ 2	2-A-(1) for large firms	2-A-(1) for large firms
Firm has outside director nominating committee (*rule adopted 1999, required from mid-2001 for large firms*).	hand-collect	*B3*	*II-3-4*	*2-B-(12), 2-B-(13)*	*2-A-(9)*	*2-A-(9)*
Audit committee of the board of directors exists (*rule adopted 1999, required from mid-2001 for large firm*)	hand-collect	*B4*	*I-6-①*	*4-(1)*	*4-(1)*	*4-(1)*
firm has compensation committee	hand-collect	hand-collect	hand-collect	hand-collect	2-A-(10)	2-A-(10)
Board Procedure Index (C)						
Directors' positions on board meeting agenda items are recorded in board minutes.	hand-collect	C2	II-2-6-②	2-B-(4)	2-B-(21)	same as 2003
Board chairman is an outside director or (from 2003)	0 firms	C3 (0 firms)	hand collect	hand collect	2-A-(5)	2-A-(5)

Date	1998-2000	mid-2001	2001	2002	2003	2004
firm has outside director as lead director.						
A system for evaluating directors exists.	hand-collect	C4	II-2-6-④	same as 2001	2-B-(39)	2-B-(34)
A bylaw to govern board meetings exists.	hand-collect	C5	average of mid-2001 and 2003	2-B-(18)	2-B-(16)	same as 2003
Firm holds four or more regular board meetings per year.	hand-collect	C6	I-4-②, II-2-3-①	2-B-(1)	2-B-(19)	2-B-(20)
Firm has one or more foreign outside directors.	hand-collect	C7	director database	2-A-(10)	2-A-(6)	2-A-(6)
Shareholders approve outside directors' aggregate pay (separate from all directors' pay).	hand-collect	C11	same as mid-2001	same as 2003	2-B-(30)	same as 2003
Outside directors attend at least 70% of meetings, on average	same as mid-2001 [missing if 0 outside directors]	C12	I-1	2-A-(2)	2-B-34	2-B-(30)
Board meeting solely for outside directors exists.	hand-collect	C15	II-3-15-③	2-A-(3)	2-B-(35)	2-B-(31)
100% outside directors on audit committee	same as mid-2001 [if committee exists]	D1	II-4-1	4-(2)	4-(2)	4-(2)
Bylaws governing audit committee (or internal auditor) exist.	hand-collect	D2	average of mid-2001 and 2002	4-(3)	4-(3)	4-(3)
Audit committee includes person with expertise in accounting	hand-collect	D3	II-4-2	average of 2001 and 2003	4-(10)	4-(11)
Audit committee (or internal auditor) approves the appointment of the internal audit head.	hand-collect	D5	average of mid-2001 and 2002	4-(4)	4-(4)	4-(5)
Audit committee meets ≥ 4 times per year	hand-collect	D10	I-6-②, II-4-7-①	4-(7)	4-(7)	4-(7)
Disclosure Index (E)						
Firm conducted investor relations activity in year 2000	same as mid-2001	E1	II-1-5	3-(1)	3-(1)	3-A-(1)
Firm website includes resumes of board members	same as mid-2001	E2	average of mid-2001 and 2002	3-(9)	3-(9)	3-B-(21)
English disclosure exists	same as mid-2001	E3	average of mid-2001 and 2002	3-(15)	3-(14)	3-A-(13)
Ownership Parity (P)						
Ownership Parity = (1 - ownership disparity); disparity = ownership by all affiliated shareholders - ownership by controlling shareholder and family members	ownership database (same as mid-2001 for financial firms)	P (ownership database)	ownership database (same as mid-2001 for financial firms)	ownership database (same as mid-2001 for financial firms)	same as 2002	same as 2002

Table 3: Summary Statistics for KCGI and Board Structure Index

Summary statistics for Board Structure Index, its components, foreign director dummy, and *KCGI*. Dates are year-end except as indicated. Large firms were required to have audit committee, outside director nominating committee, and 3 outside directors in 2000, and 50% outside directors by spring 2001. Year-end values are means. Variables are defined in *Table 4, Panel A*. Board structure row is shown in **boldface** for emphasis. This table is limited to firms with data on *KCGI,* so number of firms is somewhat smaller than for the event study and DiD analyses.

Large Firms

	Mean	Median	Std. Dev	1998	1999	2000	spr 2001	2002	2004
KCGI	51.63	54.25	15.76	28.90	32.88	45.63	50.30	61.56	67.90
Board Structure (BS = BI + BC)	**9.28**	**11.67**	**6.05**	**0.20**	**0.26**	**8.68**	**12.47**	**12.08**	**14.97**
Board Independence (BI=5*(b1 + b2))	4.29	5.00	3.60	0.20	0.26	2.54	5.80	5.48	7.92
b1 (50% outside directors dummy)	0.66	1.00	0.47	0.02	0.04	0.38	1.00	0.94	0.96
b2 (> 50% directors dummy)	0.20	0.00	0.40	0.02	0.02	0.13	0.16	0.15	0.62
Board Comm. (BC=3.33*(b3+b4+b5))	4.96	6.67	3.03	0.00	0.00	6.14	6.67	6.60	7.04
b3 (nominating committee)	0.72	1.00	0.45	0.00	0.00	0.84	1.00	1.00	1.00
b4 (audit committee)	0.73	1.00	0.44	0.00	0.00	0.98	1.00	0.94	1.00
b5 (compensation committee)	0.03	0.00	0.17	0.00	0.00	0.02	0.00	0.04	0.11
Board Procedure	10.55	10.59	3.12	6.15	9.17	10.71	10.97	11.70	12.35
Shareholder Rights	7.47	10.00	4.49	3.53	3.80	6.17	7.70	10.58	10.43
Disclosure	6.93	6.67	6.81	3.41	3.07	3.68	3.00	10.16	12.96
Ownership Parity	16.73	17.35	3.04	16.80	16.67	16.36	16.10	17.03	16.90
Number of large firms	429	429	429	51	57	57	50	52	53

Small Firms

	Mean	Median	Std. Dev	1998	1999	2000	spr 2001	2002	2004
KCGI	32.31	31.34	9.36	23.03	25.43	28.73	29.94	38.68	40.75
Board Structure (BS = BI + BC)	**1.03**	**0.00**	**2.54**	**0.01**	**0.07**	**0.50**	**0.74**	**1.95**	**1.95**
Board Independence (BI=5*(b1 + b2))	0.34	0.00	1.51	0.00	0.07	0.24	0.23	0.46	0.69
b1 (50% outside directors dummy)	0.05	0.00	0.22	0.00	0.01	0.04	0.04	0.07	0.10
b2 (> 50% directors dummy)	0.01	0.00	0.12	0.00	0.00	0.00	0.01	0.02	0.04
Board Comm. (BC=3.33*(b3+b4+b5))	0.69	0.00	1.68	0.01	0.01	0.27	0.52	1.50	1.25
b3 (nominating committee)	0.11	0.00	0.31	0.00	0.00	0.03	0.07	0.26	0.20
b4 (audit committee)	0.08	0.00	0.28	0.00	0.00	0.04	0.07	0.17	0.15
b5 (compensation committee)	0.01	0.00	0.11	0.00	0.00	0.01	0.02	0.02	0.02
Board Procedure	7.23	7.14	3.02	4.21	5.39	7.44	7.51	8.41	8.54
Shareholder Rights	4.44	5.00	4.13	0.33	1.33	2.42	3.34	7.64	8.13
Disclosure	2.04	0.00	3.81	0.71	0.73	0.73	0.73	3.24	5.16
Ownership Parity	17.48	18.91	3.27	17.65	17.78	17.63	17.63	17.45	16.97
Number of small firms	3,709	3,709	3,709	441	453	460	461	388	443

Large Versus Small Firms: Difference in Means

	Overall	1998	1999	2000	spr 2001	2002	2004
KCGI	19.32	5.87	7.45	16.90	20.36	22.88	27.15
Board Structure (BS = BI + BC)	**8.25**	**0.19**	**0.19**	**8.18**	**11.73**	**10.13**	**13.02**
Board Independence (BI=5*(b1 + b2))	3.95	0.20	0.19	2.30	5.57	5.02	7.23
b1 (50% outside directors dummy)	0.61	0.02	0.03	0.34	0.96	0.87	0.86
b2 (> 50% directors dummy)	0.19	0.02	0.02	0.13	0.15	0.13	0.58
Board Comm. (BC=3.33*(b3+b4+b5))	4.27	-0.01	-0.01	5.87	6.15	5.10	5.79
b3 (nominating committee)	0.61	0.00	0.00	0.81	0.93	0.74	0.80
b4 (audit committee)	0.65	0.00	0.00	0.94	0.93	0.77	0.85
b5 (compensation committee)	0.02	0.00	0.00	0.01	-0.02	0.02	0.09
Board Procedure	3.32	1.94	3.78	3.27	3.46	3.29	3.81
Shareholder Rights	3.03	3.20	2.47	3.75	4.36	2.94	2.30
Disclosure	4.89	2.70	2.34	2.95	2.27	6.92	7.80
Ownership Parity	-0.75	-0.85	-1.11	-1.27	-1.53	-0.42	-0.07

Table 4: Principal Variables

Definition and summary statistics for the principal dependent and independent variables used in this paper. Panel A defines each variable. Panel B provides summary statistics. Book asset values are in billion won. Book and market values are measured at year end, except that market values for mid-2001 are measured on June 30, 2001.

Panel A: Variable Definitions

Governance Variables	**Description**
KCGI	Korean Corporate Governance Index: Sum of Board Structure, Shareholder Rights, Board Procedure, Disclosure, and Ownership Parity Indices
Board Structure Index	Sum of Board Structure Subindex + Board Independence Subindex
Board Independence Subindex	BI = (b1 + b2)/10
b1	1 if firm has at least 50% outside directors, 0 otherwise
b2	1 if firm has >50% outside directors, 0 otherwise
Board Committee Subindex	(b3 + b4 + b5)/10
b3	1 if firm has outside director nomination committee, 0 otherwise
b4	1 if firm has audit committee, 0 otherwise
b5	1 if firm has compensation committee, 0 otherwise
Other Variables	
Small firm index	Return to equally weighted index of 142 small Korean public firms with assets < 1 trillion won at year-end 1998, which had not adopted 50% outside directors by year-end 2004.
Tobin's *q*	[Market value of assets / Book value of assets] measured at each year-end. Market value of assets is estimated by [book value of debt + book value of preferred stock + market value of common stock].
Market-to-Book Ratio	[Market value of common stock / Book value of common stock] measured at each year-end, winsorized at 1%/99%. We drop firms with negative book value of common stock.
Years Listed	Number of years since original listing on Korea Stock Exchange
Leverage	(Book value of debt)/ (Market value of common stock), winsorized at 1% and 99%
Sales Growth	Geometric average sales growth during past 5 fiscal years (or available period if < five years). If fiscal year changes, we only keep years which cover a full 12 months.
R&D/Sales	Ratio of research and development (*R&D*) expense to sales. Firms with missing data for *R&D* expense are assumed to have 0 values.
Advertising/Sales	Ratio of advertising expense to sales. Firms with missing data for advertising expense are assumed to have 0 values.
Exports/Sales	Ratio of export revenue to sales. Firms with missing data for export revenue are assumed to have 0 values.
PPE/Sales	Ratio of property, plant, and equipment to sales.
Capex/PPE	Ratio of capital expenditures to PPE
EBIT/Sales	Ratio of earnings before interest and taxes to sales.
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 year / Common shares held by public shareholders]. Denominator = [common shares outstanding x (1 – total affiliated ownership)]
Foreign Ownership	[Common shares held by foreign investors / common shares outstanding]
Sole Ownership	[common shares held by controlling shareholder and family members / common shares outstanding]
Large firm dummy	Equals 1 of book value of assets > 2 trillion won at end of prior year, 0 otherwise
Chaebol Dummy	1 if a member of one of the top-30 business groups (based on total group assets) as identified annually by Korea Fair Trade Commission; 0 otherwise, excluding former state-owned enterprises.
Level 1 ADR Dummy	1 if firm has level 1 American Depository Receipts (ADRs); 0 otherwise.
Level 2/3 ADR Dummy	1 if firm has level 2 or level 3 ADRs; 0 otherwise.
MSCI Index Dummy	1 if firm is in Morgan Stanley Capital International Index; 0 otherwise.

Panel B: Summary Statistics

	No of "1" values	Pooled Mean	Pooled Median	Min.	Max.	S.D.	1998 Mean	2000 Mean	2002 Mean	2004 Mean
Tobin's *q*		0.86	0.80	0.21	6.05	0.39	0.85	0.85	0.77	0.93
ln(market cap)										
ln(Tobin's *q*)		-0.22	-0.23	-1.55	1.80	0.35	-0.25	-0.23	-0.30	-0.11
ln(market/book)		-0.67	-0.73	-9.23	7.18	0.83	-0.63	-0.69	-1.00	-0.53
ln(assets)		5.53	5.33	0.70	10.69	1.44	5.41	5.55	5.60	5.47
Years Listed		2.56	2.64	0.00	3.89	0.78	2.64	2.58	2.53	2.40
Leverage		33.62	2.37	0.01	115000	1797	4.00	222.77	9.17	7.08
Sales Growth		0.27	0.08	-0.65	541.25	8.63	0.13	0.33	0.11	0.12
R&D/Sales		0.01	0.00	0.00	7.69	0.13	0.01	0.01	0.01	0.01
Advertising/Sales		0.01	0.00	0.00	0.21	0.02	0.01	0.01	0.01	0.01
Exports/Sales		0.28	0.15	0.00	1.00	0.31	0.27	0.28	0.28	0.31
PPE/Sales		0.54	0.39	0.00	36.05	1.11	0.49	0.61	0.60	0.54
Capex/PPE		0.14	0.09	0.00	7.73	0.20	0.14	0.12	0.15	0.14
EBIT/Sales		0.04	0.06	-30.78	0.97	0.52	0.04	0.03	0.05	0.05
Market Share		0.06	0.01	0.00	1.00	0.15	0.06	0.06	0.06	0.06
Share Turnover		14.93	4.65	0.03	17332	332	7.80	31.46	8.01	5.55
Foreign Ownership		7.59	0.91	0.00	94.11	13.56	8.49	6.64	6.47	5.89
Sole Ownership		19.97	19.85	0.00	78.81	16.46	19.39	19.55	19.84	20.83
Large firm dummy	429	0.10	0.00	0.00	1.00	0.31	0.09	0.10	0.11	0.10
Chaebol Dummy	831	0.20	0.00	0.00	1.00	0.40	0.19	0.20	0.19	0.23
Level 1 ADR Dummy	114	0.03	0.00	0.00	1.00	0.16	0.03	0.03	0.03	0.01
Level 2/3 ADR Dummy	13	0.00	0.00	0.00	1.00	0.06	0.00	0.00	0.00	0.00
MSCI Index Dummy	451	0.11	0.00	0.00	1.00	0.31	0.07	0.10	0.10	0.16

Table 5: Correlations

The table below shows selected correlation coefficients which may be relevant in assessing colinearity between variables. Correlations significant at p = .05 are in **bold.**

	q	m/b	IV	b1	b2	b3	b4	b5	BI	BC	BS	C	P	A	E	KCGI-BS-P	KCGI
ln(Tobin's *q*)	1.00																
ln(market/book)	**0.76**	1.00															
IV (large firm dummy)	**0.12**	**0.07**	1.00														
b1 (50% outside dummy)	**0.20**	**0.13**	**0.68**	1.00													
b2 (> 50% outside dummy)	**0.12**	**0.09**	**0.36**	**0.50**	1.00												
b3 (nomination committee)	**0.15**	**0.12**	**0.59**	**0.50**	**0.28**	1.00											
b4 (audit committee)	**0.15**	**0.11**	**0.66**	**0.55**	**0.31**	**0.55**	1.00										
b5 (compensation committee)	**0.09**	**0.06**	**0.05**	**0.08**	**0.14**	**0.14**	**0.10**	1.00									
Board Independence Subindex	**0.20**	**0.13**	**0.64**	**0.93**	**0.78**	**0.48**	**0.53**	**0.12**	1.00								
Board Committee Subindex	**0.18**	**0.13**	**0.69**	**0.59**	**0.35**	**0.87**	**0.85**	**0.32**	**0.57**	1.00							
Board Structure Index	**0.21**	**0.15**	**0.75**	**0.86**	**0.63**	**0.77**	**0.78**	**0.25**	**0.88**	**0.89**	1.00						
Board Procedure (C)	**0.14**	**0.11**	**0.37**	**0.37**	**0.21**	**0.35**	**0.36**	**0.11**	**0.35**	**0.41**	**0.43**	1.00					
Ownership Parity (P)	0.00	0.00	**-0.06**	**-0.06**	-0.03	-0.02	**-0.08**	0.02	**-0.06**	**-0.05**	**-0.06**	**-0.08**	1.00				
Shareholder Rights (A)	**0.10**	**0.11**	**0.24**	**0.21**	**0.13**	**0.28**	**0.25**	**0.07**	**0.21**	**0.30**	**0.29**	**0.28**	**-0.06**	1.00			
Disclosure (E)	**0.22**	**0.23**	**0.35**	**0.28**	**0.15**	**0.28**	**0.30**	**0.08**	**0.27**	**0.33**	**0.34**	**0.31**	**-0.07**	**0.32**	1.00		
KCGI – BS - P	**0.22**	**0.22**	**0.43**	**0.38**	**0.21**	**0.40**	**0.40**	**0.11**	**0.36**	**0.46**	**0.46**	**0.64**	**-0.09**	**0.73**	**0.81**	1.00	
KCGI	**0.24**	**0.22**	**0.57**	**0.54**	**0.35**	**0.57**	**0.56**	**0.19**	**0.54**	**0.65**	**0.67**	**0.63**	**0.21**	**0.65**	**0.72**	**0.91**	1.00
KCGI - BI	**0.23**	**0.22**	**0.51**	**0.44**	**0.26**				**0.43**			**0.61**	**0.23**	**0.67**	**0.74**	**0.92**	**0.99**
KCGI - BC	**0.23**	**0.22**	**0.47**			**0.44**	**0.42**	**0.13**	**0.48**	**0.49**		**0.62**	**0.25**	**0.67**	**0.74**	**0.93**	**0.98**
KCGI - BS	**0.21**	**0.21**	**0.40**	**0.34**	**0.20**	**0.38**	**0.36**	**0.12**		**0.43**	**0.43**	**0.58**	**0.28**	**0.69**	**0.75**	**0.93**	**0.95**
BC - b3	**0.16**	**0.12**	**0.62**			**0.55**	**0.95**			**0.88**		**0.36**	**-0.06**	**0.25**	**0.30**	**0.40**	
BC - b4	**0.16**	**0.12**	**0.56**				**0.53**			**0.89**		**0.35**	-0.01	**0.27**	**0.28**	**0.40**	
BC - b5	**0.17**	**0.13**	**0.71**					**0.14**		**0.98**		**0.40**	**-0.05**	**0.30**	**0.33**	**0.46**	

Table 6. Basic Event Study Results

Cumulative market adjusted returns (*CMARs*) and cumulative abnormal returns (CARs) are measured for each firm relative to Small Firm Index (equally weighted index of small firms with assets < 1 trillion won at year-end 1998 which had not adopted 50% outside directors by year-end 2004). Daily MAR and CAR returns are winsorized at 1%/99%, outlier CMARs and CARs are excluded if a studentized residual obtained by regressing the dependent variable on a "large plus" dummy (=1 for firms with assets > 1 trillion won at year-end 1998) is greater than ±1.96. Last column includes broad event window that includes events 1-3. Sample before excluding outliers is 64 almost large firms plus 151 or 152 firms in the Small Firm Index. Number of industries = 32. *, **, and *** indicate significance at 10%, 5%, and 1% levels. Significant results for interaction terms (at 5% level or better) are in **boldface**.

Panel A. Cumulative Market Adjusted Returns: regression results

Dependent variable is CMARs over indicated event windows. Independent variables are constant term and "large plus" dummy. *t*-statistics, based on Rogers' robust standard errors and industry-group clusters, are shown in parentheses. Number of industries = 32; number of clusters = 47.

Event	1	1	1	2	2	3	3	4	4	1-3
event window	(-1, +2)	(-2, +3)	(-2, +15)	(-1, +2)	(-2,+3)	(-1, +2)	(-2, +3)	(-1, +2)	(-2, +3)	(1: -2, +25)
calendar dates	6/2 - 6/7	6/1 - 6/8	6/1 - 6/24	6/24 - 6/29	6/23 - 6/30	7/1 - 7/6	6/30 - 7/7	8/24 ~ 8/27	8/23 - 8/30	6/1 - 7/8
Large plus dummy	**0.0583*** **	**0.0809*** **	**0.1105*** **	-0.0070	0.0049	**0.0453*** **	**0.0653*** **	**0.0217***	**0.0555*** **	**0.2121*** **
	(4.56)	**(6.45)**	**(5.57)**	(0.71)	(0.41)	**(3.55)**	**(5.88)**	**(1.79)**	**(4.36)**	**(10.41)**
Constant	-0.0069	-0.0049	-0.0122	-0.0047	-0.0037	-0.0060	-0.0042	-0.0059	-0.0081	-0.0241*
	(1.08)	(0.93)	(1.40)	(0.90)	(0.46)	(1.22)	(0.71)	(1.01)	(1.34)	(1.89)
No. of firms	206	207	204	199	204	201	202	202	205	204
Adjusted R^2	0.1666	0.2109	0.1480	-0.0021	-0.0043	0.0868	0.1235	0.0275	0.1444	0.3107

Panel B. Classic Event Study: CAR results

Cumulative abnormal returns for event study of 64 large plus firms, included in 18 industries, over indicated event windows. Estimation period is Jan-May and -Dec 1999. z-statistics are in parentheses.

Event	1	1	1	2	2	3	3	4	4	1-3
event window	(-1, +2)	(-2, +3)	(-2, +15)	(-1, +2)	(-2,+3)	(-1, +2)	(-2, +3)	(-1, +2)	(-2, +3)	(-2, +25)
Industry CARs	**0.0543*** **	**0.0984*** **	**0.1338*** **	0.0064	**0.0362*** **	**0.0466*** **	**0.0800*** **	**0.0236*** **	**0.0349*** **	**0.2218*** **
	(7.50)	**(11.09)**	**(8.71)**	(0.70)	**(3.07)**	**(6.15)**	**(9.12)**	**(3.51)**	**(4.34)**	**(11.44)**
No. of industries	18	18	18	18	18	18	18	18	18	18
Portfolio CARs	**0.0644*** **	**0.0995*** **	**0.1274*** **	0.0086	0.0444	**0.0652*** **	**0.0999*** **	0.0256	0.0527	**0.2359*** **
	(2.45)	**(3.09)**	**(2.28)**	(0.33)	(1.38)	**(2.48)**	**(3.10)**	(0.97)	(1.64)	**(3.39)**

Table 7: Event Study - Regression Discontinuity Results

Dependent variable is CMARs over indicated event windows. *t*-statistics, based on Rogers' robust standard errors and industry-group clusters, are shown in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% levels. Significant results for interaction terms (at 5% level or better) are in **boldface**

Panel A. Basic Regression Discontinuity Results

Similar to *Table 6, Panel A*, except includes ln(market capitalization at May 31, 1999) as additional independent variable. Correlation between large plus dummy and ln(market cap) = 0.75.

Event	1	3	4	1-3
event window	(-2, +3)	(-2, +3)	(-2, +3)	(-2, +25)
calendar dates	6/1 - 6/8	6/30 - 7/7	8/23 - 8/30	6/1 - 7/8
Regression set 1: Controlling for ln(market cap)				
large plus dummy	**0.0573*** **	**0.0505** **	**0.0443*** **	**0.1665*** **
	(3.89)	**(2.67)**	**(2.76)**	**(4.28)**
ln(market cap)	**0.0097** **	0.0060	0.0046	0.0190
	(2.43)	(1.07)	(0.96)	(1.60)
constant	yes	yes	yes	yes
No. of firms	207	202	205	204
Adjusted R^2	0.2211	0.1240	0.1449	0.3190
Regression set 2: Controlling for 6 powers of ln(market cap)				
large plus dummy	**0.0450** **	0.0496*	**0.0481*** **	**0.1750*** **
	(2.50)	(1.86)	**(2.85)**	**(4.71)**
Adjusted R^2	0.2260	0.1190	0.1513	0.3161

Panel B. 6 Powers Results; Alternate Specifications

Regression (1) repeats last regression in *Panel A*, regression set 2. Regression (2) is similar except uses large firm dummy (=1 if firm assets > 2 trillion won) instead of large plus dummy (=1 if firm assets > 1 trillion won), and omits firms with assets between 1 and 2 trillion won. Regressions (3-4) include add the same control variables as in Table 10, other than ln(assets).

Regression	(1)	(2)	(3)	(4)
event window		(1: -2, +25)		
calendar dates		6/1 - 7/8		
large plus dummy	**0.1750*** **		**0.2811*** **	
	(4.71)		**(5.43)**	
large firm dummy		**0.1713*** **		**0.2723*** **
		(3.57)		**(4.12)**
6 powers of ln(market cap)	Y	Y	Y	Y
other control variables	N	N	Y	Y
constant	Y	Y	Y	Y
No. of firms	204	190	184	171
Adjusted R^2	0.3161	0.3076	0.4360	0.4151

Table 8: Difference-in-Differences: Market Value

Firm fixed effects regressions for *ln*(Tobin's *q*) or *ln*(market/book). Independent variables in Regressions (1) and (4) are large firm dummy, period dummies (omitting the base date of May 31, 1999), period dummies interacted with the large firm dummy. Large firm dummy captures difference between small and large firms at May 31, 1999. Regression (2) adds first 6 powers of ln(assets); regression (3) adds other control variables as in Table 10, regression (3). Regressions are estimated over June 1996 to December 2004, but coefficients are suppressed for periods before Dec. 1997 and after Dec. 2001. Periods are every 6 months except that May 31, 1999 replaces June 30, 1999. Sample excludes small firms that voluntarily adopt 50% outside directors, and large firms that have 50% outside directors as of May 31, 1999. *, **, and *** indicate significance at 10%, 5%, and 1% levels. All regressions use firm clusters and Rogers' robust standard errors. *t*--values are reported in parentheses. Significant results (at 5% level or better) are in **boldface**.

	(1)	(2)	(3)	(4)
	***ln*(Tobin's *q*)**	***ln*(Tobin's *q*)**	***ln*(Tobin's *q*)**	***ln*(Market/Book)**
12/1997 dummy * large firm dummy	-0.0584	-0.0472	-0.0266	-0.2059*
	(1.34)	(1.11)	(0.55)	(1.71)
06/1998 dummy * large firm dummy	0.0394	0.0482	0.0586	-0.0877
	(0.93)	(1.17)	(1.40)	(0.77)
12/1998 dummy * large firm dummy	0.0024	0.0083	0.0245	-0.0839
	(0.08)	(0.28)	(0.81)	(0.96)
12/1999 dummy * large firm dummy	**0.1608*** **	**0.1498*** **	**0.1501*** **	**0.2150*** **
	(3.58)	**(3.39)**	**(3.36)**	**(2.64)**
06/2000 dummy * large firm dummy	**0.1706*** **	**0.1565*** **	**0.1520*** **	**0.2038** **
	(4.90)	**(4.52)**	**(4.51)**	**(2.23)**
12/2000 dummy * large firm dummy	**0.1747*** **	**0.1572*** **	**0.1496*** **	**0.2022** **
	(4.52)	**(3.90)**	**(3.86)**	**(2.07)**
06/2001 dummy * large firm dummy	**0.1607*** **	**0.1399*** **	**0.1219*** **	0.2057*
	(4.05)	**(3.35)**	**(3.19)**	(1.88)
12/2001 dummy * large firm dummy	**0.1740*** **	**0.1554*** **	**0.1376*** **	**0.2441** **
	(4.27)	**(3.66)**	**(3.29)**	**(2.36)**
Large firm dummy	0.0510	0.0268	0.0020	-0.0063
	(1.27)	(0.58)	(0.04)	(0.05)
Period dummies	Yes	Yes	Yes	Yes
6 powers of *ln*(assets)	No	Yes	Yes	No
Other control variables	No	No	Yes	No
Observations	3879	3879	3717	3841
No. of large firms	205	205	205	205
Adjusted R^2	0.359	0.381	0.426	0.412

Table 9: Difference-in-Differences: Profitability and Dividends

All panels: Sample period is 1996 through 2004. *, **, and *** indicate significance at 10%, 5%, and 1% levels. All regressions use firm clusters and Rogers' robust standard errors. *t*-values are reported in parentheses. Significant results for interaction terms (at 5% level or better) are in **boldface**.

Panel A: Annual results for profitability and dividends. Firm fixed effects regressions of indicated profitability and dividend variables on year dummies, large firm dummy (=1 for firms with assets > 2 trillion won, 0 otherwise), interactions between large firm dummy and year dummies, and a constant term. Large firm dummy captures difference between large and small firms in base year. Interaction terms capture difference in differences between large and small firms, relative to base year. Profitability variables are winsorized at 5% and 95% levels.

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	**EBIT/assets**	**Ordinary income/book value of equity**	**Net income/book value of equity**	**Ln(dividends/ sales)**	**EBIT/assets**	**EBIT/assets**
12/1996 dummy * large firm dummy	0.0020 (0.22)	-0.0380 (0.66)	-0.0443 (0.84)	**-0.6356**** **(2.29)**	0.0047 (0.51)	0.0146 (1.34)
12/1997 dummy * large firm dummy	0.0114 (1.42)	-0.0500 (1.01)	-0.0139 (0.32)	**-0.9568***** **(3.25)**	0.0120 (1.44)	0.0148* (1.72)
12/1998 dummy * large firm dummy	-0.0063 (0.82)	-0.0230 (0.50)	-0.0445 (0.93)	-0.6663* (1.82)	-0.0067 (0.85)	-0.0042 (0.56)
12/1999 dummy * large firm dummy	**-0.0159**** **(2.28)**	0.0280 (0.66)	0.0671 (1.44)	0.3915 (1.65)	**-0.0157**** **(2.13)**	**-0.0140**** **(1.97)**
12/2001 dummy * large firm dummy	-0.0043 (0.50)	-0.0347 (0.95)	-0.0581* (1.67)	0.0130 (0.07)	-0.0035 (0.39)	-0.0017 (0.20)
12/2002 dummy * large firm dummy	0.0053 (0.58)	**0.0804**** **(2.24)**	0.0619* (1.77)	0.3219 (1.34)	0.0059 (0.62)	0.0077 (0.84)
12/2003 dummy * large firm dummy	**0.0244***** **(2.84)**	**0.1157***** **(3.08)**	**0.1049***** **(3.12)**	**0.4957**** **(2.47)**	**0.0245***** **(2.73)**	**0.0250***** **(2.79)**
12/2004 dummy * large firm dummy	**0.0325***** **(3.78)**	**0.1662***** **(3.86)**	**0.1553***** **(3.84)**	**0.6326***** **(2.88)**	**0.0322***** **(3.45)**	**0.0309***** **(3.16)**
Large firm dummy	0.0075 (0.78)	-0.0574 (1.44)	-0.0728* (1.70)	-0.2189 (0.89)	-0.0052 (0.51)	-0.0062 (0.61)
ln(assets)					**0.0176***** **(5.23)**	**0.0443***** **(2.99)**
Firm fixed effects, year dummies, constant term	Yes	Yes	Yes	Yes	Yes	Yes
6 powers of ln(assets)	No	No	No	No	No	Yes
Observations	5,956	5,956	5,954	3,662	5,956	5,956
Large firm observations	422	422	422	272		
within R^2	0.0197	0.0203	0.0159	0.1706		
between R^2	0.0186	0.0016	0.0001	0.0762		
overall R^2	0.0044	0.0132	0.0097	0.0812		

Panel B: Combined reform period results for profitability and dividends. Similar to panel A except independent variables are reform period dummy (=1 for year-end 2002 and after), large firm dummy, interaction term, and constant term. Interaction term captures average difference in differences between large and small firms, relative to base year.

	(1)	(2)	(3)	(4)
	EBIT/assets	**Ordinary income/book value of equity**	**Net income/book value of equity**	**ln(dividends/sales)**
Reform period dummy	**-0.0072*** **	-0.0063	-0.0031	**0.2771*** **
	(3.52)	(0.74)	(0.38)	**(7.68)**
Reform period dummy * large firm dummy	**0.0241*** **	**0.1365*** **	**0.1179*** **	**0.6118*** **
	(4.00)	**(6.00)**	**(5.12)**	**(4.03)**
Large firm dummy	0.0008	-0.0560*	-0.0598	-0.1938
	(0.09)	(1.71)	(1.44)	(1.16)
Firm fixed effects, constant term	Yes	Yes	Yes	Yes
Observations	5,956	5,956	5,954	3,662
overall R^2	0.0019	0.0031	0.0020	0.0330

Panel C: Related party transactions. Similar to panel B, except dependent variables are (i) related party loans/assets; related party borrowings/debt; related party sales/revenue, and (asset sales to related parties/assets), each winsorized at 1%/99%.

	(1)	(2)	(3)	(4)
	Related party loans/assets	**Related party borrowings/ debt**	**Related party sales/revenue**	**Asset sales to related parties/assets**
Reform period dummy	0.0019	**0.0186*** **	0.0091*	**0.0081*** **
	(0.11)	**(5.59)**	(1.81)	**(2.87)**
Reform period dummy * large firm dummy	0.0696	0.0026	0.0103	**-0.0129*** **
	(1.24)	(0.25)	(0.56)	**(2.91)**
Large firm dummy	-0.0973	**0.0135** **	0.0011	**0.0096** **
	(1.17)	**(2.10)**	(0.05)	**(2.14)**
Firm fixed effects, constant term	Yes	Yes	Yes	Yes
Observations	5,957	5,957	5,956	5,957
overall R^2	0.0010	0.0032	0.0014	0.0000

Table 10: Full Sample Results for Board Structure Index

Coefficients from regressions of *ln*(Tobin's *q*) on Board Structure Index, (*KCGI* – Board Structure Index - Ownership Parity), Ownership Parity Index, and control variables. Outliers for each year are identified and dropped if the studentized residual from a regression of *ln*(Tobin's *q*) on Board Structure Index is greater than ±1.96. Fixed effects regressions omit *ADR*, *MSCI* index, and industry dummies due to minimal or no within-firm variation over time. *, **, and *** indicate significance at 10%, 5%, and 1% levels. All regressions use year dummies and Rogers' robust standard errors. *OLS* and firm fixed effects regressions use firm clusters. *t*- or *z*-values are reported in parentheses. R^2 is adjusted R^2 for *OLS*, overall R^2 for random effects, and within R^2 for fixed effects regressions. Significant results (at 5% level or better) are in **boldface**.

	(1)	(2)	(3)	(4)	(5)
	ln(Tobin's *q*)				*ln*(market/book)
	Pooled *OLS*	**Random Effects**	**Fixed Effects (Unbalanced)**	**Fixed Effects (Balanced)**	**Fixed Effects (Unbalanced)**
Board Structure Index	**0.0124*** **	**0.0109*** **	**0.0101*** **	**0.0090*** **	**0.0176*** **
	(6.87)	**(10.23)**	**(6.58)**	**(4.79)**	**(3.87)**
KCGI - Board Structure Index - Ownership Parity	**0.0025** **	**0.0014** **	0.0011	0.0004	0.0025
	(2.31)	**(2.02)**	(1.09)	(0.31)	(1.17)
Ownership Parity	**0.0091*** **	0.0038*	0.0005	0.0001	0.0010
	(3.77)	(1.86)	(0.17)	(0.02)	(0.11)
ln(assets)	**-0.0305*** **	**-0.0394*** **	**-0.0566*** **	-0.0460	**-0.1682*** **
	(3.31)	**(5.00)**	**(2.70)**	(1.30)	**(3.11)**
ln(years listed)	**-0.0520*** **	**-0.0574*** **	**-0.0974*** **	**-0.1771*** **	**-0.3087*** **
	(5.24)	**(6.09)**	**(2.86)**	**(2.83)**	**(4.16)**
leverage	-0.0000	-0.0000*	-0.0000	-0.0000	**-0.0183*** **
	(1.01)	(1.74)	(1.31)	(0.48)	**(5.19)**
sales growth	-0.0001	-0.0000	-0.0001	0.0057	-0.0005
	(0.64)	(0.26)	(0.51)	(0.13)	(0.80)
R&D/sales	**0.0672*** **	**0.0224** **	**0.0184*** **	**0.0180*** **	**0.0663*** **
	(5.94)	**(1.98)**	**(3.37)**	**(3.45)**	**(4.27)**
advertising/sales	**1.2596*** **	**1.0291*** **	0.8610	0.8515	**3.3290** **
	(2.80)	(2.67)	(1.35)	(1.36)	**(2.58)**
exports/sales	-0.0050	-0.0335	-0.0745*	-0.0100	**-0.2583*** **
	(0.16)	(1.31)	(1.95)	(0.18)	**(2.79)**
PPE/sales	-0.0238	-0.0268*	**-0.0417** **	**-0.1835*** **	**-0.1794*** **
	(1.42)	(1.85)	**(1.98)**	**(4.50)**	**(3.28)**
$(PPE/sales)^2$	0.0007	0.0005	0.0008	**0.0292*** **	**0.0049** **
	(1.20)	(1.12)	(1.28)	**(5.09)**	**(2.57)**
capex/PPE	**0.1292*** **	**0.0698*** **	0.0541*	**0.0824** **	**0.1963*** **
	(3.59)	**(2.78)**	(1.91)	**(2.43)**	**(2.72)**
EBIT/sales	**-0.0199*** **	**-0.0153*** **	**-0.0087** **	0.0682	-0.0101
	(2.86)	**(3.95)**	**(2.19)**	(1.40)	(0.08)
market share	0.1322	**0.2695*** **	**0.3072*** **	0.2356	0.1768
	(1.59)	**(2.91)**	**(2.70)**	(1.41)	(0.57)
share turnover	**0.0000*** **	0.0000	0.0000	0.0000	**0.0049*** **
	(3.23)	(1.16)	(0.56)	(0.27)	**(2.67)**
foreign ownership	**0.0022*** **	**0.0024*** **	**0.0026*** **	**0.0035*** **	**0.0097*** **
	(3.19)	**(5.52)**	**(3.82)**	**(3.89)**	**(5.96)**
sole ownership	**0.0555*** **	**0.0439*** **			
	(3.05)	**(3.08)**			
$(sole\ ownership)^2$	**-0.0047*** **	**-0.0030*** **	-0.0014	-0.0005	-0.0032
	(3.56)	**(2.74)**	(0.76)	(0.17)	(0.75)
chaebol dummy	0.0000	0.0000	-0.0000	0.0000	0.0000
	(1.20)	(0.18)	(0.11)	(0.02)	(0.52)
ADR Level 1 dummy	-0.0361	0.0234			
	(0.74)	(0.69)			
ADR Level 2-3 dummy	0.0073	0.0660			
	(0.07)	(0.19)			
MSCI index dummy	0.0380*	0.0232			
	(1.79)	(1.50)			
year dummies	yes	yes	yes	yes	yes
4-digit industry dummies	yes	yes	not avail.	not avail.	not avail.
Observations (No. of firms)	3693 (656)	3693 (656)	3693 (656)	1947 (263)	3685 (654)
R^2	0.2832	0.2788	0.2201	0.2790	0.3174

Table 11: Full Sample Results for Board Independence and Board Committee Subindices

Coefficients from regressions of *ln*(Tobin's *q*) on Board Independence and Board Committees Subindices, board structure elements, indicated control for rest of *KCGI*, and other control variables as in *Table 9*. Outliers for each year are identified and dropped if the studentized residual from a regression of *ln*(Tobin's *q*) on Board Structure Index (regressions (1) and (4)), Board Independence Subindex (reg. (2)) or Board Independence Subindex (reg. (3)) is greater than ±1.96. *, **, and *** indicate significance at 10%, 5%, and 1% levels. All regressions use year dummies, unbalanced panels, and Rogers' robust standard errors. *OLS* and firm fixed effects regressions use firm clusters. *t*- or *z*-values are reported in parentheses. *p*-values for joint significance in regression (4) are from an F-test. R^2 is adjusted R^2 for *OLS*, overall R^2 for random effects, and within R^2 for fixed effects regressions. Significant results (at 5% level or better) are in **boldface**.

		ln(Tobin's *q*)			*ln*(M/B)
Panel		**Pooled *OLS***	**Random Effects**	**Fixed Effects**	**Fixed Effects**
(1)	Board Independence Subindex	**0.0159*** (5.44)**	**0.0133*** (7.67)**	**0.0122*** (5.48)**	**0.0250*** (3.48)**
	Board Committee Subindex	**0.0087** (2.33)**	**0.0079*** (3.58)**	**0.0074** (2.48)**	0.0093 (1.23)
	KCGI - Board Structure Index	**0.0040*** (4.14)**	**0.0018*** (2.83)**	0.0010 (1.12)	0.0024 (1.21)
	R^2	0.2807	0.2771	0.2206	0.2543
(2)	b1 (50% outside director dummy)	**0.1145*** (5.29)**	**0.0864*** (6.66)**	**0.0791*** (4.85)**	**0.1543*** (3.15)**
	b2 (> 50% outside director dummy)	0.0451* (1.73)	**0.0570*** (3.43)**	**0.0588*** (2.69)**	0.1451* (1.93)
	KCGI - Board Independence Subindex	**0.0040*** (4.41)**	**0.0021*** (3.53)**	0.0012 (1.45)	0.0027 (1.35)
	R^2	0.2779	0.2742	0.2213	0.3227
(3)	Nominating committee	**0.0536** (2.47)**	**0.0380*** (3.11)**	0.0340* (1.95)	0.0079 (0.19)
	Audit committee	0.0380 (1.55)	**0.0437*** (3.05)**	**0.0439** (2.08)**	0.0872* (1.79)
	Compensation committee	0.0782 (1.58)	0.0431 (1.63)	0.0379 (0.90)	-0.0631 (0.68)
	KCGI - Board Committee Subindex	**0.0047*** (5.01)**	**0.0027*** (4.27)**	**0.0017** (1.99)**	**0.0046** (2.19)**
	R^2	0.2676	0.2646	0.2087	0.3113
(all)	year dummies, other control variables	yes	yes	yes	yes
	Observations	3708	3708	3708	0.3119
	No. of firms	658	658	658	656

Table 12: Firm Fixed Effects (Subsample Results)

Coefficients from firm fixed effects regressions for indicated subsamples of *ln*(Tobin's *q*) on (i) Board Structure Index, (ii) Board Independence and Board Committee Subindices, and (iii) 50% outside directors dummy and > 50% outside directors dummy, in each case with control for rest of KCGI and other control variables as in *Table 10*. Outliers for each year are identified and dropped if the studentized residual from a regression of *ln*(Tobin's *q*) on [Board Structure Index for specifications (i)-(ii); Board Independence Subindex for specification (iii)] is greater than ±1.96. In regression (2), large firms are treated as complying with the 1999 rules as of year-end 1999. *, **, and *** indicate significance at 10%, 5%, and 1% levels. All regressions use year dummies, unbalanced panel, firm clusters, and Rogers' robust standard errors. *t*-values are reported in parentheses. Significant results (at 5% level or better) are shown in **boldface.**

Specification			**(i)**	**(ii)**		**(iii)**	
	Subsample	Sample Size	Board Structure	Board Independence	Board Committee	50% outside directors	> 50% outside directors
(1)	Small Firms	3,449	**0.0090*** (4.85)**	**0.0119*** (4.65)**	0.0057* (1.75)	**0.0656*** (3.20)**	0.0551* (1.72)
(2)	Large Firms	412	**0.0085**** (2.18)	0.0078* (1.70)	0.0124 (1.54)	0.0911 (1.61)	0.0268 (1.27)
(3)	Financial Firms	324	**0.0092*** (3.53)**	**0.0080*** (2.71)**	**0.0119**** (2.51)**	**0.0621**** (2.10)**	0.0315 (1.07)
(4)	Non-Financial Firms	3,370	**0.0108*** (4.54)**	**0.0146*** (4.48)**	0.0059 (1.37)	**0.0832*** (3.74)**	**0.0670**** (2.02)**
(5)	*Chaebol* Firms	770	**0.0076*** (2.70)**	**0.0083**** (2.20)**	0.0064 (1.13)	**0.0611**** (2.16)**	0.0311 (0.86)
(6)	Non-*Chaebol* Firms	2,924	**0.0101*** (3.94)**	**0.0133*** (4.36)**	0.0064 (1.28)	**0.0764*** (2.77)**	0.0601 (1.64)
(7)	Non-manufacturing firms	1,049	**0.0091*** (3.90)**	**0.0132*** (3.76)**	0.0026 (0.55)	**0.0606**** (2.51)**	**0.0809*** (2.60)**
(8)	Manufacturing firms	2,645	**0.0117*** (4.16)**	**0.0129*** (3.65)**	0.0100* (1.87)	**0.0953*** (3.74)**	0.0315 (0.85)

Table 13: Two Stage Least Squares Results

Instrumental variable results using large firm dummy as an instrument for Board Structure Index, using pooled data from 1999-2004. All regressions use year dummies, and other control variables as in *Table 10* except that we exclude *MSCI* Index and *ADR* dummy variables due to high correlation with large firm dummy. *OLS* regressions use firm clusters; all regressions use Rogers' robust standard errors. Clusters are not available with random effects in Panel B. R^2 is adjusted R^2 for *OLS* and overall R^2 for random effects. *t*-values are reported in parentheses. *, **, and *** respectively indicate significance levels at 10%, 5%, and 1% levels. Significant results (at 5% level or better) are in **boldface.** No. of observations = 3,256.

Panel A: Durbin-Wu-Hausman Test for Endogeneity of Board Structure Index

The Durbin-Wu-Hausman test assumes that large firm dummy is an appropriate instrument.

	First Stage	**Second Stage**	
		ln(Tobin's *q*)	*ln*(market/book)
	(1)	(2)	(3)
Board Structure Index		**0.0150*** (3.73)**	**0.0460*** (4.24)**
Ownership Parity	**0.0828*** (2.99)**	**0.0087*** (3.44)**	0.0051 (0.76)
KCGI - Board Structure Index – Ownership Parity	**0.0408*** (3.44)**	**0.0025** (2.11)**	0.0053* (1.90)
Residual From 1st Stage		-0.0039 (0.95)	**-0.0314*** (2.78)**
Large firm dummy	**7.2925*** (15.78)**		
ln(assets)	**0.4682*** (4.15)**	**-0.0308*** (2.72)**	**-0.1212*** (4.63)**
year dummies, other control variables	yes	yes	Yes
Adjusted R^2	0.5954	0.2722	0.4016

Panel B: *2SLS* Results for Board Structure Index

Two-stage (*2SLS*) regressions of ln(Tobin's *q*) and ln(market/book) on Board Structure Index, estimated using OLS and firm random effects specifications. First stage is the same for both dependent variables and both specifications. Second stage is estimated using the fitted value for Board Structure Index from the first stage. Other control variables and treatment of outliers are the same as in *Table 10*.

	First Stage	**Second Stage**			
	Board Structure Index	*ln*(Tobin's *q*)		*ln*(market/book)	
	Pooled OLS	Pooled OLS	Random Effects	Pooled OLS	Random Effects
	(1)	(2)	(3)	(4)	(5)
Instrumented Board Structure Index		**0.0150*** **	**0.0207*** **	**0.0461*** **	**0.0543*** **
		(3.66)	**(3.04)**	**(4.20)**	**(4.16)**
Ownership Parity	**0.0828*** **	**0.0087*** **	**0.0039** **	0.0051	0.0000
	(2.99)	**(3.39)**	**(2.10)**	(0.75)	(0.00)
KCGI - Board Structure Index – Ownership Parity	**0.0408*** **	**0.0025** **	0.0007	0.0052*	0.0020
	(3.44)	**(2.10)**	(0.92)	(1.92)	(1.10)
Large firm dummy	**7.2925*** **				
	(15.78)				
ln(assets)	**0.4682*** **	**-0.0308*** **	**-0.0417*** **	**-0.1213*** **	**-0.1274*** **
	(4.15)	**(2.69)**	**(3.94)**	**(4.62)**	**(5.52)**
year dummies, other control variables	yes	Yes	Yes	Yes	Yes
Observations	3,256	3,256	3,256	3,255	3,256
R^2	0.5954	0.2600	0.2648	0.3986	0.3757

Table 14. *3SLS* Results for Board Structure Index

Three-stage least squares (*3SLS*) results, instrumenting for both Board Structure Index and ln(Tobin's *q*) or ln(market/book), estimated with *OLS*. Large firm dummy is instrument for Board Structure Index. Instrument for ln(Tobin's *q*) (or ln(market/book)) are R&D/sales, advertising/sales, and EBIT/sales as additional instruments. All of these variables predict Tobin's *q* (see *Table 10*), lack a strong theoretical connection to board structure, and do not predict board structure in unreported regressions similar to *Table 10* with Board Structure Index as dependent variable. Regressions use year dummies, and other control variables as in *Table 10* except that we exclude *MSCI* Index and *ADR* dummy variables due to high correlation with large firm dummy. Firm clusters and robust standard errors are not used because not available with the 3SLS specification. *t*-values are reported in parentheses. p-values in last row are from Hansen overidentification test. *, **, and *** respectively indicate significance levels at 10%, 5%, and 1% levels. Significant results (at 5% level or better) are in **boldface.** No. of observations = 3,256.

	Board Structure Index		*ln*(Tobin's *q*)	*ln*(market/book)
	(1)	(2)	(3)	(4)
Board Structure Index			**0.0150*** (5.59)**	**0.0460*** (7.52)**
ln(Tobin's *q*)	3.1339* (1.82)			
ln(market/book)		**1.8670** (1.99)**		
Large firm dummy	**6.9372*** (23.55)**	**6.9623*** (17.24)**		
R&D/sales			**0.0645** (2.29)**	**0.1684*** (2.58)**
advertising/sales			**1.2482*** (5.05)**	**2.3476*** (4.23)**
EBIT/sales			**-0.0200*** (2.67)**	-0.0917 (1.13)
Ownership Parity	**0.0507** (2.08)**	**0.0556*** (2.82)**	**0.0087*** (5.71)**	0.0051 (1.43)
KCGI - Board Structure Index – Ownership Parity	**0.0314*** (3.01)**	0.0215* (1.92)	**0.0025*** (3.20)**	**0.0053*** (2.95)**
ln(assets)	**0.5473*** (6.96)**	**0.5771*** (4.77)**	**-0.0308*** (4.95)**	**-0.1212*** (8.28)**
year dummies, other control variables	yes	yes	yes	yes
Hansen overidentification test			p = 0.443	p = 0.77