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Corporate Governance and Equity Prices

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CORPORATE GOVERNANCE AND EQUITY PRICES

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ABSTRACT

Shareholder rights vary across firms. Using the incidence of 24 governance rules, we construct a "Governance Index" to proxy for the level of shareholder rights at about 1500 large firms during the 1990s. An investment strategy that bought firms in the lowest decile of the index (strongest rights) and sold firms in the highest decile of the index (weakest rights) would have earned abnormal returns of 8.5 percent per year during the sample period. We find that firms with stronger shareholder rights had higher firm value, higher profits, higher sales growth, lower capital expenditures, and made fewer corporate acquisitions.

Keywords: *Corporate governance, shareholder rights, investor protection, agency problems, entrenched management, hostile takeovers, poison pills, golden parachutes, greenmail.*

I. Introduction

Corporations are republics. The ultimate authority rests with voters (shareholders). These voters elect representatives (directors) who delegate most decisions to bureaucrats (managers). As in any republic, the actual power-sharing relationship depends upon the specific rules of governance. One extreme, which tilts toward a democracy, reserves little power for management and allows shareholders to quickly and easily replace directors. The other extreme, which tilts toward a dictatorship, reserves extensive power for management and places strong restrictions on shareholders' ability to replace directors. Presumably, shareholders accept restrictions of their rights in hopes of maximizing their wealth, but little is known about the ideal balance of power. From a theoretical perspective, there is no obvious answer. In this paper, we ask an empirical question -- is there a relationship between shareholder rights and corporate performance?

Twenty years ago, large corporations had little reason to restrict shareholder rights. Proxy fights and hostile takeovers were rare, and investor activism was in its infancy. By rule, most firms were shareholder democracies, but in practice management had much more of a free hand than they do today. The rise of the junk bond market in the 1980s disturbed this equilibrium by enabling hostile-takeover offers for even the largest public firms. In response, many firms added takeover defenses and other restrictions of shareholder rights. Among the most popular were those that stagger the terms of directors, provide severance packages for managers, and limit shareholders' ability to meet or act. During the same time period, many states passed antitakeover laws giving firms further defenses against hostile bids. By 1990, there was considerable variation across firms in the strength of shareholder rights. The takeover market subsided in the early 1990s, but this variation remained in place throughout the decade.

Most research on the wealth impact of takeover defenses uses event-study methodology, where firms' stock returns are analyzed following the announcement of a new defense.[1] Such studies face the difficulty that new defenses may be driven by contemporaneous conditions at the firm, i.e., adoption of a defense may both change the governance structure and provide a signal of managers' private information about impending takeover bids. Event studies of changes in state takeover laws are mostly immune from this problem, but it is difficult to identify a single date for an event that is preceded by legislative negotiation and followed by judicial uncertainty. For these and other reasons, some authors argue that event-study methodology cannot identify the impact of governance provisions.[2]

We avoid these difficulties by taking a long-horizon approach. We combine a large set of governance provisions into an index which proxies for the strength of shareholder rights, and then study the empirical relationship between this index and corporate performance. Our analysis should be thought of as a "long-run event study": we have democracies and dictatorships, the rules stayed mostly the same for a decade -- how did each type do? Our main results are to demonstrate that, in the 1990s, democracies earned significantly higher returns, were valued higher, and had better operating performance. Our analysis is not a test of market efficiency. Because theory provides no clear prediction, there is no reason that investors in 1990 should have foreseen the outcome of this novel experiment. Also, because this "experiment" did not use random assignment, we cannot make strong claims about causality, but we do explore the implications and assess the supportive evidence for several causal hypotheses.[3]

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[1] Surveys of this literature can be found in Bhagat and Romano [2001], Bittlingmayer [2000], Comment and Schwert [1995], and Karpoff and Malatesta [1989].

[2] See Coates [2000] for a detailed review of these arguments.

[3] Other papers that analyze relationships between governance and either firm value or performance have generally focused on board composition, executive compensation, or insider ownership [Baysinger and Butler 1985, Bhagat and Black 1998, Core, Holthausen, and Larcker 1999, Hermalin and Weisbach 1991, Morck, Shleifer, and Vishny 1988, Yermack 1996]. See Shleifer and Vishny [1997] for a survey.

Our data are derived from publications of the Investor Responsibility Research Center. These publications provide 24 distinct corporate-governance provisions for approximately 1,500 firms since 1990.[4] In Section II, we describe these provisions and data sources in more detail. We divide the rules into five thematic groups and then construct a "Governance Index" as a proxy for the balance of power between shareholders and managers. Our index construction is straightforward: for every firm, we add one point for every provision that reduces shareholder rights. This reduction of rights is obvious in most cases; the few ambiguous cases are discussed. Firms in the highest decile of the index are placed in the "Dictatorship Portfolio" and are referred to as having the "highest management power" or the "weakest shareholder rights"; firms in the lowest decile of the index are placed in the "Democracy Portfolio" and are described as having the "lowest management power" or the "strongest shareholder rights".

In Section III, we document the main empirical relationships between governance and corporate performance. Using performance-attribution time-series regressions from September 1990 to December 1999, we find that the Democracy Portfolio outperformed the Dictatorship Portfolio by a statistically significant 8.5 percent per year. These return differences induced large changes in firm value over the sample period. By 1999, a one-point difference in the index was negatively associated with an 11.4 percentage-point difference in Tobin's *Q*. After partially controlling for differences in market expectations by using the book-to-market ratio, we also find evidence that firms with weak shareholder rights were less profitable and had lower sales growth than other firms in their industry.

[4] These 24 provisions include 22 firm-level provisions and six state laws (four of the laws are analogous to four of the firm-level provisions). For the remainder of the paper, we refer interchangeably to corporate governance "laws", "rules", and "provisions". We also refer interchangeably to "shareholders" and "investors" and refer to "management" as comprising both managers and directors.

The correlation of the Governance Index with returns, firm value, and operating performance could be explained in several ways. Section IV sets out three hypotheses to explain the results. Hypothesis I is that weak shareholder rights caused additional agency costs. If the market underestimated these additional costs, then a firm's stock returns and operating performance would have been worse than expected, and the firm's value at the beginning of the period would have been too high. Hypothesis II is that managers in the 1980s predicted poor performance in the 1990s, but investors did not. In this case, the managers could have put governance provisions in place to protect their jobs. While the provisions might have real protective power, they would not have caused the poor performance. Hypothesis III is that governance provisions did not cause poor performance (and need not have any protective power) but rather were correlated with other characteristics that were associated with abnormal returns in the 1990s. While we cannot identify any instrument or natural experiment to cleanly distinguish among these hypotheses, we do assess some supportive evidence for each one in Section V. For Hypothesis I, we find some evidence of higher agency costs in a positive relationship between the index and both capital expenditures and acquisition activity. In support of Hypothesis III, we find several observable characteristics that can explain up to one-third of the performance differences. We find no evidence in support of Hypothesis II. Section VI concludes the paper.

II. Data

A. Corporate-Governance Provisions

Our main data source is the Investor Responsibility Research Center (IRRC), which publishes detailed listings of corporate-governance provisions for individual firms in *Corporate Takeover Defenses* [Rosenbaum 1990, 1993, 1995, and 1998]. These data are derived from a variety of public sources including corporate bylaws and charters, proxy statements, annual reports, as well as 10-K and 10-Q documents filed with the SEC. The IRRC's universe is drawn from the Standard & Poor's (S&P) 500 as well as the annual lists of the largest corporations in the publications of *Fortune*, *Forbes*, and *Businessweek*. The IRRC's sample expanded by several hundred firms in 1998 through additions of some smaller firms and firms with high institutional-ownership levels. Our analysis uses all firms in the IRRC universe except those with dual-class common stock (less than 10 percent of the total).[5] The IRRC universe covers most of the value-weighted market: even in 1990, the IRRC tracked more than 93 percent of the total capitalization of the combined New York Stock Exchange (NYSE), American Stock Exchange (AMEX), and Nasdaq markets.

The IRRC tracks 22 charter provisions, bylaw provisions, and other firm-level rules plus coverage under six state takeover laws; duplication between firm-level provisions and state laws yields 24 unique provisions. Table I lists all of these provisions and Appendix A discusses each one in detail. We divide them into five groups: tactics for delaying hostile bidders (*Delay*); voting rights (*Voting*); director/officer protection (*Protection*); other takeover defenses (*Other*); and state laws (*State*).

[5] We omit firms with dual-class common stock because the wide variety of voting and ownership differences across these firms makes it difficult to compare their governance structures with those of single-class firms.

The *Delay* group includes four provisions designed to slow down a hostile bidder. For takeover battles that require a proxy fight to either replace a board or dismantle a takeover defense, these provisions are the most crucial. Indeed, some legal scholars argue that the dynamics of modern takeover battles have rendered all other defenses superfluous [Daines and Klausner 2001, Coates 2000]. The *Voting* group contains six provisions, all related to shareholders' rights in elections or charter/bylaw amendments. The *Protection* group contains six provisions designed to insure officers and directors against job-related liability or to compensate them following a termination. The *Other* group includes the six remaining firm-level provisions.

These provisions tend to cluster within firms. Out of (22 * 21)/2 = 231 total pairwise correlations for the 22 firm-level provisions, 169 are positive, and 111 of these positive correlations are significant.[6] In contrast, only nine of the 62 negative correlations are significant. This clustering suggests that firms may differ significantly in the balance of power between investors and management.

The IRRC firm-level data do not include provisions that apply automatically under state law. Thus, we supplement this data with state-level data on takeover laws as given by Pinnell [2000], another IRRC publication. From this publication, we code the presence of six types of so-called "second-generation" state takeover laws and place them in the *State* group.[7] Few states

[6] Unless otherwise noted, all statements about statistical significance refer to significance at the five-percent level.

[7] These laws are classified as "second-generation" in the literature to distinguish them from the "first-generation" laws passed by many states in the 60s and 70s and held to be unconstitutional in 1982. See Comment and Schwert [1995] and Bittlingmayer [2000] for a discussion of the evolution and legal status of state takeover laws and firm-specific takeover defenses. The constitutionality of almost all of the second-generation laws and the firm-specific takeover defenses was clearly established by 1990. All of the state takeover laws cover firms incorporated in their home state. A few states have laws that also cover firms incorporated outside of the state that have significant business within the state. The rules for "significant" vary from case to case, but usually cover only a few very large firms. We do not attempt to code for this out-of-state coverage.

have more than three of these laws, and only Pennsylvania has all six.[8] Some of these laws are analogues of firm-level provisions given in other groups. We discuss these analogues in Section II.B.

The IRRC dataset is not an exhaustive listing of all provisions. Although firms can review their listing and point out mistakes before publication, the IRRC does not update every company in each new edition of the book, so some changes may be missed. Also the charter and bylaws are not available for all companies and thus the IRRC must infer some provisions from proxy statements and other filings. Overall, the IRRC intends its listings as a starting point for institutional investors to review governance provisions. Thus, these listings are a noisy measure of a firm's governance provisions, but there is no reason to suspect any systematic bias. Also, all of our analysis uses data available at time t to forecast performance at time $t+1$ and beyond, so there is no possibility of look-ahead bias induced by our statistical procedures.

To build the dataset, we coded the data from the individual firm profiles in the IRRC books. For each firm, we recorded the identifying information (ticker symbol, state of incorporation) and the presence of each provision. Although many of the provisions can be made stronger or weaker (e.g., supermajority thresholds can vary between 51 and 100 percent), we made no strength distinctions and coded all provisions as simply "present" or "not present". This methodology sacrifices precision for the simplicity necessary to build an index.

For most of the analysis of this paper, we match the IRRC data to the Center for Research in Security Prices (CRSP) and, where necessary, to Standard and Poor's Compustat database. CSRP matching was done by ticker symbol and was supplemented by handchecking names, exchanges, and states of incorporation. These procedures enable us to match 100 percent of the

[8] The statistics of Table I reflect exactly the frequency of coverage under the default law in each state. A small minority of firms elect to "opt-out" of some laws and "opt-in" to others. We code these options separately and use them in the creation of our index.

IRRC sample to CRSP, with about 90 percent of these matches having complete annual data in Compustat.

B. The Governance Index

The index construction is straightforward: for every firm, we add one point for every provision that restricts shareholder rights (increases managerial power). This power distinction is straightforward in most cases, as is discussed below. While this simple index does not accurately reflect the relative impacts of different provisions, it has the advantage of being transparent and easily reproducible. The index does not require any judgments about the efficacy or wealth effects of any of these provisions; we only consider the impact on the balance of power.

For example, consider Classified Boards, a provision that staggers the terms and elections of directors and hence can be used to slow down a hostile takeover. If management uses this power judiciously, it could possibly lead to an increase in overall shareholder wealth; if management uses this power to maintain private benefits of control, then this provision would decrease shareholder wealth. In either case, it is clear that Classified Boards increase the power of managers and weaken the control rights of large shareholders, which is all that matters for constructing the index.

Most of the provisions can be viewed in a similar way. Almost every provision gives management a tool to resist different types of shareholder activism, such as calling special meetings, changing the firm's charter or bylaws, suing the directors, or just replacing them all at once. There are two exceptions: Secret Ballots and Cumulative Voting. A Secret Ballot, also called "confidential voting" by some firms, designates a third-party to count proxy votes and

prevents management from observing how specific shareholders vote. Cumulative Voting allows shareholders to concentrate their directors' votes so that a large minority holder can ensure some board representation. (See Appendix A for fuller descriptions.) These two provisions are usually proposed by shareholders and opposed by management.[9] In contrast, none of the other provisions enjoy consistent shareholder support or management opposition; in fact, many of these provisions receive significant numbers of shareholder proposals for their repeal [Ishii 2000]. Also, both Cumulative Voting and Secret Ballots tend to be negatively correlated with the presence of other firm-level provisions (19 negative out of 21 for Cumulative Voting; 11 out of 21 for Secret Ballot). Thus, we consider the presence of Secret Ballots and Cumulative Voting to be *increases* in shareholder rights. For each one, we add one point to the Governance Index when firms do *not* have it. For all other provisions, we add one point when firms do have it.[10]

Thus, the Governance Index ("*G*") is just the sum of one point for the existence (or absence) of each provision. We also construct subindices for each of the five categories: *Delay, Protection, Voting, Other,* and *State*. Recall that there are 28 total provisions listed in the five categories, of which 24 are unique. For the state laws with a firm-level analogue, we add one point to the index if the firm is covered under the firm-level provision, the state law, or both.[11] For example, a firm that has an Antigreenmail provision and is also covered by the

[9] In the case of Secret Ballots, shareholder fiduciaries argue that it enables voting without threat of retribution, such as the loss of investment-banking business by brokerage-house fiduciaries. See Gillan and Bethel [2001] and McGurn [1989].

[10] Only two other provisions – Antigreenmail and Golden Parachutes – seem at all ambiguous. Since both are positively correlated with the vast majority of other firm-level provisions and can logically be viewed as takeover defenses, we code them like other defenses and add one point to the index for each. See their respective entries in Appendix A for a discussion.

[11] Firms usually have the option to opt out of state law coverage. Also, a few state laws require firms to opt in to be covered. The firms that exercise these options are listed in the IRRC data. When we constructed the *State* subindex, we ignored these options and used the default state coverage. When we constructed the *G* index, we included the options and used actual coverage.

Antigreenmail state law would get one point added to both its *State* subindex and its *Other* subindex, but only one point (not two) would be added to its overall *G* index. Thus, *G* has a possible range from 1 to 24 and is not just the sum of the five subindices.

Table II gives summary statistics for *G* and the subindices in 1990, 1993, 1995, and 1998. Table II also shows the frequency of *G* by year, broken up into groups beginning with $G \leq 5$, then each value of *G* from $G = 6$ through $G = 13$, and finishing with $G \geq 14$. These ten "deciles" are similar but not identical in size, with relative sizes that are fairly stable from 1990 to 1995. In the remainder of the paper, we pay special attention to the two extreme portfolios: the "Dictatorship Portfolio" of the firms with the weakest shareholder rights ($G \geq 14$), and the "Democracy Portfolio" of the firms with the strongest shareholder rights ($G \leq 5$). These portfolios are updated at the same frequency as *G*.

Most of the changes in the distribution of *G* come from changes in the sample due to mergers, bankruptcies, and additions of new firms by the IRRC. In 1998, the sample size increased by about 25 percent, and these new firms tilted toward lower values of *G*. At the firm level, *G* is relatively stable. For individual firms, the mean (absolute) change in *G* between publication dates (1990, 1993, 1995, 1998) is 0.60, and the median (absolute) change between publication dates is zero.[12]

Table III shows the correlations between pairs of subindices. The *Delay*, *Protection*, *Voting*, and *Other* subindices all have positive and significant pairwise correlations with each other. *State*, however, has negative correlations with *Delay*, *Protection*, and *Voting*. It could be that firms view some of the state laws as substitutes for the firm-level provisions, but then it

[12] The IRRC gives dates for some of the provision changes – where available, this data suggests that the majority of the provisions were adopted in the 1980s. Danielson and Karpoff [1998] perform a detailed study on a similar set of provisions and demonstrate a rapid pace of change between 1984 and 1989.

would be surprising that *Other*, which contains three provisions that are direct substitutes for state laws, is the only subindex that is positively correlated with *State*. Overall, it appears that coverage under state laws is not highly correlated with the adoption of firm-level provisions. This fact has implications for the analysis of causality, as is discussed in Section IV.

Table IV lists the ten largest firms (by market capitalization) in the Democracy and Dictatorship Portfolios in 1990 and gives the value of *G* for these firms in 1990 and 1998. Of the ten largest firms in the Democracy Portfolio in 1990, six of them are still in the Democracy Portfolio in 1998, three have dropped out of the portfolio and have $G = 6$, and one (Berkshire Hathaway) disappeared from the sample.[13] The Dictatorship Portfolio has a bit more activity, with only two of the top ten firms remaining in the portfolio, four firms dropping out with $G = 13$, and three firms leaving the sample though mergers or the addition of another class of stock.[14] Thus, 40 percent (eight out of 20) of the largest firms in the extreme portfolios in 1990 were also in these portfolios in 1998. This is roughly comparable to the full set of firms: among all firms in the Democracy and Dictatorship Portfolios in 1990, 31 percent were still in the same portfolios in 1998.

There is no obvious industry concentration among these top firms; the whole portfolios are similarly dispersed. Classifying firms into 48 industries as in Fama and French [1997], the portfolios appear to be broadly similar to each other in all years, with a mix of old-economy and new-economy industries.[15] Each portfolio has an important technology component. "Computers" is the largest industry by market value in the Democracy Portfolio in 1990, with

[13] Berkshire Hathaway disappeared because it added a second class of stock before 1998. Firms with multiple classes of common stock are not included in our analysis.

[14] NCR disappeared after a merger. It reappeared in the sample in 1998 as a spin-out, but since it received a new permanent number from CRSP, we treat the new NCR as a different company.

[15] The industry names are from Fama and French [1997], but use a slightly updated version of the SIC classification of these industries that is given on Ken French's website (June 2001). In Sections III and V, we use both this updated classification and the corresponding industry returns (also from the French website).

22.4 percent of the portfolio, falling to third place with 12.3 percent of the value in 1998. "Communications" does not make the top five in market value for the Dictatorship Portfolio in 1990, but rises to first place with 25.3 percent of the portfolio in 1998.

III. Governance: Empirical Relationships

A. Summary Statistics

Table V gives summary statistics and correlations for *G* (and subindices) with a set of firm characteristics as of September 1990: book-to-market ratio, firm size, share price, monthly trading volume, Tobin's *Q*, dividend yield, S&P 500 inclusion, past five-year stock return, past five-year sales growth, and percentage of institutional ownership. The first four of these characteristics are in logs. The construction of each characteristic is described in Appendix B. The first column of Table V gives the correlation of each of these characteristics with *G*, the next two columns give the mean value in the Democracy and Dictatorship Portfolios, and the final column gives the difference between these means. These results are descriptive and are intended to provide some background for the analyses in the following sections.

The strongest relation is between *G* and S&P 500 inclusion. The correlation between these variables is positive and significant -- about half of the Dictatorship Portfolio is drawn from S&P 500 firms compared to 15 percent of the Democracy Portfolio. Given this finding, it is not surprising that *G* is also positively correlated with size, share price, trading volume, and institutional ownership. S&P firms tend to have relatively high levels of all of these characteristics. In addition, the correlation of *G* with five-year sales growth is negative and significant, suggesting that high-*G* firms had relatively lower sales growth over the second half of the 1980s, the period when many of the provisions were first adopted.

Correlations at other times in the sample period (not shown in the table) are similar. Overall, it appears that firms with weaker shareholder rights tend to be large S&P firms with relatively high share prices, institutional ownership and trading volume, relatively poor sales growth, and poor stock-market performance. The 1990s were a time of rising activism by institutional investors and more attention to governance provisions; thus, we might expect to see some reduction in the institutional ownership of high-*G* firms. In untabulated tests, we find no evidence of such a reduction, with both pairwise correlations and multivariate analysis suggesting no robust relationship between *G* and changes in institutional ownership.

B. Governance and Returns

If corporate governance matters for firm performance *and* this relationship is fully incorporated by the market, then a stock price should quickly adjust to any relevant change in the firm's governance. This is the logic behind the use of event studies to analyze the impact of takeover defenses. If such a reaction occurs, then expected returns on the stock would be unaffected beyond the event window. If, however, governance matters but is not incorporated immediately into stock prices, then realized returns on the stock would differ systematically from equivalent securities.

In this section, we examine the relationship between *G* and subsequent returns. An investment of $1 in the (value-weighted) Dictatorship Portfolio on September 1, 1990, when our data begin, would have grown to $3.39 by December 31, 1999. In contrast, a $1 investment in the Democracy Portfolio would have grown to $7.07 over the same period. This is equivalent to annualized returns of 14.0 percent for the Dictatorship Portfolio and 23.3 percent for the Democracy Portfolio, a difference of more than nine percent per year.

What can explain this disparity? One possible explanation is that the performance differences are driven by differences in the riskiness or "style" of the two portfolios. Researchers have identified several equity characteristics that explain differences in realized returns. In addition to differences in exposure to the market factor ("beta"), a firm's market capitalization (or "size"), book-to-market ratio (or other "value" characteristics), and immediate past returns ("momentum") have all been shown to significantly forecast future returns.[16] If the Dictatorship Portfolio differs significantly from the Democracy Portfolio in these characteristics, then style differences may explain at least part of the difference in annualized raw returns.

Several methods have been developed to account for these style differences in a system of performance attribution. We employ one method here and use another in Section V. The four-factor model of Carhart [1997] is estimated by:

$$(1) \qquad R_t = \alpha + \beta_1 * RMRF_t + \beta_2 * SMB_t + \beta_3 * HML_t + \beta_4 * Momentum_t + \varepsilon_t$$

where R_t is the excess return to some asset in month t, $RMRF_t$ is the month t value-weighted market return minus the risk-free rate, and the terms SMB_t (small minus big), HML_t (high minus low), and $Momentum_t$ are the month t returns on zero-investment factor-mimicking portfolios designed to capture size, book-to-market, and momentum effects, respectively.[17] Although there is ongoing debate about whether these factors are proxies for risk, we take no position on this issue and simply view the four-factor model as a method of performance attribution. Thus, we

[16] See Basu [1977] (price-to-earnings ratio), Banz [1981] (size), Fama and French [1993] (size and book-to-market), Lakonishok, Shleifer and Vishny [1994] (several value measures), and Jegadeesh and Titman [1993] (momentum).

[17] This model extends the Fama-French [1993] three-factor model with the addition of a momentum factor. For details on the construction of the factors, see Fama and French [1993] and Carhart [1997]. We are grateful to Ken French for providing the factor returns for *SMB* and *HML*. *Momentum* returns were calculated by the authors using the procedures of Carhart [1997].

interpret the estimated intercept coefficient, "alpha", as the abnormal return in excess of what could have been achieved by passive investments in the factors.

The first row of Table VI shows the results of estimating (1) where the dependent variable, R_t, is the monthly return difference between the Democracy and Dictatorship Portfolios. Thus, the alpha in this estimation is the abnormal return on a zero-investment strategy that buys the Democracy Portfolio and sells short the Dictatorship Portfolio. For this specification, the alpha is 71 basis points (bp) per month, or about 8.5 percent per year. This point estimate is statistically significant at the one-percent level. Thus, very little of the difference in raw returns can be attributed to style differences in the two portfolios.

The remaining rows of Table VI summarize the results of estimating (1) for all ten "deciles" of G, including the extreme deciles comprising the Democracy ($G \leq 5$) and Dictatorship ($G \geq 14$) Portfolios. As the table shows, the significant performance difference between the Democracy and Dictatorship Portfolios is driven both by overperformance (for the Democracy Portfolio) and underperformance (by the Dictatorship Portfolio). The Democracy Portfolio earns a positive and significant alpha of 29 bp per month, while the Dictatorship Portfolio earns a negative and significant alpha of –42 bp per month.

The results also show that alpha decreases as G increases. The Democracy Portfolio earns the highest alpha of all the deciles, and the next two highest alphas, 24 and 22 bp, are earned by the third ($G = 7$) and second ($G = 6$) deciles, respectively. The Dictatorship Portfolio earns the lowest alpha, and the second lowest alpha is earned by the eighth ($G = 12$) decile. Furthermore, the four lowest G deciles earn positive alphas, while the three highest G deciles earn negative alphas. More formally, a Spearman rank-correlation test of the null hypothesis of

no correlation between *G*-decile rankings and alpha rankings yields a test statistic of 0.842, and is rejected at the one-percent level.

Table VII reports several variations of the abnormal-return results. In each variation, we estimate the performance-attribution regression in equation (1) on the return difference between the Democracy and Dictatorship Portfolios, while changing some aspect of the portfolio construction or return calculation. We perform all of these tests using both value-weighted (VW) and equal-weighted (EW) portfolios. These tests allow us to estimate the fraction of the benchmark abnormal returns that can be attributed to industry composition, choice of cutoffs for the extreme portfolios, new provisions during the decade, legal variation across states, and different time periods.

The first row of Table VII replicates the baseline portfolio construction used above. The remaining rows of the table summarize tests using industry-adjusted returns (Row 2), two alternative constructions of the extreme portfolios (Rows 3 and 4), fixed portfolios built with 1990 levels of *G* (Row 5), a subsample that includes only Delaware firms (Row 6), and subsamples split between the first half and the second half of the sample period (Rows 7 and 8). Details of each of these constructions are given in the table note. The main themes of these results are, first, that the VW returns (Democracy minus Dictatorship) are economically large in all cases and, second, the EW abnormal returns are usually about two-thirds of the VW abnormal returns. Most of the return differential can be attributed to within-state variation already in place in 1990, and this return differential is apparent in both halves of the sample period.

Overall, we find significant evidence that the Democracy Portfolio outperformed the Dictatorship Portfolio in the 1990s. We also find some evidence of a monotonic relationship between *G* and returns. It would be useful to know which subindices and provisions drive these

results. We address this issue in depth within the broader analysis of causality and omitted-variable bias in Section V, so we defer a detailed analysis until then.

C. Governance and the Value of the Firm

It is well established that state and national laws of corporate governance affect firm value. La Porta et al. [2001] show that firm value is positively associated with the rights of minority shareholders. Daines [2001] finds that firms incorporated in Delaware have higher valuations than other U.S. firms. In this section, we study whether variation in firm-specific governance is associated with differences in firm value. More importantly, we analyze whether there was a change in the governance/value relationship during the 1990s. Since there is evidence of differential stock returns as a function of *G*, we would expect to find relative "mispricing" between 1990 and 1999 as a function of *G*.

Our valuation measure is Tobin's *Q*, which has been used for this purpose in corporate-governance studies since the work of Demsetz and Lehn [1985] and Morck, Shleifer, and Vishny [1988]. We follow Kaplan and Zingales' [1997] method for the computation of *Q* (details are listed in Appendix B) and also compute the median *Q* in each year in each of the 48 industries classified by Fama and French [1997]. We then regress

$$(2) \qquad Q'_{it} = a_t + b_t X_{it} + c_t W_{it} + e_{it},$$

where Q'_{it} is industry-adjusted *Q* (firm *Q* minus industry-median *Q*), X_{it} is a vector of governance variables (*G*, its components, or inclusion in one of the extreme portfolios) and W_{it} is a vector of firm characteristics. As elements of *W*, we follow Shin and Stulz [2000] and include

the log of the book value of assets and the log of firm age as of December of year *t*.[18] Daines [2001] found that *Q* is different for Delaware and non-Delaware firms, so we also include a Delaware dummy in *W*. Morck and Yang [2001] show that S&P 500 inclusion has a positive impact on *Q*, and that this impact increased during the 1990s; thus, we also include a dummy variable for S&P 500 inclusion in *W*.

Using a variant of the methods of Fama and MacBeth [1973], we estimate annual cross-sections of (2) with statistical significance assessed within each year (by cross-sectional standard errors) and across all years (with the time-series standard error of the mean coefficient). This method of assessing statistical significance deserves some explanation. In particular, one logical alternative would be a pooled setup with firm fixed effects and time-varying coefficients. We rejected this alternative mainly because there are few changes over time in the Governance Index, and the inclusion of fixed effects would force identification of the *G* coefficient from only these changes. In effect, our chosen method imposes a structure on the fixed effects: they must be a linear function of *G* or its components.

Table VIII summarizes the results. The first column gives the results with *G* as the key regressor. Each row gives the coefficients and standard errors for a different year of the sample; the last row gives the average coefficient and time-series standard error of these coefficients. The coefficients on *G* are negative in every year and significantly negative in nine of the ten years. The largest absolute value point estimate occurs in 1999, and the second largest is in 1998. The point estimate in 1999 is economically large; a one-point increase in *G*, equivalent to adding a single governance provision, is associated with an 11.4 percentage point lower value for

[18] Unlike Shin and Stulz [2000], we do not trim the sample of observations that have extreme independent variables. Results with a trimmed sample are nearly identical and are available from the authors.

Q. If we assume that the point estimates in 1990 and 1999 are independent, then the difference between these two estimates (11.4 – 2.2 = 9.2) is statistically significant.

In the second column of Table VIII, we restrict the sample to include only firms in the Democracy and Dictatorship Portfolios. We then estimate (2) using a dummy variable for the Democracy Portfolio. The results are consistent with the previous regressions on *G*. The point estimate for 1999 is the largest in the decade, implying that firms in the Democracy Portfolio have a *Q* that is 56 percentage points higher, other things being equal, than do firms in the Dictatorship Portfolio. This compares to an estimated difference of 19 percentage points in 1990. While the difference in coefficients between 1990 and 1999 is not statistically significant, it is similar to the total EW difference in abnormal returns estimated in Table VII.[19] There is no real pattern for the rest of the decade, however, and large standard errors toward the end of the sample period prevent any strong inference across years.

The final columns of Table VIII give results using the five governance subindices: *Delay*, *Voting*, *Protection*, *Other*, and *State*. The table shows that all subindices except *Voting* have average coefficients that are negative and significant (assuming independence across years). Over the full sample period, *Delay* and *Protection* have the most consistent impact, while the largest absolute coefficients are for *Voting* at the end of the sample period. The subindices are highly collinear, however, and the resulting large standard errors and covariances make it difficult to draw strong conclusions. For example, even in 1999 we cannot reject the null hypothesis that the coefficient on *Voting* is equal to the coefficient on *Delay*.

[19] Table VII, first row, second column, shows an alpha of 45 bp per month for the EW difference between the Democracy and Dictatorship portfolios. Over 112 months this produces a difference of approximately 50 percent, as compared to the 56 – 19 = 37 percent difference estimated for the *Q* regressions. We use the EW alpha as a comparison because the *Q* regressions are also equal-weighted.

Overall, the results for returns and prices tell a consistent story. Firms with the weakest shareholder rights (high values of *G*) significantly underperformed firms with the strongest shareholder rights (low values of *G*) during the 1990s. Over the course of the 1990s, these differences have been at least partially reflected in prices. While high-*G* firms already sold at a significant discount in 1990, this discount became much larger by 1999.

D. Governance and Operating Performance

Table IX shows the results of annual regressions for three operational measures on *G* (or a Democracy dummy). The three operational measures are the net profit margin (income divided by sales), the return on equity (income divided by book equity), and one-year sales growth. All of these measures are industry-adjusted by subtracting the median for this measure in the corresponding Fama-French [1997] industry. This adjustment uses all available Compustat firms. To reduce the influence of large outliers – a common occurrence for all of these measures -- we estimate median (least-absolute-deviation) regressions in each case. While our sample does not include a natural experiment to identify *G* as the cause of operational differences, we attempt to control for "expected" cross-sectional differences by using the log book-to-market ratio (*BM*) as an additional explanatory variable.

The odd-numbered columns give the results when *G* is the key regressor. We find that the average coefficient on *G* is negative and significant for both the net-profit-margin and sales-growth regressions, and is negative but not significant for the return-on-equity regressions. The even-numbered columns give the results for the subsample of firms from the extreme deciles, with a dummy variable for the Democracy Portfolio as the key regressor. For all three operating measures, the average coefficient on this dummy variable was positive but insignificant. Thus,

these results are consistent with the evidence for the full sample but not significant on their own. In untabulated results, we also regressed these same measures on the five subindices. The results show no clear pattern of differential influence for any particular subindex, with most coefficients having the same sign as *G*. Overall, we find some significant evidence that more democratic firms have better operating performance and no evidence that they do not.

IV. Governance: Three Hypotheses

Section III established an empirical relationship of *G* with returns, firm value, and operating performance. Since firms did not adopt governance provisions randomly, this evidence does not itself imply a causal role by governance provisions. Indeed, there are several plausible explanations for our results:

Hypothesis I) Governance provisions cause higher agency costs. These higher costs were underestimated by investors in 1990.

Hypothesis II) Governance provisions do not cause higher agency costs, but rather were put in place by 1980s managers who forecasted poor performance for their firms in the 1990s.

Hypothesis III) Governance provisions do not cause higher agency costs, but their presence is correlated with other characteristics that earned abnormal returns in the 1990s.

Most explanations of the Section III results can be fit within these three hypotheses. Under Hypothesis I, a reduction in shareholder rights causes an unexpectedly large increase in agency costs through some combination of inefficient investment, reduced operational efficiency, or self-dealing. If shareholders find it difficult or costly to replace managers, then managers may be more willing and able to extract private benefits. This is the standard justification for takeover threats as the strongest form of managerial discipline [Jensen 1986]. For Hypothesis I to be correct, these additional agency costs must have been underestimated in 1990.

Under Hypothesis II, governance does not affect performance, but there must be a perception that governance provisions are protective for management. In this case, the stock in these companies would have been relatively overvalued in 1990, even though objective measures (e.g., *Q* regressions) would suggest that it was undervalued relative to observable characteristics. When the poor operating performance occurs, the market is surprised but the managers are not. The protective provisions then supply a shield, real or imagined, for managerial jobs and compensation.

Under Hypothesis III, all of the results in the previous section would be driven by omitted-variable bias. Since governance provisions were certainly not adopted randomly, it is plausible that differences in industry, S&P 500 inclusion, institutional ownership, or other firm characteristics could be correlated both with *G* and with abnormal returns. Under this hypothesis, governance provisions could be completely innocuous, with no influence either on managerial power or on agency costs.

Ideally, we would distinguish among these three hypotheses by using random variation in some characteristic that was causal for *G*. Unfortunately, we have not been able to identify such

an instrument. One candidate would be the subset of state laws, with the *State* subindex as a proxy. Though in some states these laws were passed at the urging of large corporations, it seems reasonable to assume that their passage was exogenous to most firms. But the *State* subindex has three flaws as an instrument. First, firms can choose to reincorporate into different states; enough firms have done so that exposure to state laws is not truly exogenous [Subramanian 2001]. Second, many firms have opted out of the protections of some of the most stringent of these laws, so that a firm's state of incorporation is only a noisy measure for its actual legal exposure. Third, as shown in Table III, the *State* subindex is not positively or consistently correlated with the other components of *G*. Other potential instruments have different problems. For example, if takeover protections were adopted during industry-specific takeover waves, then we might be able to use industry as an instrument for *G*. Unfortunately, this would render it impossible to distinguish between *G* or industry as the cause of poor returns in the 1990s.

In Section V, our tests consist of a search for evidence supportive of each hypothesis, while acknowledging the impossibility of a perfect test to distinguish among them. First, if Hypothesis I is correct, then we should observe some "unexpected" differences in agency costs across firms. We discuss several previous studies on this topic and look for such differences in our sample by analyzing capital expenditure and acquisition behavior. Second, for Hypothesis II, we analyze insider-trading activity as a function of *G*. If governance provisions were put in place by prescient managers, these same managers might be net sellers of the stock in their firms. Finally, for Hypothesis III, we test whether a large set of observable firm characteristics can explain the empirical relationship between returns and *G*.

V. Governance: Tests

In this section we examine the evidence for each of the hypotheses described in Section IV. Section V.A covers Hypothesis I, Section V.B covers Hypothesis II, and Section V.C covers Hypothesis III. Section V.D summarizes and discusses the evidence.

A. Evidence on Hypothesis I

Increased agency costs at high-*G* firms can directly affect firm performance in several ways. In the specific case of state takeover laws, where causality is easier to establish, researchers have found evidence of increased agency costs through a variety of mechanisms. Borokhovich, Brunarski and Parrino [1997] show that compensation rises for CEOs of firms adopting takeover defenses. Bertrand and Mullainathan [1999a, 1999b, and 2000] find a similar result for CEOs and other employees in firms newly covered by state takeover laws. They also find that these laws cause a decrease in plant-level efficiency, measured either by total factor productivity or return on capital. Garvey and Hanka [1999] show that state takeover laws led to changes in leverage consistent with increased corporate slack. These studies provide the cleanest evidence in support of Hypothesis I, but, of course, do not make use of the full variation embodied in the *G* index. We supplement these findings by examining the empirical relationship of *G* with two other possible sources of agency costs: capital expenditure and acquisition behavior.

A substantial literature, dating back at least to Baumol [1959], Marris [1964], and Williamson [1964], holds that managers may undertake inefficient projects in order to extract private benefits. This problem is particularly severe when managers are entrenched and can resist hostile takeovers [Jensen and Ruback 1983, Shleifer and Vishny 1989]. Under this view, if

capital expenditure increases following the adoption of new takeover defenses, this increase would be a net negative for firm value.[20]

To examine the empirical relationship between capital expenditure and governance, we estimate annual median regressions for capital expenditure (CAPEX), scaled by either sales or assets, and net of the industry median. To control for the different investment opportunities available at value and growth firms, we include the log book-to-market ratio (BM) as a control variable in all specifications. Table X summarizes the results, with BM coefficients omitted. Columns (1) and (3) give results for the full sample, with *G* as the key regressor; columns (2) and (4) give results for the sample restricted to firms in the Democracy and Dictatorship Portfolios, with a Democracy dummy as the key regressor. The average coefficient on *G* is positive and significant in both sets of regressions. Consistent with these results, we find that the average coefficient on the Democracy dummy is negative and significant in both sets of regressions. We conclude that, other things equal, high-*G* firms have higher CAPEX than do low-*G* firms.

Another outlet for capital expenditure is for firms to acquire other firms. Some of the strongest evidence for the importance of agency costs comes from the negative returns to acquirer stocks after a bid is announced. Considerable evidence shows that these negative returns are correlated with other agency problems, including low managerial ownership [Lewellen, Loderer, and Rosenfeld 1985], high free-cash flow [Lang, Stulz, and Walkling 1991], and diversifying transactions [Morck, Shleifer, and Vishny 1990]. In addition to negative announcement returns, there is also long-run evidence of negative abnormal performance by

[20] For an alternative view, see Stein [1988 and 1989]. Empirical evidence on this issue is given by Daines and Klausner [2001], Johnson and Rao [1997], Meulbroek et al. [1990], Pugh, Page, and Jahera [1992], and Titman, Wei, and Xie [2001].

acquirer firms [Loughran and Vijh 1997, Rao and Vermaelen 1998].[21] Taken together, these studies suggest acquisitions as another pathway through which governance affects performance.

To analyze the relation between acquisition activity and *G*, we use the SDC database to identify all transactions in which a sample firm acted as either the acquirer or the seller during the sample period. From January 1991 through December 1999, there are 12,694 acquisitions made by sample firms; SDC gives the acquisition price for just under half of these. For each firm, we count the number of acquisitions ("Acquisition Count"). We also calculate the sum of the price of all acquisitions in each calendar year and divide this sum by the firm's average market capitalization for the first day and last day of the year ("Acquisition Ratio").

Table XI summarizes the results of annual regressions for both Acquisition Count and the Acquisition Ratio in year *t* on *G* (or a Democracy dummy), the log of size, the log of the book-to-market ratio, and 48 industry dummies, all measured at year-end *t-1*. Coefficients on all control variables are omitted from the table. Since many firms make no acquisitions in a year, the dependent variables are effectively left-censored at zero. To account for this censoring, we estimate Poisson regressions for Acquisition Count and Tobit regressions for the Acquisition Ratio. Columns (1) and (3) give results for the full sample, with *G* as the key regressor; columns (2) and (4) give results for the sample restricted to firms in the Democracy and Dictatorship portfolios, with a Democracy dummy as the key regressor. For both sets of regressions, the coefficients on *G* are positive in every year, and the average coefficient on *G* is positive and significant. Consistent with this result, the average coefficient on the Democracy dummy is negative for both sets of regressions and is significant for Acquisition Count.

[21] Mitchell and Stafford [2000] have challenged the magnitude of this long-run evidence, but still allow for some underperformance for acquisitions financed by stock. A related debate on whether diversifying acquisitions destroy value has grown too large to survey here. The seminal works are Lang and Stulz [1994] and Berger and Ofek [1995]. Recent work is summarized in Holmstrom and Kaplan [2001] and Stein [2001].

One interpretation of these results is that high-*G* firms engaged in an unexpectedly large amount of inefficient investment during the 1990s. This interpretation is consistent with contemporaneous unexpected differences in profitability, stock returns, and firm value. This inefficient investment does not necessarily mean that firms are attempting to maximize their size in a form of empire building. Indeed, empire building would be inconsistent with the negative relationship between sales growth and *G* found in Table IX. Instead, managers may be attempting to stave off "empire collapse" with high expenditure and acquisition activity. In that case, the results of this section are consistent with the evidence of Table IX.

B. Evidence on Hypothesis II

It is well established that insider trading can forecast returns. Firms whose shares have been intensively sold (bought) by insiders tend to underperform (overperform) benchmarks in subsequent periods.[22] If some 1980s insiders forecasted poor performance for their firms, we might expect them to have looked for ways to keep the shareholders from firing them, either through voting or takeovers. In this case, weak shareholder rights would be a symptom of insiders' superior information, but would not necessarily be the cause of the poor performance in the subsequent decade.

To study this possibility, we use data collected by Thomson Financial from the required SEC insider-trading filings. For each firm in our sample, we sum all (split-adjusted) open-market transactions for all insiders in each year, with purchases entering positively and sales entering negatively. We then normalize this sum by shares outstanding at the beginning of the year to arrive at a "Net Purchases" measure for each firm in each year. If insiders put new

[22] See Seyhun [1998] for a comprehensive review of this literature and a discussion of SEC rules, filing requirements, and available data.

provisions in place when they forecast poor performance, then we would expect Net Purchases to be negatively correlated with *G*.

We employ two regression specifications. First, we estimate OLS regressions of Net Purchases on *G* (or a Democracy dummy), BM, and log of size. For some firm-years, the Net Purchase measure is dominated by one large transaction. While large transactions might have information content, they might also reflect liquidity or rebalancing needs. In an OLS regression, firms with large outliers will dominate. Thus, we also estimate ordered logit regressions on the same OLS regressors, in which the dependent variable is equal to one if Net Purchases is positive, zero if Net Purchases is zero, and negative one if Net Purchases is negative.

Table XII summarizes the results of these regressions. Columns (1) and (3) give results for the full sample, with *G* as the key regressor; columns (2) and (4) give results for the sample restricted to firms in the Democracy and Dictatorship Portfolios with a Democracy dummy as the key regressor. Coefficients on all control variables are omitted from the table. We find no significant relationships between governance and insider trading. Two of four sets of regressions have positive average coefficients, two have negative average coefficients, and none of these average coefficients are significant. In untabulated results, we also estimated median regressions, replicated all of the above results using all transactions (the main difference is the inclusion of option-exercise transactions), and estimated long-horizon regressions using all years of data for each firm. In none of these cases did we find a robust relationship between governance and insider trading. Overall, we find no support for Hypothesis II in the insider-trading data.

C. Evidence on Hypothesis III

What other factors might be driving the return difference between the Democracy and Dictatorship portfolios? We saw in Table II that *G* is correlated with several firm characteristics, including S&P 500 membership, institutional ownership, trading volume, and past sales growth. If returns to stocks with these characteristics differed in the 1990s in a way not captured by the model in equation (1), then a type of omitted variable bias may drive the abnormal-return results. In this section, we explore this possibility using a cross-sectional regression approach. In addition to providing evidence on Hypothesis III, this method also supplements the analysis of Section III.B by allowing a separate regressor for each component of *G*.

For each month in the sample period, September 1990 to December 1999, we estimate

$$r_{it} = a_t + b_t X_{it} + c_t Z_{it} + e_{it}, \quad (3)$$

where, for firm *i* in month *t*, r_{it} are the returns (either raw or industry-adjusted), X_{it} is a vector of governance variables (either *G*, its components, or inclusion in one of the extreme portfolios), and Z_{it} is a vector of firm characteristics. As elements of *Z*, we include the full set of regressors used by Brennan, Chordia, and Subrahmanyam [1998], plus five-year sales growth, S&P 500 inclusion, and institutional ownership.[23] Variable definitions are given in Appendix B.

We estimate (3) separately for each month and then calculate the mean and time-series standard deviation of the 112 monthly estimates of the coefficients. Table XIII summarizes the results. The first two columns give the results, raw and industry-adjusted, for the full sample of firms in each month with *G* as the key independent variable. In both regressions, the average

[23] All of these additional variables are correlated with *G* (see Table III) and, in prior studies, with either firm value or abnormal returns. See Lakonishok, Shleifer, and Vishny [1994] (sales growth), Gompers and Metrick [2001] (institutional ownership), and Morck and Yang [2001] (*Q*).

coefficient on *G* is negative but not significant. The point estimates are not small. For example, the point estimate for the coefficient on *G* in column 3 implies a lower return of approximately four bp per month (= 48 bp per year) for each additional point of *G*, but it would require estimates nearly twice as large before statistical significance would be reached.

The next two columns give the results when the sample is restricted to stocks in either the Democracy ($G \leq 5$) or Dictatorship ($G \geq 14$) portfolios. In the first column, the dependent variable is the raw monthly return for each stock. In the second column, the dependent variable is the industry-adjusted return for each stock, where industry adjustments are relative to the Fama and French [1997] 48 industries. The key independent variable in these regressions is the Democracy dummy, set equal to one if the stock is in the Democracy Portfolio and zero if the stock is in the Dictatorship Portfolio. For both the raw and industry-adjusted returns, the coefficient on this dummy variable is positive and significant at the one-percent level. The average point estimate can be interpreted as a monthly abnormal return. These point estimates, 76 bp per month raw and 63 bp per month industry-adjusted, are similar to those found in the factor models, and provide a further robustness check to the benchmark result. Here, industry adjustments explain about one-sixth of the raw result. In the factor-model results of Table VII, the industry adjustment explained about one-third of the raw result.

Columns (5) and (6) of Table XIII give the results for the full sample of firms when the five subindices are used as the components of *X*. In principle, these regressions could help us distinguish between Hypotheses I and III. If governance provisions cause poor performance, then we might expect certain provisions to play a stronger role. In the absence of such a finding, we should wonder if the results are driven by some other characteristic. For example, some legal scholars argue that the *Delay* provisions are the only defenses with deterrent value [Coates 2000,

Daines and Klausner 2001]. If managers also believe this, then the *Delay* subindex should also be the most important driver of the results.

Unfortunately, large standard errors, due in part to the substantial multicollinearity between the regressors, makes it difficult to construct a powerful test. None of the subindex coefficients are statistically significant in either specification, but many of the point estimates are economically large. In the end, we cannot precisely measure the relative importance of *Delay* or any other subindex. This is similar to the problem that occurred in the *Q* regressions of Table VIII. For example, in both Tables VIII and XIII, the coefficients on *Voting* suggest potentially enormous economic significance, but large standard errors prevent any meaningful statistical inference.

In untabulated tests, we also included all 28 provisions from Table I as separate regressors in (3). Regressing raw returns on these 28 provisions plus the same controls as in Table XIII, we find that 16 of the coefficients are negative, and only one (Unequal Voting) is significant. (With this many regressors, we would expect one to appear "significant" just by chance.) Results for industry-adjusted returns are similar. These results highlight and magnify the lack of power in the subindex regressions. Indeed, many of the point estimates imply return effects above 20 basis points per month (2.4 percent per year), but are still far from being statistically significant. This result also suggests that the Democracy-minus-Dictatorship return differences are not driven by the presence or absence of any one provision.

D. Discussion

The evidence in sections V.A, V.B, and V.C must be interpreted with caution. Since this is an experiment without random assignment, no analysis of causality can be conclusive. The

main problem is the possibility that some unobserved characteristic is correlated with *G* and is also the main cause of abnormal returns. This type of omitted-variable bias could be something prosaic, such as imperfect industry adjustments or model misspecification, or something more difficult to quantify, such as a partially unobservable or immeasurable "corporate culture". Under the latter explanation, management behavior would be constrained by cultural norms within the firm, and democracy and dictatorship would be a persistent feature of a corporate culture; *G* would be a symptom, but not a cause, of this culture. In this case, all the results of the paper could be explained if investors mispriced culture in 1990, just as they appear to have mispriced its proxy, *G*. The policy impact of reducing *G* would be nonexistent unless it affected the culture of managerial power that was the true driver of poor performance.

In addition to the three hypotheses considered above, other explanations fall into the general class of "Type I" error. For example, one could argue that investors in 1990 had rational expectations about the expected costs and benefits of takeover defenses, where the expected costs are more severe agency problems and the expected benefits are higher takeover premia. Then, when the hostile takeover market largely evaporated in the early 1990s – perhaps because of macroeconomic conditions unrelated to takeover defenses – Dictatorship firms were left with the costs but none of the benefits of their defenses. Over the subsequent decade, the expected takeover premia eroded as investors gradually learned about the weak takeover market. Simple calculations suggest that this explanation cannot be that important. Suppose that in 1990 the expected takeover probability for Dictatorship firms was 30 percent, and the expected takeover premium conditional on takeover was also 30 percent. Further suppose that both of these numbers were zero for Democracy firms. Then, the unconditional expected takeover premium

for Dictatorship firms would have been only nine percent, which is approximately the relative underperformance of these firms for only a single year.

In sum, we find some evidence in support of Hypothesis I and no evidence in support of Hypothesis II. For Hypothesis III, we find that industry classification can explain somewhere between one-sixth and one-third of the benchmark abnormal returns, but we do not find any other observable characteristic that explains the remaining abnormal return. The subindex regressions, which might be helpful in distinguishing between Hypotheses I and III, are not powerful enough for strong inference. We conclude that the remaining performance differences, which are economically large, were either directly caused by governance provisions (Hypothesis I), or were related to unobservable or difficult-to-measure characteristics correlated with governance provisions (Hypothesis III).

What do these hypotheses imply about abnormal returns in the future? None suggests any obvious pattern for the relationship between *G* and returns. Under Hypothesis I, if we interpret our test as a long-run event study, then there is no reason to expect any relationship once the market has fully priced the underlying "event" of corporate governance. The fact that this price adjustment is taking such a long time does not seem so surprising in light of the lengthy intervals necessary for much more tangible information to be incorporated into prices.[24] Thus, to the extent that end-of-sample price adjustment is incomplete, complete, or has overreacted, the future relationship between *G* and returns could be negative, zero, or positive. Under Hypothesis II, there is a similar dependence on whether past insider information has been fully incorporated into prices. Under Hypothesis III, future return differences would be driven the relevant omitted characteristic; clearly, this hypothesis yields no clear prediction.

[24] For example, there is evidence that earnings surprises [Bernard and Thomas 1989], dividend omissions [Michaely, Thaler, and Womack 1995], and stock repurchases [Ikenberry, Lakonishok, and Vermaelen 1995] have long-term drift following the event, and all seem to be relatively simple events compared to changes in governance structure.

VI. Conclusion

The power-sharing relationship between investors and managers is defined by the rules of corporate governance. Beginning in the late 1980s, there is significant and stable variation in these rules across different firms. Using 24 distinct corporate-governance provisions for a sample of about 1,500 firms per year during the 1990s, we build a Governance Index, denoted as *G*, as a proxy for the balance of power between managers and shareholders in each firm. We then analyze the empirical relationship of this index with corporate performance.

We find that corporate governance is strongly correlated with stock returns during the 1990s. An investment strategy that purchased shares in the lowest-*G* firms ("Democracy" firms with strong shareholder rights), and sold shares in the the highest-*G* firms ("Dictatorship" firms with weak shareholder rights), earned abnormal returns of 8.5 percent per year. At the beginning of the sample, there is already a significant relationship between valuation and governance: each one-point increase in *G* is associated with a decrease in Tobin's *Q* of 2.2 percentage points. By the end of the decade, this difference has increased significantly, with a one-point increase in *G* associated with a decrease in Tobin's *Q* of 11.4 percentage points. The results for both stock returns and firm value are economically large and are robust to many controls and other firm characteristics.

We consider several explanations for the results, but the data do not allow strong conclusions about causality. There is some evidence, both in our sample and from other authors, that weak shareholder rights caused poor performance in the 1990s. It is also possible that the results are driven by some unobservable firm characteristic. These multiple causal explanations have starkly different policy implications and stand as a challenge for future research. The

empirical evidence of this paper establishes the high stakes of this challenge. If an 11.4 percentage point difference in firm value were even partially "caused" by each additional governance provision, then the long-run benefits of eliminating multiple provisions would be enormous.

Appendix A – Corporate-Governance Provisions

This appendix describes the provisions listed in Table I and used as components of the Governance Index. The shorthand title of each provision, as used in the text of the paper, is given in bold. These descriptions are given in alphabetical order and are similar to Rosenbaum [1998]. For a few provisions, we discuss their impact on shareholder rights or the logic behind their categorization in Table I.

Antigreenmail – Greenmail refers to a transaction between a large shareholder and a company in which the shareholder agrees to sell his stock back to the company, usually at a premium, in exchange for the promise not to seek control of the company for a specified period of time. Antigreenmail provisions prevent such arrangements unless the same repurchase offer is made to all shareholders or approved by a shareholder vote. Such provisions are thought to discourage accumulation of large blocks of stock because one source of exit for the stake is closed, but the net effect on shareholder wealth is unclear [Shleifer and Vishny 1986, Eckbo 1990]. Five states have specific **Antigreenmail laws**, and two other states have "recapture of profits" laws, which enable firms to recapture raiders' profits earned in the secondary market. We consider recapture of profits laws to be a version of Antigreenmail laws (albeit a stronger one). The presence of firm-level Antigreenmail provisions is positively correlated with 18 out of the other 21 firm-level provisions, is significantly positive in eight of these cases, and is not significantly negative for any of them. Furthermore, states with Antigreenmail laws tend to pass them in conjunction with laws more clearly designed to prevent takeovers [Pinnell 2000]. Since it seems likely that most firms and states perceive Antigreenmail as a takeover "defense", we treat Antigreenmail like the other defenses and code it as a decrease in shareholder rights.

Blank Check preferred stock is stock over which the board of directors has broad authority to determine voting, dividend, conversion, and other rights. While it can be used to enable a company to meet changing financial needs, its most important use is to implement poison pills or to prevent takeover by placing this stock with friendly investors. Because of this role, blank check preferred stock is a crucial part of a "delay" strategy. Companies that have this type of preferred stock but require shareholder approval before it can be used as a takeover defense are *not* coded as having this provision in our data.

Business Combination laws impose a moratorium on certain kinds of transactions (e.g., asset sales, mergers) between a large shareholder and the firm, unless the transaction is approved by the Board of Directors. Depending on the State, this moratorium ranges between two and five years after the shareholder's stake passes a prespecified (minority) threshold. These laws were in place in 25 states in 1990 and two more by 1998. It is the only state takeover law in Delaware, the state of incorporation for about half of our sample.

Bylaw and **Charter** amendment limitations limit shareholders' ability to amend the governing documents of the corporation. This might take the form of a supermajority vote requirement for charter or bylaw amendments, total elimination of the ability of shareholders to amend the bylaws, or the ability of directors (beyond the provisions of state law) to amend the bylaws without shareholder approval.

Control-share **Cash-out laws** enable shareholders to sell their stakes to a "controlling" shareholder at a price based on the highest price of recently acquired shares. This works something like fair-price provisions (see below) extended to nontakeover situations. These laws were in place in three states by 1990 with no additions during the decade.

A **Classified Board** (or "staggered" board) is one in which the directors are placed into different classes and serve overlapping terms. Since only part of the board can be replaced each year, an outsider who gains control of a corporation may have to wait a few years before being able to gain control of the board. This slow replacement makes a classified board a crucial component of the *Delay* group of provisions, and one of the few provisions that clearly retains some deterrent value in modern takeover battles [Daines and Klausner 2001].

Compensation Plans with changes-in-control provisions allow participants in incentive bonus plans to cash out options or accelerate the payout of bonuses should there be a change in control. The details may be a written part of the compensation agreement, or discretion may be given to the compensation committee.

Director indemnification **Contracts** are contracts between the company and particular officers and directors indemnifying them from certain legal expenses and judgments resulting from lawsuits pertaining to their conduct. Some firms have both "Indemnification" in their bylaws or charter and these additional indemnification "Contracts".

Control-share Acquisition laws (see Supermajority, below).

Cumulative Voting allows a shareholder to allocate his total votes in any manner desired, where the total number of votes is the product of the number of shares owned and the number of directors to be elected. By allowing them to concentrate their votes, this practice helps minority shareholders to elect directors. Cumulative Voting and Secret Ballot (see below) are the only two provisions whose presence is coded as an *increase* in shareholder rights, with an additional point to the Governance Index if the provision is absent.

Directors' Duties provisions allow directors to consider constituencies other than shareholders when considering a merger. These constituencies may include, for example,

employees, host communities, or suppliers. This provision provides boards of directors with a legal basis for rejecting a takeover that would have been beneficial to shareholders. 31 states have **Directors' Duties laws** allowing similar expansions of constituencies, but in only two of these states (Indiana and Pennsylvania) are the laws explicit that the claims of shareholders should not be held above those of other stakeholders [Pinnell 2000]. We treat firms in these two states as though they had an expanded directors' duty provision unless the firm has explicitly opted out of coverage under the law.

Fair-Price provisions limit the range of prices a bidder can pay in two-tier offers. They typically require a bidder to pay to all shareholders the highest price paid to any during a specified period of time before the commencement of a tender offer, and do not apply if the deal is approved by the board of directors or a supermajority of the target's shareholders. The goal of this provision is to prevent pressure on the target's shareholders to tender their shares in the front end of a two-tiered tender offer, and they have the result of making such an acquisition more expensive. Also, 25 states had **Fair-Price laws** in place in 1990, and two more states passed such laws in 1991. The laws work similarly to the firm-level provisions.

Golden Parachutes are severance agreements that provide cash and non-cash compensation to senior executives upon an event such as termination, demotion, or resignation following a change in control. They do not require shareholder approval. While such payments would appear to deter takeovers by increasing their costs, one could argue that these parachutes also ease the passage of mergers through contractual compensation to the managers of the target company [Lambert and Larcker 1985]. While the net impact on managerial entrenchment and shareholder wealth is ambiguous, the more important effect is the clear decrease in shareholder rights. In this case, the "right" is the ability of a controlling shareholder to fire management

without incurring an additional cost. Golden Parachutes are highly correlated with all the other takeover defenses. Out of 21 pairwise correlations with the other firm-level provisions, 15 are positive, 10 of these positive correlations are significant, and only one of the negative correlations is significant. Thus, we treat Golden Parachutes as a restriction of shareholder rights.

Director **Indemnification** uses the bylaws, charter, or both to indemnify officers and directors from certain legal expenses and judgments resulting from lawsuits pertaining to their conduct. Some firms have both this "Indemnification" in their bylaws or charter and additional indemnification "Contracts". The cost of such protection can be used as a market measure of the quality of corporate governance [Core 1997 and 2000].

Limitations on director **Liability** are charter amendments that limit directors' personal liability to the extent allowed by state law. They often eliminate personal liability for breaches of the duty of care, but not for breaches of the duty of loyalty or for acts of intentional misconduct or knowing violation of the law.

Pension Parachutes prevent an acquirer from using surplus cash in the pension fund of the target to finance an acquisition. Surplus funds are required to remain the property of the pension fund and to be used for plan participants' benefits.

Poison Pills provide their holders with special rights in the case of a triggering event such as a hostile takeover bid. If a deal is approved by the board of directors, the poison pill can be revoked, but if the deal is not approved and the bidder proceeds, the pill is triggered. Typical poison pills give the holders of the target's stock other than the bidder the right to purchase stock in the target or the bidder's company at a steep discount, making the target unattractive or diluting the acquirer's voting power. Poison pills are a crucial component of the "delay" strategy

at the core of modern defensive tactics. Nevertheless, we do not include poison pills in the *Delay* group of provisions, but include it in the *Other* group because the pill itself can be passed on less than one-day's notice, so it need not be in place for the other *Delay* provisions to be effective. The other provisions in this group require a shareholder vote, so they cannot be passed on short notice. See Coates [2000] and Daines and Klausner [2001] for a discussion of this point.

Under a **Secret Ballot** (also called confidential voting), either an independent third party or employees sworn to secrecy are used to count proxy votes, and the management usually agrees not to look at individual proxy cards. This can help eliminate potential conflicts of interest for fiduciaries voting shares on behalf of others, and can reduce pressure by management on shareholder-employees or shareholder-partners. Cumulative Voting (see above) and Secret Ballots are the only two provisions whose presence is coded as an *increase* in shareholder rights, with an additional point to the Governance Index if the provision is absent.

Executive **Severance** agreements assure high-level executives of their positions or some compensation and are not contingent upon a change in control (unlike Golden or Silver parachutes).

Silver Parachutes are similar to Golden Parachutes in that they provide severance payments upon a change in corporate control, but differ in that a large number of a firm's employees are eligible for these benefits. Since Silver Parachutes do not protect the key decision makers in a merger, we classified them in the *Other* group rather than in the *Protection* group.

Special Meeting limitations either increase the level of shareholder support required to call a special meeting beyond that specified by state law or eliminate the ability to call one entirely. Such provisions add extra time to proxy fights, since bidders must wait until the regularly scheduled annual meeting to replace board members or dismantle takeover defenses.

This delay is especially potent when combined with limitations on actions by written consent (see below).

Supermajority requirements for approval of mergers are charter provisions that establish voting requirements for mergers or other business combinations that are higher than the threshold requirements of state law. They are typically 66.7, 75, or 85 percent, and often exceed attendance at the annual meeting. In practice, these provisions are similar to **Control-Share Acquisition laws**. These laws require a majority of disinterested shareholders to vote on whether a newly qualifying large shareholder has voting rights. They were in place in 25 states by September 1990 and one additional state in 1991.

Unequal Voting rights limit the voting rights of some shareholders and expand those of others. Under time-phased voting, shareholders who have held the stock for a given period of time are given more votes per share than recent purchasers. Another variety is the substantial-shareholder provision, which limits the voting power of shareholders who have exceeded a certain threshold of ownership.

Limitations on action by **Written Consent** can take the form of the establishment of majority thresholds beyond the level of state law, the requirement of unanimous consent, or the elimination of the right to take action by written consent. Such requirements add extra time to many proxy fights, since bidders must wait until the regularly scheduled annual meeting to replace board members or dismantle takeover defenses. This delay is especially potent when combined with limitations for calling special meetings (see above).

Appendix B – Definitions for the Regression Variables

This list includes all variables used as regressors or for summary statistics in Tables V and XIII. All components are drawn from the CRSP monthly files and all variables are in natural logs unless explicitly noted otherwise. Variables are listed in alphabetical order.

BM - The ratio of book value of common equity (previous fiscal year) to market value of common equity (end of previous calendar year). Book value of common equity is the sum of book common equity (Compustat item 60) and deferred taxes (Compustat item 74). This variable, and all other variables that use Compustat data, are recalculated each July and held constant through the following June.

5-Year Return – The compounded return from month t-61 to month t-2.

IO – Shares held by institutions divided by total shares outstanding (not in logs). Institutional holdings are from SEC Form 13F quarterly filings, as provided by Thomson Financial. We use the most recent quarter as of the end of month t-1, with shares outstanding (from CRSP) measured on the same date.

NADVOL - The dollar volume of trading in month t-2 for stocks that trade on the Nasdaq. Approximated as stock price at the end of month t-2 multiplied by share volume in month t-2. For New York Stock Exchange (NYSE) and American Stock Exchange (AMEX) stocks, NADVOL equals zero.

NASDUM - A dummy variable equal to one if the firm traded on the Nasdaq Stock Market at the beginning of month t and zero otherwise.

NYDVOL - The dollar volume of trading in month t-2 for stocks that trade on the NYSE or AMEX. Approximated as stock price at the end of month t-2 multiplied by share volume in month t-2. For Nasdaq stocks, NYDVOL equals zero.

PRICE - Price at the end of month t-2.

Q - The market value of assets divided by the book value of assets (Compustat item 6), where the market value of assets is computed as book value of assets plus the market value of common stock less the sum of the book value of common stock (Compustat item 60) and balance sheet deferred taxes (Compustat item 74). All book values for fiscal year t (from Compustat) are combined with the market value of common equity at the calendar end of year t.

RET2-3 - Compounded gross returns for months t-3 and t-2.

RET4-6 - Compounded gross returns for months t-6 through t-4.

RET7-12 - Compounded gross returns for months t-12 through t-7.

SGROWTH - The growth in sales (Compustat item 12) over the previous five fiscal years (not in logs).

SIZE - Market capitalization in millions of dollars at the end of month t-2.

SP500 - membership in the S&P 500 as of the end of month t-1. Value is equal to one if the firm is in the index, and zero otherwise. Data is from CRSP S&P 500 constituent file.

VOLUME - The dollar volume of trading in month t-2 = NADVOL + NYDVOL.

YLD - The ratio of dividends in the previous fiscal year (Compustat item 21) to market capitalization measured at calendar year end (not in logs).

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TABLE I
Governance Provisions

This table presents the percentage of firms with each provision between 1990 and 1998. The data are drawn from the IRRC Corporate Takeover Defenses publications [Rosenbaum 1990, 1993, 1995, and 1998] and are supplemented by data on state takeover legislation coded from Pinnell [2000]. See Appendix A for detailed information on each of these provisions. The sample consists of all firms in the IRRC research universe except those with dual class stock.

	Percentage of firms with governance provisions in			
	1990	1993	1995	1998
Delay				
Blank Check	76.4	80.0	85.7	87.9
Classified Board	59.0	60.4	61.7	59.4
Special Meeting	24.5	29.9	31.9	34.5
Written Consent	24.4	29.2	32.0	33.1
Protection				
Compensation Plans	44.7	65.8	72.5	62.4
Contracts	16.4	15.2	12.7	11.7
Golden Parachutes	53.1	55.5	55.1	56.6
Indemnification	40.9	39.6	38.7	24.4
Liability	72.3	69.1	65.6	46.8
Severance	13.4	5.5	10.3	11.7
Voting				
Bylaws	14.4	16.1	16.0	18.1
Charter	3.2	3.4	3.1	3.0
Cumulative Voting	18.5	16.5	14.9	12.2
Secret Ballot	2.9	9.5	12.2	9.4
Supermajority	38.8	39.6	38.5	34.1
Unequal Voting	2.4	2.0	1.9	1.9
Other				
Antigreenmail	6.1	6.9	6.4	5.6
Directors' Duties	6.5	7.4	7.2	6.7
Fair Price	33.5	35.2	33.6	27.8
Pension Parachutes	3.9	5.2	3.9	2.2
Poison Pill	53.9	57.4	56.6	55.3
Silver Parachutes	4.1	4.8	3.5	2.3
State				
Antigreenmail Law	17.2	17.6	17.0	14.1
Business Combination Law	84.3	88.5	88.9	89.9
Cash-Out Law	4.2	3.9	3.9	3.5
Directors' Duties Law	5.2	5.0	5.0	4.4
Fair Price Law	35.7	36.9	35.9	31.6
Control Share Acquisition Law	29.6	29.9	29.4	26.4
Number of Firms	1357	1343	1373	1708

TABLE II
The Governance Index

This table provides summary statistics on the distribution of *G*, the Governance Index, and the subindices (*Delay, Protection, Voting, Other,* and *State*) over time. *G* and the subindices are calculated from the provisions listed in Table I as described in Section II**.** Appendix A gives detailed information on each provision. We divide the sample into ten portfolios based on the level of *G* and list the number of firms in each portfolio. The Democracy Portfolio is composed of all firms where $G \leq 5$, and the Dictatorship Portfolio contains all firms where $G \geq 14$.

	1990	1993	1995	1998
Governance Index				
Minimum	2	2	2	2
Mean	9.0	9.3	9.4	8.9
Median	9	9	9	9
Mode	10	9	9	10
Maximum	17	17	17	18
Standard Deviation	2.9	2.8	2.8	2.8
Number of Firms				
$G \leq 5$ (Democracy Portfolio)	158	139	120	215
$G=6$	119	88	108	169
$G=7$	158	140	127	186
$G=8$	165	139	152	201
$G=9$	160	183	183	197
$G=10$	175	170	178	221
$G=11$	149	168	166	194
$G=12$	104	123	142	136
$G=13$	84	100	110	106
$G \geq 14$ (Dictatorship Portfolio)	85	93	87	83
Total	1357	1343	1373	1708
Subindex Means				
Delay	1.8	2.0	2.1	2.1
Protection	2.4	2.5	2.5	2.1
Voting	2.2	2.1	2.1	2.2
Other	1.1	1.2	1.1	1.0
State	1.8	1.8	1.8	1.7

TABLE III
Correlations between the Subindices

This table presents pairwise correlations between the subindices, *Delay*, *Protection*, *Voting*, *Other*, and *State* in 1990. The calculation of the subindices is described in Section II. The elements of each subindex are given in Table I and described in detail in Appendix A. Significance at the five-percent and one-percent levels is indicated by * and ** respectively.

	Delay	*Protection*	*Voting*	*Other*
Protection	0.22**			
Voting	0.33**	0.10**		
Other	0.43**	0.27**	0.19**	
State	-0.08**	-0.04	-0.07*	0.05

TABLE IV
The Largest Firms in the Extreme Portfolios

This table presents the firms with the largest market capitalizations at the end of 1990 of all companies within the Democracy Portfolio ($G \leq 5$) and the Dictatorship Portfolio ($G \geq 14$). The calculation of G is described in Section II. The companies are listed in descending order of market capitalization.

1990 Democracy Portfolio			
	State of Incorporation	1990 Governance Index	1998 Governance Index
IBM	New York	5	6
Wal-Mart	Delaware	5	5
Du Pont de Nemours	Delaware	5	5
Pepsico	North Carolina	4	3
American International Group	Delaware	5	5
Southern Company	Delaware	5	5
Hewlett Packard	California	5	6
Berkshire Hathaway	Delaware	3	–
Commonwealth Edison	Illinois	4	6
Texas Utilities	Texas	2	4

1990 Dictatorship Portfolio			
	State of Incorporation	1990 Governance Index	1998 Governance Index
GTE	New York	14	13
Waste Management	Delaware	15	13
General Re	Delaware	14	16
Limited Inc	Delaware	14	14
NCR	Maryland	14	–
K Mart	Michigan	14	10
United Telecommunications	Kansas	14	–
Time Warner	Delaware	14	13
Rorer	Pennsylvania	16	–
Woolworth	New York	14	13

TABLE V
Summary Statistics

This table gives descriptive statistics for the relationship of *G* with several financial and accounting measures in September 1990. The first column gives the correlations for each of these variables with the Governance Index, *G*. The second and third columns give means for these same variables within the Democracy Portfolio ($G \leq 5$) and the Dictatorship Portfolio ($G \geq 14$) in 1990. The final column gives the difference of the two means with its standard error in parentheses. The calculation of *G* is described in Section II, and definitions of each variable are given in Appendix B. Significance at the five-percent and one-percent levels is indicated by * and ** respectively.

	Correlation with *G*	Mean, Democracy Portfolio	Mean, Dictatorship Portfolio	Difference
BM	0.02	-0.66	-0.54	-0.12
				(0.10)
SIZE	0.15**	12.86	13.46	-0.60**
				(0.21)
PRICE	0.16**	2.74	3.14	-0.40**
				(0.12)
VOLUME	0.19**	16.34	17.29	-0.95**
				(0.24)
Q	-0.04	1.77	1.47	0.30*
				(0.14)
YLD	0.03	4.20%	7.20%	-3.00%
				(4.34)
SP500	0.23**	0.15	0.49	-0.34**
				(0.06)
5-Year Return	-0.01	90.53%	85.41%	5.12%
				(20.74)
SGROWTH	-0.08**	62.74%	44.78%	17.96%
				(9.83)
IO	0.14**	25.89%	34.44%	-8.55%*
				(3.36)

TABLE VI
Performance-Attribution Regressions for Decile Portfolios

We estimate four-factor regressions (equation (1) from the text) of value-weighted monthly returns for portfolios of firms sorted by G. The calculation of G is described in Section II. The first row contains the results when we use the portfolio that buys the Democracy Portfolio ($G \leq 5$) and sells short the Dictatorship Portfolio ($G \geq 14$). The portfolios are reset in September 1990, July 1993, July 1995, and February 1998, which are the months after new data on G became available. The explanatory variables are *RMRF*, *SMB*, *HML*, and *Momentum*. These variables are the returns to zero-investment portfolios designed to capture market, size, book-to-market, and momentum effects, respectively. (Consult Fama and French [1993] and Carhart [1997] on the construction of these factors.) The sample period is from September 1990 through December 1999. Standard errors are reported in parentheses and significance at the five-percent and one-percent levels is indicated by * and ** respectively.

	α	*RMRF*	*SMB*	*HML*	*Momentum*
Democracy-Dictatorship	**0.71****	**-0.04**	**-0.22***	**-0.55****	**-0.01**
	(0.26)	**(0.07)**	**(0.09)**	**(0.10)**	**(0.07)**
$G \leq 5$ (Democracy)	0.29*	0.98**	-0.24**	-0.21**	-0.05
	(0.13)	(0.04)	(0.05)	(0.05)	(0.03)
$G=6$	0.22	0.99**	-0.18**	0.05	-0.08
	(0.18)	(0.05)	(0.06)	(0.07)	(0.04)
$G=7$	0.24	1.05**	-0.10	-0.14	0.15**
	(0.19)	(0.05)	(0.07)	(0.08)	(0.05)
$G=8$	0.08	1.02**	-0.04	-0.08	0.01
	(0.14)	(0.04)	(0.05)	(0.06)	(0.04)
$G=9$	-0.02	0.97**	-0.20**	0.14**	-0.01
	(0.12)	(0.03)	(0.04)	(0.05)	(0.03)
$G=10$	0.03	0.95**	-0.17**	-0.00	-0.08**
	(0.11)	(0.03)	(0.04)	(0.04)	(0.03)
$G=11$	0.18	0.99**	-0.14*	-0.06	-0.01
	(0.16)	(0.05)	(0.05)	(0.06)	(0.04)
$G=12$	-0.25	1.00**	-0.11*	0.16**	0.02
	(0.14)	(0.04)	(0.05)	(0.06)	(0.04)
$G=13$	-0.01	1.03**	-0.21**	0.14*	-0.08*
	(0.14)	(0.04)	(0.05)	(0.06)	(0.04)
$G \geq 14$ (Dictatorship)	-0.42*	1.03**	-0.02	0.34**	-0.05
	(0.19)	(0.05)	(0.06)	(0.07)	(0.05)

TABLE VII

Performance-Attribution Regressions under Alternative Portfolio Constructions

This table presents the alphas from four-factor regressions for variations on the Democracy ($G \leq 5$) minus Dictatorship ($G \geq 14$) Portfolio. The calculation of G is described in Section II. The portfolios are reset in September 1990, July 1993, July 1995, and February 1998, which are the months after new data on G became available. The sample period is September 1990 to December 1999. The first row uses the unadjusted difference between the monthly returns to the Democracy and Dictatorship Portfolios. The second row contains the results using industry-adjusted returns, with industry adjustments done relative to the 48 industries of Fama and French [1997]. The third and fourth rows use alternative definitions of the Democracy and Dictatorship Portfolios. In the third row, firms are sorted on G and the two portfolios contain the smallest set of firms with extreme values of G such that each has at least 10 percent of the sample. This implies cutoff values of G for the Democracy Portfolio of 5, 5, 6, and 5 for September 1990, July 1993, July 1995, and February 1998, respectively. The cutoffs for the Dictatorship Portfolio are always 13. In the fourth row, the two portfolios contain the largest set of firms such that each has no more than 10 percent of the sample. The cutoff values of G for the Democracy Portfolio are 4, 4, 5, and 4 for September 1990, July 1993, July 1995, and February 1998, respectively, and they are always 14 for the Dictatorship Portfolio. In the fifth row, portfolio returns are calculated maintaining the 1990 portfolios for the entire sample period. As long as they are listed in CRSP, we neither delete nor add firms to these portfolios regardless of subsequent changes in G or changes in the IRRC sample in later editions. The sixth row shows the results of restricting the sample to firms incorporated in Delaware. In the seventh and eighth rows, the sample period is divided in half at April 30, 1995, and separate regressions are estimated for the first half and second half of the period (56 months each). The explanatory variables are *RMRF*, *SMB*, *HML*, *Momentum*, and a constant. These variables are the returns to zero-investment portfolios designed to capture market, size, book-to-market, and momentum effects, respectively. (Consult Fama and French [1993] and Carhart [1997] on the construction of these factors.) All coefficients except for the alpha are omitted in this table. Standard errors are reported in parentheses and significance at the five-percent and one-percent levels is indicated by * and ** respectively.

		α, Value-Weighted	α, Equal-Weighted
(1)	Democracy-Dictatorship	0.71**	0.45*
		(0.26)	(0.22)
(2)	Industry-Adjusted	0.47*	0.30
		(0.22)	(0.19)
(3)	Big Portfolios	0.47*	0.39*
		(0.21)	(0.19)
(4)	Small Portfolios	0.78*	0.45
		(0.33)	(0.25)
(5)	1990 Portfolio	0.53*	0.33
		(0.24)	(0.22)
(6)	Delaware Portfolio	0.63	0.42
		(0.34)	(0.26)
(7)	Early Half	0.45	0.58*
		(0.23)	(0.28)
(8)	Late Half	0.75	0.04
		(0.40)	(0.27)

TABLE VIII
Q Regressions

The first column of this table presents the coefficients on *G*, the Governance Index, from regressions of industry-adjusted Tobin's *Q* on *G* and control variables. The second column restricts the sample to firms in the Democracy ($G \leq 5$) and Dictatorship ($G \geq 14$) portfolios and includes as regressors a dummy variable for the Democracy Portfolio and the controls. The third through seventh columns show the coefficients on each subindex from regressions where the explanatory variables are the subindices *Delay*, *Protection*, *Voting*, *Other*, and *State*, and the controls. We include as controls a dummy variable for incorporation in Delaware, the log of assets in the current fiscal year, the log of firm age measured in months as of December of each year, and a dummy variable for inclusion in the S&P 500 as of the end of the previous year. The coefficients on the controls and the constant are omitted from the table. The calculation of *G* and the subindices is described in Section II. *Q* is the ratio of the market value of assets to the book value of assets: the market value is calculated as the sum of the book value of assets and the market value of common stock less the book value of common stock and deferred taxes. The market value of equity is measured at the end of the current calendar year, and the accounting variables are measured in the current fiscal year. Industry adjustments are made by subtracting the industry median, where medians are calculated by matching the four-digit SIC codes from December of each year to the 48 industries designated by Fama and French [1997]. The coefficients and standard errors from each annual cross-sectional regression are reported in each row, and the time-series averages and time-series standard errors are given in the last row. * and ** indicate significance at the five-percent and one-percent levels respectively.

	(1) *G*	(2) *Democracy Portfolio*	(3) *Delay*	(4) *Protection*	(5) *Voting*	(6) *Other*	(7) *State*
1990	-0.022**	0.186	-0.015	-0.035	0.015	-0.031	-0.004
	(0.008)	(0.127)	(0.022)	(0.018)	(0.030)	(0.026)	(0.020)
1991	-0.040**	0.302*	-0.033	-0.048	-0.012	-0.059	0.003
	(0.012)	(0.143)	(0.034)	(0.028)	(0.047)	(0.040)	(0.031)
1992	-0.036**	0.340*	-0.041	-0.039	0.021	-0.054	-0.011
	(0.010)	(0.151)	(0.027)	(0.023)	(0.038)	(0.032)	(0.025)
1993	-0.042**	0.485*	-0.023	-0.055*	0.009	-0.060	-0.062*
	(0.011)	(0.204)	(0.029)	(0.026)	(0.038)	(0.035)	(0.027)
1994	-0.031**	0.335*	-0.032	-0.012	-0.032	-0.029	-0.047*
	(0.009)	(0.161)	(0.023)	(0.020)	(0.031)	(0.028)	(0.022)
1995	-0.039**	0.435*	-0.046	-0.062*	-0.086*	0.023	-0.022
	(0.011)	(0.217)	(0.030)	(0.027)	(0.041)	(0.036)	(0.028)
1996	-0.025*	0.299	-0.029	-0.030	-0.078	0.018	-0.024
	(0.011)	(0.195)	(0.031)	(0.028)	(0.041)	(0.037)	(0.028)
1997	-0.016	0.210	-0.017	-0.007	-0.055	-0.001	-0.017
	(0.013)	(0.196)	(0.035)	(0.032)	(0.047)	(0.042)	(0.032)
1998	-0.065**	0.203	-0.023	-0.096*	-0.132	-0.058	0.012
	(0.020)	(0.404)	(0.052)	(0.049)	(0.070)	(0.066)	(0.052)
1999	-0.114**	0.564	-0.067	-0.171*	-0.294**	-0.006	-0.033
	(0.027)	(0.602)	(0.071)	(0.067)	(0.098)	(0.090)	(0.073)
Mean	-0.043**	0.336**	-0.033**	-0.056**	-0.065	-0.025*	-0.020*
	(0.009)	(0.040)	(0.005)	(0.015)	(0.030)	(0.010)	(0.007)

TABLE IX
Operating Performance

The first, third, and fifth columns of this table give the results of annual median (least absolute deviation) regressions for net profit margin, return on equity, and sales growth on the Governance Index, *G*, measured in the previous year, and the book-to-market ratio, *BM*. The second, fourth, and sixth columns restrict the sample to firms in the Democracy ($G \leq 5$) and Dictatorship ($G \geq 14$) portfolios and include as regressors a dummy variable for the Democracy Portfolio and *BM*. The coefficients on *BM* and the constant are omitted from the table. The calculation of *G* is described in Section II. Net profit margin is the ratio of income before extraordinary items available for common equity to sales; return on equity is the ratio of income before extraordinary items available for common equity to the sum of the book value of common equity and deferred taxes; *BM* is the log of the ratio of book value (the sum of book common equity and deferred taxes) in the previous fiscal year to size at the close of the previous calendar year. Each dependent variable is net of the industry median, which is calculated by matching the four-digit SIC codes of all firms in the CRSP-Compustat merged database in December of each year to the 48 industries designated by Fama and French [1997]. The coefficients and standard errors from each annual cross-sectional regression are reported in each row, and the time-series averages and time-series standard errors are given in the last row. Significance at the five-percent and one-percent levels is indicated by * and ** respectively. All coefficients and standard errors are multiplied by 1000.

	(1)	(2)	(3)	(4)	(5)	(6)
	Net Profit Margin		Return on Equity		Sales Growth	
	G	*Democracy Portfolio*	*G*	*Democracy Portfolio*	*G*	*Democracy Portfolio*
1991	-0.70	10.61	-1.19*	13.54	-2.30	-3.52
	(0.39)	(7.12)	(0.60)	(11.30)	(1.38)	(17.83)
1992	-0.52	9.45	0.42	2.54	-1.43	0.10
	(0.58)	(10.43)	(0.61)	(9.21)	(1.06)	(11.52)
1993	-0.76	7.77	-0.34	2.51	-3.35**	18.55
	(0.48)	(9.98)	(0.79)	(10.98)	(1.17)	(17.71)
1994	-0.83	10.94	-1.07	2.69	-2.71*	12.58
	(0.48)	(6.59)	(0.61)	(10.36)	(1.10)	(22.81)
1995	-0.72	7.56	-1.39	14.77	-0.89	7.91
	(0.67)	(8.30)	(0.75)	(9.88)	(1.70)	(19.67)
1996	-0.43	-2.17	0.90	-2.30	-2.44	14.84
	(0.40)	(7.22)	(0.65)	(12.09)	(1.39)	(19.36)
1997	0.21	-9.61	0.66	-17.54	0.01	-4.28
	(0.55)	(9.99)	(0.81)	(9.83)	(1.64)	(26.61)
1998	-0.73	-3.99	-1.28	13.62	-1.45	-15.65
	(0.63)	(7.15)	(1.01)	(15.10)	(1.50)	(23.36)
1999	-1.27*	4.59	0.93	-15.53	-0.52	15.38
	(0.58)	(11.58)	(0.85)	(10.38)	(1.92)	(26.10)
Mean	-0.64**	3.91	-0.26	1.59	-1.68**	5.10
	(0.13)	(2.46)	(0.33)	(3.98)	(0.37)	(3.84)

TABLE X
Capital Expenditure

The first and third columns of this table present the results of annual median (least absolute deviation) regressions of CAPEX/Assets and CAPEX/Sales on the Governance Index, *G*, measured in the previous year, and *BM*. The second and fourth columns restrict the sample to firms in the Democracy ($G \leq 5$) and Dictatorship ($G \geq 14$) portfolios and include as regressors a dummy variable for the Democracy Portfolio and *BM*. The coefficients on *BM* and the constant are omitted from the table. The calculation of *G* is described in Section II. CAPEX is capital expenditures, and *BM* is the log of the ratio of book value (the sum of book common equity and deferred taxes) in the previous fiscal year to size at the close of the previous calendar year. Both dependent variables are net of the industry median, which is calculated by matching the four-digit SIC codes of all firms in the CRSP-Compustat merged database in December of each year to the 48 industries designated by Fama and French [1997]. The coefficients and standard errors from each annual cross-sectional regression are reported in each row, and the time-series averages and time-series standard errors are given in the last row. Significance at the five-percent and one-percent levels is indicated by * and ** respectively. All coefficients and standard errors are multiplied by 1000.

	(1)	(2)	(3)	(4)
	CAPEX/Assets		CAPEX/Sales	
	G	*Democracy Portfolio*	*G*	*Democracy Portfolio*
1991	1.32**	-13.02**	0.70*	-9.28
	(0.27)	(4.28)	(0.32)	(4.96)
1992	0.42	-7.03	0.54	-7.23
	(0.35)	(4.86)	(0.35)	(6.01)
1993	0.81*	-6.06	0.09	-1.68
	(0.37)	(4.48)	(0.34)	(4.98)
1994	0.51	-7.84	-0.07	-4.82
	(0.32)	(5.21)	(0.37)	(4.76)
1995	0.35	-3.40	0.32	-9.80
	(0.39)	(6.83)	(0.39)	(5.90)
1996	0.75	-6.90	0.31	-3.26
	(0.39)	(5.55)	(0.33)	(6.36)
1997	0.74*	-4.23	0.70	-8.05
	(0.34)	(3.50)	(0.40)	(5.71)
1998	0.80*	-10.57	0.37	-6.43
	(0.37)	(6.75)	(0.35)	(5.63)
1999	-0.15	3.12	-0.32	3.49
	(0.39)	(4.20)	(0.38)	(5.52)
Mean	0.62**	-6.21**	0.30*	-5.23**
	(0.13)	(1.53)	(0.11)	(1.41)

TABLE XI
Acquisitions

The first column of this table presents annual Tobit regressions of the *Acquisition Ratio* on the Governance Index, *G*, measured in the previous year, *SIZE*, *BM*, and industry dummy variables. The third column presents annual Poisson regressions of *Acquisition Count* on the same explanatory variables. In the second and fourth columns, we restrict the sample to firms in the Democracy ($G \leq 5$) and Dictatorship ($G \geq 14$) portfolios, and we include as a regressor a dummy variable that equals 1 when the firm is in the Democracy Portfolio and 0 otherwise. The coefficients on *SIZE*, *BM*, and the industry dummy variables are omitted from the table. The calculation of *G* is described in Section II. *Acquisition Ratio* is defined as the sum of the value of all corporate acquisitions during a calendar year scaled by the average of market value at the beginning and end of the year. *Acquisition Count* is defined as the number of acquisitions during a calendar year. The data on acquisitions are from the SDC database. *SIZE* is the log of market capitalization at the end of the previous calendar year in millions of dollars, and *BM* is the log of the ratio of book value (the sum of book common equity and deferred taxes) in the previous fiscal year to size at the close of the previous calendar year. Industry dummy variables are created by matching the four-digit SIC codes of all firms in the CRSP-Compustat merged database in December of each year to the 48 industries designated by Fama and French [1997]. The coefficients and standard errors from each annual cross-sectional regression are reported in each row, and the time-series averages and time-series standard errors are given in the last row. Significance at the five-percent and one-percent levels is indicated by * and ** respectively. All coefficients and standard errors are multiplied by 100.

	(1)	(2)	(3)	(4)
	Acquisition Count (Poisson Regressions)		*Acquisition Ratio (Tobit Regressions)*	
	G	*Democracy Portfolio*	*G*	*Democracy Portfolio*
1991	1.58 (1.46)	-50.81 (26.12)	0.51 (0.47)	0.14 (5.03)
1992	1.64 (1.44)	-31.39 (24.61)	0.10 (0.50)	7.91 (6.42)
1993	1.75 (1.42)	-47.67 24.51	0.70 (0.56)	-6.31 (6.85)
1994	4.09** (1.27)	-13.10 (21.02)	0.75 (0.48)	1.82 (4.14)
1995	2.57* (1.15)	-60.92** (17.85)	0.41 (0.44)	-2.95 (4.42)
1996	2.69* (1.14)	-66.06** (20.48)	1.33* (0.60)	-24.22** (9.41)
1997	2.34* (1.12)	-63.81** (19.03)	0.99* (0.51)	-9.24 (6.78)
1998	2.42* (1.09)	-52.03** (17.67)	1.47 (0.76)	-11.11 (8.51)
1999	0.52 (1.01)	-47.64** (17.27)	0.84 (0.74)	-20.87* (9.68)
Mean	2.18** (0.33)	-48.16** (5.60)	0.79** (0.14)	-7.21 (3.49)

TABLE XII
Insider Trading

The first and third columns of this table present annual OLS and ordered logit regressions of *Net Insider Purchases* on *G* measured in the previous year, *SIZE*, *BM*, and a constant. In the second and fourth columns, we restrict the sample to firms in the Democracy ($G \leq 5$) and Dictatorship ($G \geq 14$) portfolios and we include as a regressor a dummy variable that equals 1 when the firm is in the Democracy Portfolio and 0 otherwise. The coefficients on *SIZE*, *BM*, and the constant are omitted from the table. The calculation of *G* is described in Section II. *Net Insider Purchases* is the sum of split-adjusted open market purchases less split-adjusted open market sales during a year scaled by shares outstanding at the end of the previous calendar year. The ordered logit regressions use a dependent variable that equals 1 if *Net Insider Purchases* is positive, 0 if it is zero, and -1 if it is negative. The data on insider sales is from the Thomson database. *SIZE* is the log of market capitalization in millions of dollars measured at the end of the previous calendar year, and *BM* is the log of the ratio of book value (the sum of book common equity and deferred taxes) in the previous fiscal year to size at the close of the previous calendar year. The coefficients and standard errors from each annual cross-sectional regression are reported in each row, and the time-series averages and time-series standard errors are given in the last row. Significance at the five-percent and one-percent levels is indicated by * and ** respectively. All coefficients and standard errors are multiplied by 1000.

	(1)	(2)	(3)	(4)
	OLS		Ordered Logit	
	G	*Democracy Portfolio*	*G*	*Democracy Portfolio*
1991	0.07*	-0.14	-8.85	-345.18
	(0.04)	(0.53)	(21.34)	(295.15)
1992	0.10	-1.47	-66.92**	499.93
	(0.07)	(1.50)	(21.70)	(310.53)
1993	0.10	-0.23	-32.40	797.17*
	(0.07)	0.51	(21.41)	(326.87)
1994	0.07	-0.61	-28.09	323.07
	(0.04)	(1.23)	(20.58)	(290.11)
1995	0.04	-0.17	-4.66	-153.33
	(0.02)	(0.20)	(22.00)	(308.90)
1996	0.15	-0.62	12.01	-93.95
	(0.14)	(1.05)	(21.67)	(321.18)
1997	-0.01	0.89	-46.08	781.42*
	(0.10)	(0.66)	(24.33)	(369.78)
1998	-0.12	2.41	-1.88	146.49
	(0.20)	(3.17)	(24.31)	(342.22)
1999	0.36	-1.36	4.41	-117.36
	(0.48)	(2.91)	(21.09)	(323.85)
Mean	0.09	-0.15	-19.16	204.25
	(0.04)	(0.40)	(8.66)	(140.02)

TABLE XIII
Fama-MacBeth Return Regressions

This table presents the average coefficients and time-series standard errors for 112 cross-sectional regressions for each month from September 1990 to December 1999. The dependent variable is the stock return for month *t*. The results are presented using both raw and industry-adjusted returns, with industry adjustments done using the 48 industries of Fama and French [1997]. The first and second columns include all firms with data for all right-hand side variables and use *G*, the Governance Index, as an independent variable. In the third and fourth columns, the sample is restricted to firms in either the Democracy ($G \leq 5$) or Dictatorship ($G \geq 14$) portfolios, and we use the independent variable, *Democracy Portfolio*, a dummy variable that equals 1 when the firm is in the Democracy Portfolio and 0 otherwise. In the fifth and sixth columns, we again include all firms with data for each explanatory variable and use the subindices, *Delay*, *Protection*, *Voting*, *Other*, and *State* as regressors. The calculation of *G* and the subindices is described in Section II. Definitions for all other explanatory variables are provided in Appendix B. All regressions are estimated with weighted least squares where all variables are weighted by market value at the end of month *t-1*. Significance at the five-percent and one-percent levels is indicated by * and ** respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Raw	Industry-Adjusted	Raw	Industry-Adjusted	Raw	Industry-Adjusted
G	-0.04	-0.02				
	(0.04)	(0.03)				
Democracy Portfolio			0.76*	0.63*		
			(0.32)	(0.26)		
Delay					-0.03	0.02
					(0.10)	(0.07)
Protection					-0.07	-0.01
					(0.08)	(0.06)
Voting					-0.08	-0.08
					(0.13)	(0.10)
Other					0.01	-0.04
					(0.08)	(0.07)
State					0.02	-0.04
					(0.08)	(0.06)
NASDUM	-0.83	-0.42	-8.23	-10.36	-2.60	-0.29
	(6.94)	(5.26)	(6.45)	(5.94)	(6.39)	(4.98)
SP500	-0.19	-0.20	-0.42	-0.21	-0.19	-0.24
	(0.49)	(0.42)	(0.49)	(0.41)	(0.45)	(0.40)
BM	0.04	0.14	0.06	0.11	0.06	0.15
	(0.19)	(0.12)	(0.38)	(0.29)	(0.20)	(0.11)
SIZE	0.17	0.22	0.47	0.02	0.19	0.24
	(0.27)	(0.16)	(0.38)	(0.32)	(0.27)	(0.17)
PRICE	0.26	0.20	0.28	0.44	0.20	0.16
	(0.26)	(0.20)	(0.31)	(0.31)	(0.28)	(0.22)
IO	0.61	0.10	0.78	-0.16	0.59	0.14
	(0.47)	(0.33)	(0.67)	(0.60)	(0.44)	(0.33)
NYDVOL	-0.11	-0.21	-0.49	-0.03	-0.13	-0.21
	(0.29)	(0.18)	(0.36)	(0.31)	(0.28)	(0.18)
NADVOL	0.01	-0.13	-0.09	0.48	0.06	-0.15
	(0.43)	(0.29)	(0.41)	(0.39)	(0.43)	(0.29)
YLD	10.85	10.94	15.74	9.23	6.21	8.76
	(10.54)	(7.25)	(14.62)	(11.56)	(11.63)	(7.70)
RET2-3	-0.48	-0.93	-2.04	-1.82	-0.57	-1.03
	(1.40)	(1.04)	(2.33)	(1.73)	(1.43)	(1.07)
RET4-6	-0.68	-0.48	-2.21	-1.12	-0.58	-0.55
	(1.33)	(0.92)	(1.89)	(1.36)	(1.33)	(0.93)
RET7-12	2.42*	0.89	0.12	-1.67	2.69**	1.06
	(1.00)	(0.65)	(1.35)	(1.03)	(0.99)	(0.65)
SGROWTH	-0.00	0.03	0.75	0.27	-0.01	0.02
	(0.26)	(0.18)	(0.47)	(0.40)	(0.25)	(0.18)
Constant	-0.53	-0.18	1.17	-1.86	0.03	-0.16
	(2.55)	(1.71)	(3.43)	(2.99)	(2.39)	(1.69)