Endogeneity and the Dynamics of Internal Corporate Governance☆

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Abstract

We use a well-developed dynamic panel generalized method of moments (GMM) estimator to alleviate endogeneity concerns in two aspects of corporate governance research: the effect of board structure on firm performance and the determinants of board structure. The estimator incorporates the dynamic nature of internal governance choices to provide valid and powerful instruments that address unobserved heterogeneity and simultaneity. We re-examine the relation between board structure and performance using the GMM estimator in a panel of 6,000 firms over a period from 1991–2003, and find no causal relation between board structure and current firm performance. We illustrate why other commonly used estimators that ignore the dynamic relationship between current governance and past firm performance may be biased. We discuss where it may be appropriate to consider the dynamic panel GMM estimator in corporate governance research, as well as caveats to its use.

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1. Introduction

Empirical corporate finance research, which attempts to explain the causes and effects of financial decisions, often has serious issues with endogeneity. This is because it is generally difficult to find exogenous factors or natural experiments with which to identify the relations being examined. However, the implications for the empirical work's usefulness if it does not properly deal with endogeneity can be substantial. In a review article that provides guidance on addressing endogeneity issues in corporate finance, Roberts and Whited (2011) note that "endogeneity leads to biased and inconsistent parameter estimates that make reliable inference virtually impossible." A large body of empirical research suggests that certain governance structures drive improved performance, but this research is plagued with endogeneity issues. We often cannot ascertain if the causation is actually reversed (e.g., performance drives governance) or if governance is merely a symptom of an underlying unobservable factor, which also affects performance. Thus, it is difficult to determine what the parameter estimates actually suggest.

We respond to these endogeneity concerns in a specific setting, the relationship between boards and performance. This paper applies a well-developed panel GMM estimator to a data set of 6,000 firms over a 13-year period from 1991–2003. We find no relation between current board structure and current firm performance. This result is inconsistent with much earlier work and policy recommendations of many commentators. To strengthen our empirical argument we also illustrate why estimators that find a relation may be biased. We demonstrate how the panel GMM estimator can be used to control for the dynamic nature of the performance-governance relationship suggested by theorists, while accounting for other sources of endogeneity in corporate finance research.

Most empirical corporate finance researchers acknowledge at least two potential sources of endogeneity: unobservable heterogeneity and simultaneity. However, one source of endogeneity that is often ignored (explicitly or implicitly) arises from the possibility that current values of governance variables are a function of past firm performance. Neglecting this source of endogeneity can have serious consequences for inference. This is especially true since the difficulty in identifying natural experiments or exogenous instruments in many settings means that corporate governance researchers often rely on panel data and fixed-effects estimates for inference. Traditional fixed-effects estimation can potentially ameliorate the bias arising from unobservable heterogeneity. However, it does this at the expense of a strong exogeneity assumption, one that is often not explicitly recognized by researchers. That is, it assumes that current observations of the explanatory variable (e.g., board structure) are completely independent of past values of the dependent variable (typically firm performance, value, or some other governance attribute), an assumption that we argue is not realistic.

We recognize that ignoring the dynamic nature of the structure performance relationship in empirical work presents significant concerns. To deal with this issue, we have two broad goals in this paper: 1) understand the dynamic relation between boards and performance, and 2) understand how to use dynamic panel estimators in this context (and similar situations). There are four basic steps in our analysis. First, we present intuitive and theoretical arguments, and empirical results, that suggest that corporate governance is dynamically related to past firm performance. Second, we show how a well-developed dynamic estimator is well suited to deal with the dynamic nature of the relation between corporate governance and performance. Third, we apply the dynamic GMM estimator to our panel to estimate the relationship between board structure and performance and the determinants of board structure. Fourth, we discuss the implications of our results with the dynamic GMM estimator for dealing with endogeneity in the governance-performance relationship and other governance estimations, as well as caveats to its use.

We start with theoretical arguments building on Hermalin and Weisbach's (1998) model, which shows that board structure is partly a function of the bargaining process between the chief executive officer (CEO) and the board, and that since the CEO's bargaining position is a function of her ability (measured by past firm performance), board structure depends on past firm performance. Consistent with this argument, we find empirical evidence that board independence is negatively related to past firm performance.

Another argument we advance, which combines insights from theoretical work by Raheja (2005) and Harris and Raviv (2008), is that past performance has a direct influence on the firm's information environment, profit potential, and the opportunity cost of outside directors, all of which are factors that may affect the optimal board structure. Indeed, we find empirical evidence that firm characteristics that proxy for these factors (e.g., firm size, market-to-book ratio, etc.) are themselves related to past firm performance. While the theoretical models we invoke are not explicitly dynamic, the implications we draw from them, and our empirical evidence, suggest that any empirical estimation of the effect of board structure on past firm performance that ignores the dynamic relation between current board structure and past performance (as do traditional fixed-effects estimators) will yield inconsistent estimates.

Next, we show that, subject to caveats, the dynamic nature of the relation between corporate governance and performance actually sets up a powerful methodology for identifying the causal effect of governance on performance. This dynamic panel GMM estimator, developed in a series of papers by Holtz-Eakin, Newey, and Rosen (1988), Arellano and Bond (1991), Arellano and Bover (1995), and Blundell and Bond (1998), potentially improves on ordinary least squares (OLS) or traditional fixed-effects estimates in at least one of three important ways. First, unlike OLS estimation, it allows us to include firm-fixed effects to account for (fixed) unobservable heterogeneity. Second, unlike traditional fixed-effects estimates, it allows current governance to be influenced by previous realizations of, or shocks to, past performance. Third, unlike either OLS or traditional fixed-effects estimates, a key insight of the dynamic panel GMM estimator is that if the underlying economic process itself is dynamic—in our case, if current governance is related to past performance—then it may be possible to use some combination of variables from the firm's history as valid instruments for current governance to account for simultaneity. Thus, an important aspect of the methodology is that it relies on a set of "internal" instruments contained within the panel itself: past values of governance and performance can be used as instruments for current realizations of governance. This eliminates the need for external instruments.

We apply the dynamic panel GMM estimator to two often-studied aspects of corporate governance: (1) the effect of board structure on firm performance and (2) the determinants of board structure, and compare the results to those obtained from OLS or traditional fixed-effects estimates. Most prior studies of the effect of board structure on performance have estimated "static" models of the form: *performance = f(board structure, firm characteristics, fixed effects)*, where board structure reflects board size, independence, or whether or not the CEO is also the chair of the board. We posit that the appropriate empirical model should be a "dynamic" model of the form: *performance = f(past performance, board structure, firm characteristics, fixed effects)*. Our empirical analysis here reveals four key findings.

First, when we apply OLS or traditional fixed-effects to the "static" model as many previous studies have done, we find, as these previous studies have, statistically significant relations between board structure and firm performance: there is a negative relation between board size and performance, and the relation between board independence and performance varies from negative to positive as we move from OLS to traditional fixed-effects estimation.

Second, when we apply simple OLS to the "dynamic" model (including past performance but temporarily ignoring unobservable heterogeneity), we get the first clear indication of the importance of dynamics in the governance/performance relation. The R^2 rises from 27% in the "static" model to 41% in the "dynamic" model, while the magnitude of the estimated coefficients on both board size and independence fall dramatically (by over 90% in both cases) and become statistically indistinguishable from zero.

Third, when we apply the dynamic GMM panel estimator to the "dynamic" model—when we fully account for unobservable heterogeneity, simultaneity, and the relation between current board structure and past firm performance—we find no statistically significant relation between firm performance and any aspect of board structure. This is one of the key results of our paper and is in contrast with results from prior studies (where some find a positive and some find a negative relationship). Changes in board size or independence are not systematically related to higher (or lower) performance.

Finally, we apply the dynamic GMM methodology to examine how firm characteristics affect board structure. That is, we estimate an empirical model of the form: *board structure = f(past board structure, firm characteristics, fixed effects)*. We find that after accounting for unobserved heterogeneity, simultaneity, and the effect of past board structure on firm characteristics, board structure is closely associated with firm size, growth opportunities, firm risk, age, leverage, and past performance. These results are similar to those obtained in recent studies by Boone, Field, Karpoff, and Raheja (2007) and Linck, Netter, and Yang (2008), and suggest that the effect of past board structure on current firm characteristics may not be as important as the effect of past board structure on current performance. This is as expected since the explanatory variables (size, business segments, etc.) are not strongly determined by past values of the dependent variable (board size or independence); thus, any link from past governance to current firm characteristics will be indirect through the effect, if any, of governance on performance. As a result, any bias arising from the underlying dynamic nature of governance appears to be more important in regressions of governance on performance than in regressions of firm characteristics on governance.

Our results also help reconcile some of the conflicting results in the prior literature and explain how some reported correlations could arise from ignoring one or more aspects of the endogeneity inherent in the board structure-performance relation. One of the key points we raise in the paper, building on work by Wooldridge (2002) and Roodman (2008), is that if there is a dynamic relation between current values of an explanatory variable and past realizations of the dependent variable, a fixed-effects regression may be biased, and the direction of the bias will be opposite that of the dynamic relation. As we noted earlier, in our empirical analysis we find, similar to Hermalin and Weisbach (1988) and Bhagat and Black (2002), a negative relation between current board independence and past firm performance. Under these conditions, an OLS regression of board independence on performance may be negatively biased, while a traditional fixed-effects regression that ignores the dynamic relationship may be positively biased. We suggest that this may explain, at least in part, the mixed results from previous studies on the effect of board independence on firm performance.

However, the dynamic panel estimation methodology has its limitations. It relies on using the firm's history (lags of dependent and independent variables) for identification. Thus, there is a potential problem with weak instruments, which becomes greater as the number of lags of the instrumental variables increases. This represents an empirical trade-off. Increasing the instruments' lag length makes them more exogenous, but may also make them weaker. While weak instruments do not appear to drive the specific results in our paper, this may be an important issue in other settings. Further, we assume that errors are serially uncorrelated, but this may not hold with persistence for all variables. Additionally, Griliches and Hausman (1986) note that the bias resulting from errors in variables may be magnified when using panel data estimators. Since the dynamic panel GMM estimator relies, at least in part, on first-differencing, dynamic panel estimators may not eliminate measurement error bias unless we make strong and difficult-to-verify assumptions about serial correlation in the measurement error.

The use of lags as instruments also relies on a key assumption, the implications of which should be carefully considered by any researcher that wishes to apply dynamic panel data estimation. The methodology assumes, as a minimum, weak rational expectations (Muth, 1961; Lovell, 1986) on the part of actors in the firm's nexus of contracts. This means that future unexpected changes in performance are purely an expectational error, and implies that our empirical model includes every variable that could conceivably jointly affect both the dependent and explanatory variables (Hansen and Singleton, 1982). Given the imperfect nature of proxies in empirical research, this is unlikely to be the case. It is possible (perhaps even likely) that any cross-sectional regression of governance on performance is misspecified and that there are "omitted" time-varying unobserved variables that affect both governance and performance. Thus, researchers should be careful in relying too much on the statistical tests that examine the validity of the lagged instrument set in justifying their use of dynamic panel data estimation. Simulation results in our paper (and in Roberts and Whited, 2011) suggest that these statistical tests may not detect potential misspecification if the coefficient bias introduced by the misspecification falls below a certain threshold (about 25% in our own simulations). However, misspecification is likely to be as big a problem with OLS and traditional fixed-effects estimation as well, and these methods are generally not accompanied by any specification tests. Thus, even given the occasional weakness of the specification tests accompanying the dynamic GMM estimator, it likely still dominates inference from OLS or fixed-effects estimation if the underlying economic process is dynamic.

Finally, we are quick to note that the dynamic panel GMM estimator does not solve all endogeneity problems. When available, natural experiments or carefully chosen strictly exogenous instruments remain the "gold standard" for consistently identifying the effect of an explanatory variable on a dependent variable. However, given the infrequent occurrence of natural experiments, such as unexpected regulatory changes, and the relative paucity of exogenous instruments, inference in corporate finance research is likely to continue to rely on cross-sectional regressions using panel data. Our paper contributes to the literature by providing economic justification for the use of dynamic panel data estimation in corporate governance research, discussing the conditions under which it improves inference beyond OLS and traditional fixed-effects estimates, while highlighting its limitations.

The rest of the paper is organized as follows. In Section 2, we discuss related literature and develop our hypotheses. In Section 3, we lay out the theoretical basis for the biases that may arise in commonly used techniques for estimating the relation between governance and performance. We also describe the dynamic panel GMM estimator and perform numerical simulations to evaluate the power of specification tests associated with this estimator. We describe the data for our empirical applications in Section 4, and provide an empirical analysis of the relation between board structure and firm performance in Section 5. In Section 6, we re-examine the determinants of board structure in a dynamic framework. We conclude in Section 7.

2. An empirical model for board structure and performance

While endogeneity is pervasive across many aspects of corporate finance, to illustrate the specific effect of endogeneity arising from the dynamic relation between current governance and a firm's history, we focus on the relation between board structure and performance.[1] This is an area that has received substantial attention in the literature. As we show in this section, theory and prior empirical work suggest that board structure, like many aspects of a firm's organization or governance, is dynamically endogenous with respect to firm performance.

Table 1 presents a summary of prior studies that have explicitly examined the relation between board structure and firm performance. The results are mixed. For example, most find either a negative relation between board independence and performance (Agrawal and Knoeber, 1996; Klein, 1998; Bhagat and Black, 2002) or no relation at all (Hermalin and Weisbach, 1991; Mehran, 1995). Interestingly, most who argue

[1] The term *"dynamic endogeneity"* has sometimes been used to refer to the type of endogeneity that arises from the possibility that a firm's current actions will affect its control environment and future performance, which will in turn affect its future control environment; see, for example, Durlauf and Quah (1999) and Asada, Chen, Chiarella, and Flaschel (2006).

for a particular level of board independence suggest that more independent boards improve performance through better monitoring of management. Yermack (1996) and Coles, Daniel, and Naveen (2008) do find a positive relation in some specifications. However, more recent evidence suggests that independent boards are not always value-enhancing. For example, Adams (2009) finds that financial firms that faced the most severe distress during the 2008/2009 financial crisis, and consequently needed government bailout funds, actually had more independent boards than the financial firms that did not. She suggests that this may have been due to the lack of experience and industry-specific knowledge of most of the independent directors.

There appears to be more empirical regularity in studies that examine the effect of board size on performance. Most (e.g., Yermack, 1996; Eisenberg, Sundgren, and Wells, 1998) report a negative relation between firm performance and board size. The theory is that larger boards are likely to have higher coordination costs, which reduces their ability to effectively monitor management.

2.1. Related theoretical work on board structure and firm performance

In this section, we present a brief review of theoretical work related to our approach. While most of the existing theoretical models are not explicitly dynamic or multi-period, they often imply that there is a dynamic element to the determinants of board structure, which may introduce endogeneity into an estimation of firm performance with board structure. The discussion is drawn mostly from Hermalin and Weisbach (1998), Raheja (2005), and Harris and Raviv (2008).

We propose that there are at least two channels by which past performance can explicitly affect current board structure. First, we note that Hermalin and Weisbach (1998) argue that board independence is the outcome of a bargaining process between the existing CEO and the board. The CEO's bargaining power derives from his perceived ability compared to alternative managers that the firm might be able to hire. They propose that the intensity with which the board monitors the CEO: (i) decreases with its prior estimate of the CEO's ability, (ii) decreases with its precision of its prior estimate, and (iii) increases with the precision of its privately acquired signal about the CEO's ability. One implication of this model is that within any particular period, there will be a *negative* relation between board independence and the ability of the firm's managers. Another implication is that that board composition will be related to the firm's past performance. As Hermalin and Weisbach (1998, p. 97) suggest: "poor performance lowers the board's assessment of the CEO's ability, reducing his bargaining position and thus increasing the probability that the CEO will be forced to accept more independent directors." Thus, while the Hermalin and Weisbach model is not an explicitly dynamic one, one potential implication of their model is that current board independence will be *negatively* related to past firm performance. Hermalin and Weisbach (1988) and Bhagat and Black (2002) find empirical evidence of this negative relation.

Past performance can affect current board structure through another channel. If board structure is determined by firm characteristics (as suggested by Raheja, 2005) and these characteristics are related to past performance, then board structure is related to past performance through the effect of performance on firm characteristics. For example, following arguments presented by Fama and Jensen (1983), Boone et al. (2007) argue that larger firms are more hierarchical, and that the larger firm boards ratify and monitor more decisions of senior managers. It follows that the information requirements of more complex, larger firms will require larger boards. Boone et al. (2007), Coles et al. (2008), Linck et al. (2008), and Lehn, Patro, and Zhao (2009), among others, find a positive relation between board size and firm size. Firm size is likely to be *positively* related to firm performance, so board size will be *positively* related to past firm performance through the effect of performance on size.

More recently, Harris and Raviv (2008) develop a model in which board structure is neither exogeneous nor by itself a determinant of performance, but both are functions of other variables like the importance of insider information and the firm's potential profits. Their model has a number of dynamic implications. For example, one comparative static that they derive is that changes in the firm's potential profitability has a first-order effect on the optimal number of outsiders and insiders on the board.

2.2. A dynamic empirical model of firm performance

The discussion in Section 2.1 suggests that board structure is a choice variable that arises through a process of bargaining between the various actors in a firm's nexus of contracts, where the bargaining process is influenced by past performance and the actors' beliefs about the costs and benefits of particular board structures. Thus, if board structure is dynamic and firm i (given its performance at time $t-1$ or earlier) chooses a board structure $\mathbf{X}_{it}$ to achieve a particular level of expected performance at time t, then the dynamic model for board structure is:

$$\mathbf{X}_{it} = f(y_{it-1}, y_{it-2} \ldots y_{it-p}, \mathbf{Z}_{it}, \eta_i), \tag{1}$$

where $\mathbf{X}$, $\mathbf{Z}$, and y represent board structure, firm characteristics, and performance, respectively, and η represents an unobserved firm effect.

Eq. (1) suggests that estimating the effect of board structure on firm performance, conditional on firm heterogeneity, requires estimating the following empirical model:

$$y_{it} = \alpha + \sum_{s} \kappa_s y_{it-s} + \beta \mathbf{X}_{it} + \gamma \mathbf{Z}_{it} + \eta_i + \varepsilon_{it} \quad s = 1, \ldots, p, \tag{2}$$

where ε_{it} is a random error term and β is the effect of board structure on performance

A key aspect of Eq. (2) is that if board structure is a choice variable, then it must be based on some expectations of performance. However, the model does not require that we assume that expected performance is the one that maximizes firm value. For example, some firms may overestimate the effect of increasing (or decreasing) board size, while others may underestimate it. Agency and transaction costs may also mean that expected performance may be less than value-maximizing. However, once the bargaining has occurred, the board has been chosen, and associated expectations have been set, then any unexpected changes to performance would be genuine shocks with respect to the information the firm used to choose its board structure.

This assumption means that if we estimate Eq. (2), current shocks are independent of historical realizations of performance or board structure. This is not a strong assumption since it allows current performance to be influenced by past and current realizations of board structure. The assumption leaves open the possibility that firms strategically choose governance to affect current or future performance. If the board structure that we observe today is one that trades off the *anticipated* costs and benefits of particular structures, then the *unanticipated* component of performance, many years in the future, will not be related to the board structure that is chosen today. This intuition, which can be written in orthogonality form as $E(\varepsilon_{it}|y_{it-s}, \mathbf{X}_{it-s}) = 0$, is essentially the same as assuming weak rational expectations among participants in the firm's nexus of contracts (Muth, 1961; Lovell, 1986). Indeed, if Eq. (1) represented the "true" model for performance, i.e., if we had correctly identified every endogenous time-varying variable that affected performance, then ε_{it} would be an expectational error and the orthogonality assumption would be valid (Hansen and Singleton, 1982). Eq. (2) is merely a reduced-form model which means that the reduced-form error, ε_{it}, is at best an imperfect proxy for the pure expectational error. In subsequent empirical analysis, we do our best to approach the "true" model by including, as controls, as many variables that determine board structure and could conceivably affect performance that we identify from prior research. Nevertheless, we cannot completely rule out the possibility that we have omitted an endogenous time-varying variable that has an economically significant effect on both firm performance and board structure.

3. Estimating the relation between governance and firm performance

In Sections 3.1 and 3.2 we lay out the theoretical basis for the biases that arise when we use OLS or fixed-effects regressions to estimate the relation between governance and firm performance. We then discuss the dynamic panel general method of moments (GMM) estimator, which mitigates these biases, as well as specification tests of the validity of our dynamic panel assumptions. In Section 3.3, we use a numerical simulation to assess the power of these specification tests to determine unobserved misspecification.

3.1. Sources of endogeneity in the governance/performance relation

In this section, we discuss the souces of econometric endogeneity that may arise with specific reference to the empirical model in Eq. (2).

3.1.1. Simultaneity

Econometrically, simultaneity exists in Eq. (2) if $E(\varepsilon_{it}|\mathbf{X}_{it},\mathbf{Z}_{it}) \neq 0$. From an economic perspective, simultaneity can arise in the board structure/performance relation. If, as theory suggests, firms choose their board structure in any period with a view towards achieving a particular level of performance in that period, then while performance may be affected by board structure, the reverse will also be true—board structure will also be affected by performance. In this case, board structure and performance are simultaneously determined and both OLS and fixed-effects estimates of Eq. (2) will be biased.

One potential solution to the problem of simultaneity is to estimate the effect of board structure on performance using a system of equations. In one equation, performance is allowed to depend on governance and other control variables while in other equations, governance is allowed to depend on performance and other control variables. However, estimating this system requires us to identify strictly exogenous instruments—there must be at least one variable in the governance equation that is not also in the performance equation. In practice, identifying and justifying a strictly exogenous instrument is very difficult.[2] To further complicate matters, the number of such exogenous instruments increases with the number of equations in the system.

3.1.2. Endogeneity and the bias of fixed-effects estimation

Econometrically, unobservable heterogeneity exists in Eq. (2) if $E(\eta_i|\mathbf{X}_{it},\mathbf{Z}_{it}) \neq 0$. Economically, unobservable heterogeneity is a source of endogeneity if there are factors unobservable to the researcher that

[2]Our point here is not that it is always impossible to find good instruments in corporate governance research. For example, using a unique data set from Denmark, Bennedsen, Nielsen, Perez-Gonzalez, and Wolfenzen (2007) use the fact that family succession is more likely in firms where the first-born is male, to assess the effect of family succession in the performance of closely held firms. However, such clear-cut instruments are relatively rare in the corporate governance literature.

affect both performance and the explanatory variables. In the board structure/performance context, theory suggests that this is the case. For example, consider the effect of managerial ability which, while generally unobservable, certainly affects performance. However, as we discussed in Section 2, Hermalin and Weisbach (1998) suggest that firms with high-ability managers will monitor less and thus, will have less independent boards. Therefore, an OLS regression of performance on board structure that ignores this unobservable heterogeneity may find a *negative* relation between board independence and performance.

A potential solution to the time-invariant or "fixed" part of unobservable heterogeneity, if panel data are available, is a fixed-effects or "within" estimation. Consider the linear model:

$$y_t = \beta x_t + \eta + \varepsilon_t, \tag{3}$$

where η represents an unobserved fixed effect. A fixed-effects transformation, which requires time-demeaning all variables yields:

$$\ddot{y}_t = \beta\,\ddot{x}_t + \varepsilon_t, \tag{4}$$

where $\ddot{x} = x_{it} - \bar{x}_i$ and $\ddot{y} = y_{it} - \bar{y}_i$.

However, what is often not recognized are the conditions under which a fixed-effects regression would be consistent and unbiased. A fixed-effects regression of the model in Eq. (2) would be consistent only if current values of the explanatory variables (governance) were completely independent of past realizations of the dependent variable (performance), i.e., if $E(\varepsilon_{is}|\mathbf{X}_{it},\mathbf{Z}_{it}) = 0, \forall s,t$. This means that fixed-effects estimates would be biased if past performance affects current values of governance. What happens if we inadvertently apply fixed-effects estimation in the presence of this dynamic relationship? From Wooldridge (2002), the potential bias from a fixed-effects estimate of Eq. (3) is:

$$\frac{1}{T}\sum_{t-1}^{T} E(\ddot{x}_{it}'\varepsilon_{it}) = -\frac{1}{T}\sum_{t-1}^{T} E(\bar{x}_i'\varepsilon_{it}) = -E(\bar{x}_i'\bar{\varepsilon}_i). \tag{5}$$

Eq. (5) suggests that if the explanatory variable, x, is positively (negatively) related to past values of the dependent variable, y, then a fixed-effects estimate of current values of y on current values of x will be negatively (positively) biased. It also suggests that even if there is no causal relation from x to y, a fixed-effects regression could yield a spurious estimate of the effect of x on y.

To further illustrate how a spurious correlation could arise if there is dynamic relation between past performance and current governance, consider a simple model in which past performance (y) causes changes in governance (x) but x does not cause y. The model can be written as follows:

$$y_{it} = \beta x_{it} + \varepsilon_{it} \tag{6}$$

$$x_{it} = \lambda y_{i,t-1} + \varepsilon_{it}$$

$$\varepsilon_{it} \sim i.i.d.N(0,\sigma_\varepsilon^2) \qquad \varepsilon_{it} \sim i.i.d.N(0,\sigma_\varepsilon^2).$$

By recursive substitution, we can write each x_{it} as:[3]

$$x_{it} = \lambda[\varepsilon_{i,t-1} + \gamma\varepsilon_{i,t-2} + \ldots + \gamma^{t-2}\varepsilon_{i,1} + \gamma^{t-1}\varepsilon_{i,0} + \gamma^t x_{i,0}] + \nu_{it}, \tag{7}$$

where $\gamma = \lambda\beta$, $x_{i,0}$ is some initial value of x independent of future performance shocks ($E[x_{i0}\varepsilon_{it}] = 0$), and where ν_{it} is a random error such that $E[\varepsilon_{it}\nu_{js}] = 0$, $\forall i \neq j$ and $t \neq s$.

Substituting (7) into (5) and making use of our assumptions above that x is orthogonal to current or future innovations y, and that $\varepsilon_{it} \sim i.i.d.N(0,\sigma_e^2)$, we get:

$$\begin{aligned} -E(\bar{x}_i'\bar{\varepsilon}_i) &= -\frac{1}{T^2}\sum_{t=1}^{T}\left[\sum_{t=1}^{T} x_t \sum_{r=1}^{T}\varepsilon_{t-r}\right] \\ &= -\frac{1}{T^2}\sum_{t=1}^{T}\left[\sum_{t=1}^{T}\lambda[\varepsilon_{i,t-1} + \gamma\varepsilon_{i,t-2} + \ldots + \gamma^{t-2}\varepsilon_{i,1} + \gamma^{t-1}\varepsilon_{i,0} + \gamma^t x_{i,0}]\sum_{r=1}^{T}\varepsilon_{t-r}\right] \\ &= -\frac{\lambda\sigma_\varepsilon^2}{T^2}\left[\frac{1-\gamma^{T-1}}{1-\gamma} + \frac{1-\gamma^{T-2}}{1-\gamma} + \ldots + 1\right]. \end{aligned}$$

Simplifying, we obtain

$$-E(\bar{x}_i'\bar{\varepsilon}_i) = -\frac{\lambda\sigma_\varepsilon^2[(T-1) - T\gamma + \gamma^T]}{T^2(1-\gamma)^2}. \tag{8}$$

Eq. (8) suggests that even if there is no causal effect of x on y (i.e., $\gamma = 0$), a fixed-effects regression of y on x could yield a spurious but statistically significant estimate of such an effect, if T is finite. This bias could be significant in many corporate governance empirical applications where the length of the panel (T) is usually small. In general, if current values of x are related to past values of y (i.e., $\lambda \neq 0$), then a conditional regression of y on x could yield a coefficient estimate that is opposite that of the correlation between current x and past y. This will be the case if the conditioning variables (fixed effects, control variables, etc.) contain information about past values of y. Of course, the sign and magnitude of the estimated partial effect of current x on current y will depend not just on the effect of past y on current x, but also the effect of past y on current values of any control variables that are included if we carry out a regression of y on x.

[3] See Roodman (2008). Roodman (2008) also derives the potential biases from fixed-effects regressions of y on leads or lags of x for the model specified in (6).

3.2. Dynamic panel GMM estimation

To obtain consistent and unbiased estimates (under the assumption that unobserved heterogeneity exists but is fixed or time-invariant), we estimate the relation between board structure and performance using a dynamic GMM panel estimator. This estimator was introduced by Holtz-Eakin et al. (1988) and Arellano and Bond (1991), and further developed in a series of papers including Arellano and Bover (1995) and Blundell and Bond (1998). It exploits the dynamic relationships inherent in our explanatory variables. The dynamic modeling approach has been used in other areas of finance and economics where the structure of the problem suggests a dynamic relation between dependent and independent variables. Examples include capital accumulation and firm investment (Whited, 1991), the sensitivity of firm investments to available internal funds (Bond and Meghir, 1994), economic growth convergence (Caselli, Esquivel, and Lefort, 1996), estimation of a labor demand model (Blundell and Bond, 1998), the relation between financial intermediary development and economic growth (Beck, Levine, and Loayza, 2000), and the diversification discount (Hoechle, Schmid, Walter, and Yermack, 2012), among others.

The basic estimation procedure consists of two essential steps. First, we write the dynamic model of (2) in first-differenced form:

$$\Delta y_{it} = \alpha + \kappa_p \sum_p \Delta y_{it-p} + \beta \Delta \mathbf{X}_{it} + \gamma \Delta \mathbf{Z}_{it} + \Delta \varepsilon_{it}, \quad p > 0. \tag{9}$$

First-differencing eliminates any potential bias that may arise from time-invariant unobserved heterogeneity. After first-differencing, we estimate (9) via GMM using lagged values of the explanatory variables as instruments for the current explanatory variables. That is, we use historical values of performance, board structure, and other firm-specific variables as instruments for current changes in these variables.

An important aspect of the dynamic panel estimator is its use of the firm's history as instruments for our explanatory variables. This means that in estimating Eq. (2) or the first-difference transformation in Eq. (9), our instruments will be drawn from the set of lagged dependent or explanatory variables, i.e., $y_{t-k}, \mathbf{X}_{t-k}, \mathbf{Z}_{t-k}$, where $k > p$. For these instruments to be valid, they must meet two criteria. First, they must provide a source of variation for current governance, i.e., $\mathbf{X}_t = f(y_{t-k}, \mathbf{X}_{t-k}, \mathbf{Z}_{t-k})$. In our discussion on the determinants of board structure, we have already established a theoretical motivation for this assumption. In additional analysis and using a variety of empirical tests in Section 5, we show that board structure is strongly correlated to historical performance and lagged values of other explanatory variables.

Second, the historical or lagged values must provide an *exogenous* source of variation for current governance. This means that lagged variables must be uncorrelated with the error in the performance equation in Eq. (2). Theory provides motivation for this. As discussed earlier, under the assumption of weak rational expectations, if the board structure that we observe today is one that trades off the anticipated costs and benefits of particular board structures, then current shocks to performance must have been unanticipated when the boards were chosen. Any information from the firm's past is impounded into current expected performance within p time periods. This means that p lags of past performance are sufficient to capture the influence of the firm's past on the present, i.e., including p lags ensures *dynamic completeness* of Eq. (2). Provided we have included p lags of performance, any information from the firm's history that is older than that has no direct effect on current performance and only affects performance through its effect on current governance and other firm characteristics. Thus, the firm's history beyond period $t-p$ should be *exogenous* with respect to any shocks or surprises to performance in the current or future periods. In our empirical analysis, we further examine the validity of our exogeneity assumptions using a battery of empirical tests. Again, it is worth pointing out that an underlying assumption in our analysis is that we have identified, and included as control variables in our empirical model of performance, any time-varying variable that may jointly affect both performance and governance.

If the exogeneity assumptions are valid, then we can write the following orthogonality conditions:

$$E(\mathbf{X}_{it-s}\varepsilon_{it}) = E(\mathbf{Z}_{it-s}\varepsilon_{it}) = E(y_{it-s}\varepsilon_{it}) = 0, \qquad \forall\ s > p. \tag{10}$$

We can then estimate (9) using GMM and the given orthogonality conditions. However, despite the economic appeal of this procedure, it does have at least three econometric shortcomings. First, Beck et al. (2000) note that if the original model is conceptually in levels, differencing may reduce the power of our tests by reducing the variation in the explanatory variables. Second, Arellano and Bover (1995) suggest that variables in levels may be weak instruments for first-differenced equations. Third, first-differencing may exacerbate the impact of measurement errors on the dependent variables (Griliches and Hausman, 1986).

Arellano and Bover (1995) and Blundell and Bond (1998) argue that we can mitigate these shortcomings and improve the GMM estimator by also including the equations in levels in the estimation procedure. We can then use the first-differenced variables as instruments for the equations in levels in a "stacked" system of equations that includes the equations in *both* levels and differences. This produces a "system" GMM estimator, that involves estimating the following system:

$$\begin{bmatrix} y_{it} \\ \Delta y_{it} \end{bmatrix} = \alpha + \kappa \begin{bmatrix} y_{it-p} \\ \Delta y_{it-p} \end{bmatrix} + \beta \begin{bmatrix} \mathbf{X}_{it} \\ \Delta \mathbf{X}_{it} \end{bmatrix} + \gamma \begin{bmatrix} \mathbf{Z}_{it} \\ \Delta \mathbf{Z}_{it} \end{bmatrix} + \varepsilon_{it}. \tag{11}$$

Unfortunately, the equations in levels still include unobserved heterogeneity. To deal with this, we assume that while the governance and control variables may be correlated with the unobserved effects, this correlation is constant over time. This is a reasonable assumption over a relatively short time period if the unobserved effects proxy for factors like unobserved director ability, managerial productivity, etc. The assumption leads to an additional set of orthogonality conditions:

$$E[\Delta \mathbf{X}_{it-s}(\eta_i + \varepsilon_{it})] = E[\Delta \mathbf{Z}_{it-s}(\eta_i + \varepsilon_{it})] = E[\Delta y_{it-s}(\eta_i + \varepsilon_{it})] = 0, \qquad \forall\, s > p. \tag{12}$$

With the system GMM estimator, we obtain efficient estimates while controlling for time-invariant unobserved heterogeneity, simultaneity, and the dynamic relationship between current values of the explanatory variables and past values of the dependent variable.

We carry out GMM panel estimation using the orthogonality conditions of (10) and (12) under the assumption that there is no serial correlation in the error term, ε. The orthogonality conditions of (10) and (12) imply that we can use lagged levels as instruments for our differenced equations and lagged differences as instruments for the levels equations, respectively. Later, we carry out rigorous tests of the validity of the orthogonality assumptions as well as the strength of the instruments that are implied by these assumptions.

Our key exogeneity assumption, as stated in Eq. (8), is that the firm's historical performance and characteristics are exogenous with respect to current shocks or innovations in performance. Arellano and Bond (1991) suggest two key tests of this assumption.

The first test is a test of second-order serial correlation. The biggest concern is whether or not we have included enough lags to control for the dynamic aspects of our empirical relationship. If we have, then any historical value of firm performance beyond those lags is a potentially valid instrument since it will be exogenous to current performance shocks. For our GMM estimates, if the assumptions of our specification are valid, by construction the residuals in first differences ($AR(1)$) should be correlated, but there should be no serial correlation in second differences ($AR(2)$). The second test is a Hansen test of over-identification. The dynamic panel GMM estimator uses multiple lags as instruments. This means that our system is over-identified and provides us with an opportunity to carry out the test of over-identification. The Hansen test yields a J-statistic which is distributed χ^2 under the null hypothesis of the validity of our instruments.

3.3. How powerful are the specification tests in detecting model misspecification?

Above, we note that OLS and fixed-effects will be biased if our explanatory variables are not strictly exogenous and the panel's time dimension is small. We also note that the validity of system GMM estimation hinges, at least in part, on two critical specification tests. Nevertheless, it is worth noting that the tests of second-order serial correlation ($AR(2)$) and the tests of over-identification (Hansen J) of the validity of our instruments are not specification tests of our empirical specification—they are merely tests of our instrument set under the assumption that we have the "correct" specification. It is possible, for example, that even if there is some unobserved time-varying variable that affects both the dependent variable (performance) and the endogenous explanatory variable (governance), that biases our GMM estimates, the $AR(2)$ test and the Hansen J test may still "pass" at conventional levels. This could be a major problem in corporate governance research where our observable proxies are limited and there may be unmodeled factors that jointly affect both governance and performance. In other words, this is a joint hypothesis test.

Consider the case where the true model (data-generating process) for firm performance (y_{it}) is:

$$y_{it} = \beta x_{it} + \gamma z_{it} + \kappa r_{it} + \eta_i + \varepsilon_{it}^{y}, \qquad \varepsilon_{it} \sim N(0, \sigma_y). \tag{13}$$

In this model, performance, y_{it}, is determined by an endogenous governance factor, x, a strictly exogenous factor, z, a fixed unobservable firm-specific factor, η, and an unobserved time-varying variable, r. z is strictly exogenous in the sense that it does not depend on past performance or the unobservable firm factor and is generated by the process:

$$z_{it} = \alpha z_{it-1} + \varepsilon_{it} \qquad \varepsilon_{it} \sim N(0, \sigma_z^2). \tag{14}$$

We introduce "misspecification" into the system via an unobservable variable, r, which is correlated with both y and x through κ and is generated by the process:

$$r_{it} = \theta r_{it-1} + \varepsilon_{it}^{r}. \tag{15}$$

The endogenous governance factor, x, is determined by the process:

$$x_{it} = \rho x_{it-1} + \pi z_{it} + \lambda y_{it-1} + \kappa r_{it} + \delta \eta_i + \varepsilon_{it} \qquad \varepsilon_{it} \sim N(0, \sigma_x). \tag{16}$$

In this model, x is endogenous in two dimensions. First, it is related to past performance, y, through λ. Second, x is also correlated with both the time-invariant unobserved firm-specific factor, η, and the time-varying unobserved factor, r. Thus, any estimation of Eq. (13) needs to account for both dynamic relation between x and y as well as unobserved heterogeneity. The researcher, unaware of the true model, estimates Eq. (13) to draw inferences based on the magnitude and significance of the estimated coefficient, $\hat{\beta}$.

To determine how well the specification tests perform, we simulate the system of the form given by (13), (14), (15), and (16) and generate panel data sets of time and cross-sectional dimensions that are similar to those commonly found in corporate governance research. For each iteration, we select panels with N = 1,000 or N = 2000, and T = 7 (which is the length of the panel that we use in our subsequent empirical analysis of the board structure/performance relation in Section 5). Our simulation parameters are chosen such that y has a distribution similar to industry-adjusted return on assets (ROA), x has a distribution similar to that of the log of board size, and z has a distribution similar to that of the log of market value of equity.[4] In our simulations, we assume that the "true" β in (13) is 0.01. We carry out the regressions of y on y_{t-1}, x, and z, thus ignoring the unobserved time-varying variable, r. Our regressions are executed using `xtabond2` in Stata with all variables, lagged two or more periods, as instruments. Except where otherwise specified, we invoke the "collapse" option of `xtabond2`. The collapse option specifies that xtabond2 should create one instrument for each variable and lag distance, rather than one for each time period, variable, and lag distance. This option effectively constrains all of the yearly moment conditions to be the same.[5]

The results of our simulation are summarized in Figs. 1(a) – (c). In each of our figures, $Bias = (\hat{\beta} - \beta)/\beta$, where the "true" $\beta = 0.01$ and $\hat{\beta}$ is the estimated (simulated) effect of x on y. We show the probability that we will reject the null of no second-order serial correlation ($AR(2)$) at the 5% level and the probability that we will reject the null of valid instruments via the Hansen J test of over-identification at the 5% level. We allow the magnitude of "misspecification" to vary by varying κ in (13) and (16). The results reveal four important general observations about the power of the specification tests.

First, we observe that when there is a certain level of unmodeled time-varying unobservable heterogeneity that can bias the GMM estimates, the $AR(2)$ and Hansen J tests "pass" at conventional levels and "fail" to detect the misspecification. So, for example, in Fig. 1(a) (with a sample size of 1,000) we find that up to a bias of about 12%, both the $AR(2)$ and Hansen J have rejection rates of 10% or less. In other words, this

[4] See Appendix C for further details of how the data are generated for the Monte Carlo analysis.
[5] See Appendix A for further details of the "collapse" option.

magnitude of bias will go undetected by our specification tests more than 90% of the time.

Second, we note that $AR(2)$ tests do not appear to detect misspecification at any level of bias. Across all our simulations, as shown in Figs. 1(a) and (b), the rejection rate of the $AR(2)$ tests does not exceed 11% even as the bias induced by the misspecification approaches 90%. In contrast, Figs. 1(a), (b) and (c) clearly show that Hansen J test rejection rates increase as the magnitude of the induced bias increases. In the case where the sample size is 1,000 (Fig. 1(a)), Hansen J rejection rates reach 40% when the magnitude of the bias is about 50%. When the sample size is increased to 2,000 (Fig. 1(b)), Hansen J rejection rates reach 40% when the magnitude of the bias is 25%.

A third observation, which is related to the second, is that the power of the (Hansen J) specification tests to detect misspecification increases with the sample size. Figs. 1(a), (b) and (c) clearly show that the steepness of the positive relation between the rejection rates and the magnitude of the bias is much steeper when the sample size is 2,000 than when the sample size is 1,000. As we previously noted, Fig. 1(c) shows that when the sample size is 1,000, a coefficient bias with a magnitude of 50% is associated with Hansen J test rejection rates of 40%; this rejection rate rises to about 70% when the sample size is 2,000.

Finally, Fig. 1(c) shows that using the "collapse" option significantly increases the power of the Hansen J. This is not very surprising; Bowsher (2002) finds that the use of too many moment conditions can significantly reduce the power of tests of over-identifying restrictions. The "collapse" option, by constraining all of the yearly moment conditions to be the same, effectively reduces the instrument count and the number of moment conditions used in the Hansen J and makes the test more powerful.

The overall point of our Monte Carlo simulation is to illustrate one of the key caveats of using dynamic panel GMM estimation: even if we fail to reject the null of no second-order serial correlation or the null of valid instruments, it is possible that our estimates may be biased by unobservable time-varying heterogeneity. The weakness of the specification tests may be even worse in other aspects of corporate research than in the governance context we examine in this paper, and as our simulations suggest, the power of the tests is weaker in smaller samples. In other words, we need to give careful consideration to ensure, as much as possible, that observable control variables that may affect both the dependent and explanatory variables are included in our empirical specification. Our test of instrument validity cannot completely eliminate the possibility that we may have bias in our GMM estimates that arises from omitting an unobserved time-varying variable.

Next, we examine the relation between performance and board structure using actual data, and compare the results of OLS and fixed-effects estimates (used in prior studies) to those obtained with dynamic GMM estimation.

4. Data, sample selection and variables

In this section we describe the data for the empirical settings that we use to illustrate the impact of endogeneity in corporate finance: (1) the relation between board structure and firm performance and (2) the determinants of board structure.

4.1. Data and sample selection

Board structure is highly persistent. This can reduce the power of any panel data estimator (see, for example, Zhou, 2001). Dynamic estimation also requires that we assume transient errors are uncorrelated. To mitigate these concerns, we sample at two-year intervals instead of every year, using governance data from 1991, 1993, 1995, 1997, 1999, 2001, and 2003.[6]

We use the board data from Linck et al. (2008), which they collected from the Compact Disclosure database. Disclosure is a comprehensive database of over 7,000 firms starting in 1991. Since our empirical tests include a number of control variables, we match the Disclosure data with data from the Center for Research in Security Prices (CRSP) and Compustat, leaving a sample of more than 6,000 unique firms and over 20,000 firm-years. To our knowledge, this is the largest panel to date that has been used to study the performance/governance relationship. Table 2 reports summary statistics of our board and control variables. To avoid sample selection issues, we do not require a balanced panel; thus, the number of firms differs each year—the estimation strategy uses all available observations. The sample includes both large and small firms, unlike most previous studies that tend to focus on either large or small firms.

Table 3 shows the number of firms that experience changes in board size, independence, and CEO duality (whether or not the CEO is also the chair of the board) between 1991 and 2003. Within any two-year period, between 49% and 64% of our sample firms experience a change in the level of board independence (54%, on average). We also find that between 40% and 53% of firms change the size of their board over a two-year

[6] While somewhat arbitrary, sampling every three years is common in the board literature (e.g., Boone et al., 2007; Linck et al., 2008) as well other aspects of governance literature (e.g., Gompers, Ishii, and Metrick, 2003). There is a trade-off in our sampling interval choice. Sampling every three years reduces serial correlation even further than the two-year interval we employ and increases the time-series variation in board structure. However, it reduces the sample size and may reduce power. We replicate our analysis on data sampled every three years as well as sampled every year. While our conclusions remain unchanged, in the case of annual sampling we are unable to reject the null of no second-order serial correlation in post-estimation tests, which suggests that serial correlation may complicate inferences in that setting.

period, with an average of 50%. As may be expected, changes in CEO duality are less common than changes in board size or independence but are still significant, averaging about 13% a year. By the end of the sample period, about 70% of the firms have had at least one change in their level of board independence, 63% have had a change in board size, and 28% have had a change in whether or not the CEO is also the board chair. Overall, more than 70% of the firms in our sample experienced at least one change in board structure (size, independence, or duality) over the sample period. This frequency of change suggests that there is enough time-series variation in our key variables to effectively use panel data estimation techniques.

4.2. Measuring firm performance

The primary performance measure we use is return on assets (*ROA*), where *ROA* is defined as operating income before depreciation (Compustat item #13) divided by fiscal year end total assets (Compustat item #6). We also calculate industry-adjusted *ROA*, which is the firm's ROA less the industry median *ROA*, defining industry by the two-digit Standard Industrial Classification (SIC) code.

Many studies that examine the governance/performance relation use Tobin's *Q* as a measure of firm performance. This can be a problem for a number of reasons. Tobin's *Q* (usually defined as the market-to-book ratio) is a proxy for growth opportunities, and there is strong theoretical reason to expect that growth opportunities are a cause, rather than a consequence, of governance structures. Boone et al. (2007), Linck et al. (2008), and Lehn et al. (2009) provide empirical evidence to support this notion. Thus, we use market-to-book as a control variable rather than a performance measure. However, for robustness and for comparison with existing research, we estimate models using Tobin's *Q* as a performance measure. Further, we also replicate our results using return on sales (*ROS*) as a performance measure to assess whether our results are sensitive to the specific performance measures we select.

4.3. Governance variables

We consider the effect of past performance on three board structure variables: board size, board composition, and board leadership, which we define as follows:

- *LogBSIZE*, the logarithm of the number of directors on the board.
- *INDEP*, the proportion of outside (non-executive) directors on the board.
- *CEO_CHAIR*, a dummy variable equal to one if the CEO is also the chairman of the board, and zero otherwise.

4.4. Control variables

Recent studies, including those by Raheja (2005), Coles et al. (2008), Boone et al. (2007), and Linck et al. (2008), suggest that firms will choose their board structures based on the relative costs and benefits of each governance mechanism. The firm's chosen board structure will reflect the monitoring costs and private benefits of control the firm faces, as well as the scope and complexity of its operations. Thus, as suggested by prior research, we use size, age, the number of business segments, growth opportunities, and leverage as determinants of board structure. Specifically, we define our control variables as follows:

- $LogMVE$, logarithm of the market value of equity.
- MTB, ratio of market-to-book value. This is obtained as market value of equity *plus* book value of assets *minus* book value of equity *minus* deferred taxes, all *divided* by book value of assets.
- $RETSTD$, standard deviation of (the past 12 months) of the firm's stock returns.
- $LogAGE$, the logarithm of the firm's age, where age is computed from the time the firm first appears on CRSP.
- $LogSEGMENTS$, the logarithm of the number of business segments.
- $DEBT$, the ratio of the firm's long-term debt to total assets.

Since these variables might also be related to firm performance, they serve as control variables in our empirical specification of firm performance as well.

5. The relation between board structure and firm performance

In this section, we examine the empirical relation between board structure and firm performance using the dynamic model developed above. In Section 5.1, we determine how many lags of performance we need to ensure dynamic completeness. Section 5.2 presents direct empirical evidence of the dynamic relation between board structure and the firm's historical performance and characteristics. In Section 5.3, we estimate the relation between board structure and firm performance using the dynamic panel GMM estimator. We compare the results to estimates obtained from a static model in order to understand biases that arise from ignoring different aspects of endogeneity. Finally, in Section 5.4, we rigorously examine the validity of the instrument set that we use in the dynamic GMM estimation; i.e., we examine the strength and exogeneity of using the firm's history as instruments for current governance.

5.1. How many lags of performance are needed to ensure dynamic completeness?

Empirically, it is important to understand how many lags of performance we need to capture all information from the past. This is important for at least two reasons. First, failure to capture all influences of the past on the present could still mean that Eq. (2) is misspecified (i.e., there might be an omitted variable bias). Second, and perhaps more importantly, we argue that all older lags are exogenous with respect to the residuals of the present; thus, they can be used as instruments. This is important for consistent estimation using the dynamic panel GMM estimator.

Glen, Lee, and Singh (2001) and Gschwandtner (2005) suggest that two lags is sufficient to capture the persistence of profitability. Thus, we propose including two lags in our estimates of the performance/governance relation (i.e., we set $p = 4$ in Eq. (2) since our data are sampled every two years). To see if two lags are sufficient to ensure dynamic completeness, we estimate a regression of current performance on *four* lags of past performance, controlling for other firm-specific characteristics. Table 4 shows the results. We use two profitability measures: return on assets (*ROA*) and return on sales (*ROS*). Results suggest that including two lags is sufficient to capture the dynamic aspect of the governance/performance relation.[7] In columns 1 and 3, the first two lags are statistically significant while older lags are insignificant. In columns 2 and 4, we drop the recent lags and include only the older lags. In these specifications, the older lags are statistically significant. Thus, while the older lags include relevant information, that information is subsumed by the more recent lags.

5.2. How strongly is the present correlated with the past?

A central argument in our paper is that board structure (size and independence) and other firm-specific variables are related to past performance. We examine this assertion directly with a series of tests. Our first set of tests involve OLS regressions of (1) current *levels* of board size, independence, and other firm-specific variables and (2) *changes* in these levels on past performance and historical values of the firm-specific variables.

The results are shown in Table 5. In Panel A, we present results from OLS regressions of the *levels* of board structure and other firm characteristics on performance and characteristics from two years before. We find that board independence is significantly negatively related to past performance as shown by Hermalin and Weisbach (1998). We also find that board size is significantly positively related to past performance,

[7]Here again, we emphasize that the underlying assumption is that besides lags of performance and the other control variables, there are no other (unobserved) time-varying factors that affect current performance.

although the significance level drops when we control for past firm size. Further, current board size is significantly positively related to past firm size, and firm size is significantly related to past performance. The results suggest that firms that have done well in the past will be larger today and as a result will have bigger boards, as suggested by Fama and Jensen (1983) and documented by Boone et al. (2007), Coles et al. (2008), and Linck et al. (2008).[8]

Panel B of Table 5 shows the results from OLS regressions of *changes* in board structure and firm characteristics on the performance levels and characteristics from two years before. The results are similar to those obtained from using the levels as dependent variables. Changes in board independence are negatively related to past performance, while changes in board size are positively related to past performance. Again, we find that changes in board size in response to past performance are through the effect of performance on firm size.

Table 5 also shows that even the potential control variables are dynamically endogenous. Current levels and changes in market-to-book (MTB), standard deviation of stock returns ($RETSTD$), number of business segments ($LogSEGMENTS$), firm age ($LogAGE$), and leverage ($DEBT$) are all significantly related to past performance. This highlights the fact that it is not only corporate governance that can be considered endogeneous, but *all* the control variables that we may want to use as proxies for the firm's operating and contracting environment are likely to be endogenous as well.

We carry out a second test of strict exogeneity suggested by Wooldridge (2002, p. 285).[9] If $\mathbf{X}_{i,t}$ contains the explanatory governance and control variables, we can test for strict exogeneity by estimating the following fixed-effects model:

$$\mathbf{y}_{i,t} = \alpha + \beta \mathbf{X}_{i,t} + \Omega \mathbf{W}_{i,t+2} + \eta_i + \varepsilon_{it}, \qquad t = 1991, 1993, 1995, \ldots, 2001, \tag{17}$$

where $\mathbf{W}_{i,t+2}$ is a subset of future values of the corporate governance and control variables. Under the null hypothesis of strict exogeneity, $\Omega = 0$, i.e., future realizations of our governance and control variables are unrelated to current performance.

Table 6 shows the results of estimating (17), with different subsets of the governance and control vari-

[8]One possibility is that staggered boards may prevent firms from adjusting to changes in firm performance. To investigate this possibility we collected staggered board data from RiskMetrics. Unfortunately, we were only able to get staggered board data on less than 20% of the firm-years in our full data set. When we interact the staggered board dummy with past performance in regressions similar to those reported in Table 6, we find that staggered boards do not affect the relation between board structure and past firm performance. We thank the referee for pointing this out.

[9]As far as we know, this is the only explicit test of strict exogeneity that is described in the literature.

ables, $\mathbf{W}_{i,t+2}$. In every specification in which they are included, the coefficient estimates for the future values of both board size ($LogBSIZE_{t+2}$) and CEO as board chair (CEO_CHAIR_{t+2}) are significantly different from zero. This suggests that neither of these board variables are strictly exogenous and instead adjust in response to firm performance. In addition, the coefficient estimates on the future values of some control variables ($LogMVE_{t+2}$, $RETSTD_{t+2}$, and $DEBT_{t+2}$) are also significantly different from zero, suggesting that these variables also adjust to firm performance. An F-test of the joint significance of the coefficient estimates of all the future values is also significant.

Overall, the results from Table 6 suggest that neither the board structure nor the firm control variables are strictly exogenous, and confirms both our theoretical predictions and the results from the OLS regressions in Table 5.

5.3. The relation between board structure and current firm performance

In this section, we examine the results from estimating the relation between board structure and current firm performance. In order to compare to past research and highlight the potential problems from ignoring the dynamic relation between current board structure and the firm's history, we estimate the following models:

1. An OLS model
2. A fixed-effects model
3. A dynamic OLS model
4. A dynamic fixed-effects model (system GMM).

Table 7 reports the results when we use return on assets (*ROA*) as our performance measure. As we discussed earlier, we include two lags of performance in the dynamic model. This makes historical performance and historical firm characteristics, lagged three periods or more, available for use as instruments. We use variables lagged three and four periods ($t-6$ and $t-8$, respectively, since we sample at two-year intervals) as instruments for all the endogenous variables in the GMM estimates.[10] Our assumption in the GMM regression is that all the regressors except firm age and the year dummies are endogenous.

Static OLS and fixed-effects estimates suggest a *negative* relation between board size and firm performance. This finding is similar (in both direction and magnitude) to those obtained by a number of prior

[10] See Appendix B for further details of the system GMM estimation using xtabond2 in Stata 9. The large number of endogenous variables means that we have many instruments and could inadvertently overfit our endogenous variables. To reduce this possibility, we use the "collapse" option in xtabond2 which is explained further in the appendix and in Roodman (2009). However, as an additional robustness check, we conduct our analysis with all the instruments dated $t-6$ or later. The results are quantitatively and qualitatively unchanged.

studies including Yermack (1996), Eisenberg et al. (1998), and Bhagat and Black (2002). However, once we move to a dynamic model, these results disappear. In a simple dynamic OLS model, board size is no longer significantly related to firm performance. For example, the coefficient on board size is a significantly negative -0.0262 ($t = -5.67$) using a static OLS model, but is insignificant in the dynamic OLS model that includes lagged performance (-0.0033, $t = -0.73$). While the simple dynamic OLS model is an improvement over the static models, it is merely an intermediate step. One clear insight that emerges from the dynamic OLS model is the importance of lagged performance when assessing the effect of board structure on firm performance. Note that the R^2 improves from 27% in the static OLS model to 41% in the dynamic OLS model. Past performance appears to explain a significant portion of the variation in current performance. This difference is not only economically significant but a test based on Vuong (1989) suggests that the R^2s are statistically different from each other. In addition, the drop in the magnitude of the estimated coefficients on the board structure variable when we move from the static OLS model to the dynamic OLS model suggests that current board structure is correlated with past firm performance—another potential indication of the endogeneity that arises from the relation between board structure and firm performance. Nevertheless, it is possible that there is some unobservable heterogeneity that is not captured by past performance. The system GMM model enables us to estimate the governance/performance relation while including *both* past performance *and* fixed-effects to account for the dynamic aspects of the governance/performance relation and time-invariant unobservable heterogeneity, respectively.

The results show that when we include fixed-effects in a dynamic model and estimate via system GMM, the coefficient on board size is insignificant (0.0183, $t = 0.43$). This is in sharp contrast to the results from the static fixed-effects model in which the coefficient on board size is significantly negative (-0.0261, $t = -4.32$). However, the negative bias in the fixed-effects coefficient estimate is consistent with the bias we expect to have if we ignore dynamic relation between current board structure and past performance: if board size is positively related to past performance, then fixed-effects estimates of the relation between board size and firm performance will be negatively biased.

The static OLS estimate also suggests a negative relation between board independence and firm performance (-0.0266, $t = -3.56$), similar to that reported in a number of prior studies including Yermack (1996), Klein (1998), and Bhagat and Black (2002). Interestingly, when we estimate this in a static fixed-effects model, the sign flips to positive and significant (0.0202, $t = 2.48$). However, in both the dynamic OLS model and the dynamic GMM model, the relation between board independence and firm performance is insignificant (0.0061, $t = 0.82$ and -0.0109, $t = -0.14$, respectively).

The intuition behind the dramatic sign flip with respect to the effect of board independence on performance (which we illustrate with a general example in Section 3.1.2) is an interesting one and illustrates the bias that may arise from ignoring both unobservable heterogeneity and dynamic relation between board independence and past performance. As suggested by Hermalin and Weisbach (1998), managers that have a high level of ability are monitored less intently by shareholders and thus, have less independent boards. Of course, these are the firms that will have the best performance. This implies that an OLS regression that ignores the unobservable heterogeneity of managerial ability may find a negative relation between firm performance and board independence, which indeed is what our results in Table 7 suggest.

The intuition underlying the sign in the fixed-effects regression is somewhat more subtle but is explained in Roodman (2008).[11] First, note that including fixed-effects is equivalent to time-demeaning all our variables. Suppose two firms, A and B, have the same average performance over t periods (perhaps because their managers have similar abilities). Now, suppose that as of time $t-1$, firm A has performed slightly better than firm B (this may have been due to purely exogenous events). If shareholders use firm performance as a proxy for managerial ability and board independence is *negatively* related to past performance (as suggested by Hermalin and Weisbach, 1998)), then firm A will have a slightly less independent board than firm B, at time $t-1$. However, since both firms have the same average performance over the entire t periods, firm B will have better performance than firm A in period t. This would appear to be due to the fact that firm B has a more independent board than A, while it is in fact due to a mechanical mean reversion of the firms' performance. Thus, including fixed-effects in a regression of performance on board independence without controlling for the past performance would suggest a positive relationship between board independence and firm performance, which is what we observe in Table 7. This is the intuition underlying the sign flip with respect to the estimated effect of board independence on firm performance in the OLS and fixed-effects regressions in Table 7.

A similar but slightly more complicated argument can be made with respect to the estimated effect of board structure on firm performance. Board size is positively correlated with independence so that in a static OLS regression, the estimated effect of board size on firm performance is negative just as is the case with independence. However, unlike with board independence, board size tends to increase with firm size. Since firm size is positively related to past performance (as we show in Table 5), there is a *positive* relationship

[11] Roodman (2008) does this in the context of foreign aid and growth.

between current board size and past firm performance. Thus, including fixed-effects in a regression of performance on board size without controlling for the past performance would suggest a negative relationship between board size and firm performance, which is what we observe in Table 7.

In Table 7, we also report the results of the specification tests—the $AR(2)$ second-order serial correlation tests and the Hansen J test of over-identifying restrictions. The $AR(2)$ test yields a p-value of 0.87 which means that we cannot reject the null hypothesis of no second-order serial correlation. The results in Table 7 also reveal a J-statistic with a p-value of 0.41 and as such, we cannot reject the hypothesis that our instruments are valid.

In Table 7 we also report the results from a test of the exogeneity of a subset of our instruments. As discussed in Section 3.3, the system GMM estimator makes an additional exogeneity assumption: the assumption that any correlation between our endogenous variables and the unobserved (fixed) effect is constant over time (Eq. (10)). This is the assumption that enables us to include the levels equations in our GMM estimates and use lagged differences as instruments for these levels. Eichenbaum, Hansen, and Singleton (1988) suggest that this assumption can be tested directly using a difference-in-Hansen test of exogeneity. This test also yields a J-statistic which is distributed χ^2 under the null hypothesis that the subset of instruments that we use in the levels equations are exogenous. The results in Table 7 show a p-value of 0.23 for the J-statistic produced by the difference-in-Hansen test. This implies that we cannot reject the hypothesis that the additional subset of instruments used in the system GMM estimates is indeed exogenous.

In additional (untabulated) analysis, we carry out the dynamic GMM regression using the "forward" orthogonal deviations transformation, proposed by Arellano and Bover (1995). Instead of first-differencing, as in the regular system GMM estimator, it subtracts the average of all future available observations of a variable. This technique differs uniquely from system GMM in one aspect. In the cases where there are gaps in our panel, the orthogonal deviation is computable for *all* observations, which is not the case for first-differences. This increases efficiency without having to rely on the additional assumption (explicit in system GMM) of constant correlation between the endogenous variables and the firm fixed-effect. Our results using this technique are qualitatively and quantitatively similar to those reported in Table 7: we find no relation between firm performance and the different aspects of board structure.

5.4. Strength of instruments

A number of authors, including Bound, Jaeger, and Baker (1995), Staiger and Stock (1997), Stock and Yogo (2005), have shown that if the endogenous variables are only weakly correlated with the instruments, estimates from an IV regression could be biased. As far as we know, there is no single criteria for evaluating the joint strength of the instrument set of the dynamic panel system GMM estimator. However, Staiger and Stock (1997) and Stock and Yogo (2005) outline a process, and develop a set of critical values, for evaluating the strength or weakness of instruments in a standard two-stage least squares (2SLS) regression. We adapt these to assess the strength of the instruments we use in our GMM estimates. The process involves two tests. First we carry out a first-stage regression of our endogenous variables on the instruments and examine the F-statistics. Second, we compute a Cragg-Donald statistic, which may be more informative than the F-statistics from the first-stage regressions if we have more than one endogenous variable. We compare this to critical values for instrument weakness developed by Stock and Yogo (2005).

If y is performance and $\mathbf{X}$ includes all the regressors, system GMM involves estimating the following:

$$\begin{bmatrix} y_{it} \\ \Delta y_{it} \end{bmatrix} = \alpha + \beta \begin{bmatrix} \mathbf{X}_{it} \\ \Delta \mathbf{X}_{it} \end{bmatrix} + \varepsilon_{it}. \tag{18}$$

To assess the strength of our instruments, we split our system into its constituent levels and difference equations. We separately assess the strength of: (1) lag differences as instruments in the level equations and (2) lagged levels as instruments in the differenced equations. First, we examine the equations in levels:

$$y_{it} = \alpha_l + \beta_l \mathbf{X}_{it} + \nu_{it} \qquad \text{Instruments: } \Delta\mathbf{X}_{it-4}, \tag{19}$$

and the equation in differences:

$$\Delta y_{it} = \alpha_d + \beta_d \Delta\mathbf{X}_{it} + \varepsilon_{it} \qquad \text{Instruments: } \mathbf{X}_{it-6}. \tag{20}$$

Table 8 shows the results of our analysis. For the variables in levels, we obtain the F-statistics by regressing each variable on all the lagged differences used as instruments ($\Delta\mathbf{X}_{it-4}$). Similarly, for the variables in differences, we obtain the F-statistics by regressing each variable on all the lagged levels used as instruments ($\mathbf{X}_{it-6}$). To obtain the Cragg-Donald statistic, we carry out two separate two-stage least squares regressions, one each for the levels and differenced equations, respectively.[12] It is worth noting that for the system GMM regressions, these tests are merely indicative of the strength of the instruments since consistency of the GMM estimates relies on the *joint* estimation of both the levels and the difference equations.

[12] Using the ivreg2 module in Stata 9.

Table 8 shows that F-statistics for all the first-stage regressions are significant, which implies that the instruments provide significant explanatory power for the endogenous variables. With only one exception, the F-statistics are all bigger than 10.0, which is the "rule of thumb" critical value suggested by Staiger and Stock (1997) for assessing instrument strength.

Finally, we examine the Cragg-Donald statistics. For the levels equations, the Cragg-Donald statistic is 22.60. This value exceeds *all* the critical values from Table 5.1 of Stock and Yogo (2005), implying that any bias from using the instruments is less than 5% of the bias from an OLS regression, with a 5% level of significance. For the differenced equations, the Cragg-Donald statistic of 4.29 lies just below the critical value (4.37)[13], at which level we can be confident (with a 5% level of significance) that the bias from the two-stage least squares estimates is less than 30% of the bias from an OLS regression.

Overall, the results from our tests for the strength of our instruments leaves us confident that the results of our GMM estimates are not driven by weak instruments. However, these tests are based on using lags from periods $t-6$ and $t-8$ as instruments. There is a fundamental trade-off in the choice of lag-length from which to choose instruments. Since it takes several periods for board structure to adjust completely to past performance, our lag-length has to be long enough to ensure exogeneity while not so long as to drive weak instruments. Our initial choice of instruments from periods $t-6$ and $t-8$ is based on the empirical analysis from Table 4 that suggests we need two lags to make our model dynamically complete. In unreported results, we re-run the regression of performance on board structure and other controls using the dynamic GMM methodology on a model similar to that used in Table 7, but with instruments from period $t-4$, and separately, instruments from period $t-8$ or later. Our inference remains unchanged—we find no relation between board size or board independence and firm performance using either instrument set. However, it is worth noting that the instruments from period $t-4$ do not pass the Hansen over-identification tests and may not be completely exogenous. This supports our initial instrument choice from periods $t-6$ and $t-8$ and suggests that this is indeed the lag-length that best handles the trade-off between exogeneity and instrument strength.

5.5. Does board structure affect firm performance with a lag?

Our analysis thus far has focused on assessing the effect of current board structure on current board performance. However, it is possible that board structure in this period affects governance in the next

[13] This is the critical value when $K = 15$, which is the number of instruments in the equation.

period, i.e., board structure affects firm performance with a lag. Thus, we estimate an empirical model of the form:

$$y_{it} = \alpha + \kappa_1 y_{it-2} + \kappa_2 y_{it-4} + \beta \mathbf{X}_{it-2} + \gamma \mathbf{Z}_{it-2} + \eta_i + \varepsilon_{it}, \tag{21}$$

where $\mathbf{X}$ contains the board structure variables and $\mathbf{Z}$ contains the control variables.

Using lagged board variables in the regression does not eliminate either unobservable heterogeneity (since $\mathbf{X}_{it-2}$ is possibly still correlated with η_i), or the dynamic aspects of the board structure/performance relation, since values of board structure at time $t-2$ could have been determined by performance at periods before $t-2$. However, using lagged board structure as opposed to current board structure reduces the impact of simultaneity since past board structure and current performance are not determined in the same period. Thus, estimating the effect of lagged board structure on current performance enables us to do two things. First, it enables us to assess the effect of board structure on firm performance using a different set of assumptions from those in Table 7. Second, it allows us to apply an alternative dynamic panel estimator that does not rely on the instrument set that we used in the dynamic GMM (Table 7).

Table 9 shows the results of estimating the effect of current performance on lagged board structure. We show results obtained using OLS, the dynamic GMM panel estimator, and a bias-corrected fixed-effects estimator developed by Bruno (2005). As we have discussed extensively in this paper, traditional fixed-effects estimates are biased because they fail to account for the effect of firm performance on current board structure. The bias-corrected fixed-effects estimator uses a numerical procedure to estimate this bias and uses it to compute the "bias-corrected" coefficient estimates. However, the bias-corrected estimates are only consistent if there is no simultaneity between performance and board structure (or the control variables), i.e., if $E(\varepsilon_{it}|\mathbf{X}_{it-2}, \mathbf{Z}_{it-2}) = 0$ in Eq. (20). This means that while we may not be able to apply the bias-corrected fixed-effects estimator when estimating the effect of current board structure on current performance, we can apply it when estimating the effect of lagged board structure on firm performance.

The results show that regardless of estimation methodology, there is no relation between lagged board structure and firm performance. In particular, the bias-corrected fixed-effects estimates suggest that even in a framework in which we are able to account for the dynamic aspects of the board structure/performance relation and time-invariant unobservable heterogeneity without invoking the instrumental variable procedure of the system GMM methodology, our inference of the effect of board structure on firm performance remains unchanged.

6. The determinants of board structure in a dynamic framework

Our analysis thus far has focused on identifying the effect of board structure on firm performance. The analysis itself assumes that the firm characteristics that we have identified as proxies for the firm's operating and contracting environment (size, growth opportunities, risk, age, and leverage) are actual determinants of board structure. In other words, we have assumed that the *exogenous* components of these characteristics have a causal effect on board structure. While there is strong empirical evidence in the literature suggesting that this is the case (e.g., Boone et al., 2007; Linck et al., 2008; Lehn et al., 2009), not all of these studies control for all the major sources of endogenity in the board/structure performance relation that we have identified here: simultaneity, unobservable heterogeneity, and the possibility that current firm characteristics may be related to past governance structure.

In this section we examine whether firm characteristics are determinants of board structure using a dynamic model and applying the dynamic GMM panel estimator. We estimate an empirical model of the form:

$$x_{it} = \alpha + \sum_{s} \kappa_s x_{it-s} + \gamma \mathbf{Z}_{it} + \eta_i + \varepsilon_{it} \quad s = 1, \ldots, p, \tag{22}$$

where x is either board size or independence and $\mathbf{Z}_{it}$ is a vector of firm characteristics that includes firm performance. Table 10 shows the the results and compares the results obtained from the dynamic panel GMM estimator with those obtained using OLS.[14]

The GMM results show that even after controlling for simultaneity, time-invariant unobservable heterogeneity, and the potential effect of past governance on current firm characteristics, firm size, growth opportunities, age, and leverage are determinants of board structure—results similar to those obtained from OLS estimates of a static model and also similar to those obtained in recent studies such as those by Boone et al. (2007) and Linck et al. (2008).[15] However, this application also demonstrates the importance of controlling for both the dynamic relation between current governance and past firm performance and time-invariant unobservable heterogeneity in the analysis. For example, the estimated magnitude of the effect of firm size on board size (independence) from GMM regressions, while still significant, is 66% (58%) smaller than the estimated magnitude of the effect from OLS regressions. Similarly, the estimated magnitude of the effect of firm age on board size (independence) from GMM regressions, while also still significant, is 65% (61%)

[14]Note that here, we are not regressing board structure (x) on firm performance (y); we are carrying out a regression of board structure (x) on firm characteristics $\mathbf{Z}$, while controlling for lagged board structure x_{t-s}.

[15]Linck et al. (2008) report that their results are robust to the use of the dynamic panel GMM estimator.

smaller than the estimated magnitude of the effect from OLS regressions. This suggests that even in this context, OLS estimates may be biased upwards because of the combination of unobservable heterogeneity and the endogeneity arising from the effect of past governance on current firm characteristics.

One fact that emerges from this analysis is that when we examine the determinants of board structure, our overall inference is unchanged when we move from OLS estimation of a static model to estimation using the dynamic GMM panel estimator. This is in sharp contrast to what we found in our earlier analysis of the effect of board structure on performance where our inference changes significantly when we account for the effect of past performance on current board structure. This difference may provide some insight as to what aspects of empirical corporate finance analysis may be the most susceptible to biases arising from ignoring the combination of unobservable heterogeneity and the dependence of present corporate finance variables on the past, and correspondingly, where analysis using dynamic panel estimation may be most important. If we are interested in the effect of governance on performance (a "performance on structure" regression), endogeneity arising from dynamic relationships will be especially important since there is a strong relation between past values of the dependent variable (performance), and current values of the explanatory variables (governance or firm characteristics).

On the other hand, if we are interested in the effect of firm characteristics on governance (a "structure on structure" regression), then the relation between present values of the explanatory variables and past realizations of the dependent variable may be less important. The explanatory variables (size, business segments, etc.) are not strongly determined by past values of the dependent variable (governance); any link from past governance to current firm characteristics will be indirect through the effect, if any, of governance on performance. While there is no doubt that a strong relation exists between past characteristics (such as size or number of segments) and current board structure, the argument for the reverse is much weaker. Firms are not bigger today nor do they operate in more business segments merely because they had more board members in the past. Thus, when we measure the effects of firm characteristics on board structure, we should draw similar inferences from either OLS or dynamic GMM estimates, which is what the results in Table 10 suggest.

7. Conclusion

It is well known that theoretical and empirical research in corporate finance is complicated by the endogenous relation that exists between the control forces operating on a firm and its decisions. Jensen (1993) broadly classifies these control forces (i.e., governance in a broad sense) as capital markets, the regulatory system, product and factor markets, and internal governance. In much of the extant corporate finance research, researchers attempt to either explain the causes or examine the effects of corporate finance decisions as related to one or more of these control forces. Empirical research often involves determining the causal effect, if any, of a firm characteristic (X) on some measure of firm profits or value (Y). This is usually done using the inference from a regression of Y on X along with several control variables (Z). The question is often framed as: holding Z constant, does X have an economically and statistically significant causal effect on Y?

To date, most empirical research in corporate finance has explicitly recognized at least two sources of endogeneity that may bias estimates of how X affects Y: *unobservable heterogeneity* (which arises if there are unobservable factors that affect both the dependent and explanatory variables) and *simultaneity* (which arises if the independent variables are a function of the dependent variable or expected values of the dependent variable). However, we argue that empirical research often overlooks an important source of endogeneity that arises because the relations among a firm's observable characteristics are likely to be dynamic. That is, a firm's current actions will affect its control environment and future performance, which will in turn affect its future actions. For example, in the context of board structure, current firm performance will affect future governance choices and these may, in turn, affect future firm performance. We note our model fits well with the theoretical model developed by Harris and Raviv (2008), who show that board structure is not exogeneous and not a determinant of performance, but both are functions of other variables. They suggest that finding a relation between board structure and performance may simply be spurious.

As in Himmelberg, Hubbard, and Palia (1999), we do not intend to minimize or ignore the importance of agency conflicts or suggest that governance is irrelevant; rather, we argue that the cross-sectional variation in observed governance structures is driven by both unobservable heterogeneity and the firm's history. As such, any attempt to explain the determinants of governance or its effect on performance that does not recognize these sources of endogeneity may be biased.

We first discuss the theory behind the GMM estimator and explain why it is appropriate for estimating the governance/performance relation in a dynamic framework. We show the advantage over fixed-effects estimators which are biased when the dynamic relation between the variable of interest and the explanatory variables is important. We specifically apply this technique to estimate the determinants of board structure and the effect of board structure on performance in a panel of 6000 firms from 1991–2003. We find that board structure is, in part, determined by past performance, and after accounting for this, we find no causal relation between board size or independence, and firm performance. We show that bias may explain the results of earlier studies that do not consider dynamics when estimating the board structure-performance relationship. We find that the broad conclusions of existing research that examine the relation between firm characteristics and board structure are relatively unaffected even after we account for any potential effects of past governance on current values of the determinants. This suggests that dynamics are less important in this setting.

While our research concentrates on board structure and performance, others have applied dynamic panel GMM estimators in other areas (e.g., financial development and growth literature, determinants of capital structure, etc.). However, it is likely to be particularly important in corporate governance since much of this research seeks to determine the effect of governance on performance, an aspect of research that is particularly susceptible to biases that may arise by ignoring the effect of historical performance on current governance.

Appendix A Dynamic panel estimation with GMM

The following discussion draws substantially from Bond (2002) and Roodman (2009).

Consider the dynamic unobserved effects model of Eq. (2):

$$y_t = y_{i,t-1} + \beta \mathbf{x}_{it} + \gamma \mathbf{z}_{it} + \eta_i + \varepsilon_{it}. \tag{23}$$

A first-difference transformation eliminates the unobserved effects and gives:

$$\Delta y_{it} = \Delta \mathbf{X}_{it} \beta + \Delta \varepsilon_{it}, \tag{24}$$

where $\mathbf{X}_t$ is a $T-1 \times K$ vector defined as $(y_{i,t-1}, \mathbf{x}_{it}, \mathbf{z}_{it})$ and Δ is the first-difference operator.

Under the assumption of *sequential exogeneity*:

$$E(\varepsilon_{it} | \mathbf{X}_{i,t-1}, \mathbf{X}_{i,t-2}, \ldots, \mathbf{X}_{i1}) = 0. \tag{25}$$

Sequential exogeneity implies that current shocks are independent of past values of the dependent variable, but leaves open the possibility that current and future values of the dependent variable might adjust to current shocks (*simultaneity* and *dynamic endogenity*, respectively).

The sequential exogeneity assumption suggests the following set of orthogonality conditions for Eq. (25):

$$E(\mathbf{X}'_{is} \Delta \varepsilon_{it}) = 0, \qquad s = 1, \ldots, t-2. \tag{26}$$

Arellano and Bond (1991) suggest that we can use these orthogonality conditions to obtain a GMM estimate of β. If we define a matrix of instruments, $\mathbf{Z}_i$:

$$\begin{pmatrix} \mathbf{X}_{i1} & 0 & 0 & \cdots & 0 & \cdots & 0 & 0 \\ 0 & \mathbf{X}_{i2} & \mathbf{X}_{i1} & \cdots & 0 & \cdots & 0 & 0 \\ \vdots & \vdots & \vdots & \ddots & \vdots & & \vdots & \vdots \\ 0 & 0 & 0 & \cdots & \mathbf{X}_{iT-2} & \cdots & \mathbf{X}_{i2} & \mathbf{X}_{i1} \end{pmatrix} \text{or collapsed,} \begin{pmatrix} \mathbf{X}_{i1} & 0 & \cdots & 0 \\ \mathbf{X}_{i2} & \mathbf{X}_{i1} & \cdots & 0 \\ \vdots & \ddots & & \vdots \\ \mathbf{X}_{iT-2} & \cdots & \mathbf{X}_{i2} & \mathbf{X}_{i1} \end{pmatrix}, \tag{27}$$

we can do GMM based on:

$$E(\mathbf{Z}'_i \Delta \varepsilon_i) = 0. \tag{28}$$

Eq. (28) means that we can use the following instruments for each first-differenced equation:

Equation	Instruments
$\Delta y_{i3} = \Delta \mathbf{X}_{i3}\beta + \Delta\varepsilon_{i3}$	$\mathbf{X}_{i1}$
$\Delta y_{i4} = \Delta \mathbf{X}_{i4}\beta + \Delta\varepsilon_{i4}$	$\mathbf{X}_{i1}, \mathbf{X}_{i2}$
$\vdots$	$\vdots$
$\Delta y_{iT} = \Delta \mathbf{X}_{iT}\beta + \Delta\varepsilon_{iT}$	$\mathbf{X}_{i1}, \mathbf{X}_{i2}, \dots, \mathbf{X}_{iT-2}$

The asymptotically efficient GMM estimator based on the moment conditions in (28) minimizes the criterion:

$$\left[\mathbf{Z}_i'(\Delta\mathbf{y}_i - \Delta\mathbf{X}_i)\right]'\hat{\mathbf{W}}\left[\mathbf{Z}_i'(\Delta\mathbf{y}_i - \Delta\mathbf{X}_i)\right]. \tag{29}$$

The GMM estimator that minimizes this criterion is obtained as:

$$\hat{\beta}_{GMM} = \left[\left(\sum_i \Delta\mathbf{X}_i'\mathbf{Z}_i\right)\hat{\mathbf{W}}\left(\sum_i \Delta\mathbf{Z}_i'\mathbf{X}_i\right)\right]^{-1}\left(\sum_i \Delta\mathbf{X}_i'\mathbf{Z}_i\right)\hat{\mathbf{W}}\left(\sum_i \Delta\mathbf{Z}_i'\mathbf{y}_i\right), \tag{30}$$

where the optimal weighting matrix, $W = \Lambda^{-1}$, and $\Lambda = E(\mathbf{Z}_i'\Delta\varepsilon_i\Delta\varepsilon_i'\mathbf{Z}_i)$.

The GMM estimator described above is known as the "difference" GMM estimator. Arellano and Bover (1995) and Blundell and Bond (1998) suggest that we can improve on the "difference" GMM estimator using the "system" GMM estimator (see Section 3.3 for a discussion of the shortcomings of the "difference" estimator). The "system" estimator requires carrying out GMM estimation using a "stacked" system consisting of both first-differenced and level equations.

The "system" GMM estimator does not directly eliminate the unobserved effect, but if we assume that the correlation between the unobserved effect and our explanatory variables is constant over the time period of our data set, we have the following additional set of orthogonality conditions:

$$E(\Delta\mathbf{X}_{it}\eta_i) = 0, \qquad t = 2, \dots, T. \tag{31}$$

We can use (32) to define a matrix of instruments for our level equations as follows:

$$\begin{pmatrix} \Delta\mathbf{X}_{i1} & 0 & \cdots & 0 \\ 0 & \Delta\mathbf{X}_{i2} & \cdots & 0 \\ \vdots & \vdots & \cdots & \vdots \\ 0 & 0 & \cdots & \Delta\mathbf{X}_{iT-2} \end{pmatrix} \text{or collapsed,} \begin{pmatrix} \Delta\mathbf{X}_{i1} \\ \Delta\mathbf{X}_{i2} \\ \vdots \\ \Delta\mathbf{X}_{iT-2} \end{pmatrix}. \tag{32}$$

Eq. (32) means that we can use the following instruments for each level equation:

Equation	Instruments
$y_{i3} = \mathbf{X}_{i3}\beta + \varepsilon_{i3}$	$\Delta\mathbf{X}_{i1}$
$y_{i4} = \mathbf{X}_{i4}\beta + \varepsilon_{i4}$	$\Delta\mathbf{X}_{i2}$
$\vdots$	$\vdots$
$y_{iT} = \mathbf{X}_{iT}\beta + \varepsilon_{iT}$	$\Delta\mathbf{X}_{iT-2}$

Appendix B Implementing dynamic GMM estimation in Stata (Version 9)

Dynamic GMM estimation can be implemented in Stata using the `xtabond2` command. The following example illustrates the use of the `xtabond2` command. As is the case with other panel data estimators in Stata, `xtabond2` requires you to specify that your data are a panel by using the `tsset` command. See Roodman (2009) for comprehensive details of using `xtabond2`, the full range of options available, and specifications tests.

Assume the data set consists of a dependent variable, `y`, and two explanatory variables, `x1` and `x2`. One can obatin a "system" GMM estimate of the effects of `x1` and `x2` on `y` as follows:

```
xtabond2 y l.y x1 x2, gmm(y x1 x2, lag(a b)) <(options)>
```

The lagged dependent variable (`l.y`) is included as an explanatory variable as specified in (23). The `gmm` command invokes our lagged instrument set. `lag(a b)` indicates what lags we wish to include as instruments; `a` indicates the most recent lag we should use while `b` represents the most distant lag. If we think `x1` and `x2` are merely predetermined, then we can set `a` as 1. However, if we assume that `x1` and `x2` are endogenous, then we can set `a` as 2 or greater. If we wish to use all the lags greater than `a`, then we can write our `xtabond2` command as:

```
xtabond2 y l.y x1 x2, gmm(y x1 x2, lag(a .)) <(options)>
```

This command essentially invokes the instrument set described by (27) and (32) above.

If we are willing to assume that we have a *strictly exogenous* variable (say, `z`), `xtabond2` allows us to partition our dependent variables (in the spirit of Hausman and Taylor, 1981) into endogenous and exogenous variables, using '`gmmstyle`' and '`ivstyle`' commands:

```
xtabond2 y l.y x1 x2, gmm(y x1 x2, lag(a .)) iv(z) <(options)>
```

Based on the preceding discussion, we obtained the GMM results presented in Table 7, using the following code in Stata (Version 9):

```
xi: xtabond2 roa l.roa l2.roa logbsize indep ceo_chair logmve mtb retstd logsegments
logage debt i.year, gmm(roa logbsize indep ceo_chair logmve mtb retstd
logsegments debt, lag(3 4) collapse) iv(i.year logage) twostep robust small
```

The Stata command incorporates our assumption that only firm age and the year dummies are exogenous. Since our data are sampled every two years, "`lag(3 4)`" invokes instruments from $t-6$ and $t-8$, respectively. We use the "`collapse`" to avoid instrument proliferation and obtain the instrument set specified in Eqs. (27) and (32).

Appendix C Data generation for Monte Carlo analysis of system GMM specification tests

In this section, we outline how we generate the data for the Monte Carlo analysis in Section 3.3. We calibrate the Monte Carlo using three variables from our data set: industry-adjusted *ROA* (y), log of board size (x), and log of market value of equity (z).

Initial (time $t=0$) values of x, y, and z are selected from normal distributions, $N(\mu,\sigma)$, with means (μ), standard deviations (σ), and correlations chosen to approximate that of our sample data. Thus, $x_{io} \sim N(2,0.34)$, $y_{io} \sim N(0,0.13)$, and $z_{io} \sim N(19,2.1)$, $corr(x_{i0},y_{i0}) = 0.17$, $corr(x_{i0},z_{i0}) = 0.55$, and $corr(y_{i0},z_{i0}) = 0.33$.

Subsequent values of x are obtained as:

$$x_{i,t} = 0.4 + 0.8x_{it-1} + \kappa r_{it} + \varepsilon^x_{it} \qquad t > 0, \tag{33}$$

while those for z are obtained as

$$z_{i,t} = 3.8 + 0.8z_{it-1} + \varepsilon^z_{it} \qquad t > 0, \tag{34}$$

where ε^z_{it} and ε^x_{it} are drawn from normal distribution to ensure that they approximate the distribution from $t = 0$, and the degree of serial correlation (0.8) is chosen to approximate that found in the data.

Subsequent values of y are generated as

$$y_{it} = 0.06 + 0.5y_{it-1} + 0.01x_{it} + 0.05z_{it} + \kappa r_{it} + \varepsilon^y_{it}, \tag{35}$$

where the coefficients are chosen to be similar to that obtained from a simple OLS regression of *ROA* on lagged *ROA*, log of board size, and log of market value of equity from our data set; ε_{it}^{y} is chosen from a normal distribution to approximate the distribution specified at time $t = 0$.

Finally, we introduce "misspecification" into the data through the variable, r and by varying κ from -0.12 to +0.12. r is selected from a normal distribution with mean zero and a standard deviation of 2 (which is similar to the standard deviation of the log of market value of equity, z), and has a serial correlation of 0.8.

All the data generation and regressions (using `xtabond2`) are carried out in Stata, and we initialize our random generator by setting seed "12345."

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Fig. 1. How powerful are specification tests in detecting mis-specification? Figs. 1(a) and 1(b) show the bias in system GMM regressions if there is an additional unobserved variable that is time-varying, as well as the percentage of specification tests ($AR(2)$ and Hansen-*J* tests) rejected in sample sizes of 1,000 and 2,000, respectively. Fig. 1 (c) plots the power of the Hansen-*J* specification tests against the magnitude of the bias. The model estimated using system GMM (via xtabond2 in Stata)is $y_{it} = \beta x_{it} + \gamma z_{it} + \varepsilon_{it}^{y}$. "Misspecification" is introduced via an unobservable variable, *r*, which is correlated with both *y* and *x* through κ. In Figs. 1(a) and 1(b), the instrument set is "collapsed" effectively constraining yearly moment conditions to be the same (further details of this option can be found in Appendix A). The true data-generating process for *y* is of the form: $y_{it} = \beta x_{it} + \gamma z_{it} + \kappa r_{it} + \eta_i + \varepsilon_{it}^{y}$; for *x* it is $x_{it} = \rho x_{it-1} + \kappa r_{it} + \delta \eta_i + \varepsilon_{it}^{x}$; for *z* it is $z_{it} = \alpha z_{it-1} + \varepsilon_{it}^{z}$; and that for $r_{it} = \theta r_{it-1} + \varepsilon_{it}^{r}$. In all the panels, $Bias = (\hat{\beta} - \beta)/\beta$, where the "true" $\beta = 0.01$ and $\hat{\beta}$ is the estimated effect of *x* on *y*. Further details of the simulation are given in Appendix C

Fig. 1(a): Sample size = 1,000; Collapse option

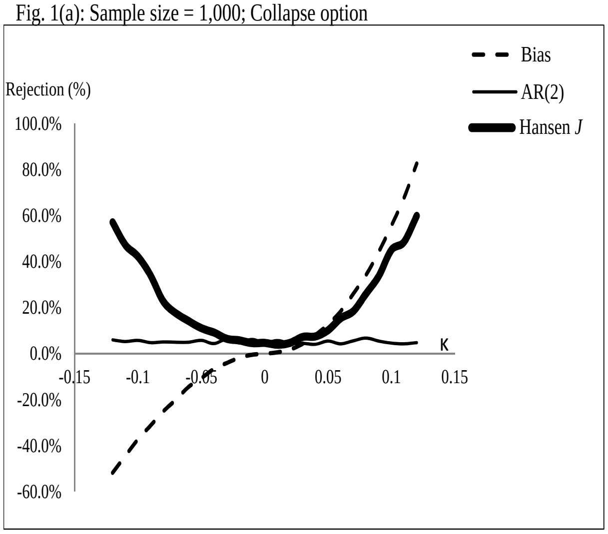

Fig. 1(b): Sample size = 2,000; Collapse option

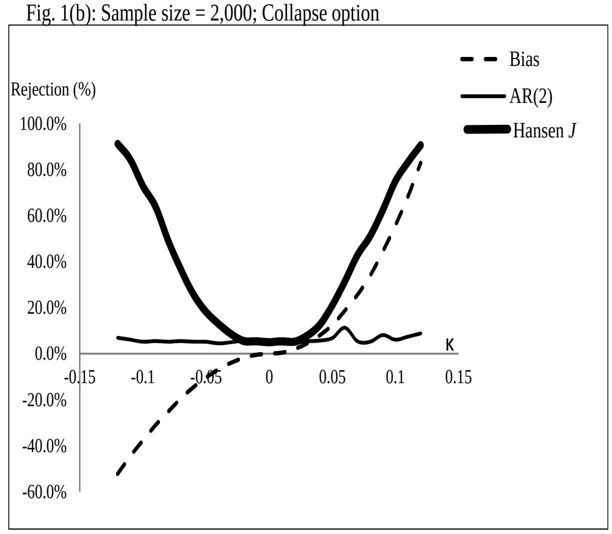

Fig. 1(c): Bias and Hansen *J* rejection probabilities

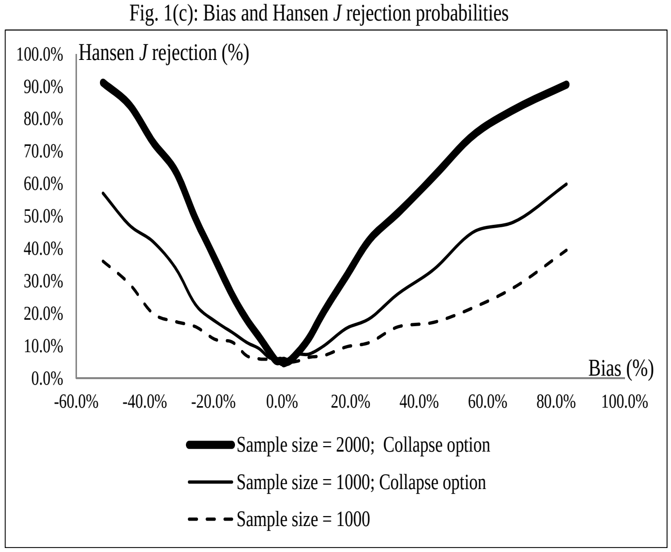

Table 1

Prior empirical analysis of the relationship between board structure and firm performance

Paper	Sample	Period	Performance measure	Methodology	Relationship
Panel A: Papers examining relationship between board independence and firm performance					
Hermalin and Weisbach (1991)	134	1971–1983	*Q*, ROA	OLS, 2SLS (Instruments: lagged value of management ownership)	None
Mehran (1995)	153	1979–1980	*Q*, ROA	OLS	None
Agrawal and Knoeber (1996)	800	1988	*Q*	2SLS (Instruments: Assets, regulatory dummy, founder dummy)	Negative
Yermack (1996)	452	1984–1991	*Q*, ROA	OLS, Fixed-effects (FE)	OLS: Negative FE: Positive
Klein (1998)	486	1992–1993	ROA, Jensen productivity measure, Market returns	OLS	Negative
Bhagat and Black (2002)	934	1988–1991	*Q*, ROA, ROS, Market returns	OLS, 2SLS	Negative
Coles, Daniel, and Naveen (2008)	8,165	1992–2001	*Q*	OLS, 3SLS	Negative for high Research and Development (R&D) firms
Panel B: Papers examining relationship between board size and firm performance					
Yermack (1996)	452	1984–1991	*Q*, ROA	OLS, FE	OLS: Negative FE: Negative
Eisenberg, Sundgren, and Wells (1998)	879	1992–1994	ROA	2SLS (Instruments: firm age, membership in group)	Negative
Bhagat and Black (2002)	934	1988–1991	*Q*, ROA, ROS, Market returns	OLS, 2SLS	Negative (None in some specifications)
Coles, Daniel, and Naveen (2008)	8,165	1992–2001	*Q*	OLS, 3SLS	Positive for large diversified firms

Table 2

Summary statistics of board and firm characteristics

The table contains the sample characteristics of the board and firm characteristics of the firms used in the study. The results are based on a sample of 6,034 firms and 20,003 firm years selected every other year (1991, 1993, 1995, 1997, 1999, 2001, and 2003). The board variable data come from the Compact Disclosure database. The firm characteristics come from CRSP and Compustat. Board size is the total number of directors on the board. CEO_Chair is one if the CEO is also the chairman of the board, zero otherwise. Board independence is the percentage of directors who are not employees of the firm. Firm size is the market value of equity. Segments is the number of business segments the firm operates in, as reported by Compustat. Firm age is computed based on the year the firm first appears on CRSP. Debt is the ratio of long-term debt to total assets. RETSTD is the standard deviation of the firm's stock returns in the previous 12 months. Market-to-book is obtained as the value of equity plus book value of assets minus book value of equity minus deferred taxes, all divided by book value of assets. Median values are shown in parentheses; standard deviations are shown in brackets.

Panel A: Mean (median) [standard deviation] of board variables							
	1991	1993	1995	1997	1999	2001	2003
Board size	7.79	7.59	7.39	7.49	7.37	7.59	7.93
	(7.00)	(7.00)	(7.00)	(7.00)	(7.00)	(7.00)	(8.00)
	[2.94]	[2.77]	[2.66]	[2.63]	[2.43]	[2.34]	[2.30]
CEO_Chair	0.59	0.60	0.59	0.59	0.59	0.59	0.56
	(1.00)	(1.00)	(1.00)	(1.00)	(1.00)	(1.00)	(1.00)
	[0.49]	[0.49]	[0.49]	[0.49]	[0.49]	[0.49]	[0.49]
Board independence	0.63	0.64	0.64	0.67	0.67	0.67	0.71
	(0.66)	(0.67)	(0.66)	(0.70)	(0.69)	(0.70)	(0.71)
	[0.18]	[0.18]	[0.19]	[0.18]	[0.17]	[0.15]	[0.14]
Panel B: Mean (median) [standard deviation] of firm characteristics							
	1991	1993	1995	1997	1999	2001	2003
Firm size (millions)	$1,250	$1,240	$1,430	$2,060	$3,130	$2,610	$3,000
	(100)	(114)	(131)	(186)	(186)	(264)	(358)
	[5,380]	[5,150]	[6,770]	[10,040]	[21,200]	[16,000]	[15,700]
Segments	1.60	1.53	1.46	1.49	2.36	2.50	2.35
	(1.00)	(1.00)	(1.00)	(1.00)	(1.00)	(1.00)	(1.00)
	[1.09]	[1.03]	[0.95]	[1.13]	[1.84]	[1.99]	[1.79]
Firm age	15.04	14.42	13.53	13.42	13.38	14.18	15.95
	(10.00)	(10.00)	(9.00)	(8.00)	(8.00)	(9.00)	(10.00)
	[14.46]	[14.36]	[14.52]	[14.66]	[14.18]	[14.47]	[14.61]
Debt	0.17	0.15	0.16	0.16	0.17	0.16	0.15
	(0.12)	(0.10)	(0.11)	(0.11)	(0.11)	(0.09)	(0.09)
	[0.16]	[0.15]	[0.16]	[0.16]	[0.17]	[0.21]	[0.15]
RETSTD	14.29%	14.62%	12.13%	14.30%	17.87%	21.62%	17.64%
	(12.68)	(12.36)	(10.77)	(12.48)	(15.48)	(18.41)	(15.13)
	[7.96]	[9.99]	[6.84]	[8.74]	[11.79]	[13.37]	[10.93]
Market-to-book	1.92	2.07	1.93	2.11	2.49	1.94	2.15
	(1.24)	(1.49)	(1.50)	(1.58)	(1.32)	(1.38)	(1.63)
	[2.88]	[1.93]	[2.14]	[1.77]	[4.19]	[1.88]	[1.73]
Return on assets	0.11	0.11	0.11	0.11	0.09	0.06	0.08
	(0.12)	(0.12)	(0.13)	(0.13)	(0.11)	(0.09)	(0.10)
	[0.14]	[0.13]	[0.13]	[0.14]	[0.14]	[0.15]	[0.13]
Number of observations	2,492	2,913	3,025	3,261	3,160	2,754	2,398

Table 3
Summary statistics of changes in board structure variables

This table contains the summary statistics of changes in board size and board independence over any two-year period between 1991 and 2003. The results are based on a sample of 6,034 firms and 20,003 firm years selected every other year (1991, 1993, 1995, 1997, 1999, 2001, and 2003). The board variable data come from the Compact Disclosure database. Board size is the total number of directors on the board. Fraction of outsiders (Board independence) is the percentage of directors who are not employees of the firm. CEO chair is whether or not the CEO is also the chair of the board

	1993	1995	1997	1999	2001	2003	1993–2003	1993–2003
				(percent of firms)				(percent of firm-years)
Change in fraction of outsiders	51.25%	50.48%	49.21%	52.53%	57.73%	63.64%	69.19%	53.68%
Change in board size	39.75%	39.60%	39.65%	42.25%	47.93%	53.17%	63.08%	49.54%
Change in whether or not CEO is chair	12.53%	12.73%	13.19%	14.02%	14.34%	14.14%	28.80%	13.46%
Change in ANY board structure variable	54.27%	53.72%	51.79%	55.35%	60.71%	66.22%	70.61%	56.55%
Number of firms (firm-years)	2,913	3,025	3,261	3,160	2,754	2,398	6,034	(17,511)

Table 4

How many lags of firm performance are significant?

In this table, we report results from the OLS estimation of the model:

$$y_{it} = \alpha_1 + \sum_{p=2}^{p=8} \kappa_p y_{it-p} + \kappa \mathbf{Z}_{it} + \eta_i + \varepsilon_{it}, \qquad t = 1999, 2001, 2003.$$

y_{it} is *ROA* or *ROS*. $\mathbf{Z}_{it}$ includes firm size (*LogMVE*), market-to-book ratio (*MTB*), standard deviation of stock returns (*RETSTD*), number of business segments (*LogSEGMENTS*), firm age (*LogAGE*), and leverage (*DEBT*). The results are based on a sample of 6,034 firms and 20,003 firm years selected every other year (1991, 1993, 1995, 1997, 1999, 2001, and 2003). The firm characteristics come from CRSP and Compustat. t-statistics are reported in parentheses. All t-statistics are based on robust, firm-clustered standard errors. a, b, c represent significance at the 1%, 5% and 10% level, respectively. Year dummies are included in all specifications.

Dependent variable	Performance *ROA*	Performance *ROA*	Performance *ROS*	Performance *ROS*
Performance$(t-2)$	0.4823^a		0.5905^a	
	(16.34)		(10.68)	
Performance$(t-4)$	0.0766^b		0.0183	
	(2.27)		(0.36)	
Performance$(t-6)$	0.0255	0.2450^a	0.0701	0.2681^a
	(0.72)	(5.97)	(5.58)	(5.58)
Performance$(t-8)$	0.0400	0.1080^a	0.0566	0.1470^a
	(1.61)	(3.38)	(1.40)	(3.24)
LogMVE	0.0036^a	0.0079^a	0.0045^a	0.0137^a
	(3.37)	(5.79)	(3.22)	(7.45)
MTB	0.0120^a	0.0141^a	0.0124^a	0.0126^a
	(5.71)	(5.39)	(6.11)	(4.99)
RETSTD	-0.0991^a	-0.1711^a	-0.1109^a	-0.1943^a
	(−3.42)	(−5.07)	(−3.28)	(−4.63)
LogSEGMENTS	0.0024	0.0018	0.0018	−0.0021
	(1.10)	(0.62)	(0.67)	(−0.56)
LogAGE	−0.0043	-0.0081^b	−0.0016	-0.0108^b
	(−1.35)	(−2.10)	(−0.41)	(−2.20)
DEBT	0.0166	0.0019	0.0311^b	0.0356^b
	(1.23)	(0.12)	(2.28)	(2.17)
R^2	0.47	0.27	0.51	0.30

Table 5

Relationship between board structure, firm-specific variables, and past performance

In this table we report the results of OLS regressions of current board size (*LogBSIZE*), independence (*INDEP*), and current firm-specific variables, on past performance and historic values of the firm-specific variables. Performance is measured by return on assets (*ROA*). The firm-specific variables include firm size (*LogMVE*), market-to-book ratio (*MTB*), standard deviation of stock returns (*RETSTD*), number of business segments (*LogSEGMENTS*), firm age (*LogAGE*), and leverage (*DEBT*). The results are based on a sample of 6,034 firms and 20,003 firm years selected every other year (1991, 1993, 1995, 1997, 1999, 2001, and 2003). The board variable data come from the Compact Disclosure database. The firm characteristics come from CRSP and Compustat. Panel A reports the results of the regressions in which the dependent variables are current levels. Panel B reports the results of the regressions in which the dependent variable is the change from $t-1$ to t. All t-statistics (in parentheses) are based on robust standard errors. Year dummies are included in all specifications. Items in **boldface** are significant at the 10% level or higher.

Panel A: Dependent variable is level at time t

	Indep	LogBsize	LogBsize	LogMVE	MTB	Retstd	LogSeg	Debt
ROA($t-2$)	**−0.0185**	**0.0956**	0.0048	**4.2779**	**−0.7200**	**−0.1637**	**−0.1868**	**−0.1027**
	(−1.76)	(6.02)	(0.30)	(21.73)	(−2.47)	(−18.32)	(−4.35)	(−6.31)
LogMVE ($t-2$)	**0.0085**		**0.0241**		**0.2288**	**−0.0105**	**0.0634**	**0.0208**
	(10.31)		(18.96)		(21.63)	(−16.63)	(17.98)	(16.02)
MTB ($t-2$)	**−0.0017**	**−0.0037**	**−0.0024**	**0.1731**		**0.0051**	**−0.0188**	**−0.0113**
	(−2.26)	(1.14)	(−5.46)	(4.17)		(5.21)	(−3.75)	(−3.73)
Retstd ($t-2$)	0.0230	**−0.0533**	**−0.0827**	**−3.1313**	**2.2846**		**−0.2483**	**−0.0876**
	(1.83)	(−4.23)	(−2.73)	(−10.07)	(6.89)		(−3.56)	(−4.29)
LogSeg ($t-2$)	**0.0060**	**0.0010**	0.0067	**0.5535**	**−0.2642**	**−0.0029**		**0.0228**
	(2.97)	(3.03)	(0.32)	(16.83)	(−10.61)	(−1.89)		(6.58)
LogAge ($t-2$)	0.0006	**0.0101**	**0.0082**	**0.3079**	**−0.1773**	**−0.0151**	**0.1225**	**−0.0109**
	(0.55)	(5.53)	(4.61)	(16.00)	(−10.12)	(−17.44)	(22.63)	(−5.66)
Debt ($t-2$)	**−0.0190**	**0.0482**	0.0083	**2.1498**	**−2.0266**	**−0.0118**	**0.1592**	
	(−2.61)	(4.54)	(0.81)	(15.11)	(−16.46)	(−2.03)	(4.46)	
Indep ($t-2$)	**0.6010**	0.0036	**− 0.0301**					
	(65.13)	(0.29)	(−2.45)					
LogBsize ($t-2$)	**0.0193**	**0.7836**	**0.7242**					
	(3.94)	(115.34)	(93.38)					
R^2	0.4753	0.7036	0.7147	0.2645	0.0946	0.2657	0.2023	0.0684

Panel B: Dependent variable is change from t − 1 to t

	ΔIndep	ΔLogBsize	ΔLogBsize	ΔLogMVE	ΔMTB	ΔRetstd	ΔLogSeg	ΔDebt
ROA($t-2$)	**−0.0254**	**0.0158**	0.0067	**0.1667**	**−0.5963**	**−0.1540**	**0.0563**	**−0.0220**
	(−2.40)	(6.13)	(0.42)	(1.88)	(−2.35)	(−17.43)	(1.96)	(−2.20)
LogMVE ($t-2$)	**0.0108**		**0.0244**	**−0.0231**	**0.1082**	**−0.0083**	**0.0167**	**0.0040**
	(14.38)		(20.78)	(−5.65)	(4.03)	(−13.93)	(8.06)	(6.45)
MTB ($t-2$)	**−0.0022**	0.0034	**−0.0024**	**−0.0323**	**−0.7290**	**0.0036**	**−0.0044**	−0.0005
	(−2.65)	(1.36)	(−5.17)	(−5.84)	(10.60)	(5.25)	(−2.81)	(−0.77)
Retstd ($t-2$)	0.0165	**−0.0379**	**−0.1695**	**−0.1358**	**0.9684**	**−0.8251**	−0.0361	−0.0162
	(1.27)	(−3.61)	(−1.96)	(−1.76)	(2.69)	(−46.10)	(−0.68)	(−1.39)
LogSeg ($t-2$)	**0.0057**	**0.0010**	0.0067	**−0.0420**	**−0.1568**	−0.0007	**−1889**	**0.0051**
	(2.71)	(3.17)	(0.32)	(−3.52)	(−4.54)	(−0.49)	(−28.28)	(2.49)
LogAge ($t-2$)	0.0011	**0.0099**	**0.0076**	**0.0149**	**−0.0667**	**−0.0111**	**0.0141**	**−0.0057**
	(0.30)	(4.34)	(4.19)	(2.10)	(−2.77)	(−12.63)	(3.87)	(−4.77)
Debt ($t-2$)	**−0.0129**	**0.0358**	**0.0008**	**−0.1647**	**0.0132**	**−0.0235**	−0.0174	**−0.2644**
	(−1.67)	(3.24)	(0.07)	(−3.83)	(2.54)	(−3.79)	(−0.83)	–(20.60)
Indep ($t-2$)	**−0.4008**							
	(−44.27)							
LogBsize ($t-2$)		**−0.2265**	**−0.2905**					
		(−31.45)	(−35.40)					
R^2	0.2251	0.1823	0.1623	0.0586	0.5375	0.4791	0.2004	0.0684

Table 6

Does board structure adjust to past performance? Tests of strict exogeneity

In this table, we report results from the fixed-effects estimation of the model:

$$\mathbf{y}_{i,t} = \alpha + \beta \mathbf{X}_{i,t} + \Omega \mathbf{W}_{i,t+2} + \eta_i + \varepsilon_{it}, \qquad t = 1991, 1993, 1995 \ldots 2001,$$

where $\mathbf{W}_{it+1}$ is a subset of forward values of the corporate governance and control variables, $\mathbf{X}$. $\mathbf{y}$ is firm performance (*ROA*). $\mathbf{X}$ includes board size (*LogBSIZE*), board independence (*INDEP*), a dummy variable which is one if the CEO is the board chair (*CEO_CHAIR*), firm size (*LogMVE*), market-to-book ratio (*MTB*), standard deviation of stock returns (*RETSTD*), number of business segments (*LogSEGMENTS*), firm age (*LogAGE*), and leverage (*DEBT*). The results are based on a sample of 6,034 firms and 20,003 firm years selected every other year (1991, 1993, 1995, 1997, 1999, 2001, and 2003). The board variable data come from the Compact Disclosure database. The firm characteristics come from CRSP and Compustat. $\Omega = 0$ is the null hypothesis of strict exogeneity. All t-statistics (in parentheses) are based on robust standard errors. Year dummies are included in all specifications. * indicates significance at the 10% level or smaller.

Dependent variable: *ROA*(*t*)	1	2	3	4	5
LogBSIZE(*t*)	−0.0267*	−0.0282*	−0.0247*	−0.0234*	−0.0251*
	(−3.59)	(−3.80)	(−3.29)	(−3.13)	(−3.32)
INDEP(*t*)	0.0082	0.0088	0.0059	0.0055	0.0050
	(0.81)	(0.86)	(0.57)	(0.54)	(0.49)
CEO_CHAIR(*t*)	0.0005	0.0005	−0.0005	−0.0005	−0.0009
	(0.21)	(0.20)	(−0.17)	(−0.16)	(−0.33)
LogMVE(*t*)	0.0442*	0.0438*	0.0441*	0.0443*	0.0416*
	(18.13)	(17.98)	(17.87)	(17.99)	(14.74)
MTB(*t*)	0.0008	0.0008	0.0008	0.0009	0.0011
	(0.75)	(0.79)	(0.80)	(0.76)	(0.97)
RETSTD(*t*)	−0.0085	−0.0082	−0.0091	−0.0091	−0.0371*
	(−0.56)	(−0.55)	(−0.59)	(−0.59)	(−2.24)
LogSEGMENTS(*t*)	−0.0070*	−0.0068*	−0.0066*	−0.0067*	−0.0042
	(−2.39)	(−2.34)	(−2.27)	(−2.29)	(−1.34)
LogAGE(*t*)	−0.0083*	−0.0087*	−0.0093*	−0.0089*	−0.0059
	(−2.20)	(−2.21)	(−2.49)	(−2.35)	(−0.33)
DEBT(*t*)	−0.0869*	−0.0864*	−0.0866*	−0.0869*	−0.0758*
	(−5.51)	(−5.46)	(−5.44)	(−5.43)	(−4.51)
LogBSIZE(*t*+2)	−0.0136*			−0.0116*	−0.0150*
	(−2.08)			(−1.72)	(−2.18)
INDEP(*t*+2)		−0.0030		−0.0023	−0.0027
		(−0.31)		(−0.23)	(−0.26)
CEO_CHAIR(*t*+2)			0.0064*	0.0062*	0.0055*
			(2.26)	(2.20)	(1.95)
LogMVE(*t*+2)					0.0076*
					(2.64)
MTB(*t*+2)					0.0003
					(0.17)
RETSTD(*t*+2)					0.0997*
					(−5.05)
LogSEGMENTS(*t*+2)					−0.0041
					(−1.43)
LogAGE(*t*+2)					−0.0060
					(−0.19)
DEBT(*t*+2)					−0.0257*
					(−2.19)

Table 7

The effect of board structure on current firm performance

In this table, we report results from the estimation of the model:

$$y_{it} = \alpha_1 + \kappa_1 y_{it-2} + \kappa_2 y_{it-4} + \beta \mathbf{X}_{it} + \gamma \mathbf{Z}_{it} + \theta \mathbf{D}_{it} + \eta_i + \varepsilon_{it}, \qquad t = 1997, 1999, 2001, 2003.$$

y_{it} is return on assets (*ROA*) which is defined as operating income divided by assets. $\mathbf{X}_{it}$ includes board size (*LogBSIZE*), board independence (*INDEP*), and a dummy variable which is one if the CEO is the board chair (*CEO_CHAIR*). $\mathbf{Z}_{it}$ includes firm size (*LogMVE*), market-to-book ratio (*MTB*), standard deviation of stock returns (*RETSTD*), number of business segments (*LogSEGMENTS*), and leverage (*DEBT*). $\mathbf{D}_{it}$ includes firm age (*LogAGE*) and year dummies. The results are based on a sample of 6,034 firms and 20,003 firm years selected every other year (1991, 1993, 1995, 1997, 1999, 2001, and 2003). The board variable data come from the Compact Disclosure database. The firm characteristics come from CRSP and Compustat. t-statistics are reported in parentheses. For the static models, it is assumed that $\kappa_1 = \kappa_2 = 0$. All t-statistics are based on robust, firm-clustered standard errors. a, b, c represent significance at the 1%, 5% and 10% level, respectively. $AR(1)$ and $AR(2)$ are tests for first-order and second-order serial correlation in the first-differenced residuals, under the null of no serial correlation. The Hansen test of over-identification is under the null that all instruments are valid. The Diff-in-Hansen test of exogeneity is under the null that instruments used for the equations in levels are exogenous. The instruments used in the GMM estimation are: differenced equations: y_{it-6}, y_{it-8}, $\mathbf{X}_{it-6}$, $\mathbf{X}_{it-8}$, $\mathbf{Z}_{it-6}$, $\mathbf{Z}_{it-8}$, $\Delta\mathbf{D}_{it}$; level equations: Δy_{it-4}, $\Delta\mathbf{X}_{it-4}$, $\Delta\mathbf{Z}_{it-4}$, $\mathbf{D}_{it}$.

	Static model		Dynamic model	
Dependent variable (*ROA*)	Pooled OLS	Fixed effects	Pooled OLS	System GMM
LogBSIZE	-0.0262^a	-0.0261^a	-0.0033	0.0183
	(-5.67)	(-4.32)	(-0.73)	(0.43)
INDEP	-0.0266^a	0.0202^b	0.0061	-0.0109
	(-3.56)	(2.48)	(0.82)	(-0.14)
CEO_CHAIR	0.0025	0.0003	0.0018	-0.0127
	(1.09)	(0.13)	(0.83)	(-0.55)
LogMVE	0.0234^a	0.0429^a	0.0070^a	0.0160^a
	(27.97)	(21.29)	(6.89)	(2.87)
MTB	-0.0025^a	0.0014	0.0070^a	-0.0137^b
	(-2.91)	(1.34)	(3.36)	(-2.11)
RETSTD	-0.2047^a	-0.0117	-0.0832^a	-0.5207^c
	(-11.69)	(-0.94)	(-4.44)	(-1.77)
LogSEGMENTS	-0.0087^a	-0.0074^a	-0.0012	-0.0068
	(-4.47)	(-3.06)	(-0.76)	(-0.75)
LogAGE	0.0056^a	0.0008	-0.0003	-0.0295^a
	(4.26)	(0.27)	(-0.20)	(-2.63)
DEBT	-0.0307^a	-0.0625^a	-0.0040	-0.0345
	(-3.37)	(-3.99)	(-0.45)	(-0.52)
$ROA(t-2)$			0.4833^a	0.7590^a
			(24.40)	(3.06)
$ROA(t-4)$			0.1054^a	-0.1212
			(6.00)	(-0.22)
R^2	0.27	0.11	0.41	
$AR(1)$ test (p-value)				(0.00)
$AR(2)$ test (p-value)				(0.87)
Hansen test of over-identification (p-value)				(0.41)
Diff-in-Hansen tests of exogeneity (p-value)				(0.23)

Table 8

First stage regression and Cragg-Donald statistics for System GMM estimates

In this table, we report the F-statistics and R^2s of OLS first-stage regressions of levels and first-differenced variables on lagged differences and lagged levels respectively. The variables are board size($LogBSIZE$), board independence ($INDEP$), a dummy variable which is one if the CEO is the board chair (CEO_CHAIR), firm size ($LogMVE$), market-to-book ratio (MTB), standard deviation of stock returns ($RETSTD$), number of business segments ($LogSEGMENTS$), and leverage ($DEBT$). The results are based on a sample of 6,034 firms and 20,003 firm years selected every other year (1991, 1993, 1995, 1997, 1999, 2001, and 2003). The board variable data come from the Compact Disclosure database. The firm characteristics come from CRSP and Compustat. For the levels variables (X), the dependent variables are: $\Delta LogBSIZE(t-4)$, $\Delta INDEP(t-4)$, $\Delta CEO\_CHAIR(t-4)$, $\Delta LogMVE(t-4)$, $\Delta MTB(t-4)$, $\Delta RETSTD(t-4)$, $\Delta LogSEGMENTS(t-4)$, $\Delta DEBT(t-4)$, $\Delta ROA(t-4)$, $LogAGE$, and year dummies. For the first-differenced variables (ΔX), the dependent variables are: $LogBSIZE(t-6)$, $INDEP(t-6)$, $CEO\_CHAIR(t-6)$, $LogMVE(t-6)$, $MTB(t-6)$, $RETSTD(t-6)$, $LogSEGMENTS(t-6)$, $DEBT(t-6)$, $LogAGE(t-6)$, $ROA(t-6)$, $LogAGE$, and year dummies.

Panel A: Dependent variable (X) is in levels	F-statistic	p-value	R^2
$LogBSIZE$	59.14	0.00	0.1605
$INDEP$	19.19	0.00	0.0584
CEO_CHAIR	7.53	0.00	0.0238
$LogMVE$	61.26	0.00	0.1653
MTB	12.34	0.00	0.0384
$RETSTD$	47.59	0.00	0.1334
$LogSEGMENTS$	86.15	0.00	0.2179
$DEBT$	11.54	0.00	0.0360
	Cragg-Donald statistic: 22.60		
Panel B: Dependent variable (ΔX) is in first-differences			
$\Delta LogBSIZE$	20.26	0.00	0.0402
$\Delta INDEP$	18.44	0.00	0.0368
ΔCEO_CHAIR	19.05	0.00	0.0379
$\Delta LogMVE$	19.11	0.00	0.0380
ΔMTB	21.45	0.00	0.0425
$\Delta RETSTD$	38.43	0.00	0.0737
$\Delta LogSEGMENTS$	97.33	0.00	0.1677
$\Delta DEBT$	25.07	0.00	0.0493
	Cragg-Donald statistic: 4.29		

Table 9

The effect of lagged board structure on current firm performance

In this table, we report results from the estimation of the model:

$$y_{it} = \alpha_1 + \kappa_1 y_{it-2} + \kappa_2 y_{it-4} + \beta \mathbf{X}_{it-2} + \gamma \mathbf{Z}_{it-2} + \theta \mathbf{D}_{it} + \eta_i + \varepsilon_{it}, \qquad t = 1997, 1999, 2001, 2003,$$

y_{it} is return on assets (*ROA*) which is defined as operating income divided by assets. $\mathbf{X}_{it}$ includes board size(*LogBSIZE*), board independence (*INDEP*), and a dummy variable which is one if the CEO is the board chair (*CEO_CHAIR*). $\mathbf{Z}_{it}$ includes firm size (*LogMVE*), market-to-book ratio (*MTB*), standard deviation of stock returns (*RETSTD*), number of business segments (*LogSEGMENTS*), and leverage (*DEBT*). $\mathbf{D}_{it}$ includes firm age (*LogAGE*) and year dummies. The results are based on a sample of 6,034 firms and 20,003 firm years selected every other year (1991, 1993, 1995, 1997, 1999, 2001, and 2003). The board variable data come from the Compact Disclosure database. The firm characteristics come from CRSP and Compustat. t-statistics are reported in parentheses. For the static models, it is assumed that $\kappa_1 = \kappa_2 = 0$. Two lags of performance (not shown) are included in all specifications. All t-statistics are based on robust, firm-clustered standard errors. a, b, c represent significance at the one percent, 1%, 5% and 10% level, respectively. $AR(1)$ and $AR(2)$ are tests for first-order and second-order serial correlation in the first-differenced residuals, under the null of no serial correlation. The Hansen test of over-identification is under the null that all instruments are valid. The Diff-in-Hansen tests of exogeneity is under the null that instruments used for the equations in levels are exogenous. The instruments used in the GMM estimation are: differenced equations: y_{it-6}, y_{it-8}, $\mathbf{X}_{it-6}$, $\mathbf{X}_{it-8}$, $\mathbf{Z}_{it-6}$, $\mathbf{Z}_{it-8}$, $\Delta\mathbf{D}_{it}$; level equations: Δy_{it-4}, $\Delta\mathbf{X}_{it-4}$, $\Delta\mathbf{Z}_{it-4}$, $\mathbf{D}_{it}$.

Dependent variable ($ROA(t)$)	Pooled OLS	System GMM	Bias-corrected fixed effects
$LogBSIZE(t-2)$	0.0078	0.0115	0.0078
	(1.55)	(0.33)	(1.01)
$INDEP(t-2)$	0.0043	−0.0655	0.0052
	(0.52)	(-1.04)	(0.46)
$CEO\_CHAIR(t-2)$	0.0015	−0.0081	0.0031
	(0.61)	(−0.57)	(0.97)
$LogMVE(t-2)$	0.0025^a	0.0146^a	0.0229^a
	(2.66)	(3.20)	(9.16)
$MTB(t-2)$	−0.0013	-0.0088^c	0.0018^b
	(−1.23)	(−1.68)	(2.00)
$RETSTD(t-2)$	-0.0574^a	−0.0658	−0.0072
	(−3.63)	(−0.41)	(0.45)
$LogSEGMENTS(t-2)$	-0.0037^c	−0.0007	−0.0025
	(−1.84)	(-0.11)	(0.83)
$LogAGE(t-2)$	0.0001	−0.0112	0.0015
	(0.08)	(−1.59)	(0.20)
$DEBT(t-2)$	0.0382^a	−0.0019	0.0733^a
	(4.29)	(−0.06)	(5.77)
R^2	0.38		0.07
$AR(1)$ test (p-value)		(0.00)	
$AR(2)$ test (p-value)		(0.11)	
Hansen test of over-identification (p-value)		(0.21)	
Diff-in-Hansen tests of exogeneity (p-value)		(0.85)	

Table 10

The determinants of board structure

In this table, we report the results from OLS and dynamic GMM regressions of board size and board independence on firm size ($LogMVE$), market-to-book ratio (MTB), standard deviation of stock returns ($RETSTD$), number of business segments ($LogSEGMENTS$), firm age ($LogAGE$), leverage ($DEBT$), and (ROA) which is defined as operating income divided by assets. t-statistics are reported in parentheses. The GMM models includes two lags of the dependent variable. Year dummies are included in all specifications. All t-statistics are based on robust, firm-clustered standard errors. a, b, c represent significance at the 1%, 5% and 10% level, respectively. $AR(1)$ and $AR(2)$ are tests for first-order and second-order serial correlation in the first-differenced residuals, under the null of no serial correlation. The Hansen test of over-identification is under the null that all instruments are valid. The Diff-in-Hansen tests of exogeneity is under the null that instruments used for the equations in levels are exogenous.

	Board size		Board independence	
	OLS	Dynamic GMM	OLS	Dynamic GMM
$LogMVE$	0.0903	0.0305	0.0246	0.0101
	$(38.49)^a$	$(7.32)^a$	$(18.78)^a$	$(5.89)^a$
MTB	−0.0790	−0.0292	−0.0097	−0.0097
	$(11.45)^a$	$(-2.81)^a$	$(-2.49)^b$	$(-2.05)^b$
$RETSTD$	−0.1362	0.0615	0.0042	0.0374
	$(-3.99)^a$	(0.98)	(0.22)	(1.33)
$LogSEGMENTS$	0.0258	0.0140	0.0157	0.0010
	$(4.13)^a$	$(2.11)^b$	$(4.57)^a$	(0.32)
$LogAGE$	0.0510	0.0114	0.0180	0.0044
	$(11.47)^a$	$(2.95)^a$	$(7.15)^a$	$(2.71)^a$
$DEBT$	0.2081	0.0977	0.0306	0.0212
	$(10.24)^a$	$(3.20)^a$	$(2.56)^b$	$(1.66)^c$
$ROA(t-2)$	−0.0354	0.0114	−0.0153	−0.0316
	(−1.53)	(0.63)	(−1.16)	$(-3.96)^a$
R^2	0.41		0.15	
$AR(1)$ test (p-value)		(0.00)		(0.00)
$AR(2)$ test (p-value)		(0.51)		(0.74)
Hansen test of over-identification (p-value)		(0.11)		(0.40)
Diff-in-Hansen tests of exogeneity (p-value)		(0.28)		(0.52)