Does Corporate Governance Matter in Competitive Industries?*

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Abstract

By reducing the fear of a hostile takeover, business combination (BC) laws weaken corporate governance and create more opportunity for managerial slack. Using the passage of BC laws as a source of variation in corporate governance, we examine if these laws have a different effect on firms in competitive and non-competitive industries. We find that while firms in non-competitive industries experience a significant drop in performance after the laws' passage, firms in competitive industries experience virtually no effect. While consistent with the general notion that competition mitigates managerial agency problems, our results are, in particular, supportive of the (stronger) Alchian-Friedman-Stigler hypothesis that competitive industries leave no room for managerial slack. When we examine which agency problem competition mitigates, we find evidence in support of a "quiet-life" hypothesis. While capital expenditures are unaffected by the passage of the BC laws, input costs, wages, and overhead costs all increase, and only so in non-competitive industries. We also conduct event studies around the dates of the first newspaper reports about the BC laws. We find that while firms in non-competitive industries experience a significant stock price decline, firms in competitive industries experience a small and insignificant price impact.

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1 Introduction

It is a widely held view among economists that product market competition mitigates managerial agency problems.[1] Views differ, however, when it comes to the issue of how "perfect" managerial incentives are in competitive industries. Some, like Leibenstein (1966), argue that competition reduces managerial slack but stop short of arguing that it resolves all (X-) inefficiencies. Others, like Alchian (1950), Friedman (1953), and Stigler (1958) go further, essentially arguing that managerial slack cannot exist, or survive, in competitive industries.[2]

The argument that competitive industries leave no room for managerial slack, provided it is true, has several important implications.[3] For instance, it implies that "the managerial extension and enrichment of the firm was not needed except where firms in the industry were [...] not under the pressure of competition" (Machlup (1967, p. 11)). In other words, topics that have been studied extensively over the past decades, such as managerial discretion and agency problems between shareholders and management leading to deviations from profit-maximizing behavior, might have little bearing on firms in competitive industries. Second, empirical studies on corporate governance might benefit from including, or conditioning on, measures of industry competition (see also Conclusion). Finally, efforts to improve corporate governance might benefit from focusing primarily on firms in non-competitive industries. Moreover, such efforts could be broadened to also include policy measures aimed at improving an industry's competitiveness, such as deregulation and antitrust laws.

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[1] Despite its intuitive appeal, attempts to formalize the notion that competition mitigates managerial agency problems have proven difficult. For example, while Hart (1983) shows that competition reduces managerial slack, Scharfstein (1988) shows that Hart's result can be easily reversed. Subsequent models generally find ambiguous effects (e.g., Hermalin (1992), Schmidt (1997)). In an early review of the literature, Holmström and Tirole (1989, p. 97) conclude that "apparently, the simple idea that product market competition reduces slack is not as easy to formalize as one might think."

[2] Scherer (1980, p. 38) summarizes the argument as follows: "Over the long pull, there is one simple criterion for the survival of a business enterprise: Profits must be nonnegative. No matter how strongly managers prefer to pursue other objectives [...] failure to satisfy this criterion means ultimately that a firm will disappear from the economic scene."

[3] Not surprisingly, the Alchian-Friedman-Stigler hypothesis is controversial. Referring to Alchian (1950) and Stigler (1958), Shleifer and Vishny (1997, p. 738) write in their survey of corporate governance: "While we agree that product market competition is probably the most powerful force toward economic efficieny in the world, we are skeptical that it alone can solve the problem of corporate governance."

To examine the empirical relevance of the above arguments, we use exogenous variation in corporate governance in the form of 30 business combination (BC) laws passed between 1985 and 1991 on a state-by-state basis.[4] By reducing the fear of a hostile takeover, these laws weaken corporate governance and increase the opportunity for managerial slack. Typically, BC laws impose a moratorium on certain kinds of transactions, including mergers and asset sales, between a large shareholder and the firm for a period ranging from three to five years after the shareholder's stake has passed a prespecified threshold. This moratorium hinders corporate raiders from gaining access to the target firm's assets for the purpose of paying down acquisition debt, thus making hostile takeovers more difficult and often impossible.[5]

Using the passage of BC laws as a source of identifying variation, we ask a straightforward question. Does corporate governance have a different effect on firm's operating performance in competitive and non-competitive industries? We obtain three main results. First, consistent with the notion that BC laws create more opportunity for managerial slack, we find that firms' return on assets (ROA) drops by 0.6 percentage points on average after the laws' passage. Second, the drop in ROA becomes increasingly stronger the less competitive the industry is. For example, ROA drops by only 0.1 percentage points in the lowest Herfindahl quintile but by 1.5 percentage points in the highest Herfindahl quintile. Third, the effect is close to zero and statistically insignificant in highly competitive industries. This last finding, in particular, is supportive of the argument by Alchian (1951), Friedman (1953), and Stigler (1958) that competitive industries leave no room for managerial slack.

The contribution of this paper is not the introduction of a novel source of identifying variation. Many papers have used the passage of BC laws as a source of exogenous variation in corporate governance, including Hackl and Testani (1988), Garvey and Hanka (1999), Bertrand and Mullainathan (1999, 2003), Cheng, Nagar, and Rajan (2005), and Rauh (2006).[6] Rather,

[4] Many authors share the view that antitakeover laws are exogenous for all but perhaps a few firms motivating these laws, e.g., Romano (1987), Karpoff and Malatesta (1989), Comment and Schwert (1995), Garvey and Hanka (1999), Bertrand and Mullainathan (1999, 2003), Cheng, Nagar, and Rajan (2004), and Rauh (2006). We specifically address the endogeneity of BC laws in our study. For further information on BC laws, see Sroufe and Gelband (1990) and Suggs (1995).

[5] Bertrand and Mullainathan (2003, p. 1045) conclude: "The reduced fear of a hostile takeover means that an important disciplining device has become less effective and that corporate governance overall was reduced."

[6] While the source of exogenous variation is often of interest in itself, it is first and foremost a means to ensure that an empirical relationship is identified.

our contribution is that we document that corporate governance has a different effect on firms' operating performance in competitive and non-competitive industries and, especially, that it does not appear to matter much in the former. We believe this is an important insight, both from a researcher's and a policymaker's perspective.

Our findings turn out to be robust across many specifications. For example, our main competition measure is the 3-digit SIC Herfindahl index computed from COMPUSTAT. However, we obtain similar results if we use 2- and 4-digit SIC Herfindahl indices, lagged Herfindahl indices, and historic Herfindahl indices predating the first BC laws. We also obtain similar results if we use the Herfindahl index provided by the U.S. Bureau of the Census (which includes both public and private firms), import penetration, and industry net profit margin as competition measures, though the first two measures are only available for manufacturing industries. Finally, we obtain similar results if we drop Delaware firms as well as firms incorporated in states that never passed a BC law, if we use alternative performance measures such as return on equity and return on sales, and if we run "horse races" between the Herfindahl index and other variables for which the Herfindahl index might be proxying.

Our identification strategy benefits from a general lack of congruence between a firm's industry, state of location, and state of incorporation. For instance, a firm's state of incorporation says little about its industry. Likewise, only 38 percent of the firms in our sample are incorporated in their state of location. BC laws, in turn, apply to all firms in a given state of incorporation, regardless of their state of location or industry. This lack of congruence allows us to control for local and industry shocks and thus to separate out the effects of shocks contemporaneous with the BC laws from the effects of the laws themselves. This can address, among other things, concerns that the BC laws might be the outcome of lobbying at the local and industry level, respectively. To address the issue of broad-based lobbying at the state of incorporation level, we furthermore investigate the dynamic effects of the BC laws.

Besides showing that competition mitigates managerial agency problems, we also examine which agency problem competition mitigates. Does competition curb managerial empire building? Or does it prevent managers from enjoying a "quiet life" by forcing them to "undertake cognitively difficult activities" (Bertrand and Mullainathan (2003, p. 1067))? We find no evidence for empire building: Capital expenditures are unaffected by the passage of the BC laws. By contrast, input costs, wages, and overhead costs all increase after the passage of the BC laws, and only so in non-competitive industries. Overall, our findings are consistent with a "quiet-life" hypothesis whereby managers insulated from hostile takeovers and competitive pressure seek to avoid cognitively difficult activities, such as haggling with input suppliers, labor unions, and organizational units demanding bigger overhead budgets.[7]

We also conduct event studies around the dates of the first newspaper reports about the BC laws. On average, we find a small but significant cumulative abnormal return (CAR) of -0.32%. Importantly, when we compute CARs separately for low- and high Herfindahl portfolios, we find that the average CAR for the low-Herfindahl portfolio is small and insignificant, whereas the average CAR for the high-Herfindahl portfolio is -0.54% and significant. A similar pattern emerges if we form three portfolios: While the average CAR for the low-Herfindahl portfolio is small and insignificant, the average CARs for the medium- and high-Herfindahl portfolios are -0.44% and -0.67%, respectively, both of which are significant.

In terms of research question, the paper most closely related to ours is Nickell (1996), who shows that more competition leads to higher productivity growth in a sample of U.K. manufacturing firms.[8] While consistent with a managerial agency explanation, Nickell's result is also consistent with alternative explanations unrelated to corporate governance. For example, firms in competitive industries might have higher productivity growth because there are more industry peers from whose successes and failures they can learn. Our paper is also related to a growing literature that documents a link between competition and firm-level corporate governance. Most of these papers find that firm-level corporate governance instruments covary with competition, for example, managerial incentive schemes (Aggarwal and Samwick (1999)), board structure (Karuna (2007)), and firm-level takeover defenses (Cremers, Nair, and Peyer (2006)). Finally, Guadalupe and Pérez-González (2005) show that competition affects private benefits of control as measured by the voting premium between shares with different voting rights.

The rest of this paper is organized as follows. Section 2 presents the data and lays out the empirical methodology. Section 3 presents our main results and robustness checks. Section 4 presents event study results. Section 5 concludes.

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[7] See Bertrand and Mullainathan (2003) for further evidence on the "quiet-life" hypothesis. The "quiet-life" hypothesis is closely related to the expense-preference hypothesis, which posits that managers share rents with workers to have a more comfortable life (e.g., Edwards (1977), Hannan (1979)).

[8] See also Bloom and van Reenen (2007), who find that poor management practices are more prevalent in less competitive industries.

2 Data

2.1 Sample Selection

Our main data source is Standard and Poor's COMPUSTAT. To be included in our sample, a firm must be located and incorporated in the United States. We exclude all observations for which the book value of assets or net sales are either missing or negative. We also exclude regulated utility firms (SIC 4900-4999).[9] The sample period is from 1976 to 1995, which is the same sample period as in Bertrand and Mullainathan (2003).

These selection criteria leave us with 10,960 firms and 81,095 firm-year observations. **Table I** shows how many firms are located and incorporated in each state. The state of location, as defined by COMPUSTAT, indicates the state in which a firm's headquarters are located. The state of incorporation is a legal concept and determines, inter alia, which BC law, if any, applies to a given firm. Unfortunately, COMPUSTAT only reports the state of incorporation for the latest available year. However, anecdotal evidence suggests that changes in states of incorporation are quite rare (e.g., Romano (1993)). To provide further evidence, Bertrand and Mullainathan (2003) have randomly sampled 200 firms from their panel and checked if any of these firms had changed their state of incorporation during the sample period. Only three firms had changed their state of incorporation, all of them to Delaware. Importantly, all three changes predated the 1988 Delaware BC law by several years. Similarly, Cheng, Nagar, and Rajan (2004) report that none of the 587 *Forbes* 500 firms in their panel had changed their state of incorporation during the sample period from 1984 to 1991.

2.2 Definition of Variables and Summary Statistics

Our main measure of product market competition is the Herfindahl-Hirschman index, which is well-grounded in industrial organization theory.[10] The Herfindahl index is defined as the sum of squared market shares,

$$HHI_{jt} := \sum_{i=1}^{N_j} s_{ijt}^2,$$

[9] Whether or not we exclude regulated utilities makes no difference for our results. We also obtain similar results if we exclude financial firms (SIC 6000-6999). Likewise, we obtain similar results if we consider only manufacturing firms (SIC 2000-3999); see Section 3 for details.

[10] See Curry and George (1983) and Tirole (1988, pp. 221-223).

where s_{ijt} is the market share of firm i in industry j in year t. Market shares are computed from COMPUSTAT using firms' sales (item #12). In robustness checks, we also compute market shares using total assets (item #6). Our benchmark measure is the Herfindahl index based on 3-digit SIC codes. The 3-digit partition is a compromise between too coarse a partition, in which unrelated industries may be pooled together, and too narrow a partition, which may be subject to misclassification. For example, the 2-digit SIC code 38 (instruments and related products) pools together ophthalmic goods such as intra ocular lenses (3-digit SIC code 385) and watches, clocks, clockwork operated devices and parts (3-digit SIC code 387), two industries that are unlikely to compete against each other. On the other hand, the 4-digit SIC partition treats upholstered wood household furniture (4-digit SIC code 2512) and non-upholstered wood household furniture (4-digit SIC code 2511) as unrelated industries, even though common sense suggests that they compete against each other. We consider Herfindahl indices based on 2- and 4-digit SIC codes in robustness checks.

A look at the empirical distribution of the Herfindahl index shows that it has a (small) "spike" at the right endpoint, which points to misclassification. To avoid that outliers and misclassification drive our results, we drop 2.5% of the firm-year observations at the right tail of the distribution.[11] We further address the issue of measurement error in robustness checks by using Herfindahl dummies. Also in robustness checks, we consider non-COMPUSTAT measures of competition that are only available for manufacturing industries.

Our main measure of firms' operating performance is the return on assets (ROA), which is defined as operating income before depreciation and amortization (EBITDA, item #13) divided by the book value of total assets (item #6). Since ROA is a ratio, it can take on extreme values (in either direction) if the scaling variable becomes too small. To mitigate the effect of outliers, we drop 1% of the firm-year observations at each tail of the ROA distribution. This reduces our initial sample of 81,095 firm-year observations. For instance, in column [1] of Table III, our final sample consists of $81,095 \times 0.98 = 79,474$ firm-year observations. We consider additional performance measures in robustness checks.

[11] The 3-digit partition comprises 270 industries. In some cases, the industry definition is rather narrow, with the effect that some industries consist of a single firm even though common sense suggests that they should be pooled together with other industries. By construction, these industries have a Herfindahl index equal to one, which explains the small "spike" at the right endpoint of the empirical distribution. Dropping 2.5% of the firm-year observations at the right tail of the distribution corrects for the misclassification.

The remaining variables are defined as follows. Size is the natural logarithm of total assets. Age is the natural logarithm of one plus the firm's age, which is the number of years the firm has been in COMPUSTAT. Leverage is long-term debt (item #9) plus debt in current liabilities (item #34) divided by total assets. Tobin's Q is the market value of total assets divided by the book value of total assets. The market value of total assets is the book value of total assets (item #6) plus the market value of equity (item #24 times item #25) minus the sum of the book value of equity (item #60) and balance sheet deferred taxes (item #74). E-Index is the entrenchment index by Bebchuk, Cohen, and Ferrell (2005) and is obtained from Lucian Bebchuk's webpage. G-index is the governance index by Gompers, Ishii, and Metrick (2003), and Poison Pills is a dummy variable that equals one if the firm has a poison pill. Both variables are obtained from the IRRC database. E-index, G-index, and Poison Pills are only available for the years 1990, 1993, and 1995 during the sample period. Additional COMPUSTAT variables will be introduced at a later point in time.

Table II provides summary statistics for firms incorporated in states that passed a BC law during the sample period ("Eventually Business Combination") and firms incorporated in states that did not pass a BC law ("Never Business Combination"). Splitting the sample this way shows that firms in passing states are bigger and slightly older on average. On the other hand, there are no significant differences with respect to leverage, Herfindahl index, and E-index. That firms in passing states have a higher G-index is partly mechanical, because the G-index assigns one index point if the firm is incorporated in a state that passed a BC law. That firms in passing states are bigger and slightly older deserves more attention, because it raises the question if the control group is an appropriate one.[12] There are several reasons why this should not be a serious concern. First, due to the staggering of the BC laws over time, firms in the "Eventually Business Combination" group are first control firms (before the BC law) and subsequently treatment firms (after the BC law). Second, we control for age and size in all our regressions. Third, we show in robustness checks that our results are unchanged if we focus only on states that passed a BC law during the sample period.

[12] The issue about the control group is that firms in passing and non-passing states may differ for reasons unrelated to the passage of BC laws. If firms differ along endogenous dimensions (e.g., G-index), this may reflect the simple fact that firms in passing and non-passing states make different choices. And yet, to address any remaining concerns that firms in passing and non-passing states differ for reasons unrelated to the passage of BC laws, we include leverage, E-index, G-index, and other variables in robustness checks (see Table V).

2.3 Empirical Methodology

We examine if the passage of 30 BC laws between 1985 and 1991 affects firms' operating performance differently depending on how competitive the firm's industry is. The basic equation we estimate is

$$y_{ijklt} = \alpha_i + \alpha_t + \beta_1 BC_{kt} + \beta_2 Herfindahl_{jt} + \beta_3 \left(BC_{kt} \times Herfindahl_{jt}\right) + \boldsymbol{\gamma}' \mathbf{X}_{ijklt} + \epsilon_{ijklt}, \quad (1)$$

where i indexes firms, j indexes industries, k indexes states of incorporation, l indexes states of location, t indexes time, y_{ijklt} is the dependent variable of interest (e.g., ROA), α_i and α_t are firm and year fixed effects, BC_{kt} is a dummy variable that equals one if a BC law has been passed in state k by time t, $Herfindahl_{jt}$ is the Herfindahl-Hirschman index for industry j at time t, $\mathbf{X}_{ijklt}$ is a vector of control variables, and ϵ_{ijklt} is the error term.

The total effect of the passage of BC laws on operating performance can be computed as $\beta_1 + \beta_3 Herfindahl$. The coefficient β_1 measures the (limit) effect as the Herfindahl index goes to zero. The coefficient β_3 measures how the effect varies with product market competition, where it should be noted that a higher Herfindahl index implies *weaker* competition. The coefficient β_2 measures the direct effect of competition on operating performance. Here, the conjecture is that an increase in competition (lower Herfindahl index) reduces firms' profits. We include age and size as control variables in all our regressions to account for systematic differences between the control and treatment groups (see Section 2.2).

We use a differences-in-differences-in-differences methodology. The first difference compares firms' operating performance before and after the passage of BC laws separately for firms in the control and treatment group. This yields two differences, one for the control group and one for the treatment group. The second difference takes the difference between these two differences. The result is an estimate of the effect of the BC laws on firms' operating performance. The interaction term $BC \times Herfindahl$ allows us to estimate a third difference, namely, whether the BC laws have a different effect on firms' operating performance in competitive and non-competitive industries. Importantly, the staggered passage of the BC laws implies that the control group is not restricted to firms incorporated in states that never passed a BC law. The control group includes all firms incorporated in states that have not passed a BC law by time t. Thus, it includes firms incorporated in states that never passed a BC law as well as firms incorporated in states that passed a law after time t.

Our identification strategy benefits from a general lack of congruence between a firm's industry, state of location, and state of incorporation. For instance, a firm's state of incorporation says little about its industry. Likewise, Table I shows that only 37.8% of all firms are incorporated in their state of location. BC laws, in turn, apply to all firms in a given state of incorporation, regardless of their state of location or industry. This lack of congruence allows us to include time-varying industry- and state-year controls to account for industry shocks and shocks specific to a state of location (see Bertrand and Mullainathan (2003)).[13] The time-varying industry- and state-year controls are computed as the mean of the dependent variable (e.g., ROA) in the firm's industry and state of location, respectively, in each year, excluding the firm itself.

Controlling for local and industry shocks helps us to separate out the effects of shocks contemporaneous with the BC laws from the effects of the laws themselves. This addresses several important concerns. First, our estimate of the laws' effects could be biased, reflecting in part the impact of contemporaneous shocks. Second, our results could be spurious, coming entirely from shocks contemporaneous with the BC laws. Third, and perhaps most important, economic conditions could influence the passage of BC laws. For example, poor economic conditions in a particular state might induce local firms to lobby for an antitakeover law to gain better protection from hostile takeovers.[14]

While the inclusion of state- and industry-year controls can address concerns that the BC laws are the outcome of lobbying at the local and industry level, respectively, it remains the possibility that lobbying occurs at the state of incorporation level. For this to be a serious concern, however, it would have to be the case that a broad coalition of firms incorporated in the same state, which all experience a decline in profitability and, in our case, moreover operate in less competitive industries, successfully lobby for an antitakeover law. Given the anecdotal evidence in Romano (1987), who portrays lobbying for antitakeover laws as an exclusive political process, this is rather unlikely. Typically, antitakeover laws were adopted, often during emergency sessions,

[13] Table I shows that about 82% of the firms incorporated outside their state of location are incorporated in Delaware. While this is an interesting fact of U.S. corporate law, it has no bearing on the identification of the state-year coefficient. What matters is that the set of firms affected by a local shock is not congruent with the set of firms affected by the BC law in the same state.

[14] While we control for local and industry shocks, it should be noted that it is not obvious how these shocks could easily explain our results. Local and industry shocks would have to primarily affect firms in less competitive industries. Moreover, affected firms would have to be primarily incorporated in states that passed a BC law.

under the political pressure of a single firm facing a takeover threat, not a broad coalition of firms. Hence, for all but a few select firms, the laws were exogenous.[15]

Following Bertrand and Mullainathan (2003), we explicitly address the issue of broad-based lobbying by investigating the dynamic effects of BC laws. Specifically, we replace the interaction term in equation (1) with five interaction terms: $Before(-2) \times Herfindahl$, $Before(-1) \times Herfindahl$, $Before(0) \times Herfindahl$, $After(1) \times Herfindahl$, and $After(2+) \times Herfindahl$, where $Before(-2)$ and $Before(-1)$ are dummy variables that equal one if the firm is incorporated in a state that will pass a BC law in two years and one year from now, respectively, $Before(0)$ is a dummy variable that equals one if the firm is incorporated in a state that passes a BC law this year, and $After(1)$ and $After(2+)$ are dummy variables that equal one if the firm is incorporated in a state that passed a BC law one year and two or more years ago, respectively. If the BC laws were passed in response to political pressure of a broad coalition of firms, then we should see an "effect" of the laws already prior to their passage. In particular, if the coefficients on $Before(-2) \times Herfindahl$ or $Before(-1) \times Herfindahl$ were significant, then this would be symptomatic of reverse causation.

Another important issue is the potential endogeneity of the Herfindahl index. The main concern here is reverse causation. Fortunately, as Nickell (1996) points out, reverse causation predicts the opposite sign. It predicts that a drop in profits, possibly caused by the passage of the BC laws, leads to firm exits and thus higher industry concentration (higher Herfindahl index). Likewise, a boost in profits leads to the entry of new firms and lower industry concentration. Hence, a negative coefficient β_2 in equation (1) would be symptomatic of reverse causation, while a positive coefficient would be consistent with the (conventional) interpretation that an increase in competition reduces firms' profits. We further address the issue of reverse causation using lagged values of the Herfindahl index as well as the average Herfindahl index from 1976 to 1984 (the first BC law was passed in 1985) in robustness checks.

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[15] Using newspaper reports (see Section 4), we have identified firms that motivated the passage of BC laws. For example, the Minnesota BC law was adopted under the political pressure of the Dayton Hudson (now Target) Corporation when it was attacked by the Dart Group Corporation. Similar to other studies (e.g., Garvey and Hanka (1999)), we find that excluding such motivating firms from our sample does not affect our results. Most commentators share the view that antitakeover laws are exogenous for all but perhaps a few motivating firms, e.g., Romano (1987), Karpoff and Malatesta (1989), Comment and Schwert (1995), Garvey and Hanka (1999), Bertrand and Mullainathan (1999, 2003), Cheng, Nagar, and Rajan (2004), and Rauh (2006).

Throughout the paper, we cluster standard errors at the state of incorporation level. This allows for arbitrary correlations of the error terms across firms in the same state of incorporation in any given year as well as over time.[16] Clustering at the state of incorporation level addresses two important concerns. First, the fact that all firms in a given year and state of incorporation are affected by the same "shock" can induce correlation of the error terms within each state-year cell (Moulton (1990), Donald and Lang (2007)). Second, and this is an intrinsic problem of the differences-in-differences approach, the fact that the BC dummy changes little over time, being zero before and one after the passage of the BC law, can induce serial correlation (Bertrand, Duflo, and Mullainathan (2004)). While clustering at the state of incorporation level is a natural choice given that the BC dummy is a likely source of both cross-sectional and serial correlation, our results also hold if we cluster at the state of location level. We discuss alternative methods to account for cross-sectional and serial correlation below.

3 Results

While many economists have argued that competition reduces managerial slack, some economists, like Alchian (1950), Friedman (1953), and Stigler (1958) go further, essentially arguing that managerial slack cannot survive in competitive industries. We investigate the empirical relevance of these arguments by examining if the passage of 30 BC laws between 1985 and 1991 affects firms' operating performance differently depending on how competitive the firm's industry is. By reducing the fear of a hostile takeover, BC laws weaken corporate governance and increase the opportunity for managerial slack. If competitive industries leave no room for managerial slack, then we should see a smaller drop in performance, if any, in competitive industries.

Main Results

Table III contains our main results. In column [1] we confirm that the passage of the BC laws indeed causes a drop in operating performance. The BC dummy has a coefficient of -0.006, implying that ROA decreases by 0.6 percentage points on average. In column [3] we examine if and how this drop in ROA varies with product market competition. The interaction term between the BC dummy and the Herfindahl index has a coefficient of -0.025, which implies that the drop in ROA is larger for firms in less competitive industries. (That firms in competitive

[16] By implication, this allows for the error terms of any given firm to be serially correlated.

industries have a lower ROA to begin with is accounted for by the inclusion of firm fixed effects and the Herfindahl index as a control variable.) Of equal interest is that the BC dummy is close to zero and insignificant. Since the BC dummy in column [3] captures the limit effect as the Herfindahl index goes to zero, this implies that the BC laws have no significant effect on firms in highly competitive industries. Finally, note that the Herfindahl index has a mean value of 0.226. We can thus compute the average effect of the BC laws from column [3] as $-0.001 - 0.025 \times 0.226 = -0.007$, which is similar to the estimate in column [1]. Performing an $F-$test shows that the BC dummy and the interaction term between the BC dummy and the Herfindahl index in column [3] are jointly significant at the 1% level.

Columns [2] and [4] show the same regressions with control variables. The BC dummy in column [2] has a coefficient of -0.006, which is the same as in column [1]. Hence, whether or not we include control variables, ROA drops by 0.6 percentage points on average. The control variables all have the expected signs. The industry- and state-year coefficients are both positive and significant, which shows that controlling for industry and local shocks is important. Size and the Herfindahl index both have positive coefficients, while age has a negative coefficient.[17] The insignificance of the Herfindahl index in column [2] is due to the fact that it captures two different effects of competition on operating performance, which have opposite signs. As we will show below, when we disentangle the two effects they both become significant.

Column [4], which represents our "basic" regression, disentangles the direct effect of competition on performance from the indirect "managerial-incentive effect". The direct effect is captured by including the Herfindahl index as a control variable. The Herfindahl index has a coefficient of 0.025, which implies that an increase in competition reduces firms' ROA. The coefficient in column [4] is larger than in column [2] because the latter coefficient additionally includes the indirect effect. The indirect effect is captured by the interaction term between the BC dummy and the Herfindahl index. The interaction term has a coefficient of -0.033, which implies that the decrease in ROA is larger for firms in less competitive industries. The coefficient in column [4] is smaller than in column [3] because the latter coefficient additionally includes the

[17] We have experimented with squared terms for size, age, and the Herfindahl index to capture possible nonlinearities. Column [2] shows that the squared term for size is negative and significant, which implies that the relationship between size and ROA is concave. The squared term for age was significant but rendered the coefficient on age itself insignificant with almost no effect on the other variables. All our results are similar if we include age-squared instead of age. The squared term for the Herfindahl index was insignificant.

direct effect of competition on operating performance.[18] Finally, the BC dummy in column [4] is close to zero and insignificant, which implies that the passage of the BC laws has no significant effect on firms in highly competitive industries.

To illustrate the magnitude of the (indirect) managerial-incentive effect, note that the Herfindahl index has a standard deviation of 0.156. Thus, an increase in the Herfindahl index by one standard deviation is associated with a decrease in ROA of $-0.033 \times 0.156 = -0.005$, or 0.5 percentage points. Alternatively, we can divide the sample into Herfindahl quintiles. The mean value of the Herfindahl index in the lowest and highest quintile is 0.067 and 0.479, respectively. Accordingly, the passage of the BC laws has virtually no effect on firms in the lowest Herfindahl quintile: ROA drops by only $0.001 - 0.033 \times 0.067 = -0.001$, or 0.1 percentage points, compared to $0.001 - 0.033 \times 0.479 = -0.015$, or 1.5 percentage points, in the highest Herfindahl quintile. Finally, we can compute the average effect of the BC laws from column [4] as $0.001 - 0.033 \times 0.226 = -0.006$, which is the same as in columns [1] and [2]. Performing an $F-$test shows that the BC dummy and the interaction term between the BC dummy and the Herfindahl index in column [4] are jointly significant at the 2% level.

Let us summarize our main results. By reducing the fear of a hostile takeover, BC laws increase the opportunity for managerial slack. And yet, the passage of BC laws appears to have no significant effect on firms in highly competitive industries, which suggests that these industries leave little room, if any, for managerial slack. On the other hand, we observe a significant drop in operating performance for firms in less competitive industries, which suggests that changes in corporate governance do matter in these industries.

Broad-based Lobbying

Column [5] of **Table III** addresses the issue of broad-based lobbying. If a broad coalition of firms incorporated in the same state, which all experience a drop in operating performance and additionally operate in non-competitive industries, successfully lobbies for an antitakeover law in their state of incorporation, the causality would be reversed. In this case, it would not be the BC laws causing a drop in operating performance for firms in non-competitive industries, but rather (a large number of) firms in non-competitive industries experiencing a drop in operating

[18] The difference is entirely due to including the Herfindahl index as a control variable. If we run the same regression as in column [4] without including the Herfindahl index as a control variable, we find that the interaction term has a coefficient of -0.025 ($t-$statistic of 4.46), which is the same estimate as in column [3].

performance would be causing the BC laws. Note that this issue is very much minimized here since we control for both local and industry shocks. This accounts for the possibility that, for example, poor economic conditions in a given state might induce local firms to lobby for an antitakeover law in that state. Moreover, given the anecdotal evidence in Romano (1987), who portrays lobbying for antitakeover laws as an exclusive political process, it is unlikely that the BC laws are the outcome of broad-based lobbying.[19]

As described in Section 2.3, we address the issue of broad-based lobbying by investigating the dynamic effects of the BC laws. If the laws were passed in response to political pressure of a broad coalition of firms incorporated in the same state and operating in non-competitive industries, then we should see an "effect" of the laws already prior to their passage. In particular, if the coefficients on either $Before(-2) \times Herfindahl$ or $Before(-1) \times Herfindahl$ were significant, then this would be symptomatic of reverse causation. However, neither of the two coefficients is significant. Moreover, both coefficients are small, especially in comparison to those on $Before(0) \times Herfindahl$ (the year of the law's passage), $After(1) \times Herfindahl$, and $After(2+) \times Herfindahl$.

Endogeneity of the Herfindahl Index

Based on the results in **Table III**, we can also address the potential endogeneity of the Herfindahl index. As discussed previously, the main issue here is reverse causation. A drop in profits, possibly caused by the passage of the BC laws, might lead to firm exits and thus higher industry concentration (higher Herfindahl index). Likewise, a boost in profits might lead to the entry of new firms and a lower Herfindahl index. Accordingly, reverse causation would predict that the coefficient β_2 in equation (1) should be negative (see also Nickell (1996)). However, Table III shows that this coefficient is positive, which is consistent with the (standard) interpretation that an increase in competition reduces firms' profits.

Another way to address the issue of reverse causation is to use lagged values of the Herfindahl index. In columns [1] and [2] of **Table IV** we use 1- and 2-year lagged Herfindahl indices, respectively. The results are similar to those in Table III.[20] In column [3] we use the average Herfindahl index from 1976 to 1984 to specifically address concerns that the drop in profits

[19] This is also confirmed by newspaper reports (see Section 4). In many cases, the BC law was motivated by a single firm facing a hostile takeover attempt. Excluding such motivating firms does not affect our results.

[20] The results are also similar if we use 3-, 4-, and 5-year lagged Herfindahl indices.

caused by the passage of the BC laws might feed back into the Herfindahl index. (The first BC law was passed in 1985). The results are again similar to those in Table III. [21]

"Horse Races"

Our results could be spurious if they were not driven by the Herfindahl index but by some (omitted) variable Z that is correlated with the Herfindahl index and for which the Herfindahl index is merely proxying. We address this issue in **Table V** by running "horse races" between the Herfindahl index and various other variables, including size, age, leverage, ROA, Tobin's Q, G-Index, E-Index, and Poison Pills.[22] In each case, we estimate our basic regression in column [4] of Table III with two additional terms: an interaction term $BC \times Z$ and a control term Z, where Z is the variable in question. The results are consistently similar to those in Table III. In particular, the coefficient on $BC \times Herfindahl$ is remarkably stable throughout with values ranging from -0.026 to -0.032 ($t-$statistics from 3.02 to 4.09), which is similar to the -0.033 reported in column [4] of Table III.

Estimating the limit effect of the BC laws as the Herfindahl index goes to zero is more subtle. This is because the BC dummy now measures the limit effect as *both* the Herfindahl index *and* Z approach zero. Ideally, however, we would like to have an estimate of the laws' effect on firms in highly competitive industries for some representative value of Z, not when Z is zero. A natural candidate is the mean of Z, denoted by $\bar{Z}$. We can estimate the effect of the BC laws as the Herfindahl index goes to zero, evaluated at the mean of Z, by adding up the coefficient on the BC dummy and the coefficient on $BC \times Z$ multiplied by $\bar{Z}$. Whether this expression is significant can be tested using a standard $F-$test. As Table V shows, the estimates are small and the $p-$values are high, which is consistent with our results in Table III.

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[21] Note that the coefficient on the Herfindahl index as a control variable is missing in column [3]. Since the average Herfindahl index from 1976 to 1984 has no "within" variation, this coefficient is not identified.

[22] To minimize the endogeneity problem, we use lagged values for size, age, leverage, ROA, and Tobin's Q. We obtain similar results if we use industry averages (lagged or contemporaneous). Unfortunately, lagged values are not available for the G-Index, E-Index, and Poison Pills since the data is only available from 1990 onwards. To mitigate the endogeneity problem, we use industry averages for the year 1990 and hold these values constant throughout the sample period. This implies, among other things, that we can use the G-index, E-index, and Poison Pills only interacted with the BC dummy but not as separate controls due to lack of "within" variation. Finally, note that poison pills were uncommon until the mid 1980s. This is not a concern, however, since Poison Pills is interacted with the BC dummy, which is always zero prior to 1985.

The coefficients on $BC \times Z$ and other controls are not reported for brevity. As one might expect, the coefficients on $BC \times Leverage$, $BC \times Size$, $BC \times G - Index$, $BC \times E - Index$ and $BC \times Poison\ Pills$ are all positive, albeit only the first three are significant. This is consistent with the casual impression that size, leverage, and firm-level takeover defenses act as partial substitutes to BC laws in deterring takeovers.[23] Importantly, however, the fact that some of these interaction terms are significant does not seem to affect much the coefficient on the interaction term $BC \times Herfindahl$, neither economically nor statistically.

Differences in Exit Rates

A possible alternative explanation for our results is that the passage of the BC laws caused a drop in operating performance for *all* firms, but in competitive industries firms experiencing a significant drop in profits went bankrupt and exited the industry, given that profit margins in such industries are likely small to begin with. Since the remaining (or surviving) firms in competitive industries are those that experienced no, or only a small, drop in operating performance, it might appear as if firms in competitive industries are seemingly unaffected by the passage of the BC laws.

To examine this hypothesis, we pooled all firms incorporated in treatment states in the year prior to the BC law ("benchmark sample") and then split the pooled sample into subsamples according to the Herfindahl index (terciles, quartiles, and quintiles). For each Herfindahl subsample, we computed exit rates by comparing how many of the firms present in the year before the BC law were still present in the year of the law, the year after the law, and so on. We repeated this exercise using time-varying benchmarks by comparing firms present in the year before the BC law with those present in the year of the law, firms present in the year of the law with those present in the year after the law, and so on. Irrespective of the method we used, the results were always similar: There appears to be no difference in exit rates across Herfindahl subsamples, suggesting that our results are not driven by differences in exit rates.[24]

[23] See Mueller and Panunzi (2004) for a model in which target firm leverage acts as a takeover deterrent.

[24] To gain further confidence, we collapsed our sample into state-year-Herfindahl cells by grouping all firms in a given year and state of incorporation into low-, medium-, and high-Herfindahl subsamples. For each cell, we then computed separate exit rates and performed a difference-in-difference estimation with exit rate as the dependent variable. Apart from the usual controls and state and year fixed effects, the independent variables included the BC dummy and interaction terms between the BC dummy and each of the three Herfindahl dummies. The null hypothesis that all three interaction terms are equal could not be rejected ($p-$value of 0.83).

Heterogeneous Time Trends and State Effects

Another alternative explanation for our results is that firms incorporated in BC states *and* operating in high-Herfindahl industries differ from the rest of the sample in other dimensions, e.g., they may be especially large. If in addition large firms experienced substantial negative shocks around the dates of the BC laws, then this could explain our results. To examine this hypothesis, we interacted each of the control variables (except the Herfindahl index) with time dummies. The results were always similar to those in Table III. In particular, the BC dummy was always close to zero and insignificant, while the interaction term between the BC dummy and the Herfindahl index was always negative and highly significant.

The above argument does not readily extend to the Herfindahl index. By construction, firms incorporated in BC states *and* operating in high-Herfindahl industries have an above average Herfindahl index. However, it might be possible that BC states have a disproportionately large share of high-Herfindahl firms, in which case our results could be explained by negative shocks to high-Herfindahl firms around the dates of the BC laws.[25] To test this hypothesis, we dropped the interaction term $BC \times Herfindahl$ from our specification and interacted the Herfindahl index with time dummies. If it is true that BC states have a disproportionately large share of high-Herfindahl firms and the latter experienced substantial negative shocks around the dates of the BC laws, then interacting the Herfindahl index with time dummies should render the BC dummy (economically and statistically) insignificant. However, this is not the case. In this modified specification, the BC dummy had a coefficient of -0.006 ($t-$statistic of 2.14), which is identical to the estimate in column [2] of Table III.[26]

To allow for heterogeneous state effects, we interacted a treatment state dummy with all control variables. The results were consistently similar to those in Table III. In particular, the BC dummy was always close to zero and insignificant, while the interaction term between the BC dummy and the Herfindahl index was remarkably stable with values ranging between -0.032 and -0.033 ($t-$statistics between 4.78 and 5.01).

Herfindahl Dummies

In columns [1] and [2] of **Table VI** we replace the continuous Herfindahl index with dummies indicating whether the Herfindahl index is above or below the median. We drop the BC dummy

[25] See Table II, however, showing that the average Herfindahl index in BC and non-BC states is almost identical.

[26] The BC dummy in column [2] of Table III has a coefficient of -0.006 ($t-$statistic of 2.25).

and one of the two Herfindahl dummies to avoid perfect multicollinearity. The results are similar to those in Table III. Whether or not we include control variables, the passage of the BC laws has no effect on firms in competitive industries (Herfindahl index below the median). By contrast, firms in non-competitive industries experience a significant drop in ROA between 1.0 and 1.1 percentage points.

In columns [3] and [4] we repeat this exercise using three Herfindahl dummies. The results are again similar. While the passage of the BC laws has no significant effect on firms in competitive industries, firms in less competitive industries (medium and top Herfindahl terciles) experience a significant drop in ROA. Note that, while monotonic, this relationship is not perfectly linear: The difference in ROA between the lowest and medium Herfindahl terciles is more than twice the difference between the medium and top terciles.

Alternative Competition Measures

In **Table VII** we consider alternative competition measures. Our main competition measure is the Herfindahl index based on 3-digit SIC codes. The 3-digit partition is a compromise between the coarse 2-digit partition, in which unrelated industries may be pooled together, and the narrow 4-digit partition, which may be subject to misclassification error. In columns [1] and [2] we verify that our results also hold for 2- and 4-digit SIC Herfindahl indices. The only major difference compared to Table III is that the coefficient on the 2-digit Herfindahl index as a control is not significant, which is due to lack of sufficient "within" variation.[27] In column [3] we use the 3-digit SIC Herfindahl index based on firms' assets in place of sales (see Hou and Robinson (2006)). The idea is that sales are rather volatile, with the effect that changes in the Herfindahl index based on sales may overstate actual changes in industry concentration. As can be seen, it makes little difference if we use asset- or sales-based Herfindahl indices.[28]

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[27] In contrast, the 3- and 4-digit SIC Herfindahl indices have sufficient "within" variation to allow the coefficient to be identified. Also, note that while the interaction terms in columns [1] and [2] of Table VII and column [4] of Table III have different coefficients, the average effect of the BC laws is the same. The mean values of the 2- and 4-digit SIC Herfindahl indices are 0.103 and 0.274, respectively. We can thus compute the average effect from columns [1] and [2] of Table VII as $-0.000 - 0.056 \times 0.103 = -0.006$ and $0.000 - 0.022 \times 0.274 = -0.006$, respectively, which is identical to the average effect in Table III.

[28] We obtain similar results using 2- and 4-digit SIC asset-based Herfindahl indices. Alternatively, we could use smoothed competition measures. If we run our basic regression using a 3-year moving average Herfindahl index, we obtain that the interaction term has a coefficient of -0.029 ($t-$statistic of 3.94), which is similar to the

In column [4] we use a margin-based competition measure: the median 3-digit SIC industry net profit margin (NPM). At the firm level, NPM is defined as operating income before depreciation and amortization (COMPUSTAT item #13) divided by sales (item #12). Industry NPM is commonly used in the industrial organization literature as an empirical proxy for the Lerner Index. The intuition is straightforward: monopolists and oligopolists can set prices in excess of marginal costs, which yields higher margins. As is shown, the results are similar to those in column [4] of Table III, which are based on the 3-digit SIC Herfindahl index. While the BC dummy is close to zero and insignificant, the interaction term between the BC dummy and Industry NPM is negative and significant. Note that Industry NPM as a control is positive and significant, which is consistent with what we found using the Herfindahl index. Finally, the average effect of the BC laws is also the same as in Table III. The sample mean of Industry NPM is 0.109, which implies an average effect of $0.000 - 0.054 \times 0.109 = -0.006$.

Non-Delaware and "Eventually Business Combination" Samples

In column [1] of **Table VIII** we exclude Delaware firms from the treatment group. Given that half of the firms in our sample are incorporated in Delaware, one might wonder if our results are driven by a single law. As can be seen, the results are similar to those in column [4] of Table III, except that the interaction term between the BC dummy and the Herfindahl index has a smaller $t-$statistic. This is likely due to differences in the samples. By excluding Delaware firms we lose about 58% of the observations in the treatment group, which considerably reduces the number of observations available for identifying the coefficient on the interaction term.[29]

In column [2] we exclude firms incorporated in states that did not pass a BC law during the sample period ("Never Business Combination"), implying that the control group consists only of firms incorporated in treatment states that have not yet passed a BC law. As can be seen, it makes little difference if we exclude those firms from the sample.

Alternative Performance Measures

Table IX considers alternative performance measures. Column [1] considers ROA after depreciation, which is defined as operating income after depreciation and amortization (EBIT,

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estimate in column [4] of Table III. The coefficient on the BC dummy is again close to zero and insignificant.

[29] While Delaware firms are excluded from the treatment group, they remain in the control group until 1988, when the Delaware BC law was passed. (See Section 2.3 regarding the empirical methodology). As a result, the sample size in column [1] of Table VIII is much larger than one half the sample size in Table III.

COMPUSTAT item #178) divided by total assets (item #6). The correlation between ROA before and after depreciation is 97%. Accordingly, it does not surprise that the results are similar to those in Table III. In columns [2] and [3] we consider return on sales (ROS), which is defined as operating income before depreciation and amortization (EBITDA, item #13) divided by sales (item #12), and return on equity (ROE), which is defined as net income (item #172) divided by common equity (item #60). While the results are similar to those in Table III, they are somewhat weaker. In particular, the interaction term between the BC dummy and the Herfindahl index has a smaller $t-$statistic than in column [4] of Table III. That the results are weaker does not surprise. ROS and, especially, ROE, are often viewed as less suitable measures of firm performance compared to ROA.

Manufacturing Industries

Table X focuses on manufacturing industries (SIC 2000-3999). For these industries only, the U.S. Bureau of the Census provides a Herfindahl index that includes both public and private firms. While the Census Herfindahl index is broader than the Herfindahl index computed from COMPUSTAT, it entails some limitations. First, the index is only available for the years 1982, 1987, and 1992 during the sample period. To fill in the missing years, we always use the index value from the latest available year. For the years prior to 1982, we use the index value from 1982. Second, the index is only available on the narrow 4-digit SIC code level, which implies that it is likely subject to misclassification (see Section 2.2). Third, the index is only available for manufacturing industries, which implies that the sample is much smaller. Fourth, there is no match for COMPUSTAT firms whose 4-digit SIC code ends with a “zero”, which further reduces the available sample size.[30]

In column [1] we re-estimate our basic specification for manufacturing industries only using the Herfindahl index based on 3-digit SIC codes computed from COMPUSTAT. The results are similar to those in column [4] of Table III, except that the interaction term between the BC dummy and the Herfindahl index has a smaller $t-$statistic. This is likely due to two reasons. First, as discussed above, the sample is considerably smaller. Second, there is less variation in the Herfindahl index within manufacturing industries, which makes it more difficult to identify the coefficient on the interaction term.

[30] 4-digit COMPUSTAT industries ending with a “zero” are effectively 3-digit industries.

In column [2] we estimate the same specification using the Census Herfindahl index.[31] The results are similar to those in Table III, except that the interaction term between the BC dummy and the Herfindahl index has a larger coefficient and smaller $t-$statistic. The smaller $t-$statistic is likely due to the same reasons as above. And while the coefficient is larger, its economic significance is similar to Table III. The Census Herfindahl index has a mean of 0.058 and a standard deviation of 0.046. We can thus compute the average effect of the BC laws from column [2] as $-0.003 - 0.081 \times 0.058 = -0.008$, which implies that ROA decreases by 0.8 percentage points on average. Likewise, we can compute the managerial-incentive effect as $-0.081 \times 0.046 = -0.004$, which implies that an increase in the Census Herfindahl index by one standard deviation is associated with a decrease in ROA of 0.4 percentage points. Both estimates are similar to those in Table III.

Whether we use the Herfindahl index computed from COMPUSTAT or that from the Census Bureau, we only capture domestic competition. To measure competition from foreign companies, we use data on import penetration (imports divided by the sum of domestic shipments plus imports minus exports). The data is from Peter Schott's website and is described in Feenstra (1996) and Feenstra, Romalis, and Schott (2002). Using import penetration as a competition measure entails some limitations. First, the data is only available on the narrow 4-digit SIC code level, which implies that it is likely subject to misclassification. Second, for the same reasons as above, the sample is much smaller. Third, and perhaps most important, it is not clear that import penetration is a suitable measure of competition. For example, import penetration may be high yet an industry may be non-competitive because all imports come from a few producers. Likewise, import penetration may be low yet an industry may be highly competitive because domestic competition is fierce. In fact, import penetration may be low *because* domestic competition is fierce.

In column [3] we estimate our basic specification using a dummy indicating whether import penetration is above or below the sample mean.[32] We use one minus the import penetration dummy as our competition measure to make it comparable to the Herfindahl index and industry net profit margin, both of which are decreasing in competition. The results are similar to those

[31] The Herfindahl index as a control variable is omitted in column [2]. Except for two "jumps" in 1982 and 1987 the Census Herfindahl index is a constant, which implies that the coefficient is not well identified.

[32] We use a dummy because the import penetration data exhibits relatively little variation. We obtain similar albeit statistically weaker results if we use a continuous import penetration measure.

in Table III, except that the interaction term between the BC dummy and import penetration has a smaller $t-$statistic. The smaller $t-$statistic is likely due to two reasons. First, the sample is much smaller. Second, and equally important, import penetration may simply be a poor measure of competition. As we argued above, there need be no relation between import penetration and the competitiveness of an industry. Finally, the mean of one minus the import penetration dummy is 0.602. Hence, we can compute the average effect of the BC laws from column [3] as $-0.004 - 0.007 \times 0.602 = -0.008$, which is the same as in column [2].

Perhaps the most meaningful way to use import penetration as a competition measure is in connection with the domestic Herfindahl index. In columns [4] and [5] we include import penetration along with Herfindahl index computed from COMPUSTAT and that from the Census Bureau, respectively. In those specifications, the BC dummy measures the effect of the BC laws on firms in industries with *both* high domestic competition *and* high import penetration, which is arguably a more complete way of capturing the essence of highly competitive industries. In both regressions, the results are similar to our previous results. The BC dummy is again close to zero and insignificant, while the interaction term between the BC dummy and the Herfindahl index is negative and significant. On the other hand, the interaction term between the BC dummy and import penetration is no longer significant, even though the magnitude of the coefficient is similar to column [3]. Hence, while both domestic and foreign competition may play an important role, the former appears to have a stronger impact on managerial incentives.

Cross-Sectional and Serial Correlation

We cluster standard errors at the state of incorporation level to account for the presence of cross-sectional and serial correlation of the error terms. Cross-sectional correlation is a concern because all firms in a given year and state of incorporation are affected by the same "shock" (namely, the passage of the BC law). Serial correlation is a concern because the BC dummy changes little over time, being zero before and one after the passage of the BC law. Not accounting for either issue can lead to a serious understatement of the standard errors. Simulation-based studies that compare different correction methods show that clustering does very well (Bertrand, Duflo, and Mullainathan (2004), Petersen (2007)). This is especially true if the number of clusters is large, as is the case here (51 clusters). Given that the BC dummy is a likely source of both cross-sectional and serial correlation, it is natural to cluster at the state of incorporation level. However, our results also hold if we cluster at the state of location level.

Table XI considers alternative methods to account for the presence of cross-sectional and serial correlation of the error terms. The methods are all described in Bertrand, Duflo, and Mullainathan (2004). Columns [1] to [3] deal with serial correlation, while column [4] deals with cross-sectional correlation.

The first correction method is a parametric one. We assume that the error term follows an AR(1) process and estimate the first-order autocorrelation coefficient by regressing the residuals from our basic regression in column [4] of Table III on their lagged values. We then form an estimate of the covariance matrix of the residuals and estimate our basic specification using GLS. As column [1] shows, the results are similar to those in column [4] of Table III. In particular, the coefficient on the BC dummy is close to zero and insignificant, while the coefficient on the interaction term is significant with a value similar to that in Table III. It should be noted, however. that parametric correction methods perform rather poorly in simulations (Bertrand, Duflo, and Mullainathan (2004)). The following methods are all non-parametric.

The second method is block bootstrapping. According to Bertrand, Duflo, and Mullainathan (2004), this method constitutes a reliable solution to the serial correlation problem if the number of blocks is sufficiently large, which is the case here (51 blocks). We construct a large number (200) of bootstrap samples by drawing with replacement 51 states of incorporation from our sample. For each bootstrap sample, we estimate our basic specification using OLS and compute for each covariate the absolute $t-$statistic $t_r := abs[(\hat{\beta}_r - \hat{\beta})/SE(\hat{\beta}_r)]$, where $\hat{\beta}$ is the estimated coefficient from column [4] of Table III, and where $\hat{\beta}_r$ is the estimated coefficient from the r^{th} bootstrap. We compute $p-$values as the relative frequency that t_r is larger than t, where $t := abs[\hat{\beta}/SE(\hat{\beta})]$ is the absolute $t-$statistic from the OLS estimation of the specification in column [4] of Table III. Since the $p-$values refer to the significance of the original coefficients from Table III, we again report those coefficients. We reject the null of a zero coefficient at the 95 percent confidence level if 95 percent of the t_r values are smaller than t. As column [2] shows, the results are again similar to those in column [4] of Table III.[33]

The third method is to collapse the data into two periods, before and after the BC law, and run an OLS regression on this two-period panel. Bertrand, Duflo, and Mullainathan (2004) find that this method performs well in simulations. Of course, it is rather crude. By collapsing 20

[33] We have also computed the mean and median values of each coefficient based on the 200 bootstraps. The values are very close to those in column [4] of Table III.

years of data into two periods we lose many observations, which reduces the power of our tests. What is more, due to the staggering of the BC laws over time, "before" and "after" are not the same for each treatment state. And for control states, "before" and "after" are not even defined. To address these issues, we use the two-step procedure by Bertrand, Duflo, and Mullainathan (2004). In the first step, we regress ROA on fixed effects and covariates, except for the BC dummy and the interaction term between the BC dummy and the Herfindahl index. For treatment states only, we then collect the residuals and compute the average residuals for the pre- and post-BC law periods. This provides us with a two-period panel, where the first period is before the law and the second period is after the law. In the second step, we then regress the average residuals on the BC dummy and the interaction term between the BC dummy and the average post-BC Herfindahl index. We use White standard errors to correct for heteroskedasticity. As column [3] shows, the results are similar to those in column [4] of Table III.[34] While the coefficient on the BC dummy is close to zero and insignificant, the coefficient on the interaction term between the BC dummy and the Herfindahl index is negative and significant.

The fourth correction method, which deals with the issue of cross-sectional rather than serial correlation, is to collapse the data into state of incorporation-industry-year cells.[35] The idea is that our main variables of interest, the BC dummy and the Herfindahl index, are of a higher level of aggregation, namely, the state of incorporation level and the industry level, respectively. The drawback of this method is that we again lose many observations, which reduces the power of our tests. Similar to the method in column [3], we proceed in two steps. In the first step, we regress ROA on time dummies and covariates, except for the BC dummy and the interaction term between the BC dummy and the Herfindahl index. We then collect the residuals and compute the average residual for each state of incorporation-industry-year portfolio. In the second step, we regress the average residuals on portfolio fixed effects, the BC dummy, and the interaction term between the BC dummy and the Herfindahl index. We use White standard errors to correct for heteroskedasticity. As column [4] shows, the results are again similar to those in column [4] of Table III.

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[34] Note that the dependent variable in columns [3] and [4] is not ROA but the average residual from the respective first-stage regressions. The coefficients are thus not comparable to those in column [4] of Table III.

[35] This method is described in footnote 14 of Bertrand, Duflo, and Mullainathan (2004). It is essentially the same method as in column [3], except that it is applied to the cross-section instead of the time-series.

Empire Building or Quiet Life?

While our results suggest that competition mitigates managerial agency problems, they do not say which agency problem is mitigated. Does competition curb managerial empire building? Or does it prevent managers from enjoying a "quiet life" as suggested by Bertrand and Mullainathan (2003)? To investigate the first possibility, we estimate our basic specification using capital expenditures (COMPUSTAT item #30) divided by total assets (item #6) as the dependent variable. To investigate the second possibility, we use a number of different dependent variables: selling, general, and administrative expenses ("overhead costs", item #189) and R&D expenses (item #46), both divided by total assets, advertising expenses (item #45) and costs of goods sold ("input costs", item #41), both divided by sales (item #12), and real wages, defined as the natural logarithm of labor and related expenses (item #42) divided by the number of employees (item #29) and deflated by the Consumer Price Index (CPI). The idea is that, in order to keep the firm's costs low, managers must haggle with labor unions and input suppliers and resist pressure from individual units within the organization demanding bigger overhead, advertising, and R&D budgets.

The results are shown in **Table XII**. Column [1] considers the effect of the BC laws on capital expenditures. As it turns out, there is no effect. Neither the BC dummy nor the interaction term between the BC dummy and the Herfindahl index is significant, neither individually nor jointly.[36] The remaining results in Table XII are mixed. Columns [3] and [4] consider the effect of the BC laws on advertising and R&D expenses, respectively. While the coefficients associated with the interaction term have the right sign, they are not significant.

Columns [2], [5], and [6] show the effect of the BC laws on selling, general, and administrative expenses, costs of goods sold, and real wages, respectively. In all three columns, the pattern is similar to our ROA regressions. While the coefficient on the BC dummy is close to zero and insignificant, the coefficient on the interaction term between the BC dummy and the Herfindahl index is positive and significant. (The sign of the coefficient is the opposite as in our ROA regressions because the dependent variables are negatively related to ROA). Hence, while the BC laws have no significant effect on firms in competitive industries, they cause a significant increase in overhead costs, input costs, and wages for firms in non-competitive industries. These

[36] The $F-$test that the two variables are jointly significant has a $p-$value of 0.82. Another way to test whether the BC laws have a significant effect on capital expenditures is to run the same regression as in column [1] without the interaction term. In that regression, the BC dummy has a coefficient of -0.000 ($t-$statistic of -0.24).

results are consistent with a "quiet-life" hypothesis whereby managers insulated from *both* hostile takeovers *and* competitive pressure seek to avoid cognitively difficult activities, such as haggling with input suppliers, labor unions, and individual units within the organization demanding bigger overhead budgets.

We conclude with two caveats. First, the $t-$statistics in columns [2], [5], and [6] are smaller than in our previous ROA regressions, presumably because the dependent variables are individual components of ROA. That is, while the passage of the BC laws may have a small effect on any individual component of ROA, its overall effect on ROA may be substantial. Second, the wage result in column [6] should be taken with caution. For one thing, the sample is rather small, which is due to the fact that only few firms in COMPUSTAT report wage data. More important, however, the COMPUSTAT wage data is very noisy.[37] For example, some firms report wage data only intermittently, while others report no data at all. What is more, COMPUSTAT only provides aggregate data on labor and related expenses, which also includes pension costs, payroll taxes, and employee benefits, to name just a few. On a positive note, our wage results are consistent with Bertrand and Mullainathan (1999, 2003), who report wage increases between 1% and 2% after the passage of the BC laws. In our case, we can compute the average wage increase (i.e., across all industries) from column [6] as $-0.003 + 0.103 \times 0.218 = 0.019$, or 1.9%, which is of the same order of magnitude.[38]

4 Event-Study Results

Does the stock market anticipate that firms in competitive industries are largely unaffected by the passage of the BC laws? The main difficulty in answering this question lies in the choice of event date. Since the passage of the BC laws is likely well anticipated, the passage date itself is unlikely to contain much new information. Rather, one must find an early date at which significant news about the law is disseminated to the public, e.g., the date of the first newspaper report about the BC law. For instance, consider the event study by Karpoff and Malatesta (1989), who examine the stock price impact of 40 antitakeover laws, including 11 BC laws, from

[37] See Bertrand and Mullainathan (1999) for a discussion of the COMPUSTAT wage data.

[38] The Herfindahl index in column [6] has a mean of 0.218, which is slightly different from the mean of 0.226 in our ROA regressions because of differences in the sample size.

1982 to 1987.[39] The authors find no significant abnormal returns when using either the date of the law's introduction in the state legislature, its final passage, or its signing by the governor as the relevant event date. However, they do find significant abnormal returns when using the first date on which they found a newspaper report about the law as the event date.

Finding the first newspaper report about a law is often a formidable task. Electronic archives of local newspapers often do not go back to the 1980s, and larger out-of-state newspapers such as the *Wall Street Journal* and the *New York Times* often provide no coverage, especially if the state in question is small and only few firms are incorporated there. After a careful search of all major newspaper databases (ProQuest, Lexis-Nexis, Factiva, Newsbank America's Newspapers, Google News Archive), we could find newspaper reports for 19 of the 30 BC laws in our sample: Arizona, Connecticut, Delaware, Georgia, Illinois, Kentucky, Maryland, Massachusetts, Minnesota, New Jersey, New York, Ohio, Oklahoma, Pennsylvania, South Carolina, Tennessee, Virginia, Washington, and Wisconsin.[40] Most of the remaining 11 states are small in terms of number of incorporated firms. In fact, seven of them had fewer than 20 firms—and only one had more than 100 firms (Nevada)—in the merged CRSP-COMPUSTAT sample during the year in which the BC law was passed. Based on the numbers in Table I, the 19 states for which we found newspaper reports represent 92% of all firms incorporated in states that passed a BC law during the sample period.

The event-study methodology is based on the assumption that the events are independent (MacKinlay (1997, p. 27)). While this assumption is satisfied in many applications where

[39] Most other event studies focus on a single antitakeover law; see Table 2 in Bhagat and Romano (2002).

[40] The dates of the newspaper reports are as follows: Arizona on July 27, 1987 (*Arizona Business Gazette*); Connecticut on February 7, 1988 (*New Haven Register*); Delaware on June 1, 1987 (*New York Times*, see also Jahera and Pugh (1991, p. 415)); Georgia on April 23, 1987 (*The Atlanta Journal-Constitution*); Illinois on November 30, 1988 (*Chicago Sun-Times*); Kentucky on March 28, 1986 (*Lexington Herald-Leader*); Maryland on February 5, 1988 (*Washington Post*); Massachusetts on February 5, 1989 (*Boston Globe*); Minnesota on June 19, 1987 (*Star Tribune*); New Jersey on March 25, 1986 (*The Record*); New York on June 26, 1985 (*New York Times*, see also Schuman (1988, p. 563)); Ohio on April 6, 1990 (*Dayton Daily News*); Oklahoma on March 7, 1991 (*The Journal Record*); Pennsylvania on February 17, 1988 (*Philadelphia Inquirer*); South Carolina on April 17, 1988 (*The State*); Tennessee on January 25, 1988 (*Memphis Business Journal*); Virginia on February 8, 1988 (*Richmond Times-Dispatch*); Washington on July 29, 1987 (*Seattle Times*, see also Karpoff and Malatesta (1989, p. 315)); Wisconsin on September 10, 1987 (*Star Tribune*). If the newspaper report was published on a non-trading day, we specify the next trading day as the event date.

the event in question is firm-specific, such as earnings or dividend announcements, it is clearly violated in our setting. Since all firms incorporated in the same state are affected by the same event, their abnormal returns are likely correlated. As a result, standard errors will be biased, leading to incorrect inferences (see Bernard (1987)). The common way to address this problem is to form portfolios consisting of all firms incorporated in a given state. Since the event dates are different for each state portfolio, the issue of cross-sectional correlation becomes negligible (Karpoff and Malatesta (1989), MacKinlay (1997)).

Our empirical methodology is similar to Karpoff and Malatesta (1989). For each state portfolio j, we estimate the market model using CRSP daily return data from 241 to 41 trading days prior to the event date.[41] Precisely, we estimate the parameters α_j and β_j of the equation

$$R_{jt} = \alpha_j + \beta_j R_{mt} + e_{jt}, \tag{2}$$

using OLS, where R_{jt} is the daily return of the equally-weighted portfolio of firms incorporated in state j, and R_{mt} is the daily return of the equally-weighted CRSP market portfolio. Substituting the estimates back into (2), we obtain an estimate of the normal portfolio return $\hat{R}_{jt}$. The abnormal return of state portfolio j can then be calculated as

$$AR_{jt} := R_{jt} - \hat{R}_{jt}.$$

To obtain cumulative abnormal returns (CAR), we sum the abnormal returns over the desired time interval. We report CARs for the same time intervals as Karpoff and Malatesta (1989): [-40,-2], [-3,-2], [-1,0], [1,2], and [1,10], where [-1,0] is the two-day event window. To see if there had been any trend in the weeks preceding the event date, we also report CARs for the time intervals [-30,-2], [-20,-2], and [-10,-2].

The methodology described above yields an estimate of the average impact of the BC laws on stock prices. To examine if the price impact is different for firms in competitive and non-competitive industries, we subdivide each state portfolio into smaller portfolios. For each state j we form low- and high-Herfindahl portfolios representing those firms whose Herfindahl index

[41] Choosing the estimation window adjacent to the first time interval for which cumulative abnormal returns are computed (here: the time interval [-40,-2]) is standard practice (e.g., MacKinlay (1997, p. 19)). However, we obtain similar results if we estimate the market model over the interval from 300 to 100 trading days before the event date. The market model is the most common statistical model to calculate normal returns at daily frequency. However, we also obtain similar results using either a 3- or 4-factor model to estimate returns.

lies below and above the median, respectively. We also do the same with low-, medium-, and high-Herfindahl portfolios representing those firms whose Herfindahl index lies in the lowest, medium, and highest tercile, respectively. The remaining steps are the same as above.

Column [1] of **Table XIII** shows the average CARs based on the 19 state portfolios. The average CAR for the two-day event window is -0.32% with a $z-$statistic of -2.58, which has a $p-$value of 0.010. Furthermore, 14 of the 19 average two-day CARs are negative. While our number is smaller than the average two-day CAR of -0.47% reported by Karpoff and Malatesta (1989, Panel (A) of Table 5), it is of the same order of magnitude.[42] Moreover, the average two-day CARs immediately before and after the event window are small and insignificant. This indicates that, on average, newspaper reports about the BC laws are associated with a significant decrease in stockholder wealth.

Columns [2] and [3] show the average CARs for the low- and high-Herfindahl portfolios. The average CAR for the low-Herfindahl portfolio in the two-day event window is close to zero and insignificant. By contrast, the average CAR for the high-Herfindahl portfolio in the two-day event window is -0.54% with a $z-$statistic of -2.36, which has a $p-$value of 0.018. Hence, while firms in competitive industries experience no significant stock price impact around the dates of the first newspaper reports about the BC laws, firms in non-competitive industries experience a significant abnormal stock price decline.

Columns [4] to [6] show the average CARs for the low-, medium-, and high-Herfindahl portfolios. The results are again similar. While firms in competitive industries experience no significant stock price impact, firms in less competitive industries (medium and top Herfindahl terciles) experience a significant abnormal stock price decline. Interestingly, and consistent with our results in Table VI, the relationship between the CARs and the Herfindahl index is monotonic. The average CAR for the medium-tercile portfolio in the two-day event window is -0.44% with a $z-$statistic of -1.67, which has a $p-$value of 0.095. By contrast, the average CAR for the top-tercile portfolio in the two-day event window is -0.67% with a $z-$statistic of -2.31, which has a $p-$value of 0.021.[43]

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[42] That our number is smaller is likely due to differences in the sample size: We have 19 BC laws while Karpoff and Malatesta (1989) have 11 BC laws. Moreover, among those 11 BC laws is California even though the legislation there never became law. California is not included in our sample.

[43] The average CAR for the low-Herfindahl portfolio in the two-day event window is 0.08% with a $z-$statistic of -0.53. We obtain a similarly monotonic pattern using median CARs. The median CARs for the low-, medium-,

5 Conclusion

Does competition mitigate managerial agency problems? And moreover, is there merit to the stronger view by Alchian (1950), Friedman (1953), and Stigler (1958) that competitive industries leave no room for managerial slack? The evidence presented in this paper suggests that the answer to both questions is yes. Using the passage of 30 business combination (BC) laws as a source of exogenous variation in corporate governance, we examine if these laws have a different impact on firms in competitive and non-competitive industries. Consistent with the notion that BC laws weaken corporate governance and create more opportunity for managerial slack, we find that firms' operating performance drops significantly on average after the passage of the BC laws. Most important, we find that this drop in operating performance is exclusively driven by non-competitive industries. By contrast, firms in competitive industries remain virtually unaffected by the passage of the BC laws, which is consistent with the Alchian-Friedman-Stigler argument that competitive industries have no tolerance for managerial slack.

Our results have several important implications. For one thing, our results imply that efforts to improve corporate governance could benefit from focusing primarily on non-competitive industries. For another, our results imply that empirical studies on corporate governance could benefit from including, or conditioning on, measures of industry competition. The empirical relationships documented in such studies might be stronger, both economically and statistically, for firms in non-competitive industries. For example, preliminary research by the authors suggests that the positive alpha generated by the democracy-dictatorship hedge portfolio in Gompers, Ishii, and Metrick (GIM, 2003) is largely driven by non-competitive industries (Giroud and Mueller (2007)). In fact, after dividing the sample into low-, medium- and high-Herfindahl portfolios it turns out that the alpha in the low-Herfindahl portfolio is small and insignificant, whereas that in the high-Herfindahl portfolio is large and highly significant. Since the alpha reported in GIM averages across all three portfolios, this means that the alpha in the high-Herfindahl portfolio is much higher than the alpha reported in GIM. While these preliminary findings are encouraging, more research is needed before we can conclude that firm-level corporate governance instruments are moot in competitive industries.

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and high-Herfindahl portfolios are 0.06%, −0.46%, and −0.67%, respectively. The corresponding ratios of positive to negative CARs for the two-day event window are 10:9, 4:15, and 5:14, respectively.

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Table I
States of Incorporation and States of Location

"BC year" indicates the year in which a business combination (BC) law was passed. "State of location" indicates the state in which a firm's headquarters are located. BC years are from Bertrand and Mullainathan (2003). States of location and states of incorporation are both from COMPUSTAT. The sample period is from 1976 to 1995.

State	BC Year	State of Incorporation	State of Location	Number (Percentage) of Firms Incorporated in:		
		Number of Firms	Number of Firms	State of Location	Delaware	Other States
Delaware	1988	5,587	39	35 (89.7%)		4 (10.3%)
California		529	1,711	489 (28.6%)	1,034 (60.4%)	188 (11.0%)
New York	1985	515	1,129	366 (32.4%)	673 (59.6%)	90 (8.0%)
Nevada	1991	302	97	55 (56.7%)	28 (28.9%)	14 (14.4%)
Florida		290	584	240 (41.1%)	261 (44.7%)	83 (14.2%)
Minnesota	1987	287	342	243 (71.1%)	88 (25.7%)	11 (3.2%)
Massachusetts	1989	280	527	236 (44.8%)	253 (48.0%)	38 (7.2%)
Colorado		266	363	160 (44.1%)	147 (40.5%)	56 (15.4%)
Pennsylvania	1989	264	428	219 (51.2%)	169 (39.5%)	40 (9.3%)
Texas		263	951	240 (25.2%)	555 (58.4%)	156 (16.4%)
New Jersey	1986	255	585	194 (33.2%)	305 (52.1%)	86 (14.7%)
Ohio	1990	224	375	198 (52.8%)	151 (40.3%)	26 (6.9%)
Maryland	1989	197	200	82 (41.0%)	103 (51.5%)	15 (7.5%)
Georgia	1988	142	277	123 (44.4%)	121 (43.7%)	33 (11.9%)
Virginia	1988	137	243	106 (43.6%)	103 (42.4%)	34 (14.0%)
Michigan	1989	120	209	109 (52.2%)	81 (38.8%)	19 (9.1%)
Indiana	1986	119	144	97 (67.4%)	41 (28.5%)	6 (4.2%)
Utah		111	97	60 (61.9%)	29 (29.9%)	8 (8.2%)
Washington	1987	102	149	87 (58.4%)	44 (29.5%)	18 (12.1%)
Wisconsin	1987	94	124	86 (69.4%)	34 (27.4%)	4 (3.2%)
North Carolina		92	173	85 (49.1%)	66 (38.2%)	22 (12.7%)
Missouri	1986	80	169	60 (35.5%)	92 (54.4%)	17 (10.1%)
Oregon		69	89	61 (68.5%)	15 (16.9%)	13 (14.6%)
Tennessee	1988	67	134	59 (44.0%)	54 (40.3%)	21 (15.7%)
Oklahoma	1991	58	121	45 (37.2%)	58 (47.9%)	18 (14.9%)
Illinois	1989	57	444	47 (10.6%)	353 (79.5%)	44 (9.9%)
Connecticut	1989	56	307	48 (15.6%)	209 (68.1%)	50 (16.3%)
Arizona	1987	39	152	35 (23.0%)	76 (50.0%)	41 (27.0%)
Iowa		38	67	31 (46.3%)	27 (40.3%)	9 (13.4%)
Louisiana		35	67	30 (44.8%)	30 (44.8%)	7 (10.4%)
South Carolina	1988	35	77	34 (44.2%)	37 (48.1%)	6 (7.8%)
Kansas	1989	34	70	26 (37.1%)	33 (47.1%)	11 (15.7%)
Kentucky	1987	29	67	28 (41.8%)	31 (46.3%)	8 (11.9%)
Rhode Island	1990	18	37	14 (37.8%)	18 (48.6%)	5 (13.5%)
Wyoming	1989	18	13	7 (53.8%)	1 (7.7%)	5 (38.5%)
Mississippi		16	47	15 (31.9%)	21 (44.7%)	11 (23.4%)
New Mexico		15	26	9 (34.6%)	10 (38.5%)	7 (26.9%)
Maine	1988	13	14	5 (35.7%)	8 (57.1%)	1 (7.1%)
New Hampshire		13	47	11 (23.4%)	28 (59.6%)	8 (17.0%)
Hawaii		12	20	8 (40.0%)	9 (45.0%)	3 (15.0%)
Alabama		10	67	9 (13.4%)	54 (80.6%)	4 (6.0%)
District of Columbia		10	30	4 (13.3%)	22 (73.3%)	4 (13.3%)
Idaho	1988	10	16	2 (12.5%)	11 (68.8%)	3 (18.8%)
Arkansas		9	35	9 (25.7%)	20 (57.1%)	6 (17.1%)
Nebraska	1988	9	29	8 (27.6%)	18 (62.1%)	3 (10.3%)
West Virginia		8	19	7 (36.8%)	9 (47.4%)	3 (15.8%)
Montana		7	13	7 (53.8%)	4 (30.8%)	2 (15.4%)
Vermont		7	16	6 (37.5%)	9 (56.3%)	1 (6.3%)
Alaska		6	6	4 (66.7%)	2 (33.3%)	0 (0.0%)
South Dakota	1990	4	10	4 (40.0%)	5 (50.0%)	1 (10.0%)
North Dakota		2	4	1 (25.0%)	2 (50.0%)	1 (25.0%)
Total		10,960	10,960	4,144 (37.8%)	5,552 (50.7%)	1,264 (11.5%)

Table II
Summary Statistics

"All States" refers to all states in Table I. "Eventually Business Combination" refers to all states that passed a BC law during the sample period. "Never Business Combination" refers to all states that did not pass a BC law during the sample period. Size is the natural logarithm of total assets (COMPUSTAT item #6). Age is the natural logarithm of one plus the number of years the firm has been in COMPUSTAT. Leverage is long-term debt (item #9) plus debt in current liabilities (item #34) divided by total assets. Herfindahl is the Herfindahl-Hirschman index, which is computed as the sum of squared market shares of all firms in a given 3-digit SIC industry. Market shares are computed from COMPUSTAT using sales (item #12). E-Index is the entrenchment index by Bebchuk, Cohen, and Ferrell (2005) and is obtained from Lucian Bebchuk's webpage. G-index is the governance index by Gompers, Ishii, and Metrick (2003) and is obtained from the IRRC database. Both indices are available for the years 1990, 1993 and 1995 during the sample period. All figures are sample means. Standard deviations are in parentheses. The sample period is from 1976 to 1995.

	All States	Eventually Business Combination	Never Business Combination
	[1]	[2]	[3]
Size	4.450	4.585	3.629
	(2.283)	(2.270)	(2.185)
Age	2.252	2.293	2.002
	(0.918)	(0.924)	(0.837)
Leverage	0.263	0.264	0.256
	(0.391)	(0.388)	(0.407)
Herfindahl	0.225	0.226	0.214
	(0.155)	(0.156)	(0.148)
E-Index	2.304	2.319	2.127
	(1.381)	(1.371)	(1.479)
G-Index	9.342	9.498	7.450
	(2.883)	(2.828)	(2.869)

Table III
Does Corporate Governance Matter in Competitive Industries?

Return on assets is operating income before depreciation and amortization (COMPUSTAT item #13) divided by total assets (item #6). BC is a dummy variable that equals one if the firm is incorporated in a state that passed a BC law during the sample period. Before(-2) and Before(-1) are dummy variables that equal one if the firm is incorporated in a state that will pass a BC law in two years and one year from now, respectively. Before(0) is a dummy variable that equals one if the firm is incorporated in a state that passes a BC law this year. After(1) and After(2+) are dummy variables that equal one if the firm is incorporated in a state that passed a BC law one year and two or more years ago, respectively. "State-year" and "industry-year" refers to the mean of the dependent variable in the firm's state of location and industry, respectively, in that year, excluding the firm itself. All other variables are defined in Table II. Standard errors are clustered at the state of incorporation level. The sample period is from 1976 to 1995. *t*-statistics are in parentheses. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	Dependent Variable: Return on Assets				
	[1]	[2]	[3]	[4]	[5]
BC	-0.006**	-0.006**	-0.001	0.001	0.001
	(2.41)	(2.25)	(0.32)	(0.35)	(0.13)
BC x Herfindahl			-0.025***	-0.033***	
			(4.93)	(4.95)	
Industry-year		0.206***		0.206***	0.206***
		(9.67)		(9.60)	(9.44)
State-year		0.249***		0.249***	0.249***
		(8.86)		(8.83)	(8.88)
Size		0.096***		0.097***	0.097***
		(20.27)		(20.38)	(20.33)
Size-squared		-0.007***		-0.007***	-0.007***
		(20.09)		(20.42)	(20.41)
Age		-0.021***		-0.021***	-0.021***
		(5.34)		(5.44)	(5.41)
Herfindahl		0.015		0.025***	0.026***
		(1.66)		(2.58)	(2.90)
Before(-2) x Herfindahl					-0.003
					(0.34)
Before(-1) x Herfindahl					-0.016
					(0.97)
Before(0) x Herfindahl					-0.039***
					(2.85)
After(1) x Herfindahl					-0.041***
					(4.15)
After(2+) x Herfindahl					-0.033***
					(4.24)
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	79,474	77,460	77,481	77,460	77,460
Adj. R-squared	0.66	0.68	0.66	0.68	0.68

Table IV
Lagged Herfindahl Indices and Pre-1984 Herfindahl Index

All variables are defined in Tables II and III. Standard errors are clustered at the state of incorporation level. The sample period is from 1976 to 1995. *t*-statistics are in parentheses. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	Dependent Variable: Return on Assets		
	Lagged Herfindahl (1-year Lag)	Lagged Herfindahl (2-year Lag)	Average Herfindahl 1976-1984
	[1]	[2]	[3]
BC	0.001	-0.000	0.003
	(0.19)	(0.05)	(0.73)
BC x Herfindahl	-0.029***	-0.024***	-0.028***
	(4.63)	(3.45)	(4.82)
Industry-year	0.204***	0.204***	0.206***
	(9.63)	(9.76)	(10.92)
State-year	0.244***	0.248***	0.252***
	(8.54)	(8.81)	(9.09)
Size	0.097***	0.097***	0.096***
	(20.42)	(20.39)	(20.60)
Size-squared	-0.007***	-0.007***	-0.007***
	(20.28)	(20.02)	(20.63)
Age	-0.021***	-0.021***	-0.021***
	(5.14)	(5.33)	(5.09)
Herfindahl	0.035***	0.032***	
	(3.88)	(3.61)	
Firm Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Observations	77,385	77,273	77,123
Adj. R-squared	0.68	0.68	0.68

Table V
"Horse Races"

Size, age, ROA, and leverage are defined in Tables II and III. Tobin's Q is the market value of total assets divided by the book value of total assets (COMPUSTAT item #6). The market value of total assets is the book value of total assets plus the market value of equity (item #24 times item #25) minus the sum of the book value of equity (item #60) and balance sheet deferred taxes (item #74). All these variables are lagged by one fiscal year. G-Index and E-Index are defined in Table II. Poison Pills is a dummy variable that equals one if the firm has a poison pill. The poison pills data is obtained from the IRRC database. G-Index, E-Index, and Poison Pills are computed in 1990 at the industry level and held constant throughout the sample period. All regressions have ROA as the dependent variable and, except for the additional controls BC x Z and Z, the same controls as in Table III. The coefficient in the first row is computed as the sum of the coefficient on BC and the coefficient on the interaction term between BC and Z multiplied by the mean value of Z. The numbers in brackets are *p*-values for the *F*-test of the null hypothesis that the sum of the coefficient on BC and the coefficient on the interaction term between BC and Z multiplied by the mean value of Z is zero. Standard errors are clustered at the state of incorporation level. The sample period is from 1976 to 1995. *t*-statistics are in parentheses. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

Z	Size (Lagged)	Age (Lagged)	Tobin's Q (Lagged)	ROA (Lagged)	Leverage (Lagged)	G-Index (1990)	E-Index (1990)	Poison Pills (1990)
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
BC + (BC x Z) $\overline{Z}$	0.002	0.003	0.002	0.003	0.003	0.001	0.000	0.001
	[0.646]	[0.421]	[0.504]	[0.374]	[0.392]	[0.850]	[0.959]	[0.868]
BC x Herfindahl	-0.026***	-0.030***	-0.027***	-0.025***	-0.032***	-0.031***	-0.030***	-0.031***
	(3.02)	(3.74)	(3.45)	(4.07)	(4.05)	(4.09)	(3.94)	(4.01)
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	68,023	68,023	65,469	67,818	67,748	72,819	72,596	72,819
Adj. R-squared	0.69	0.69	0.69	0.70	0.69	0.68	0.68	0.68

Table VI
Herfindahl Dummies

Herfindahl < 50%, Herfindahl ≥ 50%, Herfindahl ≤ 35%, Herfindahl ∈ (35%, 65%), and Herfindahl ≥ 65% are dummy variables that equal one if the Herfindahl index lies in the specified range of its empirical distribution. All other variables are defined in Tables II and III. Standard errors are clustered at the state of incorporation level. The sample period is from 1976 to 1995. *t*-statistics are in parentheses. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	Dependent Variable: Return on Assets			
	Two Herfindahl Dummies		Three Herfindahl Dummies	
	[1]	[2]	[3]	[4]
BC x Herfindahl < 50%	-0.003	-0.002		
	(1.13)	(0.49)		
BC x Herfindahl ≥ 50%	-0.010***	-0.011***		
	(4.28)	(4.13)		
BC x Herfindahl ≤ 35%			-0.000	0.002
			(0.13)	(0.68)
BC x Herfindahl ∈ (35%, 65%)			-0.008***	-0.008**
			(3.33)	(2.56)
BC x Herfindahl ≥ 65%			-0.011***	-0.012***
			(4.31)	(4.59)
Industry-year		0.206***		0.206***
		(9.63)		(9.61)
State-year		0.250***		0.248***
		(8.96)		(8.77)
Size		0.096***		0.097***
		(20.04)		(20.34)
Size-squared		-0.007***		-0.007***
		(19.92)		(20.53)
Age		-0.022***		-0.021***
		(5.56)		(5.37)
Herfindahl ≥ 50%		0.004		
		(1.51)		
Herfindahl ∈ (35%, 65%)				0.006*
				(1.88)
Herfindahl ≥ 65%				0.008**
				(2.12)
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	77,481	77,460	77,481	77,460
Adj. R-squared	0.66	0.68	0.66	0.68

Table VII
Alternative Competition Measures

The asset-based Herfindahl index is computed using firms' total assets (COMPUSTAT item #6) in place of sales. Industry net profit margin (NPM) is the median net profit margin in the respective 3-digit SIC industry. NPM is operating income before depreciation and amortization (item #13) divided by sales (item #12). All other variables are defined in Tables II and III. Standard errors are clustered at the state of incorporation level. The sample period is from 1976 to 1995. *t*-statistics are in parentheses. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	Dependent Variable: Return on Assets			
	[1]	[2]	[3]	[4]
BC	-0.000	0.000	0.002	0.000
	(0.15)	(0.11)	(0.60)	(0.07)
BC x Herfindahl (2-digit)	-0.056***			
	(5.15)			
BC x Herfindahl (4-digit)		-0.022***		
		(3.23)		
BC x Herfindahl (Assets)			-0.037***	
			(4.73)	
BC x Industry NPM				-0.054***
				(3.03)
Industry-year	0.203***	0.201***	0.202***	0.136***
	(9.90)	(9.72)	(9.34)	(9.67)
State-year	0.251***	0.249***	0.246***	0.255***
	(8.76)	(9.26)	(8.83)	(10.98)
Size	0.096***	0.096***	0.097***	0.089***
	(19.30)	(21.35)	(20.31)	(19.40)
Size-squared	-0.007***	-0.007***	-0.007***	-0.006***
	(18.57)	(21.25)	(20.15)	(17.98)
Age	-0.021***	-0.020***	-0.021***	-0.020***
	(5.21)	(4.99)	(5.52)	(6.26)
Herfindahl (2-digit)	0.011			
	(0.76)			
Herfindahl (4-digit)		0.017**		
		(2.13)		
Herfindahl (Assets)			0.033***	
			(4.15)	
Industry NPM				0.098***
				(4.58)
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	77,135	77,446	77,470	76,365
Adj. R-squared	0.68	0.68	0.68	0.68

Table VIII
Non-Delaware and "Eventually Business Combination" Samples

Firms incorporated in Delaware are excluded from the treatment group in the "Non-Delaware" sub-sample. "Eventually Business Combination" is defined in Table II. All other variables are defined in Tables II and III. Standard errors are clustered at the state of incorporation level. The sample period is from 1976 to 1995. *t*-statistics are in parentheses. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	Dependent Variable: Return on Assets	
	Non-Delaware	Eventually Business Combination
	[1]	[2]
BC	0.002	0.002
	(0.04)	(0.57)
BC x Herfindahl	-0.032**	-0.032***
	(2.41)	(4.74)
Industry-year	0.232***	0.189***
	(10.30)	(14.27)
State-year	0.260***	0.237***
	(7.02)	(12.63)
Size	0.092***	0.096***
	(14.25)	(19.51)
Size-squared	-0.007***	-0.007***
	(13.64)	(18.27)
Age	-0.019***	-0.023***
	(3.63)	(6.19)
Herfindahl	0.031**	0.025**
	(2.28)	(2.39)
Firm Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
Observations	55,920	66,623
Adj. R-squared	0.68	0.69

Table IX
Alternative Performance Measures

Return on assets (after depreciation) is operating income after depreciation and amortization (COMPUSTAT item #178) divided by total assets (item #6). Return on equity is net income (item #172) divided by the book value of common equity (item #60). Return on sales is operating income before depreciation and amortization (item #13) divided by sales (item #12). All other variables are defined in Tables II and III. Standard errors are clustered at the state of incorporation level. The sample period is from 1976 to 1995. *t*-statistics are in parentheses. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	Return on Assets (after Depreciation)	Return on Sales	Return on Equity
	[1]	[2]	[3]
BC	0.000	-0.001	0.000
	(0.06)	(0.36)	(0.03)
BC x Herfindahl	-0.031***	-0.035***	-0.032***
	(4.73)	(3.24)	(2.74)
Industry-year	0.243***	0.234***	0.143***
	(8.95)	(12.02)	(9.98)
State-year	0.254***	0.137***	0.181***
	(7.56)	(5.09)	(5.17)
Size	0.111***	0.090***	0.104***
	(22.53)	(17.70)	(11.25)
Size-squared	-0.008***	-0.005***	-0.007***
	(21.93)	(11.62)	(9.37)
Age	-0.037***	-0.017***	-0.068***
	(8.77)	(4.48)	(8.41)
Herfindahl	0.021**	0.028**	0.021
	(2.11)	(2.22)	(1.17)
Firm Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Observations	78,698	73,571	76,412
Adj. R-squared	0.67	0.66	0.43

Table X
Manufacturing Industries

Herfindahl (Census) is the Herfindahl index computed by the U.S. Bureau of the Census. The index is only available for 4-digit SIC manufacturing industries for the years 1982, 1987, and 1992 during the sample period. For all other years we always use the latest available value. Import penetration is a dummy variable that equals one if the import penetration in a given industry and year lies above the industry mean in that year. Import penetration is defined as imports divided by the sum of total shipments minus exports plus imports. The import data is only available for 4-digit SIC manufacturing industries and is obtained from Peter Schott's webpage and described in Feenstra (1996) and Feenstra, Romalis and Schott (2002). All other variables are defined in Tables II and III. Standard errors are clustered at the state of incorporation level. The sample period is from 1976 to 1995. *t*-statistics are in parentheses. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	Dependent Variable: Return on Assets				
	[1]	[2]	[3]	[4]	[5]
BC	0.003	-0.003	-0.004	0.002	-0.000
	(0.67)	(0.83)	(0.95)	(0.38)	(0.09)
BC x Herfindahl (Compustat)	-0.035***			-0.045**	
	(3.08)			(2.50)	
BC x Herfindahl (Census)		-0.081***			-0.104***
		(2.84)			(2.62)
BC x (1 - Import Penetration)			-0.007*	-0.006	-0.007
			(1.90)	(1.41)	(1.29)
Industry-year	0.188***	0.148***	0.177***	0.173***	0.154***
	(9.96)	(6.21)	(8.07)	(7.58)	(6.08)
State-year	0.277***	0.284***	0.348***	0.306***	0.273***
	(7.64)	(3.99)	(5.87)	(6.04)	(2.60)
Size	0.111***	0.115***	0.097***	0.120***	0.091***
	(21.14)	(13.13)	(18.57)	(15.74)	(13.77)
Size-squared	-0.008***	-0.009***	-0.007***	-0.009***	-0.007***
	(20.52)	(12.45)	(17.55)	(15.96)	(13.96)
Age	-0.034***	-0.043***	-0.031***	-0.029***	-0.037***
	(5.07)	(5.39)	(5.12)	(4.55)	(5.01)
Herfindahl (Compustat)	0.036***			0.068***	
	(3.13)			(3.69)	
1 - Import Penetration			0.011***	0.010***	0.011**
			(3.09)	(2.68)	(2.44)
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	35,371	19,244	21,031	20,948	17,551
Adj. R-squared	0.70	0.73	0.69	0.72	0.71

Table XI
Serial and Cross-Sectional Correlation

The methods employed to correct for serial and cross-sectional correlation of the error terms are described in Section 3.3. All variables are defined in Tables II and III. The sample period is from 1976 to 1995. *t*-statistics are in parentheses; *p*-values are in brackets. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	Dependent Variable: Return on Assets			
	AR(1)-GLS	Block Bootstrapping	Time Collapsing	Cross-Sectional Collapsing
	[1]	[2]	[3]	[4]
BC	0.003	0.001	-0.001	0.002
	(0.98)	[0.805]	(0.56)	(0.97)
BC x Herfindahl	-0.032***	-0.033***	-0.015**	-0.020***
	(3.90)	[0.000]	(2.52)	(2.59)
Industry-year	0.168***	0.206***		
	(13.94)	[0.000]		
State-year	0.176***	0.249***		
	(7.11)	[0.000]		
Size	0.114***	0.097***		
	(54.69)	[0.000]		
Size-squared	-0.008***	-0.007***		
	(38.93)	[0.000]		
Age	-0.020***	-0.021***		
	(6.80)	[0.000]		
Herfindahl	0.012	0.025**		
	(1.45)	[0.025]		
Firm Fixed Effects	Yes	Yes	Yes	No
Year Fixed Effects	Yes	Yes	Yes	Yes
Portfolio Fixed Effects	No	No	No	Yes
Observations	66,739	77,460	10,192	26,344
Adj. R-squared	0.72	0.68	0.00	0.42

Table XII
Empire Building or Quiet Life?

Capital expenditures (investment) are property, plant, and equipment capital expenditures (COMPUSTAT item #30) divided by total assets (item #6). Selling, general & admin. expenses (overhead) are SG&A expenses (item #189) divided by total assets. Advertising expenses (item #45) and costs of goods sold (item #41) are divided by sales (item #12). R&D expenses (item #46) are divided by total assets. Wages (real) are the natural logarithm of labor and related expenses (item #42) divided by the number of employees (item #29) and deflated by the consumer price index (CPI) from the U.S. Bureau of Labor Statistics. All other variables are defined in Tables II and III. Standard errors are clustered at the state of incorporation level. The sample period is from 1976 to 1995. *t*-statistics are in parentheses. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	Capital Expenditures (Investment)	Selling, General & Admin. Expenses (Overhead)	Advertising Expenses	R&D Expenses	Costs of Goods Sold	Wages (Real)
	[1]	[2]	[3]	[4]	[5]	[6]
BC	-0.000	0.005	0.000	0.001	-0.002	-0.003
	(0.27)	(0.80)	(0.59)	(0.49)	(0.20)	(0.12)
BC x Herfindahl	0.001	0.029**	0.003	0.007	0.053**	0.103**
	(0.18)	(2.51)	(1.04)	(1.39)	(2.44)	(2.00)
Industry-year	0.258***	0.110***	0.001	0.333***	0.101***	0.087***
	(10.92)	(5.47)	(0.04)	(8.50)	(3.97)	(3.49)
State-year	0.246***	0.013	0.106***	0.173***	0.038	0.003
	(6.20)	(0.42)	(2.77)	(3.28)	(1.38)	(0.09)
Size	0.013***	-0.286***	0.001	-0.068***	-0.134***	-0.110***
	(4.08)	(25.05)	(0.96)	(4.47)	(4.49)	(4.49)
Size-squared	-0.001***	0.019***	-0.000	0.005***	0.008***	0.007***
	(2.72)	(17.56)	(0.96)	(4.00)	(3.72)	(4.73)
Age	-0.034***	0.117***	-0.008***	0.024**	0.000	0.109***
	(10.96)	(12.10)	(12.27)	(2.66)	(0.00)	(6.26)
Herfindahl	-0.009	-0.056***	0.005**	-0.010	-0.073**	-0.115
	(1.35)	(3.27)	(2.04)	(1.47)	(2.11)	(0.89)
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	74,435	68,561	28,389	39,359	74,758	8,651
Adj. R-squared	0.55	0.81	0.80	0.76	0.60	0.89

Table XIII
Event-Study Results

The methodology used to calculate cumulative abnormal returns (CARs) is described in Section 4. The event date is the date of the first newspaper report about the respective BC law. The two-day event window is [-1,0]. The numbers reported in the table are average portfolio CARs based on 19 state- or state-Herfindahl (sub-)portfolios, respectively. The 19 states are Arizona, Connecticut, Delaware, Georgia, Illinois, Kentucky, Maryland, Massachusetts, Minnesota, New Jersey, New York, Ohio, Oklahoma, Pennsylvania, Tennessee, South Carolina, Virginia, Washington, and Wisconsin. *z*-statistics are in parentheses. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	All	Herfindahl		Herfindahl		
		≤ 1/2	> 1/2	≤ 1/3	∈ (1/3, 2/3)	≥ 2/3
	[1]	[2]	[3]	[4]	[5]	[6]
[-40, -2]	0.98	1.25	0.61	1.51	2.11	-0.30
	(1.44)	(1.40)	(0.49)	(1.53)	(1.13)	(0.04)
[-30, -2]	0.43	0.83	0.08	0.78	0.52	-0.34
	(0.94)	(1.08)	(0.07)	(1.02)	(0.36)	(0.07)
[-20, -2]	0.08	0.15	-0.01	0.33	-0.07	-0.41
	(0.53)	(0.47)	(0.22)	(0.78)	(-0.03)	(0.15)
[-10, -2]	0.52	0.44	0.57	0.44	1.15	0.10
	(1.35)	(1.31)	(0.54)	(1.19)	(1.24)	(0.21)
[-3, -2]	-0.02	0.22	-0.24	0.38	0.09	-0.24
	(0.05)	(0.47)	(-0.50)	(0.75)	(-0.26)	(-0.25)
[-1, 0]	-0.32***	-0.10	-0.54**	0.08	-0.44*	-0.67**
	(-2.58)	(-1.29)	(-2.36)	(-0.53)	(-1.67)	(-2.31)
[1, 2]	0.09	-0.03	0.20	0.01	0.25	0.03
	(0.37)	(0.07)	(0.45)	(-0.05)	(1.02)	(-0.28)
[1, 10]	-0.07	0.03	-0.17	0.30	-0.74	-0.27
	(-0.08)	(0.07)	(-0.07)	(0.78)	(-0.53)	(-0.61)