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 exogenous variation, 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 operating performance after the laws' passage, firms in competitive industries experience virtually no effect. Our results are consistent with the notion that competition mitigates managerial slack. When we examine which agency problem competition mitigates, we find evidence in support of a "quiet-life" hypothesis. Input costs, wages, and overhead costs all increase after the laws' passage, and only so in non-competitive industries. We find no evidence for empire building. 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 stock price impact.

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1. Introduction

Going back to Adam Smith, economists have long argued that managerial slack is first and foremost an issue for firms in non-competitive industries. As Sir John Hicks succinctly put it, managers of such firms enjoy the "quiet life".[1] By contrast, managers in competitive industries are under constant pressue to reduce slack and improve efficiency, or else they will no survive:

> "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" (Scherer, 1980).

The hypothesis that competition mitigates managerial slack, provided it is true, has several important implications. For instance, it implies that topics that have been studied extensively over the past decades, such as managerial agency problems leading to deviations from value-maximizing behavior, might have little bearing on firms in competitive industries. Second, on a practical note, researchers who want to study the effects of governance might benefit from interacting governance with measures of competition. Third, and perhaps most important, policy efforts to improve corporate governance might benefit from focusing on non-competitive industries. Moreover, such efforts could be broadened to also include measures aimed at improving an industry's competitiveness, such as deregulation and antitrust laws.

We test the hypothesis that competition reduces managerial slack using 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. BC laws impose a moratorium on certain transactions, including mergers and asset sales, between a large shareholder and the firm for a period ranging from three to five years after the large shareholder's stake has passed a prespecified threshold. This moratorium severely hinders corporate raiders from gaining access to the target firm's

[1] "The best of all monopoly profits is a quiet life" (Hicks, 1935). Similarly, "Monopoloy is a great enemy to good management" (Smith, 1776). Despite its intuitive appeal, attempts to formalize the notion that competition mitigates managerial slack 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) conclude that "apparently, the simple idea that product market competition reduces slack is not as easy to formalize as one might think."

assets for the purpose of paying down acquisition debt, thus making hostile takeovers more difficult and often impossible. Thus, by reducing the fear of a hostile takeover, BC laws weaken corporate governance and increase the opportunity for managerial slack.[2]

Using the passage of BC laws as a source of identifying variation, we examine if these laws have a different effect on firms in competitive and non-competitive industries. We obtain three main results. First, consistent with the notion that BC laws increase the opportunity for managerial slack, we find that firms' return on assets (ROA) drops by 0.6 percentage points on average. Given that the average (median) ROA is about 7.4 percent (10.4 percent), this implies a drop in ROA of 8.1 percent for the average firm and 5.8 percent for the median firm. Second, the drop in ROA is larger for firms in non-competitive industries. For example, while ROA drops by 1.5 percentage points in the highest HHI (Herfindahl-Hirschman index) quintile, it only drops by 0.1 percentage points in the lowest quintile. Third, the effect is close to zero and insignificant in highly competitive industries. Overall, our results are consistent with the notion that competition reduces managerial slack.

The contribution of this paper is not to introduce a novel source of exogenous variation. Many papers have used the passage of BC laws as a source of exogenous variation, including Garvey and Hanka (1999), Bertrand and Mullainathan (1999, 2003), Cheng, Nagar, and Rajan (2005), and Rauh (2006). Rather, the contribution is to show that governance has a different effect on firms in competitive and non-competitive industries and, especially, that it does not seem to matter much in competitive industries.

ROA is an accounting measure that can be manipulated. Accordingly, a drop in ROA after the passage of the BC laws does not necessarily imply a reduction in operating profitability. It may simply reflect a change in the extent to which firms manage their earnings. While it is difficult to completely rule out this alternative story, we can offer some pieces of evidence that seem inconsistent with it. First, if a BC law is passed only a few months prior to the fiscal year's end, then it would seem hard to imagine that the current year's ROA should drop by much, given that most of the fiscal year is already over. In this case, a significant drop in ROA might be indicative of an earnings management story. However, we find that the drop in ROA is small and insignificant if the BC law is passed late in the fiscal year. Second, using discretionary accruals

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

as proxies for earnings management, we directly test whether firms' earnings management has changed after the passage of the BC laws. We find no evidence for this.

Our findings are robust across many specifications. Our main competition measure is the HHI based on 3-digit SIC codes computed from Compustat. We obtain similar results if we use 2-digit and 4-digit HHIs, asset-based HHIs, lagged HHIs (up to five years), and the average HHI from 1976 to 1984 (the first BC law was passed in 1985). We also obtain similar results if we use the Census HHI (which includes both public and private firms), import penetration, and industry net profit margin (or Lerner index). Finally, we obtain similar results if we run "horse races" between the HHI and other variables that could be correlated with the HHI, if we exclude Delaware firms from the treatment group, if we use alternative performance measures, such as return on equity and net profit margin, if we restrict the sample to only those firms that were present during the entire period from 1981 to 1995 (to purge the sample of exits and entries), if we alter the sample period, and if we interact all control variables with time dummies and treatment state dummies.

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, less than 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 contemporaneous shocks from the effect of the BC laws themselves. Among other things, this alleviates concerns that the BC laws might the outcome of lobbying at the local and industry level, respectively. To address concerns that the BC laws might the outcome of broad-based lobbying at the state of incorporation level, we examine if there was already an "effect" of the laws prior to their passage. We find not evidence for this.

While our results suggest that competition mitigates managerial agency problems, they do not say which agency problem is being mitigated. 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)? We find no evidence for empire building. Capital expenditures, asset growth, PPE growth, the volume of acquisitions made by a firm, and the likelihood of being an acquirer are all unaffected by the BC laws. In

contrast, we find that input costs, overhead costs, and wages all increase after the laws' passage, and only so in non-competitive industries. Overall, our 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 units within the company demanding bigger overhead budgets.

To see whether our findings are also reflected in stock prices, we conduct event studies around the dates of the first newspaper reports about the BC laws. Across all industries, we find a significant cumulative abnormal return (CAR) of -0.32%. When we compute CARs separately for low- and high-HHI portfolios, we find that the CAR for the low-HHI portfolio is small and insignificant, while the high-HHI portfolio has a significant CAR of -0.54%. A similar pattern emerges when we form three HHI-based portfolios. While the CAR for the low-HHI portfolio is small and insignificant, the medium- and high-HHI portfolios have significant CARs of -0.44% and -0.67%, respectively.

Our empirical methodology follows Bertrand and Mullainathan (2003), who consider the same 30 BC laws as we do. Using plant-level data from the Census Bureau's Longitudinal Research Database (LRD), the authors examine the effect of the BC laws on wages, employment, plant births and deaths, investment, total factor productivity, and return on capital.[3] We extend their results by investigating whether the BC laws have a different effect on firms in competitive and non-competitive industries. In terms of research question, our paper is closely related to Nickell (1996), who finds that more competition leads to higher productivity growth in a sample of U.K. manufacturing firms.[4] While consistent with a managerial agency explanation, this result is also consistent with alternative explanations that are unrelated to corporate governance. For instance, 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 governance instruments correlate with measures of competition, e.g., managerial incentive schemes (Aggarwal and Samwick, 1999),

[3] Using plant-level data is superior to Compustat data in many respects. For instance, it allows to estimate total factor productivity. Also, it allows to include both plant fixed effects and state of incorporation fixed effects, thus permitting a tighter identification.

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

board structure (Karuna, 2007), and firm-level takeover defenses (Cremers, Nair, and Peyer, 2007). Finally, our paper is related to Guadalupe and Pérez-González (2005), who find that competition affects private benefits of control, as measured by the voting premium between shares with differential 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 examines which agency problem competition mitigates. Section 5 presents event-study results. Section 6 concludes.

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).[5] The sample period is from 1976 to 1995, which is the same sample period as in Bertrand and Mullainathan (2003).

The above selection criteria leave us with 10,960 firms and 81,095 firm-year observations. **Table 1** 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 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 (Romano, 1993). To gain further confidence, Bertrand and Mullainathan (2003) randomly sampled 200 firms from their panel and checked (using *Moody's Industrial Manual*) if any of these firms had changed their state of incorporation during the sample period. Only three firms had changed their state of incorporation, and all of them to Delaware. Importantly, all three changes predate 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.

[5] Whether we exclude regulated utilities makes no difference for our results. We also obtain similar results if we exclude financial firms (SIC 6000-6999), and if we restrict the sample to manufacturing firms (SIC 2000-3999).

2.2. Definition of variables and summary statistics

Our main measure of competition is the Herfindahl-Hirschman index (HHI), which is well-grounded in industrial organization theory (see Tirole, 1988). A higher HHI value indicates weaker competition. The HHI is defined as the sum of squared market shares,

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

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 firms' assets. Our benchmark measure is the HHI 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 unlikely compete with each other. On the other hand, the 4-digit 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, although common sense suggests that they compete with each other. We consider HHIs based on 2- and 4-digit SIC codes in robustness checks. Also in robustness checks, we consider additional competition measures, such as the Census HHI, industry net profit margin (or Lerner index), and import penetration. Finally, a look at the empirical distribution of the HHI 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 HHI distribution.[6]

Our main measure of operating performance is the return on assets (ROA), which is defined as operating income before depreciation and amortization (EBITDA, item #13) divided by 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

[6]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 definition, these industries have an HHI of one, which explains the small "spike" at the right endpoint of the empirical HHI distribution. Dropping 2.5% of the firm-year observations at the right tail of the distribution is an attempt to correct for this misclassification.

firm-year observations at each tail of the ROA distribution. **Panel (A) of Table 2** provides summary statistics on the mean, median, and range of observed ROA values based on the trimmed sample. We consider alternative methods to deal with ROA outliers in robustness checks. Also in robustness checks, we consider alternative performance measures, such as the return on equity and net profit margin.

We control for firm age and firm size in all our regressions. 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. We also control for local and industry shocks in a way made precise below.

Panel (B) of Table 2 provides summary statistics for firms incorporated in states that passed a Business Combination (BC) law during the sample period ("Eventually BC") and firms incorporated in states that never passed a BC law ("Never BC"). As can be seen, there are no significant differences with respect to the HHI. On the other hand, firms in passing states are slightly bigger and older on average, which raises the question if the control group is an appropriate one. There are several reasons why this should not be a concern. First, due to the staggering of the BC laws over time, firms in the "Eventually BC" group are first control firms (before the BC law) and then treatment firms. Second, we control for age and size in all our regressions. Third, we show in robustness checks that our results are similar if we restrict the control group to firms incorporated in treatment states that have not yet passed a BC law.

2.3. Empirical methodology

We examine whether the passage of 30 BC laws between 1985 and 1991 has a different effect on firms' operating performance in competitive and non-competitive industries. We estimate

$$y_{ijklt} = \alpha_i + \alpha_t + \beta_1 BC_{kt} + \beta_2 HHI_{jt} + \beta_3 \left(BC_{kt} \times HHI_{jt}\right) + \boldsymbol{\gamma}' \mathbf{X}_{ijklt} + \epsilon_{ijklt}, \qquad (2)$$

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 (mainly ROA), α_i and α_t are firm and year fixed effects, BC_{kt} is a dummy that equals one if a BC law has been passed in state k by time t, HHI_{jt} is the HHI associated with industry j at time t, $\mathbf{X}_{ijklt}$ is a vector of controls (age, size, industry- and state-year controls), and ϵ_{ijklt} is the error term.

For any given HHI, we can compute the total effect of the BC laws as $\beta_1 + \beta_3 HHI$. The coefficient β_1 on the BC dummy measures the (limit) effect as the HHI goes to zero. Thus, it

measures the laws' effect on firms in highly competitive industries. The coefficient β_3 measures how the effect varies with the degree of competition. The coefficient β_2 measures the direct effect of competition. For instance, when the dependent variable is ROA, the standard conjecture is that the coefficient β_2 should be positive, implying that firms in more competitive industries (lower HHI) make fewer profits.

Equation (2) amounts to a difference-in-difference-in-difference (DDD) specification. In the case where the dependent variable is ROA, the first difference compares firms' ROA before and after the passage of the 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 laws' effect on firms' ROA. The interaction term $BC \times HHI$ estimates a third difference, namely, whether the laws' effect is different for firms 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 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 1 shows that only 37.8% of all 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. In theory, this lack of congruence allows us to fully control for any industry shocks and shocks specific to a state of location by including a full set of industry and state of location dummies, each interacted with time dummies. Alas, computational difficulties make it practically infeasible to estimate a specification with so many covariates. Instead, we follow Bertrand and Mullainathan (2003) and control for local and industry shocks by including a full set of time-varying industry- and state-year controls, which are computed as the mean of the dependent variable in the firm's industry and state of location, respectively, in a given year, excluding the firm itself.

Controlling for local and industry shocks helps to separate out the effects of shocks contemporaneous with the BC laws from the effect of the laws themselves. This addresses several

important concerns. First, our estimate of the laws' effect could be biased, reflecting in part the effects of contemporaneous shocks. Second, our results could be spurious, coming entirely from contemporaneous shocks. Third, and perhaps most important, economic conditions could influence the passage of the BC laws. For example, poor economic conditions in a particular state might induce local firms to lobby for an anti-takeover law to gain better protection from hostile takeovers. While the inclusion of state- and industry-year controls mitigates 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. We will address this issue in Section 3.2 below.

The HHI is an imperfect measure of competition. The classic example is that where every city has one (say, cement) company. In that case, there would be many companies in the industry, but given the high transportation costs for cement, each company is a local monopoly. Clearly, the HHI would seriously misrepresent the true level of competition in this situation. This concern applies more generally whenever markets are regionally segmented. Nevertheless, as long as this measurement error is not systematically related to the passage of the BC laws, which is a reasonable assumption, our coefficients will remain unbiased. The only harm caused by this measurement error is that it leads to an increase in noise, which makes it harder to find any significant results.

Throughout, we cluster standard errors at the state of incorporation level. This accounts for arbitrary correlations of the error terms i) across different firms in a given state of incorporation and year (cross-sectional correlation), ii) across different firms in a given state of incorporation over time (across-firm serial correlation), and iii) within the same firm over time (within-firm serial correlation). Cross-sectional correlation is a concern because all firms in a given state of incorporation are affected by the same "shock" (namely, the BC law) (Moulton, 1990). 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 (Bertrand, Duflo, and Mullainathan, 2004). We discuss alternative ways to correct for cross-sectional and serial correlation in robustness checks.

3. Results

3.1. Main results

Our main results are in **Panel (A) of Table 3**. Column [1] shows the average effect of the

passage of the BC laws across all firms. The coefficient on the BC dummy is -0.006, implying that ROA drops on average by 0.6 percentage points. Given that the average (median) ROA is about 7.4 percent (10.4 percent), this implies a drop in ROA of 8.1 percent for the average firm and 5.8 percent for the median firm. The control variables all have the expected signs. The industry- and state-year coefficients are both positive and significant, which underscores the importance of controlling for industry and local shocks. The coefficients on size and the HHI are both positive, while the coefficient on age is negative.[7] The weak significance of the HHI as a control variable in column [1] is due to the fact that it captures two different effects of competition on profits, which have opposite signs. As we will see below, when we disentangle the two effects, they will both become significant.

In column [2], we examine whether the drop in ROA is different for firms in competitive and non-competitive industries. The interaction term between the BC dummy and the HHI has a coefficient of -0.033 ($t-$statistic of 4.95), which implies that the drop in ROA is larger for firms in non-competitive industries. (That these firms have higher profits to begin with is accounted for by the inclusion of the HHI as a control variable.) To illustrate the economic magnitude of this effect, note that the HHI has a standard deviation of 0.156. Accordingly, an increase in the HHI by one standard deviation is associated with a drop in ROA of $-0.033 \times 0.156 = -0.005$, or 0.5 percentage points. We can alternatively divide the sample into HHI quintiles. The mean value of the HHI in the lowest and highest HHI quintile is 0.067 and 0.479, respectively. Hence, while ROA drops by 1.5 percentage points in the highest HHI quintile, it only drops by 0.1 percentage points in the lowest HHI quintile. Of equal interest is the fact that the BC dummy is close to zero and insignificant. Since the BC dummy captures the limit effect as the HHI goes to zero, this implies that the passage of the BC laws has no significant effect on firms in highly competitive industries. Finally, we can disentangle the two different effects of competition on profits. The interaction term captures the *indirect* (or "managerial-slack") effect. The negative coefficient implies that the indirect effect is positive, i.e., firms in more competitive industries

[7] We have experimented with including squared terms for size, age, and the HHI (both in the interaction term and as a control variable) to capture possible non-linearities. As is shown in Table 3, 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 virtually no effect on the other variables. All our results are similar if we include age-squared instead of age. The squared term for the HHI was insignificant, both in the interaction term and as a control variable.

experience a smaller drop in ROA after the laws' passage. By contrast, the HHI as a control variable captures the *direct* effect of competition on profits. The positive coefficient implies that the direct effect is negative, i.e., firms in more competitive industries tend to make fewer profits.

The positive sign of the HHI as a control variable also mitigates potential endogeneity concerns regarding the HHI. A main concern here is reverse causation. A drop in profits, possibly caused by the passage of the BC laws, might lead to firm exits and higher industry concentration. Accordingly, as pointed out by Nickell (1996), reverse causation would predict that the HHI as a control variable should have a negative sign. However, the sign is positive, which is consistent with the (conventional) interpretation that competition reduces profits. We will further address this issue in robustness checks using lagged values of the HHI as well as the average HHI from 1976 to 1984 (the first BC law was passed in 1985).

In column [3], we use HHI dummies in place of a continuous measure. The dummies indicate whether the HHI lies in the bottom, medium, or top tercile of its empirical distribution. We drop the BC dummy and one of the HHI dummies as a control variable to avoid perfect multicollinearity. The results are similar to those in column [2]. While the BC laws have no significant effect on firms in competitive industries (lowest HHI tercile), firms in less competitive industries experience a significant drop in ROA of 0.8 percentage points (medium HHI tercile) and 1.2 percentage points (highest HHI tercile), respectively.

Overall, our results are consistent with the arguments by Alchian (1950), Friedman (1953), and Stigler (1958), based on survivorship, that competitive industries leave no room for managerial slack. However, since it is difficult to identify where exactly this "zero-slack point" is, our results are also consistent with the (weaker) notion that there is some positive "baseline level" of slack in all firms, where firms in competitive industries may already operate at this minimum level, while firms in non-competitive industries may only be driven down to this level if there is a threat of a hostile takeover.

3.2. Endogeneity of the BC laws?

While the inclusion of state- and industry-year controls alleviates 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. Such lobbying is a concern because it opens up the possibility of reverse causation. Precisely, if a broad coalition of firms incorporated in the same state, which all experience a decline in profitability and moreover all

operate in non-competitive industries, successfully lobby for an antitakeover law in their state of incorporation, then causality might be reversed.

Given the anecdotal evidence in Romano (1987), who portrays lobbying for antitakeover laws as an exclusive political process, the notion of broad-based lobbying seems unlikely. Typically, antitakeover laws were adopted, often during emergency sessions, under the political pressure of a single firm facing a concrete takeover threat, not a broad coalition of firms. Hence, for all but perhaps a few select firms, the laws were likely exogenous.[8] This mitigating evidence notwithstanding, the possibility of reverse causation warrants closer investigation. Following Bertrand and Mullainathan (2003), we address this issue by replacing the BC dummy in equation (2) with four dummies: $BC\ Year(-1)$, $BC\ Year(0)$, $BC\ Year(1)$, and $BC\ Year(2+)$, where $BC\ Year(-1)$ is a dummy that equals one if the firm is incorporated in a state that will pass a BC law in one year from now, $BC\ Year(0)$ is a dummy that equals one if the firm is incorporated in a state that passes a BC law this year, and $BC\ Year(1)$ and $BC\ Year(2+)$ are dummies that equal one if the firm is incorporated in a state that passed a BC law one year ago and two or more years ago, respectively. If the BC laws were passed in response to political pressure of a broad coalition of firms, which all experience a decline in profitability and, moreover, all operate in non-competitive industries, then we should see an "effect" of the laws already prior to their passage. In particular, if the coefficient on $BC\ Year(-1) \times HHI$ is negative and significant, then this might be symptomatic of reverse causation.

The results are in **Panel (B) of Table 3**. As is shown, the coefficient on $BC\ Year(-1) \times HHI$ is small and insignificant, while the coefficients on the other interaction terms are large and significant. Thus, there appears to be no "effect" of the BC laws on firms in non-competitive industries prior to the laws' passage, which is consistent with a causal interpretation of our results. Moreover, and also consistent with a causal interpretation, the coefficient on $BC\ Year(0) \times HHI$ is smaller than the coefficient on either $BC\ Year(1) \times HHI$ or $BC\ Year(2+) \times HHI$.

3.3. Decrease in operating profitability or change in firms' earnings management?

ROA is an accounting measure that can be manipulated. Accordingly, a drop in ROA after

[8] Using newspaper reports (see Section 5), we have identified firms motivating the passage of the 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 does not affect our results.

the passage of the BC laws does not necessarily imply a reduction in operating profitability. It could simply reflect a change in the extent to which firms manage their earnings. For example, firms might overstate their earnings to appear more profitable in order to ward off hostile takeovers. Consequently, firms' earnings might drop after the laws' passage not because of a decrease in operating profitability, but simply because the need for earnings overstatement is reduced. If, in addition, the threat of being taken over is primarily a concern for firms in non-competitive industries, then this alternative story, based on earnings management, might explain our results.[9]

While it is difficult to completely rule out this alternative story, we can offer some pieces of evidence that seem inconsistent with it. First, the likelihood of being taken over appears no different in competitive and non-competitive industries. In Table 5 below, which presents a regression predicting the likelihood of being taken over, the HHI dummies as controls are all insignificant. (To avoid perfect multicollinearity, we drop one of the HHI dummies, implying that the other HHI dummies measure the takeover likelihood relative to firms in the lowest HHI tercile.) Second, we can examine whether the BC laws have a different effect on firms' ROA depending on whether the laws were passed early or late in the fiscal year. If a BC law is passed only a few months prior to the fiscal year's end, then it would seem hard to imagine that the current year's ROA should drop by much, given that most of the fiscal year is already over. In this case, a significant drop in ROA might be indicative of an earnings management story.

In **Panel (A) of Table 4**, we estimate a regression similar to that in Panel (B) of Table 3, except that the reference point is not the calender year in which the BC law was passed but the effective month of the law, denoted by "$0m$". Thus, the dummy $BC(0m\ to\ 6m)$ indicates that ROA is measured within six months after the law's passage, the dummy $BC(6m\ to\ 12m)$ indicates that ROA is measured between six and twelve months after the law's passage, and so forth. For example, the Delaware BC law was passed on February 8, 1988. A Delaware company whose fiscal year ends in June thus has its fiscal year end within six months after the law's passage. For this company, the dummy $BC(0m\ to\ 6m)$ is set to one in 1988. In contrast, a Delaware company whose fiscal year ends in December has its fiscal year end between six and twelve months after the law's passage. For this company, the dummy $BC(6m\ to\ 12m)$ is set

[9] There is a variant of this story in which firms do not change their earnings management after the passage of the BC laws but rather the mix of projects they invest in. While some of the evidence presented here also applies to this alternative story, it is harder to rule out.

to one in 1988.[10] The main variable of interest is the interaction term $BC(0m\ to\ 6m) \times HHI$, which captures the effect of the BC laws on firms in non-competitive industries when the law is passed late in the fiscal year. If the coefficient on this interaction term is significant, then this might be indicative of an earnings management story. However, as is shown, the coefficient is small and insignificant. Moreover, the coefficients on all subsequent interaction terms are large and significant, implying that it takes about six months until the effect of the BC laws shows up in the ROA numbers.

Third, we can *directly* measure whether firms' earnings management changes after the passage of the BC laws. A commonly used proxy for earnings management are discretionary accruals, which are those parts of total accruals over which management has discretion. Total accruals are computed as the difference between earnings and operating cash flows, or equivalently, as the change between non-cash current assets minus the change in current liabilities, excluding the portion that comes from the maturation of the firm's long-term debt, minus depreciation and amortization, scaled by total assets in the previous fiscal year. To identify those components of total accruals that are discretionary, we follow Dechow, Sloan, and Sweeney (1995). The authors show that a modified version of the Jones (1991) model has the most power in detecting earnings management relative to alternative accrual-based models. The modified Jones model regresses total accruals on the inverse of total assets in the previous fiscal year, the change in sales less the change in accounts receivable, and property, plant and equipment. Discretionary accruals are the residuals from this regression.

To test whether firms' earnings management has changed after the passage of the BC laws, we estimate our basic specification using discretionary accruals as the dependent variable. The results are presented in **Panel (B) of Table 4**. As is shown in column [1], the coefficients on BC and $BC \times HHI$ are both small and insignificant, suggesting that firms did not change their earnings management after the laws' passage. A related proxy for earnings management are discretionary *current* accruals, as used in Teoh, Welch, and Wong (1998). The authors decompose discretionary accruals into a short-term (or current) component and a long-term component and argue that managers have more discretion over the current component. Thus,

[10] Likewise, in 1987, the dummy $BC(-12m\ to\ -6m)$ is set to one for the first company, while the dummy $BC(-6m\ to\ 0m)$ is set to one for the second company. In contrast, in Panel (B) of Table 2, which is based on calender years, the dummy $BC\ Year(-1)$ is set to one for both companies in 1987, the dummy $BC\ Year(0)$ is set to one for both companies in 1988, and so forth.

discretionary current accruals might be a superior proxy for earnings management. The results, which are shown in column [2], are similar to those in column [1].

While it is hard to completely rule out that the drop in ROA is due to a change in earnings management, the evidence presented here is inconsistent with this idea. Additional evidence will be presented in Section 5, where we show that BC laws not only have an impact on accounting variables, but also on firms' equity prices.

3.4. Do BC laws reduce the takeover threat?

A key assumption underlying our identification strategy is that BC laws reduce the takeover threat. This assumption may seem in conflict with evidence by Comment and Schwert (1995) showing that BC laws (and anti-takeover laws in general) do not significantly reduce the takeover likelihood.[11] However, as Garvey and Hanka (1999) point out, since the takeover likelihood is an equilibrium outcome, it is quite possible that anti-takeover laws reduce the takeover threat, yet the takeover likelihood remains unchanged. The idea is that, by reducing the takeover threat, anti-takeover laws lead to an increase in managerial slack, which in turn increases the gains from mounting a hostile takeover. Thus, while hostile takeovers become more difficult, the gains from mounting a hostile takeover increase as well. In equilibrium, there need be no change in the observed takeover frequency.

Alas, this "equilibrium argument" comes only partly to our rescue. After all, we argue in this paper that managerial slack increases only in non-competitive industries. Consequently, we should thus observe that the takeover likelihood in competitive industries is reduced. To investigate the effect of the BC laws on the takeover likelihood, we estimate our basic specification using as the dependent variable a dummy that equals one if the firm is acquired in the following year and zero otherwise. The takeover data are obtained from the Securities Data Corporation's (SDC) database. Since these data begin in 1979, our initial sample period is reduced to 1978–1995 (with observed takeovers from 1979–1996). Importantly, our specification includes firm size

[11] If anti-takeover laws were indeed ineffective, why were so many of these laws passed in the 1980s and 90s? And why do these laws have so much predictive power in empirical studies (e.g., Karpoff and Malatesta, 1989; Garvey and Hanka, 1999; Bertrand and Mullainathan, 1999, 2003; Cheng, Nagar, and Rajan, 2005; Rauh, 2006)? Contrary to his previous findings, Schwert (2000) finds that hostile takeovers have become significantly less likely after 1989, which he partly attributes to the passage of anti-takeover laws: "This probably reflects the effects of [...] state antitakeover laws. In contrast, Comment and Schwert (1995) were unable to identify a statistically significant decline in hostile offers based on an analysis of transactions through 1991".

as a control variable, which, as Schwert (2000) argues, is the only variable that is consistently significant in empirical studies of the takeover likelihood.

The results are presented in **Table 5**. Column [1] shows the average effect (i.e., across all firms) of the BC laws on the takeover likelihood. While the coefficient on the BC dummy is negative, it is not significant. Thus, consistent with Comment and Schwert's (1995) findings, BC laws do not significantly reduce the *average* takeover likelihood. In column [2], we examine whether the BC laws have a different effect on the takeover likelihood in competitive and non-competitive industries. We obtain two main results. First, the effect of the BC laws on the takeover likelihood is monotonic in the HHI. Second, and consistent with our hypothesis, while the BC laws have no significant effect on the takeover likelihood in non-competitive industries (medium and highest HHI terciles), they significantly reduce the takeover likelihood in competitive industries (lowest HHI tercile).

3.5. Robustness

3.5.1. Alternative competition measures

Our main competition measure is the HHI based on 3-digit SIC codes. In **Table 6**, we use HHIs based on 2-digit SIC codes (column [1]) and 4-digit SIC codes (column [2]), respectively. As is shown, the results are similar to our baseline results in Table 3. The only difference is that the 2-digit HHI as a control variable is not significant, which is due to lack of sufficient "within" variation of this variable. As for the magnitude of the "managerial-slack" effect, note that the 2-digit and 4-digit HHIs have a standard deviation of 0.076 and 0.190, respectively. Thus, an increase in the 2-digit HHI by one standard deviation is associated with a drop in ROA of $-0.056 \times 0.076 = -0.004$, or 0.4 percentage points, which is similar to the estimate in Table 3. Likewise, an increase in the 4-digit HHI by one standard deviation is associated with a drop in ROA of $-0.022 \times 0.190 = -0.004$, or 0.4 percentage points.

In untabulated regressions, we use 2-digit, 3-digit, and 4-digit HHIs based on firms' assets in place of sales. The idea behind using asset-based HHIs is that sales can be rather volatile, with the effect that changes in the HHI may overstate actual changes in industry concentration (Hou and Robinson, 2006). The results using asset-based HHIs are similar to those in Table 3. Another way to address the issue of sales volatility is to use smoothed HHI measures. For instance, using a 3-year moving average HHI based on 3-digit SIC codes, we find that the interaction term between the BC dummy and the HHI has a coefficient of -0.029 ($t-$statistic

of 3.94), which is similar to the estimate in Table 3.

In column [3], we consider a margin-based competition measure, namely, the median industry net profit margin (NPM) based on 3-digit SIC Codes. At the firm level, NPM is computed 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, or price-cost margin. The Lerner index measures the extent to which firms can set prices above marginal cost. Under the commonly made assumption that marginal cost can be approximated by the average variable cost (Carlton and Perloff, 1989, p. 367), the Lerner index and the industry NPM are equivalent. As is shown, the results are similar to our baseline results in Table 3.

In **Table 7**, we consider competition measures that are only available for manufacturing industries (SIC 2000-3999). In column [1], we use the Census HHI, which is based on all public and private firms. While the Census HHI is broader, 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. Third, the index is only available for manufacturing industries, which implies that the sample is much smaller. As is shown, the results are similar to our baseline results in Table 3. Note that the HHI as a control variable is omitted in this regression. Except for three "jumps" in 1982, 1987, and 1992, the Census HHI has no "within" variation, implying that the coefficient is not well identified. As for the magnitude of the "managerial-slack" effect, note that the Census HHI has a standard deviation of 0.046. Thus, an increase in the Census HHI by one standard deviation is associated with a drop in ROA of $-0.081 \times 0.046 = -0.004$, or 0.4 percentage points, which is similar to the estimate in Table 3.

Whether we use the HHI from Compustat or that from the Census Bureau, we only capture domestic competition. In column [2], we use import penetration as our competition measure. Like the Census HHI, this measure is only available for manufacturing industries, and only on the narrow 4-digit SIC code level, which implies that it is likely subject to misclassification. Moreover, it is not clear that import penetration is a good measure of competition. For instance, import penetration may be high, yet an industry may be non-competitive because all of the

imports come from a single foreign producer. 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. While the results are similar to those in Table 3, they are statistically weaker. This may be partly due to the smaller sample size. But it may also be due to the fact that import penetration is a weak measure of competition. Perhaps the most meaningful way to use import penetration is together with the Census HHI, as is done in column [3]. In this regression, the BC dummy measures the effect of the BC laws on industries with both high domestic competition and high import penetration. As is shown, the results are similar to those in column [1]. Note, in particular, that the interaction term between the BC dummy and import penetration is no longer significant. While the coefficient on the interaction term is the same as in column [2], import penetration appears to lose the "horse race" against the Census HHI.

3.5.1. Miscellaneous robustness checks

This section presents some additional robustness checks. For brevity's sake, the results are not tabulated. In many cases, tabulated results as well as extensive discussions can be found in an earlier version of this paper (Giroud and Mueller, 2008). All of the results presented in this section are available from the authors upon request.

"Horse races". Our results could be spurious if they were not driven by the HHI but by some (omitted) variable Z that is merely correlated with the HHI. We address this issue by running "horse races" between the HHI and various candidate variables for Z, including size, age, leverage, ROA, Tobin's Q, G-Index, E-index, and poison pills. To mitigate potential endogeneity concerns, we use lagged values and industry averages. In each case, we estimate our basic specification with two additional terms: an interaction term $BC \times Z$ and a control term Z. The results are consistently similar to those in Table 3. In particular, the coefficient on the interaction term between the BC dummy and the HHI is remarkably stable with values from -0.026 to -0.032 ($t-$statistics from 3.02 to 4.09). To estimate the limit effect of the BC laws as the HHI goes to zero, we sum up the coefficient on the BC dummy and the coefficient on $BC \times Z$ multiplied by $\bar{Z}$, where $\bar{Z}$ is the sample mean of Z. Whether this expression is significant can be tested using a standard $F-$test. In each case, we find that the estimate is close to zero (values from 0.001 to 0.003) while the $p-$value is large (values from 0.374 to 0.959), which is consistent with our baseline results in Table 3.

Lagged HHIs and average HHI from 1976 to 1984. As discussed above, the positive coefficient on the HHI as a control variable mitigates potential endogeneity concerns regarding the HHI. To further address this issue, we estimate our basic specification using lagged values of the HHI (up to five years) as well as the average HHI from 1976 to 1984 (the first BC law was passed in 1985). In each case, the results are similar to our baseline results in Table 3. For instance, using the average HHI from 1976 to 1984, we find that the BC dummy is close to zero and insignificant, while the interaction term between the BC dummy and the HHI has a coefficient of -0.028 ($t-$statistic of 4.82).

Non-Delaware and "Eventually BC" samples. Given that about one half of the firms in our sample are incorporated in Delaware, one might be worried that our results are driven by a single law. When we exclude Delaware firms from the treatment group, we obtain results that are very similar to those in Table 2. While the BC dummy is close to zero and insignificant, the interaction term between the BC dummy and the HHI has a coefficient of -0.032 ($t-$statistic of 2.41), which is almost identical to the estimate in Table 3.[12] Another potential concern is that the control and treatment groups might differ for reasons unrelated to the passage of the BC laws. To address this issue, we restrict the control group to firms incorporated in treatment states that have not yet passed a BC law. The results are again similar to those in Table 3. While the BC dummy is close to zero and insignificant, the interaction term between the BC dummy and the HHI has a coefficient of -0.032 ($t-$statistic of 4.74).

Alternative performance measures. Our main performance measure is ROA before depreciation. In robustness checks, we use a variety of alternative performance measures: ROA after depreciation, which is defined as operating income after depreciation and amortization (Compustat item # 178) divided by total assets (item #6), net profit margin (NPM), which is defined as operating income before depreciation and amortization (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). Arguably, ROA (before or after depreciation) is the most suited of these measures. NPM is based on firms' sales, which can be rather volatile, while ROE depends on leverage. Regardless of which measure we use, we obtain results that are similar to those in Table 3. While the BC dummy is always close to zero and insignificant, the interaction term

[12] The weaker significance is likely due to the fact that by exluding Delaware firms, we lose about 58% of the treatment group, which considerably reduces the number of observations available for identifying the coefficient.

between the BC dummy and the HHI is always negative and significant with coefficients ranging from -0.031 to -0.035 ($t-$statistics from 2.74 to 4.73).

Different sample periods. Our sample period is from 1976 to 1995, which is the same period as in Bertrand and Mullainathan (2003). However, it has been questioned whether the BC laws had any significant effect prior to June 1989, when an appellate court upheld Wisconsin's BC law in *Amanda Acquisition Corp. v. Universal Foods Corp.*[13] To address this issue, we estimate our basic specification for the truncated sample period from 1976 to 1988. The results are again similar to our baseline results in Table 3. While the BC dummy is close to zero and insignificant, the interaction term between the BC dummy and the HHI has a coefficient of -0.029 ($t-$statistic of 4.35). A possible explanation is that firms always believed that the BC laws would be enforced, so they acted as if the laws were constitutional. This conviction might in part stem from earlier rulings on other types of anti-takeover laws, such as the decision by the U.S. Supreme Court in 1987 to uphold Indiana's control share acquisition law (*CTS Corp. v. Dynamics Corp. of America*).

In further robustness checks, we impose a symmetry condition by using a sample period that begins n years before the first BC was passed and ends n years after the last BC law was passed. Thus, our sample period is from $1985 - n$ to $1991 + n$, where $n = 4, 5, 6$, and 7. In each case, the results are similar to our baseline results in Table 3.

Entry and exit of firms. A possible alternative story is that the passage of the BC laws caused a drop in profits for *all* firms, but in competitive industries firms who experienced a significant drop in profits went bankrupt and exited the sample, in which case our results would be driven by survivorship bias. To purge the sample of any entry and exit effects, we restrict the sample to only those firms that were present during the entire period from 1981 to 1995, which is the period four years before the first BC law was passed until four years after the last BC law was passed. The results are again similar to those in Table 3. While the BC dummy is close to zero and insignificant, the interaction term between the BC dummy and the HHI has a coefficient of -0.027 ($t-$statistic of 2.19), where the weaker significance is likely due to the fact that the sample size is reduced by more than 63%.

[13] Already in 1988, the Delaware BC law was held to be constitutional in *RP Acquisition Corp. v. Staley Continental, Inc.* The Wisconsin ruling in 1989 is widely regarded as a landmark decision, though, because the Wisconsin BC law was more stringent than the Delaware BC law, and because it was upheld by a federal appellate court and ended up with the U.S. Supreme Court, which sustained the decision of the appellate court.

Accounting for ROA outliers. Since ROA is a ratio, it can take on extreme values if the scaling variable (total assets) becomes too small, feeding concerns that our results might be driven by ROA outliers. In our basic specification, we trim 1% at each tail of the ROA distribution. Our results are similar if we trim either 5% or 10% at each tail, if we use log(1+ROA) as the dependent variable, if we exclude firms with assets below $1 million, if we estimate a median regression (with industry-fixed effects in place of firm-fixed effects), and if we estimate a Poisson regression in which ROA is converted into a count variable ranging from 1 to 10. In the case of the median and Poisson regressions, we compute standard errors using block bootstrapping with 51 blocks based on 200 bootstrap samples.

Heterogeneous time trends and state effects. To allow for heterogeneous time trends and state effects, we interact all control variables with time dummies and treatment-state dummies. In either case, the results are similar to those in Table 3.

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 the law's passage and one thereafter. Given that the BC dummy is a likely source of both cross-sectional and serial correlation, we cluster the standard errors at the state of incorporation level. However, we obtain similar results if we cluster at the state of location level.

In addition to clustering, we consider a number of alternative correction methods. The methods are all described in Bertrand, Duflo, and Mullainathan (2004). For instance, we obtain similar results if we use an AR(1) correction method, or if we use block bootstrapping with 51 blocks based on 200 bootstrap samples. An effective, albeit crude, way to deal with the issue of serial correlation is to collapse the data into two periods, before and after the BC law. Since "before" and "after" are different for each treatment state (while for control states "before" and "after" are not even defined), it is necessary to use a two-step procedure. 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 HHI. 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 regress the average residuals on the BC dummy and the interaction

term between the BC dummy and the average post-BC HHI. We use robust standard errors to correct for heteroskedasticity. We use a similar two-step procedure to deal with the issue of cross-sectional correlation. We collapse the data into state of incorporation-industry-year cells, based on the argument that our main variables of interest, the BC dummy and the HHI, are both of a higher level of aggregation. Under either of the two "collapsing methods", we obtain results that are similar to our baseline results in Table 3.

4. Empire building or quiet life?

While our results suggest that competition mitigates managerial agency problems, they do not say with agency problem is being mitigated. 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)? In **Table 8**, we attempt to distinguish between these two hypotheses. For brevity's sake, we only report the coefficients on the BC dummy and the interaction term between the BC dummy and the HHI, which are our main variables of interest.

In Panel (A), we estimate our basic specification using various proxies for empire building as the dependent variable. In column [1], we use capital expenditures (Compustat item #30) divided by total assets (item #6). Arguably, capital expenditures will be a poor proxy if much of the empire building activity comes in the form of acquisitions. To address this issue, we use total asset growth in column [2] and PPE growth in column [3]. Total asset growth is the percentage increase in total assets, while PPE growth is the percentage increase in property, plant, and equipment (item #8). We also construct more direct proxies for acquisition activity using data from the Securities Data Corporation's (SDC) database. Since the SDC data begin in 1979, the sample period is from 1979 to 1995. In column [4], our proxy is the sum of the value of all acquisitions made by a firm in a given year divided by the firm's average market capitalization in that year ("acquisition ratio"). In column [5], our proxy is the likelihood of being an acquirer, measured via a dummy that equals one if the firm makes at least one acquisition during the year and zero otherwise. As is shown, regardless of which proxy we use, neither the BC dummy nor the interaction term between the BC dummy and the HHI are significant, neither individually nor jointly.

In Panel (B), we estimate our basic specification using various proxies for "quiet life" as the dependent variable. In column [1], we use selling, general, and administrative expenses

("overhead costs", item #189) divided by total assets. In column [2], we use advertising expenses (item #45) divided by sales (item #12). In column [3], we use R&D expenses (item #46) divided by total assets. In column [4], we use costs of goods sold ("input costs", item #41) divided by sales. In column [5], we use real wages, which are computed 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 from the U.S. Bureau of Labor Statistics. As is shown, the BC dummy is always close to zero and insignificant. In columns [2] and [3], the interaction term between the BC dummy and the HHI has the right sign but is insignificant. In columns [1], [4], and [5], the interaction term between the BC dummy and the HHI is positive and significant, implying that overhead costs, input costs, and wages all increase after the passage of the BC laws, and only so in non-competitive industries.

Our 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 units within the company demanding bigger overhead budgets. Our results are not consistent with empire building. Let us conclude with two caveats. First, the various interaction terms in Panel (B) have smaller $t-$statistics than in our previous ROA regressions, presumably because the dependent variables are individual components of ROA. In other words, while the passage of the BC laws may have a large and significant effect on ROA overall, the effect on any single component of ROA may be relatively small. Second, the wage result in Panel (B) should be taken with caution. Not only is the sample relatively small, which is due to the fact that only few firms in Compustat report wage data, but the data is also extremely noisy (see Bertrand and Mullainathan, 1999). For instance, some firms report wage data only intermittently, while others report no wage 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. 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. Based on our results, we can compute the average wage increase (i.e., across all industries) as $-0.003 + 0.103 \times 0.218 = 0.019$, or 1.9%, which is roughly of the same order of magnitude.[14]

[14] The HHI in column [5] of Panel (B) has a mean of 0.218, which differs slightly from the mean of 0.226 in our ROA regressions because of differences in the samples.

5. Event-study results

Does the stock market anticipate that firms in competitive industries will be 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 date itself is well anticipated, it is unlikely to contain any new information. Rather, one must find an early date at which significant news about the BC law is disseminated to the public, e.g., the date of the first newspaper report about the law. For instance, Karpoff and Malatesta (1989), in their event study of anti-takeover laws, 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 event date. However, they do find significant abnormal returns when using the first date for which they found a newspaper report about the law as the event date.

Finding the first newspaper report about a BC law is sometimes 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. 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 (Nevada) had more than 100 firms—in the merged CRSP-Compustat sample in the year when the BC law was passed. 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). While this assumption is satisfied in many applications, it is not satisfied here. Since all firms incorporated in a given state are affected by the same event, their abnormal returns are likely correlated, leading to biased standard errors. A 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 (MacKinlay, 1997; Karpoff and Malatesta, 1989).

Our empirical methodology follows 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.[15] That is, we estimate

$$R_{jt} = \alpha_j + \beta_j R_{mt} + e_{jt}, \tag{3}$$

where R_{jt} is the daily return of the equally-weighted portfolio of all firms incorporated in state j, and where R_{mt} is the daily return of the equally-weighted CRSP market portfolio. Substituting the estimates back into (3), we obtain an estimate of the normal portfolio return $\hat{R}_{jt}$. The abnormal return of state portfolio j can then be computed as

$$AR_{jt} := R_{jt} - \hat{R}_{jt}. \tag{4}$$

To obtain cumulative abnormal returns (CAR), we sum up the abnormal returns over the desired time interval. We report average CARs based on the 19 state portfolios for the same time intervals as in 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 is any trend in the weeks prior to the event date, we also report average CARs for the time intervals [-30,-2], [-20,-2], and [-10,-2].

The above methodology 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 divide each state portfolio into equal-sized smaller portfolios. For each state j, we form a low- and high-HHI portfolio by sorting firms based on whether their HHI is below or above the median, respectively. We also do the same with low-, medium-, and high-HHI portfolios by sorting firms based on whether their HHI lies in the lowest, medium, and highest tercile, respectively. The remaining steps are the same as above.

The results are in Panel (A) of **Table 9**. As is shown in column [1], the average CAR in the two-day event window is -0.32% ($z-$statistic of -2.58). Moreover, 14 of the 19 individual CARs are negative. Of equal interest is that the average two-day CARs immediately before and after the two-day event window are small and insignificant. Taken together, this indicates that

[15] Choosing the estimation window next to the first time interval for which cumulative abnormal returns are computed (here: the time interval [-40,-2]) is common practice (MacKinlay, 1997). However, we obtain similar results if we estimate the market model over the time interval from 300 to 100 trading days before the event date. We also obtain similar results when using either a 3- or 4-factor model instead of the market model to calculate normal returns.

newspaper reports about the BC laws are associated with a significant decline in stockholder wealth.

Columns [2] and [3] show the average CARs separately for the low- and high-HHI portfolios. The average two-day event CAR for the low-HHI portfolio is close to zero and insignificant. By contrast, the average two-day event CAR for the high-HHI portfolio is -0.54% ($z-$statistic of -2.36). Thus, while firms in competitive industries experience no significant stock price impact around the date of the first newspaper report about the BC law, 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-HHI 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 HHI tercile) experience a significant abnormal stock price decline. Moreover, and consistent with our previous results in column [3] of Table 3, the stock price decline is monotonic in the HHI. While the average two-day event CAR for the medium-HHI portfolio is -0.44% ($z-$statistic of -1.67), the average two-day event CAR for the high-HHI portfolio is -0.67% ($z-$statistic of -2.31).[16]

In Panel (B), we test whether the average two-day event CARs are significantly different between HHI groups. For each state j, we form a hedge portfolio that is long in the top-HHI group and short in the bottom-HHI group, where "top" and "bottom" are defined by the underlying HHI partition. Average CARs and $z-$statistics are then computed based on the 19 hedge portfolios. In column [1], we form a hedge portfolio that is long in the above-median HHI group and short in the below-median HHI group. As is shown, the average CARs are not significantly different between the two groups ($z-$statistic of 1.41). This is not surprising. As is often the case with stock returns, significant differences can only be found in the extremes. In column [2], we form a hedge portfolio that is long in the top-tercile HHI group and short in the bottom-tercile HHI group. The difference between the two groups is now marginally significant ($z-$statistic of 1.76). If we consider finer HHI partitions, this difference is even more pronounced. In columns [3] and [4], we form hedge portfolios based on HHI quartiles and quintiles, respectively. In either case, the difference between the bottom- and top-HHI group is significant at the 5% level ($z-$statistics of 2.02 and 2.06, respectively).

[16] We obtain a similar monotonic pattern using median CARs. The median two-day event CARs for the low-, medium-, and high-HHI portfolios are 0.06%, -0.46%, and -0.67%, respectively. The ratios of positive to negative CARs in the two-day event window are 10:9, 4:15, and 5:14, respectively.

Let us conclude with a word of caution. In some way, the event-study results also partly address concerns that our main results are based on accounting variables, which can be manipulated (see Section 3.3). As we have just shown, the results are qualitatively similar if we consider stock returns. And yet, the event-study results do not offer conclusive evidence that firms in non-competitive industries experience a larger drop in operating profitability after the passage of the BC laws. For instance, it could be that, for various reasons, the value gains from (hostile) takeovers are higher in non-competitive industries, in which case the larger stock price decline in these industries might simply reflect the capitalized value of higher forgone value gains.

6. Conclusion

Does competition mitigate managerial agency problems? The evidence presented in this paper suggests that the answer 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 effect on firms in competitive and non-competitive industries. Consistent with the notion that BC laws weaken corporate governance and increase the 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 notion that competitive industries have little 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, 2008). In fact, after dividing the sample into low-, medium- and high-HHI portfolios it turns out that the alpha in the low-HHI portfolio is small and insignificant, whereas that in the high-HHI 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-HHI portfolio is much higher

than the alpha reported in GIM. While these preliminary findings are encouraging, more research is needed before we can firmly conclude that firm-level corporate governance instruments are moot in competitive industries.

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Table 1
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 taken from Bertrand and Mullainathan (2003). States of location and states of incorporation are both taken 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 2
Summary Statistics

In Panel (A), return on assets (ROA) is operating income before depreciation and amortization (Compustat item #13) divided by total assets (item #6). In Panel (B), "All States" refers to all states in Table 1. "Eventually BC" refers to all states that passed a BC law during the sample period. "Never BC" refers to all states that never passed a BC law during the sample period. Size is the natural logarithm of total assets. Age is the natural logarithm of one plus the number of years the firm has been in Compustat. HHI 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 firms' sales (item #12). All figures in Panel (B) are sample means. Standard deviations are in parentheses. The sample period is from 1976 to 1995.

Panel (A): Sample Distribution of ROA (trimmed at 1%)

Mean	Median	Minimum	Maximum
0.074	0.104	-1.051	0.417

Panel (B): "Eventually BC" States vs. "Never BC" States

	All States	Eventually BC	Never BC
	[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)
HHI	0.225	0.226	0.214
	(0.155)	(0.156)	(0.148)

Table 3
Does Corporate Governance Matter in Competitive Industries?

BC is a dummy variable that equals one if the firm is incorporated in a state that has passed a BC law. HHI(Low), HHI(Medium), and HHI(High) are dummy variables that equal one if the HHI lies in the bottom, medium, and top tercile, respectively, of its empirical distribution. "Industry-year" and "state-year" are variables that indicate the mean of the dependent variable in the firm's industry and state of location, respectively, in a given year, excluding the firm itself. BC Year(-1) is a dummy variable that equals one if the firm is incorporated in a state that will pass a BC law in one year from now. BC Year(0) is a dummy variable that equals one if the firm is incorporated in a state that passes a BC law this year. BC Year(1) and BC Year(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. All other variables are defined in Table 2. 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.

Panel (A): Main Results

	[1]	[2]	[3]
Dependent Variable:	ROA	ROA	ROA
BC	-0.006**	0.001	
	(2.25)	(0.35)	
BC x HHI		-0.033***	
		(4.95)	
BC x HHI(Low)			0.002
			(0.68)
BC x HHI(Medium)			-0.008**
			(2.56)
BC x HHI(High)			-0.012***
			(4.59)
Industry-year	0.206***	0.206***	0.206***
	(9.67)	(9.60)	(9.61)
State-year	0.249***	0.249***	0.248***
	(8.86)	(8.83)	(8.77)
Size	0.096***	0.097***	0.097***
	(20.27)	(20.38)	(20.34)
Size-squared	-0.007***	-0.007***	-0.007***
	(20.09)	(20.42)	(20.53)
Age	-0.021***	-0.021***	-0.021***
	(5.34)	(5.44)	(5.37)
HHI	0.015*	0.025***	
	(1.66)	(2.58)	
HHI(Medium)			0.006*
			(1.88)
HHI(High)			0.008**
			(2.12)
Firm Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Observations	77,460	77,460	77,460
Adj. R-squared	0.68	0.68	0.68

Panel (B): Endogeneity of BC Laws?

Dependent Variable: ROA	
BC Year(-1)	-0.001
	(0.17)
BC Year(0)	-0.002
	(0.39)
BC Year(1)	-0.000
	(0.07)
BC Year(2+)	0.004
	(0.74)
BC Year(-1) x HHI	0.001
	(0.07)
BC Year(0) x HHI	-0.027**
	(2.06)
BC Year(1) x HHI	-0.032***
	(4.33)
BC Year(2+) x HHI	-0.034***
	(4.15)
Industry-year	0.210***
	(7.70)
State-year	0.256***
	(7.74)
Size	0.097***
	(20.37)
Size-squared	-0.007***
	(20.44)
Age	-0.020***
	(5.44)
HHI	0.025**
	(2.53)
Firm Fixed Effects	Yes
Year Fixed Effects	Yes
Observations	77,460
Adj. R-squared	0.68

Table 4
Decrease in Operating Profitability or Change in Firms' Earnings Management?

BC(-12m to -6m) is a dummy variable that equals one if the firm is incorporated in a BC state and the firm's fiscal year end lies between 12 months and 6 months prior to the month of the law's passage (which is denoted as "0m"). BC(-6m to 0m), BC(0m to 6m), BC(6m to 12m), and BC(12m +) are all defined analogously. Discretionary accruals are computed as in Dechow, Sloan, and Sweeney (1995). Discretionary current accruals are computed as in Teoh, Welch, and Wong (1998). All other variables are defined in Tables 2 and 3. 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.

Panel (A): Fiscal Year Ends

Dependent Variable: ROA	
BC(-12m to -6m)	-0.001
	(0.23)
BC(-6m to 0m)	0.002
	(0.51)
BC(0m to 6m)	-0.003
	(0.70)
BC(6m to 12m)	0.000
	(0.04)
BC(12m +)	0.003
	(0.79)
BC(-12m to -6m) x HHI	0.001
	(0.08)
BC(-6m to 0m) x HHI	-0.006
	(0.39)
BC(0m to 6m) x HHI	-0.019
	(0.81)
BC(6m to 12m) x HHI	-0.031***
	(2.62)
BC(12m +) x HHI	-0.036**
	(4.45)
Industry-year	0.207***
	(9.61)
State-year	0.250***
	(8.95)
Size	0.097***
	(20.38)
Size-squared	-0.007***
	(20.43)
Age	-0.021***
	(5.42)
HHI	0.025***
	(2.59)
Firm Fixed Effects	Yes
Year Fixed Effects	Yes
Observations	77,460
Adj. R-squared	0.68

Panel (B): Earnings Management

Dependent Variable:	[1] Discretionary Accruals	[2] Discretionary Current Accruals
BC	-0.000	0.000
	(0.12)	(0.15)
BC x HHI	-0.001	-0.003
	(0.28)	(0.39)
Industry-year	0.375***	0.403***
	(13.97)	(21.94)
State-year	0.007	0.054**
	(0.99)	(2.52)
Size	-0.012***	-0.016***
	(5.50)	(10.65)
Size-squared	0.000	0.001***
	(1.40)	(4.86)
Age	-0.038***	-0.030***
	(17.33)	(13.90)
HHI	-0.004	-0.004
	(0.55)	(0.76)
Firm Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
Observations	63,749	64,070
Adj. R-squared	0.29	0.30

Table 5
Do BC Laws Reduce the Takeover Threat?

"Likelihood of being acquired" is a dummy variable that equals one if the company is acquired in the next calendar year. The acquisition data are obtained from the Securities Data Corporation's (SDC) database. All other variables are defined in Tables 2 and 3. Standard errors are clustered at the state of incorporation level. The sample period is from 1978 to 1995. *t*-statistics are in parentheses. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

Dependent Variable: Likelihood of Being Acquired	[1]	[2]
BC	-0.004	
	(1.39)	
BC x HHI(Low)		-0.007**
		(2.19)
BC x HHI(Medium)		-0.004
		(1.05)
BC x HHI(High)		-0.001
		(0.44)
Industry-year	0.087***	0.087***
	(4.87)	(4.86)
State-year	0.027	0.027
	(0.84)	(0.84)
Size	-0.002	-0.002
	(1.21)	(1.27)
Size-squared	-0.001***	-0.001***
	(3.85)	(3.76)
Age	0.041***	0.041***
	(13.98)	(14.03)
HHI(Medium)	0.001	0.000
	(0.65)	(0.02)
HHI(High)	0.002	0.001
	(0.97)	(0.31)
Firm Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
Observations	77,142	77,142
Adj. R-squared	0.29	0.29

Table 6
Alternative Competition Measures I (All Industries)

HHI (2-digit) and HHI (4-digit) are HHIs based on 2-digit and 4-digit SIC industries, respectively. NPM is operating income before depreciation and amortization (Compustat item #13) divided by sales (item #12). Industry NPM is the median NPM in a given year and 3-digit SIC industry. All other variables are defined in Tables 2 and 3. 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.

	[1]	[2]	[3]
Dependent Variable: ROA	HHI (2-digit)	HHI (4-digit)	Industry NPM
BC	-0.000	0.000	0.000
	(0.15)	(0.11)	(0.07)
BC x HHI (2-digit)	-0.056***		
	(5.15)		
BC x HHI (4-digit)		-0.022***	
		(3.23)	
BC x Industry NPM			-0.054***
			(3.03)
Industry-year	0.203***	0.201***	0.136***
	(9.90)	(9.72)	(9.67)
State-year	0.251***	0.249***	0.255***
	(8.76)	(9.26)	(10.98)
Size	0.096***	0.096***	0.089***
	(19.30)	(21.35)	(19.40)
Size-squared	-0.007***	-0.007***	-0.006***
	(18.57)	(21.25)	(17.98)
Age	-0.021***	-0.020***	-0.020***
	(5.21)	(4.99)	(6.26)
HHI (2-digit)	0.011		
	(0.76)		
HHI (4-digit)		0.017**	
		(2.13)	
Industry NPM			0.098***
			(4.58)
Firm Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Observations	77,135	77,446	76,365
Adj. R-squared	0.68	0.68	0.68

Table 7
Alternative Competition Measures II (Manufacturing Industries)

HHI (Census) is the HHI based on 4-digit SIC manufacturing industries (SIC 2000-3999) provided by the Census Bureau. The index is 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. 'Import penetration" is a dummy variable that equals one if the import penetration in a given 4-digit SIC manufacturing industry lies above the industry mean. Import penetration is defined as imports divided by the sum of total shipments minus exports plus imports. The import data are obtained from Peter Schott's webpage and are described in Feenstra (1996) and Feenstra, Romalis and Schott (2002). All other variables are defined in Tables 2 and 3. 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.

	[1]	[2]	[3]
Dependent Variable: ROA	HHI (Census)	Import Penetration	HHI (Census) & Import Penetration
BC	-0.003	-0.004	-0.000
	(0.83)	(0.95)	(0.09)
BC x HHI (Compustat)			
BC x HHI (Census)	-0.081***		-0.104***
	(2.84)		(2.62)
BC x (1 - Import Penetration)		-0.007*	-0.007
		(1.90)	(1.29)
Industry-year	0.148***	0.177***	0.154***
	(6.21)	(8.07)	(6.08)
State-year	0.284***	0.348***	0.273***
	(3.99)	(5.87)	(2.60)
Size	0.115***	0.097***	0.091***
	(13.13)	(18.57)	(13.77)
Size-squared	-0.009***	-0.007***	-0.007***
	(12.45)	(17.55)	(13.96)
Age	-0.043***	-0.031***	-0.037***
	(5.39)	(5.12)	(5.01)
1 - Import Penetration		0.011***	0.011**
		(3.09)	(2.44)
Firm Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Observations	19,244	21,031	17,551
Adj. R-squared	0.73	0.69	0.71

Table 8
Empire Building or Quiet Life?

In Panel (A), capital expenditures (Compustat item #30) are divided by total assets (item #6). Asset growth is the percentage increase in total assets from one year to the next. PPE growth is the percentage increase in property, plant, and equipment (item #8) from one year to the next. "Acquisition ratio" is the sum of the value of all acquisitions made by the firm in a given year divided by the firm's average market capitalization in that year (from CRSP). The acquisition data are obtained from the Securities Data Corporation's (SDC) database. "Likelihood of being acquirer" is a dummy variable that equals one if the firm makes at least one acquisition during the year. In Panel (B), selling, general & admin. expenses is SG&A expenses (item #189) divided by total assets. Advertising expenses (item #45) and costs of goods sold (item #41) are both 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 from the U.S. Bureau of Labor Statistics. All other variables are defined in Tables 2 and 3. For brevity's sake, only the coefficients on BC and BC × HHI are reported. Standard errors are clustered at the state of incorporation level. The sample period is from 1976 to 1995, except for columns [4] and [5] of Panel (A), where the sample period is from 1979 to 1995. *t*-statistics are in parentheses. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

Panel (A): Empire Building

	[1]	[2]	[3]	[4]	[5]
Dependent Variable:	Capital Expenditures	Asset Growth	PPE Growth	Acquisition Ratio	Likelihood of Being Acquirer
BC	-0.000	-0.004	-0.003	0.000	0.003
	(0.27)	(0.70)	(0.53)	(0.02)	(0.42)
BC x HHI	0.001	-0.004	-0.000	0.001	-0.010
	(0.18)	(0.39)	(0.04)	(0.43)	(0.48)
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	74,435	67,806	64,449	70,248	75,415
Adj. R-squared	0.55	0.35	0.22	0.29	0.36

Panel (B): Quiet Life

	[1]	[2]	[3]	[4]	[5]
Dependent Variable:	Selling, General & Admin. Expenses	Advertising Expenses	R&D Expenses	Costs of Goods Sold	Wages (Real)
BC	0.005	0.000	0.001	-0.002	-0.003
	(0.80)	(0.59)	(0.49)	(0.20)	(0.12)
BC x HHI	0.029**	0.003	0.007	0.053**	0.103**
	(2.51)	(1.04)	(1.39)	(2.44)	(2.00)
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	68,561	28,389	39,359	74,758	8,651
Adj. R-squared	0.81	0.80	0.76	0.60	0.89

Table 9
Event-Study Results

The methodology used to calculate cumulative abnormal returns (CARs) is described in Section 5. The event date is the date of the first newspaper report about the BC law. The two-day event window is denoted by [-1,0]. The numbers reported in the table are average portfolio CARs based on 19 state portfolios. 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. The construction of the state-HHI portfolios, bottom-HHI portfolios, and top-HHI portfolios is described in Section 5. The hedge portfolio is long in the top-HHI portfolio and short in the bottom-HHI portfolio. *z*-statistics are in parentheses. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

Panel (A): Event-Study Results

	[1] All Firms	[2] HHI (Low)	[3] HHI (High)	[4] HHI (Low)	[5] HHI (Medium)	[6] HHI (High)
[-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)

Panel (B): Hedge Portfolios

HHI Partition	Median	Terciles	Quartiles	Quintiles
Bottom-HHI Portfolio	-0.10	0.08	0.17	0.19
	(-1.29)	(-0.53)	(-0.62)	(-0.64)
Top-HHI Portfolio	-0.54**	-0.67**	-0.75**	-0.78**
	(-2.36)	(-2.31)	(-2.44)	(-2.49)
Hedge Portfolio	0.44	0.75*	0.92**	0.97**
	(1.41)	(1.76)	(2.02)	(2.06)

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Does Corporate Governance Matter?

A brief synopsis of **Does Corporate Governance Matter in Competitive Industries?** (working paper, 2007) by Xavier Giroud and Holger M. Mueller

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GOVERNANCE

Does Corporate Governance Matter?

Competition may be the more effective mechanism.

There is a time and a place when corporate governance has little influence over performance, say two researchers at New York University's Leonard N. Stern School of Business, because competitive forces cut away at management fat.

Holger M. Mueller and Xavier Giroud, associate professor of finance and doctoral student, respectively, studied the performance of companies before and after 30 anti-takeover laws were passed in different states between 1985 and 1991, to determine how the provisions affected performance. In theory, laws that make hostile takeovers more difficult weaken corporate governance and undermine the ability of directors (and investors) to hold management to task. With more opportunity for managers to slack off, financial performance could suffer.

Sure enough, that's what Mueller and Giroud found — but not for every industry. "A negative change in corporate governance [caused by the passage of an anti-takeover law], has a negative effect in noncompetitive industries," explains Mueller. "But a change in corporate governance has no effect in competitive industries."

In the 2007 working paper, *Does Corporate Governance Matter in Competitive Industries?* Mueller and Giroud explain how they tracked 10,960 companies from 1976 to 1995, and used the Herfindahl index — the sum of the squares of competitors' market shares in a single industry — as a measure of competitiveness. With this index, if one company has a monopoly position, the value approaches one; the more evenly a larger group of competitors divides market share, the closer the value approaches zero. What they found is that the passage of an anti-takeover law reduced return on assets by an average of 0.6% in that state's companies relative to the nation as a whole, but most of that effect was driven by companies with higher Herfindahl indices — the noncompetitive industries.

The analysis suggests more about how managers in noncompetitive industries are acting, by looking at companies' cost structures. The authors find evidence that leaders in those industries are softer on spending. "CEOs want to have a quiet life. They don't want to argue and quarrel with other parties," says Mueller. "So they fight less hard with unions and with input suppliers. …Cutting costs is a tough thing." When competition fails to enforce discipline on managers, weaker corporate governance indeed allows managers to taste the quiet life.

To be sure, the authors studied the corporate governance regime, not the governance of specific companies. So while they establish a causal relationship between a governance regime and corporate performance, they don't offer much on how to make boards more effective. In actuality, there is a great deal of heterogeneity of governance approaches and abilities among companies operating within a governance regime, a point the authors concede.

In addition, the study's measure of governance covers anti-takeover provisions; "bad governance" means shielding a company from hostile takeovers. Any director will tell you that the practice of governance involves many activities — including compliance with financial regulations, setting CEO pay and advising on strategy — to which anti-takeover provisions are only peripherally related. So anti-takeover provisions may not be a perfect window into the boardroom.

Still, Mueller says, "It's one example of a change in corporate governance. We wanted to see if the effect is different for competitive and noncompetitive industries. It may be that another [kind of] change in corporate governance has a different effect. But this is one piece of evidence that firms in competitive industries are not affected by corporate governance."

The granddaddy of corporate governance laws, of course, was the Sarbanes-Oxley Act of 2002. Since the regulation applied across the entire country, it's hard to find a control to compare against, using the authors' methodology. However, Mueller says, "I would conjecture, if there were any way to measure it against a control group, that changes that improve governance should improve performance mainly for firms in noncompetitive industries."

What does that tell directors? "You have to watch management much more closely if it's sitting in a very comfortable position where it doesn't face much competition," says Mueller. "On the one hand, you can make a lot more profit if you don't face competition. On the other hand, those profits could be squandered by management."

For policy-makers, Mueller suggests a different conclusion. "If competition is really an effective governance mechanism, perhaps we should think about laws to make markets more competitive. It kills two birds with one stone: We don't have antitrust problems and we will have better corporate governance.

"In a sense it all boils down to Adam Smith, I suppose," Mueller adds. "We think about all these complex corporate governance mechanisms, but competitive pressure is perhaps the best mechanism."

For more information, download the paper from http://ssrn.com/abstract_id=1006118, or contact Holger Mueller at hmueller@stern.nyu.edu.

— Larry Yu

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