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 identifying variation, we examine if such laws have a different effect on firms in competitive and non-competitive industries. We find that while firms in non-competitive industries experience a substantial drop in operating performance, firms in competitive industries experience virtually no effect. Though consistent with the general notion that competition mitigates managerial agency problems, our results are, in particular, supportive of the stronger Alchian-Friedman-Stigler hypothesis that managerial slack cannot exist, or survive, in competitive industries. When we examine which agency problem competition mitigates, we find evidence in favor of a "quiet-life" hypothesis. While capital expenditures are unaffected by the passage of BC laws, input costs, wages, and overhead costs all increase, and only so in non-competitive industries. We also conduct event studies around the dates of the first newspaper reports about the BC laws. We find that while firms in non-competitive industries experience a significant decline in their stock prices, firms in competitive industries experience a small and insignificant price impact.

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1 Introduction

It is a widely held view among many economists that product market competition mitigates managerial agency problems.[1] Views differ, however, when it comes to the issue of how "perfect" managerial incentives are in competitive industries. Some, like Leibenstein (1966), argue that competition reduces managerial slack but stop short of claiming that it resolves all (X-) inefficiencies. Others, like Alchian (1950), Friedman (1953), and Stigler (1958), go much further, essentially arguing that managerial slack cannot exist, or survive, in competitive industries.[2] If managers seek to maximize their long-run income, and if inefficient firms are wiped out from the market by the forces of competition (or "natural selection"), then, as Machlup (1967, p. 19) concludes, "maximization of managerial income and maximization of profits come to the same thing if competition is effective."

The argument that managerial slack cannot exist, or survive, in competitive industries has far-reaching implications.[3] For instance, it implies that "the managerial extension and enrichment of the firm was not needed except where firms in the industry were large and few, and not under the pressure of competition" (Machlup (1967, p. 11)). In other words, topics that have been studied extensively over the past decades, such as managerial discretion and agency conflicts between shareholders and management leading to deviations from profit-maximizing behavior, might have little bearing on firms in competitive industries.[4] By implication, then,

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

[2]Scherer (1980, p. 38) summarizes the argument as follows: "When forced into the trenches on the question of whether firms maximize profits, economists resort to the ultimate weapon in their arsenal: a variant of Darwin's natural selection theory. Over the long pull, there is one simple criterion for the survival of a business enterprise: Profits must be nonnegative. No matter how strongly managers prefer to pursue other objectives [...] failure to satisfy this criterion means ultimately that a firm will disappear from the economic scene."

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

[4]Scherer (1980, p. 38-41) notes that organizational slack, "princely managerial salaries," and income redistri-

firms in competitive industries could, at least as a first approximation, be rightfully viewed as a "profit-maximizing black box," as is done by neoclassical price theory. Second, empirical studies on corporate governance could benefit from including measures of industry competition, such as the Herfindahl index (see also Conclusion). Finally, efforts to improve corporate governance could benefit from focusing primarily on firms in non-competitive industries. Moreover, such efforts could be broadened to include measures aimed at improving an industry's competitiveness, such as deregulation and antitrust laws.

To examine the empirical relevance of these arguments, we use exogenous variation in corporate governance in the form of 30 business combination (BC) laws passed between 1985 and 1991 on a state-by-state basis.[5] By reducing the fear of a hostile takeover, such laws weaken corporate governance and create more opportunity for managerial slack.[6] Typically, BC laws impose a moratorium on certain kinds of transactions, including mergers and asset sales, between a large shareholder and the firm for a period usually ranging from three to five years after the shareholder's stake has passed a prespecified threshold. This moratorium severely hinders corporate raiders from gaining access to the target firm's assets for the purpose of paying down acquisition debt, thus making hostile takeovers more difficult and often impossible.[7] Like Bertrand and Mullainathan (2003), we focus on BC laws because they are more stringent than other antitakeover laws, providing a source of variation that can hopefully generate statistically and economically significant results. For instance, Karpoff and Malatesta (1989) find that only BC laws, but not fair price statutes or control share acquisition statutes, cause a negative stock price reaction for the firms incorporated in passing states.

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bution from stockholders to management are primarily a problem of firms possessing market power. By contrast, "the natural selection process is a stern master in a competitive environment."

[5] Many authors share the view that antitakeover laws are exogenous for all but perhaps a few firms motivating the laws, e.g., Romano (1987), Karpoff and Malatesta (1989), Comment and Schwert (1995), Garvey and Hanka (1999), Bertrand and Mullainathan (1999, 2003), Cheng, Nagar, and Rajan (2004), and Rauh (2006). Perhaps the most prominent study using BC laws as a source of identifying variation is Bertrand and Mullainathan (2003). The authors argue on p. 1045 that "[t]hese laws avoid the endogeneity problem to the extent that they are passed by states and are not endogenously driven by firm-specific conditions." We specifically address the potential endogeneity of BC laws in our empirical study.

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

[7] For background information on BC laws, see Sroufe and Gelband (1990) and Suggs (1995).

Our identification strategy benefits from the lack of congruence between a firm's industry, state of location, and state of incorporation. For instance, a firm's state of incorporation says little about its industry. Likewise, only 38 percent of the firms in our sample are incorporated in their state of location. BC laws, in turn, apply to all firms in a given state of incorporation, regardless of their state of location or industry. This lack of congruence helps us to separate out the effects of local and industry shocks contemporaneous with the BC laws from the effects of the laws themselves (see Bertrand and Mullainathan (2003)). This addresses several concerns. First, our estimate of the laws' effects could be biased, reflecting in part the impact of contemporaneous shocks. Second, our results could be spurious, coming entirely from contemporaneous shocks. Third, and perhaps most important, economic conditions could influence the passage of BC laws. For example, poor economic conditions in a particular state might induce local firms to lobby for an antitakeover law in that state.

Using BC laws as a source of identifying variation, we ask a simple question. Assuming these laws have an effect on firms' operating performance (which is a testable assumption), does the effect differ for firms in competitive and non-competitive industries? We obtain three main results, which turn out to be robust across many specifications. First, consistent with the notion that BC laws create more opportunity for managerial slack, we find that they have a negative effect on operating performance. On average, the return on assets (ROA) drops by 0.6 percentage points. Second, the effect becomes increasingly more negative the less competitive the industry is. For example, ROA drops by 0.1 percentage points in the lowest Herfindahl quintile, but by 1.5 percentage points in the highest Herfindahl quintile. Generally, a one-standard deviation increase in the Herfindahl index is associated with a drop in ROA of 0.5 percentage points. Third, the effect is virtually zero and insignificant in highly competitive industries.

While all three results are consistent with the notion that competition mitigates managerial agency problems, the third result, in particular, is supportive of the stronger hypothesis by Alchian (1951), Friedman (1953), and Stigler (1958) that managerial slack cannot exist, or survive, in competitive industries. We also try to examine which agency problem competition mitigates. Does it curb managerial empire building? Or does it prevent managers from enjoying a "quiet life" by forcing them to "undertake cognitively difficult activities" (Bertrand and Mullainathan (2003, p. 1067))? We find no evidence for empire building: Capital expenditures are seemingly unaffected by the passage of BC laws. By contrast, we find that input costs, wages, and overhead costs all increase after the passage of BC laws. Notably, this result is entirely driven by non-competitive industries. In competitive industries, by contrast, the effect is virtually zero and insignificant. Overall, our findings are supportive of a "quiet-life" hypothesis whereby managers insulated from *both* takeover pressure *and* competitive pressure seek to avoid cognitively difficult activities, such as haggling with input suppliers, labor unions, and individual organizational units demanding bigger overhead budgets.[8]

To collect further evidence, we conduct event studies around the dates of the first newspaper reports about the BC laws. On average, we find a small but significant cumulative abnormal return (CAR) of −0.32%. While this is somewhat smaller than the average CAR of −0.47% reported by Karpoff and Malatesta (1989), the numbers are roughly of the same order of magnitude. Importantly, when we perform the event study separately for portfolios with low and high Herfindahl indices, we find that the average CAR for the low-Herfindahl portfolio is close to zero and insignificant, whereas the average CAR for the high-Herfindahl portfolio is −0.54% and significant. A similar pattern emerges when we form three portfolios: While the average CAR for the low-Herfindahl portfolio is again close to zero and insignificant, the average CARs for the medium- and high-Herfindahl portfolios are −0.44% and −0.67%, respectively, both of which are significant. Not only do these findings suggest that the stock market anticipates that BC laws have a negative impact only on firms in less competitive industries, they also suggest that the market knows that firms in high-Herfindahl industries fare worse than firms in medium-Herfindahl industries.

This paper is the first to provide direct evidence of the Alchian-Friedman-Stigler hypothesis that competitive industries leave no room for managerial slack. Given an exogenous increase in the opportunity for managerial slack, we show that operating performance drops for firms in non-competitive industries but not for firms in competitive industries. In terms of research question, the paper most closely related to ours is perhaps Nickell (1996), who shows that higher competition leads to higher productivity growth in a sample of U.K. manufacturing firms. While consistent with a managerial agency explanation, Nickell's finding is also consistent with alternative explanations. For example, firms in competitive industries may have higher productivity growth because there are more industry peers from whose successes and failures they can learn.

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

More generally, our paper is related to a growing literature that documents a link between product market competition and corporate governance. Most papers find that (endogenous) firm-level corporate governance instruments vary with industry competition. Examples include managerial incentive schemes (Aggarwal and Samwick (1999)), board structure (Karuna (2007)), and firm-level takeover defenses (Cremers, Nair, and Peyer (2006)), Karuna (2007)). Finally, Guadalupe and Pérez-González (2005) show that competition affects private benefits of control, as measured by the voting premium between shares with different voting rights. Their study is especially interesting because it provides some first-hand evidence of the bankruptcy-risk channel that also implicitly underlies the Alchian-Friedman-Stigler hypothesis.

The rest of this paper is organized as follows. Section 2 presents the data. Section 3 lays out the empirical strategy. Section 4 presents our main results as well as robustness checks. Section 5 presents evidence from event studies. Section 6 concludes.

2 Data

2.1 Sample Selection

Our main data source is Standard and Poor's COMPUSTAT. All COMPUSTAT items are from the annual data files, except for states of incorporation, which are from the quarterly files. To be included in our sample, a firm must be located and incorporated in the United States. We exclude all observations for which the book value of assets or net sales are either missing or negative. We also exclude regulated utility firms (SIC 4900-4999).[9] The sample period is from 1976 to 1995, which is the same period as in Bertrand and Mullainathan (2003), who consider the same 30 business combination (BC) laws as we do.

These selection criteria leave us with 10,960 firms and 81,095 firm-year observations. **Table I** shows how many firms are located and incorporated in each state. The state of location, as defined by COMPUSTAT, indicates the state in which a firm's headquarters are located. The state of incorporation is a legal concept and determines, inter alia, which BC law, if any, is relevant for 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

[9]Whether or not we exclude regulated utilities makes no difference for our results. We also obtain similar results if we exclude financial firms (SIC 6000-6999). Likewise, we obtain similar results if we consider only manufacturing firms (SIC 2000-3999); see Section 3.2.

incorporation are rare (e.g., Romano (1993)). To provide further evidence on this issue, Bertrand and Mullainathan (2003) have randomly sampled 200 firms from their panel and checked if any of these firms had changed their state of incorporation during the sample period. Only three firms had changed their state of incorporation, all of them to Delaware. Importantly, all three changes 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.

2.2 Definition of Variables and Summary Statistics

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

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

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

A look at the empirical distribution of the Herfindahl index shows that it has a small "spike" at the right endpoint, which points to misclassification. To avoid that outliers and misclassification drive our results, we drop 2.5% of the firm-year observations at the right tail of the

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

distribution.[11] We will further address the issue of measurement error in robustness checks by using Herfindahl dummies. Also in robustness checks, we will consider non-COMPUSTAT measures of competition that are only available for manufacturing industries.

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

The remaining variables are defined as follows. Size is the natural logarithm of total assets. Age is the natural logarithm of one plus the firm's age, which is the number of years the firm has been in COMPUSTAT. Leverage is long-term debt (item #9) plus debt in current liabilities (item #34) divided by total assets. E-Index is the entrenchment index by Bebchuk, Cohen, and Ferrell (2005) and is obtained from Lucian Bebchuk's webpage. G-index is the governance index by Gompers, Ishii, and Metrick (2003) and is obtained from the IRRC database. Both indices are only available for the years 1990, 1993, and 1995 during the sample period. Additional COMPUSTAT variables will be introduced in Section 3.4.

Table II provides separate summary statistics for firms incorporated in states that passed a BC law during the sample period ("Eventually Business Combination") and firms incorporated in states that did not pass a BC law ("Never Business Combination"). Splitting the sample this way shows that firms in passing states are bigger and slightly older on average. On the other hand, there are no significant differences with respect to leverage, Herfindahl index, and E-index. That firms in passing states have a higher G-index is partly mechanical, because the G-index assigns one index point if the firm is incorporated in a state that passed a BC law. That firms in passing states are bigger and slightly older deserves more attention, because it raises

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

the question if the control group is an appropriate one.[12] There are several reasons why this should not be a serious concern. First, due to the staggering of the BC laws over time, firms in the "Eventually Business Combination" group are both control firms (before the BC law) and treatment firms (after the BC law). Second, we control for age and size in all our regressions. Third, we show in robustness checks that our results are unchanged if we focus only on states that passed a BC law during the sample period.

2.3 Empirical Methodology

Using a differences-in-differences approach, we examine whether the passage of 30 BC laws between 1985 and 1991 affects operating performance differently depending on how competitive the firm's industry is. The basic equation we estimate is

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

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

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

The differences-in-differences approach is easily explained. The first difference compares

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

operating performance before and after the passage of BC laws separately for firms in the control and treatment group. This yields two differences, one for the control group and one for the treatment group. The second difference takes the difference between these two differences. The result is an estimate of the effect of BC laws on operating performance. The interaction term $BC \times Herfindahl$ allows us to estimate a third difference, namely, whether the BC laws affect operating performance differently depending on how competitive the firm's industry is. 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 during the sample period. The control group includes all firms incorporated in states that did not pass a BC law by time t. Thus, it includes firms incorporated in states that never passed a BC law during the sample period as well as firms incorporated in states that passed a law after time t.

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

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

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

protection from hostile takeovers.[14]

While the inclusion of state- and industry-year controls can address concerns that the BC laws are the outcome of lobbying at the local and industry level, respectively, it remains the possibility that lobbying occurs at the state of incorporation level. However, as Bertrand and Mullainathan (2003) point out, for this to be a serious concern, it would have to be the case that a broad coalition of firms incorporated in the same state, which all experience a decline in profitability and, in our case, additionally operate in less competitive industries, successfully lobby for an antitakeover law in their state of incorporation. Given the evidence in Romano (1987), who portrays lobbying for antitakeover laws as an exclusive political process, this seems rather unlikely. Typically, antitakeover laws were adopted, often during emergency sessions, under the political pressure of a single firm facing a takeover threat, not a broad coalition of firms. Hence, for all but a few firms, the laws were exogenous.[15]

Similar to Bertrand and Mullainathan (2003), we address the issue of broad-based lobbying by investigating the dynamic effects of BC laws. Specifically, we replace the interaction term in equation (1) with five interaction terms: $Before(-2) \times Herfindahl$, $Before(-1) \times Herfindahl$, $Before(0) \times Herfindahl$, $After(1) \times Herfindahl$, and $After(2+) \times Herfindahl$, where $Before(-2)$ and $Before(-1)$ are dummy variables that equal one if the firm is incorporated in a state that will pass a BC law in two years and one year from now, respectively, $Before(0)$ is a dummy variable that equals one if the firm is incorporated in a state that passes a BC law this year, and $After(1)$ and $After(2+)$ are dummy variables that equal one if the firm is incorporated in a state that passed a BC law one year and two or more years ago, respectively. If BC laws were passed in response to political pressure of a broad coalition of firms, then we

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

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

should see an "effect" of the laws already prior to their passage. In particular, if the coefficients on $Before(-2) \times Herfindahl$ or $Before(-1) \times Herfindahl$ were significant, this would be symptomatic of reverse causation.

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

We cluster standard errors at the state of incorporation level in all our regressions. This addresses two important concerns. First, the fact that all firms in a given year and state of incorporation are affected by the same "shock" (namely, the passage of a BC law) may induce correlation of the error terms within each state-year cell (Moulton (1990), Donald and Lang (2007)). Second, and this is an intrinsic problem of the differences-in-differences approach, the fact that the BC dummy changes little over time, being zero before and one after the passage of the BC law, may induce serial correlation of the error terms (Bertrand, Duflo, and Mullainathan (2004)). Either problem can lead to a serious understatement of the standard errors. While clustering at the state of incorporation level is a natural choice in our case, given that the BC dummy is a likely source of cross-sectional and serial correlation, we have verified that our results also hold if we cluster at the state of location level. We will discuss alternative methods to account for cross-sectional and serial correlation in Section 3.3.

3 Results

3.1 Main Results

While many economists would probably agree that product market competition reduces managerial slack, some, like Alchian (1950), Friedman (1953), and Stigler (1958), go much further, essentially arguing that managerial slack cannot exist, or survive, in competitive industries (see Introduction). We investigate the empirical relevance of these arguments by examining how the passage of 30 BC laws between 1985 and 1991 affects operating performance depending on how competitive the firm's industry is. By reducing the fear of a hostile takeover, BC laws weaken corporate governance and create more opportunity for managerial slack. If competitive industries have less tolerance for slack than non-competitive industries do, then we should see a smaller drop in operating performance, if any, in competitive industries.

Table III contains our main results. In column [1] we first confirm that the passage of BC laws leads to a drop in operating performance. The BC dummy has a coefficient of -0.006, which implies that ROA decreases by 0.6 percentage points on average. In column [3] we examine how this drop in performance varies with product market competition. The interaction term between the BC dummy and the Herfindahl index has a coefficient of -0.025, which implies that the drop in ROA is larger for firms in less competitive industries. Of equal interest is that the BC dummy is close to zero and insignificant. Since the BC dummy in column [3] captures the effect as the Herfindahl index goes to zero, this implies that the passage of BC laws has no significant effect on firms in highly competitive industries. Finally, note that the Herfindahl index has a mean value of 0.226. We can thus compute the average effect of the passage of BC laws as $-0.001 - 0.025 \times 0.226 = -0.007$, which is similar to column [1]. Performing an $F-$test shows that the BC dummy and the interaction term between the BC dummy and the Herfindahl index are jointly significant at the 1% level.

Columns [2] and [4] show the same regressions but with control variables. The BC dummy in column [2] has a coefficient of -0.006, which is the same as in column [1]. Hence, whether or not we include controls, ROA drops by 0.6 percentage points on average. The control variables all have the expected signs. The industry- and state-year coefficients are both positive and significant, which shows that controlling for industry and local shocks is important. Size and the Herfindahl index both have a positive coefficient, while age has a negative coefficient.[16] The insignificance of the Herfindahl index is due to the fact that it captures two different effects of

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

competition on operating performance, which have opposite signs. As we will see below, when we disentangle the two effects, they both become significant.

Column [4], which represents our "basic" regression, separates out the direct effect of competition on operating performance from the indirect managerial-incentive effect. The direct effect is captured by including the Herfindahl index as a control variable. The Herfindahl index has a coefficient of 0.025, which implies that an increase in competition reduces firms' profits. The coefficient is larger than in column [2], because the latter additionally includes the indirect effect. The indirect effect is captured by the interaction term between the BC dummy and the Herfindahl index. The interaction term has a coefficient of -0.033, which implies that the decrease in ROA is larger for firms in less competitive industries. The coefficient is smaller than in column [3], because the latter additionally includes the direct effect.[17] Finally, the BC dummy is close to zero and insignificant, which implies that the passage of BC laws has no significant effect on firms in highly competitive industries.

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

Let us summarize our findings so far. By reducing the fear of a hostile takeover, BC laws create more opportunity for managerial slack. And yet, we find that the passage of BC laws has no significant effect on firms in highly competitive industries, which suggests that these industries exhibit little tolerance, if any, for managerial slack. On the other hand, we find a significant drop

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

in operating performance in less competitive industries, which suggests that changes in corporate governance do matter in these industries. This has implications for corporate governance reform. In particular, it implies that efforts to improve corporate governance could benefit from focusing primarily on firms in less competitive industries.

Some comments are in order. First, the correlation between the Herfindahl index and the interaction term between the BC dummy and the Herfindahl index is only 34%. Also, adding or subtracting either of the two variables from the regression has no profound effect on the other variable (see Table III). Hence, multicollinearity is unlikely to be an issue. Second, we can address the potential endogeneity of the Herfinahl index. As discussed in Section 2.3, the main issue is reverse causation. Fortunately, as Nickell (1996) points out, reverse causation predicts the opposite sign. It predicts that the coefficient on the Herfindahl index should be negative. However, column [4] of Table III shows that the coefficient is positive. While inconsistent with reverse causation, this result is consistent with the conventional interpretation that an increase in competition reduces firms' profits.

Third, our results could be spurious if they are driven by some other (omitted) variable that is highly correlated with the Herfindahl index. To address this issue, we have run "horse races" between the Herfindahl index and various other variables, including size, age, leverage, Tobin's Q, G-index, and E-index. In each case, we have estimated our basic regression with two additional terms: an interaction term $BC \times Z$ and a control term Z, where Z is the variable in question. The results were always similar to those in column [4] of Table III. In particular, the coefficient on the BC dummy was always close to zero and insignificant, while the coefficient on the interaction term was always highly significant with values ranging from -0.025 to -0.040. We do not report these regressions here as many of the variables in question are endogenous, which challenges causal interpretations.

Fourth, we can address the issue that BC laws are the outcome of lobbying by a broad coalition of firms incorporated in the same state, which all experience a drop in performance and operate in less competitive industries. Note that this issue is minimized here since we can control for both local and industry shocks (see Bertrand and Mullainathan (2003)). This accounts for the possibility that, for example, poor economic conditions in a particular state might induce local firms to lobby for an antitakeover law in that state. Moreover, given the evidence in Romano (1987), who portrays lobbying for antitakeover laws as an exclusive political process, it seems unlikely that BC laws are the outcome of broad-based lobbying.[18]

In column [5] of Table III we directly address the issue of broad-based lobbying by investigating the dynamic effects of BC laws. If the BC laws were passed in response to political pressure of a broad coalition of firms incorporated in the same state, then we should see an "effect" of the laws already prior to their passage (see Bertrand and Mullainathan (2003)). In particular, if the coefficients on either $Before(-2) \times Herfindahl$ or $Before(-1) \times Herfindahl$ were significant, this would be symptomatic of reverse causation. As is shown, however, neither of the two coefficients is significant. Moreover, both coefficients are small, especially in comparison to those on $Before(0) \times Herfindahl$ (the year of the passage of the BC law), $After(1) \times Herfindahl$, and $After(2+) \times Herfindahl$. In sum, we find no "effect" of the BC laws prior to their passage, which is consistent with a causal interpretation of our results.

Table IV addresses issues of measurement error. In columns [1] and [2] we replace the (continuous) Herfindahl index with dummies indicating whether the Herfindahl index is above or below the median. We drop the BC dummy and one of the Herfindahl dummies to avoid perfect multicollinearity. The results are similar to Table III. Whether or not we include control variables, the passage of BC laws has no effect on firms in competitive industries (Herfindahl below the median). By contrast, firms in less competitive industries experience a significant drop in ROA between 1.0 and 1.1 percentage points. The average effect of the BC laws can be computed as $-0.003 \times 0.5 - 0.010 \times 0.5 = -0.007$ and $-0.002 \times 0.5 - 0.011 \times 0.5 = -0.007$, respectively, which is similar to Table III.

In columns [3] and [4] we repeat the exercise with three Herfindahl dummies. The results are again similar. While the passage of BC laws has no significant effect on firms in competitive industries, firms in less competitive industries (medium and top Herfindahl tercile) experience a significant drop in ROA. While monotonic, the relationship is not perfectly linear. The difference in ROA between the lowest and medium Herfindahl tercile is more than twice the difference between the medium and top tercile. We can again compute the average effect of the BC laws as $-0.000 \times 0.35 - 0.008 \times 0.3 - 0.011 \times 0.35 = -0.006$ and $0.002 \times 0.35 - 0.008 \times 0.3 - 0.012 \times 0.35 = -0.006$, respectively, which is similar to Table III.

Table V continues with issues of measurement error. This time, we consider alternative

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

Herfindahl indices. Our basic measure of competition is the Herfindahl index based on 3-digit SIC codes. The 3-digit partition is a compromise between the coarse 2-digit partition, in which unrelated industries may be pooled together, and the narrow 4-digit partition, which may be subject to misclassification. In columns [1] and [2] we examine if our results also hold for 2- and 4-digit SIC Herfindahl indices. They do. The only major difference compared to Table III is that the coefficient on the 2-digit Herfindahl index as a control is not significant, which likely is due to lack of sufficient "within" variation.[19] In columns [3] to [5] we use 2-, 3-, and 4-digit SIC Herfindahl indices based on firms' assets in place of sales (see Hou and Robinson (2006)). The idea is that sales are relatively volatile, with the effect that changes in the Herfindahl index based on sales may overstate actual changes in industry concentration.[20] As is shown, it makes little difference if we use Herfindahl indices based on sales or assets.

In column [1] of **Table VI** we exclude Delaware firms from the treatment group. Given that half of the firms in our sample are incorporated in Delaware (see Table I), one might be worried that our results are driven by a single law. As is shown, the coefficients on both the BC dummy and the interaction term between the BC dummy and the Herfindahl index are very similar to column [4] of Table III. While consistent with previous findings by Bertrand and Mullainathan (2003, Table 10), this result contrasts with the often-heard argument that the Delaware BC law was less stringent than other BC laws.[21] In column [2] we exclude firms incorporated in states that did not pass a BC law during the sample period ("Never Business Combination").

[19] While the interaction terms in columns [1] and [2] of Table V and column [4] of Table III have different coefficients, the average effect of the BC laws is the same in all three cases. The mean values of the 2- and 4-digit SIC Herfindahl indices are 0.103 and 0.274, respectively. Thus, we can compute the average effect from columns [1] and [2] of Table V as $-0.000-0.056\times0.103=-0.006$ and $0.000-0.022\times0.274=-0.006$, respectively, which is the same as in column [4] of Table III. We can also compute the magnitude of the indirect effect. The standard deviations of the 2- and 4-digit SIC Herfindahl indices are 0.075 and 0.190, respectively. Thus, an increase in the Herfindahl index by one standard deviation is associated with a drop in ROA of $-0.056\times0.075=-0.004$ and $-0.022\times0.190=-0.004$, respectively.

[20] An alternative is to use smoothed industry concentration measures. If we run our basic regression using a 3-year moving average Herfindahl index, we find that the interaction term has a coefficient of -0.029 ($t-$statistic of 3.94), which is similar to estimate in column [4] of Table III. The coefficient on the BC dummy is again close to zero and insignificant.

[21] That the $t-$statistic on the interaction term is smaller than in Table III is likely due to the smaller sample size. In column [1] we lose about 58% percent of our treatment group, which significantly reduces the number of observations available for identifying the coefficient on the interaction term.

As discussed in Section 2.2, firms incorporated in non-passing states are smaller and slightly younger on average, feeding concerns that the control group might not be an appropriate one. As is shown, it makes little difference if we exclude those firms.

We have remarked earlier that the positive coefficient on the Herfindahl index as a control variable is inconsistent with reverse causation. Another way to address the issue of reverse causation, which we explore in **Table VII**, is to use past values of the Herfindahl index. In columns [1] and [2] we use 1- and 2-year lagged Herfindahl indices, respectively. The results are similar to column [4] of Table III.[22] In column [3] we use the average Herfindahl index from 1976 to 1984 to specifically address concerns that the drop in profitability caused by the passage of BC laws might feed back into the Herfindahl index. (The first BC law was passed in 1985). The results are again similar to column [4] of Table III. Note the missing coefficient on the Herfindahl index as a control variable. Since the average Herfindahl index from 1976 to 1984 has no "within" variation, the coefficient is not identified.

Table VIII considers alternative performance measures. Column [1] considers ROA after depreciation, which is defined as operating income after depreciation and amortization (EBIT, item #178) divided by total assets (item #6). The correlation between ROA before and after depreciation is 97%. Hence, it does not surprise that the results are similar to Table III. In columns [2] and [3] we consider return on sales (ROS), which is defined as operating income before depreciation and amortization (EBITDA, item #13) divided by sales (item #12), and return on equity (ROE), which is defined as net income (item #172) divided by common equity (item #60). While the results are again similar, they are somewhat weaker. In particular, the interaction term has a smaller $t-$statistic than in column [4] of Table III. That the results are weaker does not surprise. Compared to ROA, ROS and, especially, ROE, are arguably less well-suited as measures of operating performance.[23]

3.2 Manufacturing Industries

In **Table IX** we focus exclusively on manufacturing industries (SIC 2000-3999). For these industries only, the U.S. Bureau of the Census provides a Herfindahl index that includes both public and private firms. While the Census Herfindahl index is somewhat broader, it entails

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

[23] The correlation between ROA and ROE (ROS) is 61% (53%).

some serious limitations. First, and perhaps most important, the index is only available for the years 1982, 1987, and 1992 during our sample period. To fill in the missing years, we always use the index from the latest available year. For the years prior to 1982, we use the value from 1982. Second, the index is only available on the narrow 4-digit SIC code level, which implies that it is likely subject to misclassification (see Section 2.2). Third, the index is only available for manufacturing industries, which implies that we lose more than half of our observations. Fourth, there is no match for COMPUSTAT firms whose 4-digit SIC code ends with a "zero", which implies that we lose additional observations.[24] In conjunction with missing values (e.g., non-disclosure of information by the Census Bureau), this leaves us with $19,244$ firm-year observations. Finally, albeit only a minor quibble, the Census Herfindahl index is a truncated measure that includes only the top 50 firms in each industry.

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

In column [2] we run the same regression, except that we use the Census Herfindahl index.[25] The results are again similar to column [4] of Table III, except that the interaction term has a larger coefficient and a smaller $t-$statistic. The smaller $t-$statistic is likely due to the same reasons as above. The larger coefficient matters only insofar as it implies different economic magnitudes. However, the magnitudes are similar to Table III: The Census Herfindahl index has a mean value of 0.058 and a standard deviation of 0.046. We can thus compute the average effect as $-0.003 - 0.081 \times 0.058 = -0.008$, which implies that ROA decreases by 0.8 percentage

[24] In COMPUSTAT, 4-digit industries ending with a "zero" are effectively 3-digit industries. In some cases, COMPUSTAT also assigns 4-digit SIC codes ending with a "zero" if the firm operates in more than one 4-digit industry.

[25] The Herfindahl index as a control variable is omitted in column [2]. Except for two "jumps" in 1982 and 1987, the Census Herfindahl index is a constant, which implies that the coefficient is not well identified. By contrast, the 3-digit SIC Herfindahl index computed from COMPUSTAT, while it has a substantial degree of serial correlation, has sufficient "within" variation to allow the coefficient to be well identified. (The same is not necessarily true of the 2-digit SIC Herfindahl index; see Table V).

points. Likewise, we can compute the indirect effect as $-0.081 \times 0.046 = -0.004$, which implies that an increase in the Census Herfindahl index by one standard deviation is associated with a drop in ROA of 0.4 percentage points.

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

In column [3] we estimate our basic specification using import penetration in place of the Herfindahl index. To ensure that the BC dummy has the same interpretation as before, we use one minus import penetration as our measure of competition. The results are qualitatively similar to those in column [4] of Table III, except that the coefficient on the interaction term between the BC dummy and the Herfindahl index is not significant. This is likely due to two reasons. First, as discussed above, the manufacturing sample is much smaller. Recall that already in columns [1] and [2] the interaction term had a relatively small $t-$statistic (compared to the $t-$statistics in previous tables). Second, and most likely the relevant explanation, import penetration may be simply a poor measure of competition. As we have pointed out above, there need be no relation between import penetration and the competitiveness of an industry. Not surprisingly, it turns out that the correlation between import penetration and the Herfindahl index is extremely small (-0.37% for the 3-digit SIC Herfindahl index).

3.3 Cross-Sectional and Serial Correlation

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

Table X considers alternative methods to account for the presence of cross-sectional and serial correlation. The methods are described in Bertrand, Duflo, and Mullainathan (2004), which is why we shall be brief. Columns [1] to [3] deal with serial correlation; column [4] deals with cross-sectional correlation.

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

The second method is block bootstrapping. According to Bertrand, Duflo, and Mullainathan (2004), this method constitutes a reliable solution to the serial correlation problem if the number of blocks is sufficiently large, which is the case here (51 blocks). We construct a large number

(200) of bootstrap samples by drawing with replacement 51 states of incorporation from our sample. For each bootstrap sample, we estimate our basic specification using OLS and compute for each covariate the absolute $t-$statistic $t_r := abs[(\hat{\beta}_r - \hat{\beta})/SE(\hat{\beta}_r)]$, where $\hat{\beta}$ is the estimated coefficient from column [4] of Table III, and where $\hat{\beta}_r$ is the estimated coefficient from the r^{th} bootstrap. We compute $p-$values as the relative frequency that t_r is larger than t, where $t := abs[\hat{\beta}/SE(\hat{\beta})]$ is the absolute $t-$statistic from the OLS estimation of the specification in column [4] of Table III. Since the $p-$values refer to the significance of the original coefficients in Table III, we again report those coefficients. We reject the null of a zero coefficient at the 95 percent confidence level if 95 percent of the t_r values are smaller than t. As is shown in column [2], the results are again similar to column [4] of Table III.[26]

The third method is to collapse the data into two periods, before and after the BC law, and run an OLS regression on this two-period panel. Bertrand, Duflo, and Mullainathan (2004) show that this method performs well in simulations. Of course, the method is rather crude—by collapsing 20 years of data into two periods, we lose many observations—which reduces the power of our tests. What is more, due to the staggering of the BC laws over time, "before" and "after" are not the same for each treatment state. And for control states, "before" and "after" are not even defined. We address this issue using the two-step procedure proposed by Bertrand, Duflo, and Mullainathan (2004). In the first step, we regress ROA on fixed effects and covariates, except for the BC dummy and the interaction term between the BC dummy and the Herfindahl index. For treatment states only, we then collect the residuals and compute the average residuals for the pre- and post-BC law periods. This provides us with a two-period panel, where the first period is before the BC 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 Herfindahl index during the post-BC law period. We use White standard errors to correct for heteroskedasticity. As column [3] shows, the results are similar to column [4] of Table III.[27] While the coefficient on the BC dummy is close to zero and

[26] We can also compute the mean and median values of each coefficient based on the 200 bootstraps. The values are very close to those in column [4] of Table III. For example, the mean coefficient on the BC dummy is 0.002, while the mean coefficient on the interaction term between the BC dummy and the Herfindahl index is −0.033. The median values are virtually identical.

[27] Columns [3] and [4] only display the second-stage regressions. The results of the first-stage regressions are available upon request. Note that the dependent variable in columns [3] and [4] is not ROA but the average

insignificant, the coefficient on the interaction term between the BC dummy and the Herfindahl index is negative and significant.

The fourth correction method, which deals with cross-sectional correlation, is to collapse the data into state of incorporation-industry-year cells.[28] The basic idea is that our variables of interest, the BC dummy and the Herfindahl index, are on a higher level of aggregation, namely, the state of incorporation and industry level, respectively. The drawback of this method is that we again lose many observations, which reduces the power of our tests. Similar to the method in column [3], we again proceed in two steps. In the first step, we regress ROA on time dummies and covariates, except for the BC dummy and the interaction term between the BC dummy and the Herfindahl index. We then collect the residuals and compute the average residual for each state of incorporation-industry-year portfolio. In the second step, we regress average residuals on portfolio fixed effects, the BC dummy, and the interaction term between the BC dummy and the Herfindahl index. We use White standard errors to correct for heteroskedasticity. As column [4] shows, the results are similar to column [4] of Table III. The coefficient on the BC dummy is again close to zero and insignificant, while the coefficient on the interaction term between the BC dummy and the Herfindahl index is negative and significant.

3.4 Empire Building or Quiet Life?

By reducing the threat of a hostile takeover, BC laws create more opportunity for managerial slack. And yet, we have seen that the passage of BC laws has no significant effect on firms in competitive industries. While this suggests that product market competition mitigates managerial agency problems, it does not address which agency problem is mitigated. Does competition curb managerial empire building? Or does it prevent managers from enjoying a "quiet life," as suggested by Bertrand and Mullainathan (2003)? To investigate the first possibility, we estimate our basic specification using capital expenditures (COMPUSTAT item #30) divided by total assets (item #6) as the dependent variable. To investigate the second possibility, we use a number of dependent variables: selling, general, and administrative expenses ("overhead costs", item #189) and R&D expenses (item #46), both divided by total assets, advertising expenses

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residual from the respective first-stage regressions. The coefficients are thus not directly comparable to those in column [4] of Table III.

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

(item #45) and costs of goods sold ("input costs", item #41), both divided by sales (item #12), and real wages, defined as the natural logarithm of labor and related expenses (item #42) divided by the number of employees (item #29) and deflated by the Consumer Price Index (CPI). The idea is that, in order to keep the firm's costs low, managers must haggle with labor unions and input suppliers and resist pressure from individual units within the organization demanding bigger overhead, advertising, and R&D budgets.

The results are shown in **Table XI**. Column [1] considers the effect of BC laws on capital expenditures. As it turns out, there is no effect. Neither the BC dummy nor the interaction term between the BC dummy and the Herfindahl index is significant, neither individually nor jointly.[29] While inconsistent with empire building, this is consistent with previous findings by Bertrand and Mullainathan (2003), who also find that BC laws have no significant effect on (plant-level) capital expenditures. The remaining results in Table XI are mixed. Columns [3] and [4] consider the effect of BC laws on advertising and R&D expenses, respectively. While the relevant coefficients all have the right signs, they are not significant.

Columns [2], [5], and [6] show the effect of BC laws on selling, general, and administrative expenses, costs of goods sold, and real wages, respectively. In all three columns, the pattern is similar to our previous ROA regressions. While the coefficient on the BC dummy is close to zero and insignificant, the coefficient on the interaction term between the BC dummy and the Herfindahl index is positive and significant. (The sign of the coefficient is the opposite as in our ROA regressions, because the dependent variables are negatively related to ROA). Hence, while BC laws have no significant effect on firms in competitive industries, they cause a significant increase in overhead costs, input costs, and wages in less competitive industries. These results are consistent with a "quiet-life" hypothesis whereby managers insulated from both takeover pressure and competitive pressure seek to avoid cognitively difficult activities, such as haggling with input suppliers, labor unions, and individual units within the organization demanding bigger overhead budgets.

We conclude with two caveats. First, the $t-$statistics in columns [2], [5], and [6] are smaller than in our previous ROA regressions, presumably because the dependent variables are all individual components of ROA. That is, while BC laws may have a relatively small effect on any

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

individual component of ROA, the overall effect on ROA may be substantial. Second, the wage result in column [6] should be taken with caution. For one thing, the sample is relatively small, which is due to the fact that only few firms in COMPUSTAT report wage data. More important, however, the COMPUSTAT wage data is quite noisy.[30] For example, some firms report wage data only intermittently, while others report no data at all. What is more, COMPUSTAT only provides aggregate data on labor and related expenses, which also includes pension costs, payroll taxes, and employee benefits, to name just a few. On a positive note, our wage results are consistent with those by Bertrand and Mullainathan (1999, 2003), who report wage increases between 1% and 2% after the passage of BC laws. In our case, we can compute the average wage increase after the passage of BC laws from column [6] as $-0.003 + 0.103 \times 0.218 = 0.019$, or 1.9%, which is roughly of the same order of magnitude.[31]

4 Event-Study Results

Does the stock market anticipate that firms in competitive industries will be largely unaffected by the passage of BC laws, whereas firms in less competitive industries will likely experience a drop in performance? The main difficulty in answering this question lies in the choice of event date. Since the passage of BC laws is presumably well anticipated, the passage date is unlikely to contain much new information. Rather, one needs to find an early date at which significant news about the law was disseminated to the public, e.g., the date of the first newspaper report. For instance, take the event study by Karpoff and Malatesta (1989), who examine the stock price impact of 40 antitakeover laws, including 11 BC laws, from 1982 to 1987.[32] The authors find no significant abnormal returns when using either the date of the law's introduction in the state legislature, its final passage, or its signing by the governor as the event date. However, they do find significant abnormal returns when using the first date on which they could find a newspaper report as the event date.

Finding the first newspaper report about a BC law is often a formidable task. Electronic archives of local newspapers often do not go back to the 1980s, and larger out-of-state newspa-

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

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

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

pers, like the *Wall Street Journal* or *New York Times*, often provide no coverage, especially if the state is small and only few firms are incorporated there. Indeed, after a careful search of all major newpaper databases (ProQuest, Lexis-Nexis, Factiva, Newsbank America's Newspapers, Google News Archive), we could only 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.[33] Most of the 11 states for which we could not find a newspaper report are small in terms of number of incorporated firms. In fact, seven of them have fewer than 20 firms—and only one has more than 100 firms (Nevada, 122 firms)—in the merged CRSP-COMPUSTAT sample in the year the BC law was passed. Based on the numbers in Table I, the 19 states for which we could find newspaper reports represent 92% of all firms incorporated in states that passed a BC law during the sample period.

The event-study methodology is based on the assumption that the events are independent (MacKinlay (1997, p. 27)). While this assumption is satisfied in many applications where the event is firm-specific, such as earnings or dividend announcements, it is violated in our setting. Since all firms incorporated in the same state are affected by the same event, their abnormal returns will likely be correlated. As a result, standard errors will be biased, leading to incorrect inferences (see Bernard (1987)). The common way to address this problem in event studies is to form portfolios consisting of all firms incorporated in a given state. Since the event dates are different for each portfolio, the issue of cross-sectional correlation does not arise, or at least becomes negligible (Karpoff and Malatesta (1989), MacKinlay (1997)).

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

Our empirical methodology is similar to Karpoff and Malatesta (1989). For each state portfolio j, we estimate the market model using CRSP daily return data from 241 to 41 trading days prior to the event date.[34] Precisely, using OLS we estimate the parameters α_j and β_j of the equation

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

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

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

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

The methodology described above yields an estimate of the average impact of BC laws on stock prices. To examine if the price impact is different for firms in competitive and non-competitive industries, we subdivide each state portfolio into smaller portfolios. Precisely, for each state j, we form low- and high-Herfindahl portfolios consisting of all firms whose Herfindahl index lies below and above the median, respectively. We do the same with low-, medium-, and high-Herfindahl portfolios. The remaining steps are as above.

In column [1] of **Table XII** we report average CARs based on the 19 state portfolios. The average CAR for the two-day event window is -0.32% with a $z-$statistic of -2.58, which has a $p-$value of 0.010. Furthermore, 14 of the 19 average two-day CARs are negative. While our number is somewhat smaller than the average two-day CAR of -0.47% reported by Karpoff and

[34] Choosing the estimation window adjacent to the first time interval for which cumulative abnormal returns are computed (here: the interval [-40,-2]) is standard practice (e.g., MacKinlay (1997, p. 19)). However, we obtain similar results if we estimate the market model over the interval from 300 to 100 trading days before the event date. The market model is the most common statistical model to calculate normal returns at daily frequency. However, we also obtain similar results if we use either the Fama-French (1993) 3-factor model or Carhart's (1997) 4-factor model.

Malatesta (1989, Panel (A) of Table 5), it is roughly of the same order of magnitude.[35] Moreover, the average two-day CARs immediately before and after the event window are small and insignificant. This indicates that, on average, newspaper reports about BC laws are associated with a significant decrease in stockholder wealth.

In columns [2] and [3] we report average CARs for low- and high-Herfindahl portfolios consisting of firms whose Herfindahl index lies below and above the median, respectively. For the low-Herfindahl portfolio, the average CAR for the two-day event window is close to zero and insignificant. By contrast, for the high-Herfindahl portfolio, the average CAR for the two-day event window is -0.54% with a $z-$statistic of -2.36, which has a $p-$value of 0.018. Thus, while firms in competitive industries experience no significant stock price impact around the first newspaper report, firms in less competitive industries experience a significant abnormal decline in their stock prices. Overall, this suggest that the stock market anticipates that BC laws have a negative impact only on firms in less competitive industries.

In columns [4] to [6] we report average CARs for low-, medium-, and high-Herfindahl portfolios consisting of firms whose Herfindahl index lies in the bottom, medium, and top tercile, respectively. The results are again similar. While firms in competitive industries experience no significant stock price impact, firms in less competitive industries (medium and top terciles) experience a significant abnormal decline in their stock prices. Interestingly, and consistent with Table IV, the relationship between the CARs and the Herfindahl index is monotonic. For the medium-tercile portfolio, the average CAR for the two-day event window is -0.44% with a $z-$statistic of -1.67, which has a $p-$value of 0.095. By contrast, for the top-tercile portfolio, the average CAR for the two-day event window is -0.67% with a $z-$statistic of -2.31, which has a $p-$value of 0.021.[36] Not only do these results confirm that the stock market anticipates that BC laws have a negative impact only on firms in non-competitive industries, they also suggest that the market knows that high-Herfindahl firms fare worse than medium-Herfindahl firms.

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

[36] For the low-tercile portfolio, the average CAR for the two-day event window is 0.08% with a $z-$statistic of -0.53. We obtain a similar monotonic pattern using median CARs. The median CARs for the bottom-, medium-, and high-tercile portfolios are 0.06%, -0.46%, and -0.67%, respectively. The corresponding ratios of positive to negative CARs for the two-day event window are 10:9, 4:15, and 5:14, respectively.

5 Conclusion

Does competition mitigate managerial agency problems? And is there merit to the considerably stronger view expressed by Alchian (1950), Friedman (1953), and Stigler (1958) that managerial slack cannot exist, or survive, in competitive industries? The evidence from our study suggests that the answer to both questions is yes. Using the passage of 30 business combination (BC) laws as a source of identifying variation, we examine if such laws have a different impact on firms in competitive and non-competitive industries. Consistent with the notion that BC laws weaken corporate governance and create more opportunity for managerial slack (see Bertrand and Mullainathan (2003)), we find that, on average, firms' operating performance drops significantly after the passage of BC laws. Most important, we find that this result is exclusively driven by firms in non-competitive industries. By contrast, firms in competitive industries remain virtually unaffected, which is supportive of the Alchian-Friedman-Stigler hypothesis that competitive industries leave no room for managerial slack.

The insight that (variation in) corporate governance has little or no effect on firms in competitive industries, if true, has far-reaching implications. For one thing, it implies that efforts to improve corporate governance could benefit from focusing primarily on firms in non-competitive industries. For another, it implies that empirical studies on corporate governance could benefit from including measures of industry competition. Results might be stronger, both economically and statistically, for firms operating in non-competitive industries. For instance, preliminary research by the authors suggests that the positive alphas generated by the democracy-dictatorship hedge portfolios in Gompers, Ishii, and Metrick (GIM, 2003) are driven by firms in non-competitive industries (Giroud and Mueller (2007)). In fact, if we split the annual samples into low-, medium- and high-Herfindahl samples, we find that the alphas in the low-Herfindahl samples are small and insignificant, whereas those in the high-Herfindahl samples are large and highly significant. Naturally, since the alphas in GIM are averages across all three samples, this implies that the alphas in the high-Herfindahl samples are higher than those in GIM. Not only are these results robust across different measures of industry competition, they also extend to other governance indices, such as the E-index by Bebchuk, Cohen, and Ferrell (2005). A similar, albeit weaker, picture emerges with respect to the Q-regressions 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.

6 References

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Table I
States of Incorporation and States of Location

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

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

Table II
Summary Statistics

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

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

Table III
Does Corporate Governance Matter in Competitive Industries?

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

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

Table IV
Herfindahl Dummies

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

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

Table V
2- and 4-Digit Herfindahl Indices and Asset-Based Herfindahl Indices

Asset-based Herfindahl indices are computed using total assets (COMPUSTAT item #6). All other variables are defined in Tables II and III. Standard errors are clustered at the state of incorporation level. The sample period is from 1976 to 1995. *t*-statistics are in parentheses. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	Dependent Variable: Return on Assets				
	[1]	[2]	[3]	[4]	[5]
BC	-0.000	0.000	-0.000	0.002	0.001
	(0.15)	(0.11)	(0.10)	(0.60)	(0.19)
BC x Herfindahl (2-digit)	-0.056***				
	(5.15)				
BC x Herfindahl (4-digit)		-0.022***			
		(3.23)			
BC x Herfindahl (2-digit, assets)			-0.053***		
			(5.27)		
BC x Herfindahl (3-digit, assets)				-0.037***	
				(4.73)	
BC x Herfindahl (4-digit, assets)					-0.021***
					(3.25)
Industry-year	0.203***	0.201***	0.206***	0.202***	0.196***
	(9.90)	(9.72)	(9.73)	(9.34)	(9.52)
State-year	0.251***	0.249***	0.248***	0.246***	0.253***
	(8.76)	(9.26)	(8.78)	(8.83)	(9.09)
Size	0.096***	0.096***	0.096***	0.097***	0.096***
	(19.30)	(21.35)	(19.38)	(20.31)	(21.23)
Size-squared	-0.007***	-0.007***	-0.007***	-0.007***	-0.007***
	(18.57)	(21.25)	(19.23)	(20.15)	(20.96)
Age	-0.021***	-0.020***	-0.020***	-0.021***	-0.020***
	(5.21)	(4.99)	(5.07)	(5.52)	(4.96)
Herfindahl (2-digit)	0.011				
	(0.76)				
Herfindahl (4-digit)		0.017**			
		(2.13)			
Herfindahl (2-digit, assets)			0.027		
			(1.43)		
Herfindahl (3-digit, assets)				0.033***	
				(4.15)	
Herfindahl (4-digit, assets)					0.030***
					(3.29)
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	77,135	77,446	77,106	77,470	77,490
Adj. R-squared	0.68	0.68	0.68	0.68	0.68

Table VI
1- and 2-Year Lagged Herfindahl Indices and Pre-1984 Average Herfindahl Index

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

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

Table VII
Non-Delaware and "Eventually Business Combination" Sub-samples

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

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

Table VIII
Alternative Performance Measures

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

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

Table IX
Manufacturing Industries

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

	Dependent Variable: Return on Assets		
	[1]	[2]	[3]
BC	0.003	-0.003	0.003
	(0.67)	(0.83)	(0.27)
BC x Herfindahl	-0.035***		
	(3.08)		
BC x Herfindahl (Census)		-0.081***	
		(2.84)	
BC x (1 - Import Penetration)			-0.017
			(1.15)
Industry-year	0.188***	0.148***	0.202***
	(9.96)	(6.21)	(8.08)
State-year	0.277***	0.284***	0.333***
	(7.64)	(3.99)	(6.26)
Size	0.111***	0.115***	0.123***
	(21.14)	(13.13)	(16.80)
Size-squared	-0.008***	-0.009***	-0.009***
	(20.52)	(12.45)	(15.40)
Age	-0.034***	-0.043***	-0.030***
	(5.07)	(5.39)	(3.00)
Herfindahl	0.036***		
	(3.13)		
1 - Import Penetration			0.052**
			(2.25)
Firm Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Observations	35,371	19,244	20,250
Adj. R-squared	0.70	0.73	0.71

Table X
Serial and Cross-Sectional Correlation

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

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

Table XI
Empire Building or Quiet Life?

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

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

Table XII
Event-Study Results

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

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