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Does corporate governance matter in competitive industries?[☆]

Xavier Giroud[a], Holger M. Mueller[a,b,c,d,*]

[a] Stern School of Business, New York University, New York, NY 10012, USA
[b] National Bureau of Economic Research (NBER), Cambridge, MA 02138, USA
[c] Centre for Economic Policy Research (CEPR), London EC1V 0DG, UK
[d] European Corporate Governance Institute (ECGI), 1180 Brussels, Belgium

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ABSTRACT

By reducing the threat of a hostile takeover, business combination (BC) laws weaken corporate governance and increase the opportunity for managerial slack. Consistent with the notion that competition mitigates managerial slack, we find that while firms in non-competitive industries experience a significant drop in operating performance after the laws' passage, firms in competitive industries experience no significant effect. When we examine which agency problem competition mitigates, we find evidence in support of a "quiet-life" hypothesis. Input costs, wages, and overhead costs all increase after the laws' passage, and only so in non-competitive industries. Similarly, when we conduct event studies around the dates of the first newspaper reports about the BC laws, we find that while firms in non-competitive industries experience a significant stock price decline, firms in competitive industries experience a small and insignificant stock price impact.

1. Introduction

Going back to Adam Smith, economists have long argued that managerial slack is first and foremost an issue for firms in non-competitive industries. As Sir John Hicks succinctly put it, managers of such firms tend to enjoy the "quiet life".[1] By contrast, managers of firms in competitive industries are under constant pressure to reduce slack and improve efficiency:

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

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* Corresponding author at: Stern School of Business, New York University, New York, NY 10012, USA.
E-mail addresses: xgiroud@stern.nyu.edu (X. Giroud), hmueller@stern.nyu.edu (H.M. Mueller).

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[1] "The best of all monopoly profits is a quiet life" (Hicks, 1935). Similarly, "Monopoly [...] is a great enemy to good management" (Smith, 1776). Despite its intuitive appeal, attempts to formalize the notion that competition mitigates managerial slack have proven difficult. For example, while Hart (1983) shows that competition reduces managerial slack, Scharfstein (1988) shows that Hart's result can be easily reversed. Subsequent models generally find ambiguous effects (e.g., Hermalin, 1992; Schmidt, 1997). In an early review of the literature, Holmström and Tirole (1989) conclude that "apparently, the simple idea that product market competition reduces slack is not as easy to formalize as one might think."

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

We test the hypothesis that competition mitigates managerial slack by using exogenous variation in corporate governance in the form of 30 business combination (BC) laws passed between 1985 and 1991 on a state-by-state basis. BC laws impose a moratorium on certain transactions, especially mergers and asset sales, between a large shareholder and the firm for a period ranging from three to five years after the large shareholder's stake has passed a prespecified threshold. This moratorium 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. By reducing the threat of a hostile takeover, BC laws thus weaken corporate governance and increase the opportunity for managerial slack.[2]

Using the passage of BC laws as a source of identifying variation, we examine if these laws have a different effect on firms in competitive and non-competitive industries. We obtain three main results. First, consistent with the notion that BC laws increase the opportunity for managerial slack, we find that firms' return on assets (ROA) drops by 0.6 percentage points on average. Given that the average ROA in our sample is about 7.4%, this implies a drop in ROA of about 8.1%. Second, the drop in ROA is larger for firms in non-competitive industries. While ROA drops by 1.5 percentage points in the highest Herfindahl-Hirschman index (HHI) quintile, it only drops by 0.1 percentage points in the lowest HHI quintile. Third, the effect is close to zero and statistically insignificant for firms in highly competitive industries. Thus, while the *opportunity* for managerial slack increases equally across all industries, managerial slack appears to increase only in non-competitive industries, but not in highly competitive industries, where competitive pressure enforces discipline on management. It is in this sense that our results suggest that competition mitigates managerial slack.

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

ROA is an accounting measure that can be manipulated. Accordingly, a drop in ROA after the passage of the BC laws does not necessarily imply a reduction in operating profitability. It could simply reflect a change in the extent to which firms manage their earnings. While it is difficult to completely rule out this alternative story, we can offer some pieces of evidence that are inconsistent with it. First, if a BC law is passed only a few months prior to the fiscal year's end, it would seem hard to imagine that the current year's ROA should drop by much, given that most of the fiscal year is already over. In this case, a significant drop in ROA might be indicative of an earnings management story. However, we find that if a BC law is passed late in the fiscal year, the drop in ROA is small and insignificant. Second, using discretionary accruals as proxies for earnings management, we find no evidence that firms' earnings management has changed after the passage of the BC laws. In a similar vein, it could be that the drop in ROA reflects a change in firms' asset mix towards lower risk/lower return projects. However, we find that neither cash-flow volatility nor firms' asset betas have changed after the laws' passage.

Our findings are robust across many alternative specifications. Our main competition measure is the HHI based on three-digit standard industry classification (SIC) codes computed from Compustat based on firms' sales. We obtain similar results if we use HHIs based on two-digit and four-digit SIC codes, asset-based HHIs, lagged HHIs (up to five years), and the average HHI from 1976 to 1984 (the first BC law was passed in 1985). We also obtain similar results if we use the Census HHI, which includes both public and private firms, import penetration, and industry net profit margin (or Lerner index) as our competition measure. Finally, we obtain similar results if we run "horse races" between the HHI and other firm or industry characteristics for which the HHI might be merely proxying, if we exclude Delaware firms from the treatment group, if we use alternative performance measures, such as return on equity and net profit margin, if we restrict the sample to firms that are present during the entire period from 1981 to 1995 (to purge the sample of entry and exit effects), if we use different sample periods, and if we interact all covariates with time dummies and treatment state dummies.

Our identification strategy benefits from a general lack of congruence between a firm's industry, state of location, and state of incorporation. For instance, the state of incorporation of a firm says little about the firm's industry. Likewise, less than 38% of the firms in our sample are incorporated in their state of location. This lack of congruence allows us to control for local and industry shocks and thus, to separate out the effects of shocks contemporaneous with the BC laws from the effects of the laws themselves. Among other things, this alleviates concerns that the BC laws might be the outcome of lobbying at the local and industry level, respectively. To address concerns that the BC laws might be the outcome of broad-based lobbying at the state of incorporation

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

level, we examine if the laws already had an "effect" prior to their passage. We find no evidence for such an "effect."

While the above results suggest that competition mitigates managerial agency problems, they do not say which agency problem is being mitigated. Does competition curb managerial empire building? Or does it prevent managers from enjoying a "quiet life" by forcing them to "undertake cognitively difficult activities" (Bertrand and Mullainathan, 2003)? We find no evidence for empire building. Capital expenditures, asset growth, property, plant, and equipment (PPE) growth, the volume of acquisitions made by a firm, and the likelihood of being an acquirer are all unaffected by the passage of the BC laws. In contrast, we find that input costs, overhead costs, and wages all increase after the laws' passage, and only so in non-competitive industries. Our results are broadly consistent with a "quiet-life" hypothesis, whereby managers insulated from hostile takeovers and competitive pressure seek to avoid cognitively difficult activities, such as haggling with input suppliers, labor unions, and organizational units within the company demanding bigger overhead budgets.

To see whether the effect also shows up in stock prices, we conduct event studies around the dates of the first newspaper reports about the BC laws. Across all industries, we find a significant cumulative abnormal return (CAR) of -0.32%. When we compute CARs separately for low- and high-HHI portfolios, we find that the CAR for the low-HHI portfolio is small and insignificant, while the CAR for the high-HHI portfolio is large (-0.54%) and significant. Similarly, if we compute CARs for low-, medium-, and high-HHI portfolios, we find that the CAR for the low-HHI portfolio is small and insignificant, while the CARs for the medium- and high-HHI portfolios are large (-0.44% and -0.67%) and significant.

Our empirical methodology closely follows Bertrand and Mullainathan (2003), who consider the same 30 BC laws as we do. Using plant-level data from the U.S. Census Bureau, they investigate the laws' effect on wages, employment, plant births and deaths, investment, total factor productivity, and return on capital.[3] We extend their analysis by investigating whether the laws have a different effect on firms in competitive and non-competitive industries. In terms of research question, our paper is closely related to Nickell (1996), who finds that more competition is associated with higher productivity growth in a sample of U.K. manufacturing firms.[4] While consistent with a managerial agency explanation, this result is also consistent with alternative explanations that are unrelated to corporate governance. For instance, firms in competitive industries might have higher productivity growth because there are more industry peers from whose successes and failures they can learn. Our paper is also related to a growing literature that documents a link between competition and firm-level governance instruments, such as managerial incentive schemes (Aggarwal and Samwick, 1999), board structure (Karuna, 2008), and firm-level takeover defenses (Cremers, Nair, and Peyer, 2008).

The rest of this paper is organized as follows. Section 2 describes the data and empirical methodology. Section 3 presents our main results. Section 4 examines which agency problem competition mitigates. Section 5 presents event-study evidence. Section 6 concludes.

2. Data

2.1. Sample selection

Our main data source is Standard & Poor's Compustat. To be included in our sample, a firm must be located and incorporated in the United States. We exclude all observations for which the book value of assets or net sales are either missing or negative. We also exclude regulated utility firms (SIC 4900–4999).[5] The sample period is from 1976 to 1995, which is the same period as in Bertrand and Mullainathan (2003).

The above selection criteria leave us with 10,960 firms and 81,095 firm-year observations. Table 1 shows how many firms are located and incorporated in each state. The state of location, as defined by Compustat, indicates the state in which a firm's headquarters are located. The state of incorporation is a legal concept and determines which business combination (BC) law, if any, applies to a given firm. While Compustat only reports the state of incorporation for the latest available year, anecdotal evidence suggests that changes in states of incorporation during the sample period are rare (Romano, 1993). To gain further confidence, Bertrand and Mullainathan (2003) randomly sampled 200 firms from their panel and checked (using *Moody's Industrial Manual*) if any of these firms had changed their state of incorporation. Only three firms had changed their state of incorporation, and all of them to Delaware. Importantly, all three changes predated the 1988 Delaware BC law by several years. Similarly, Cheng, Nagar, and Rajan (2005) report that none of the 587 *Forbes* 500 firms in their panel changed their state of incorporation during the sample period from 1984 to 1991.

2.2. Definition of variables and summary statistics

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

[3] Using plant-level data from the U.S. Census Bureau is superior to using Compustat data in many respects. For instance, one can estimate total factor productivity. Moreover, it allows the inclusion of both plant fixed effects and state of incorporation fixed effects, thus permitting a tighter identification.

[4] See also Bloom and van Reenen (2007), who find that poor management practices are more prevalent in non-competitive industries, and Guadalupe and Pérez-González (2005), who find that competition affects private benefits of control, as measured by the voting premium between shares with differential voting rights.

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

Table 1
States of incorporation and states of location.
"BC year" indicates the year in which a business combination (BC) law was passed. "State of location" indicates the state in which a firm's headquarters are located. BC years are from Bertrand and Mullainathan (2003). States of location and states of incorporation are both from Compustat. The sample consists of all Compustat firms except regulated utility firms (SIC 4900–4999). The number of firms in the sample is 10,960, and 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%)

sum of squared market shares,

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

where s_{ijt} is the market share of firm i in industry j in year t. Market shares are computed from Compustat based on firms' sales (item #12). In robustness checks, we also compute market shares based on firms' assets. Our benchmark measure is the HHI based on three-digit SIC codes. The three-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 two-digit SIC code 38 (instruments and related products) pools together ophthalmic goods such as intraocular lenses (three-digit SIC code 385) and watches, clocks, clockwork operated devices and parts (three-digit SIC code 387), two industries that unlikely compete with each other. On the other hand, the four-digit partition treats upholstered wood household furniture (four-digit SIC code 2512) and non-upholstered wood household furniture (four-digit SIC code 2511) as unrelated industries, although common sense suggests that they compete with each other. We consider HHIs based on two- and four-digit SIC codes in robustness checks. There, we also consider alternative competition measures, such as the Census HHI, industry net profit margin (or Lerner index), and import penetration. Finally, a look at the empirical distribution of the HHI shows that it has a (small) "spike" at the right endpoint, which points to misclassification. To correct for this misclassification, we drop 2.5% of the firm-year observations at the right tail of the HHI distribution.[6]

Our main measure of operating performance is return on assets (ROA), which is defined as operating income before depreciation and amortization (EBITDA, item #13) divided by total assets (item #6). Since ROA is a ratio, it can take on extreme values (in either direction) if the scaling variable becomes too small. To mitigate the effect of outliers, we drop 1% of the firm-year observations at each tail of the ROA distribution. Panel A of Table 2 presents summary statistics for the mean, median, and range of observed ROA values for the trimmed sample. We consider alternative methods to deal with ROA outliers in robustness checks. Also in robustness checks, we consider alternative measures of operating performance, such as return on equity and net profit margin.

Panel B of Table 2 provides summary statistics for firms incorporated in states that passed a BC law during the sample period ("Eventually BC") and firms incorporated in states that never passed a BC law ("Never BC"). As is shown, firms in passing states are slightly bigger and older on average, which raises the question of whether the control group is an appropriate one. There are several reasons why this should not be a concern. First, due to the staggering of the BC laws over time, firms in the "Eventually BC" group are first control firms (before the law) and then treatment firms. Second, we control for size and age in all our regressions. Size is the natural logarithm of total assets, while age is the natural logarithm of one plus the firm's age, which is the number of years the firm has been in Compustat. Third, we show in robustness checks that results are similar if we limit the control group to firms incorporated in treatment states that have not yet passed a BC law.

Table 2
Summary statistics.

In Panel A, return on assets (ROA) is operating income before depreciation and amortization (Compustat item #13) divided by total assets (item #6). In Panel B, "All states" refers to all states in Table 1. "Eventually BC" refers to all states that passed a BC law during the sample period. "Never BC" refers to all states that never passed a BC law during the sample period. Size is the natural logarithm of total assets. Age is the natural logarithm of one plus the number of years the firm has been in Compustat. HHI is the Herfindahl-Hirschman index, which is computed as the sum of squared market shares of all firms in a given three-digit SIC industry. Market shares are computed from Compustat based on firms' sales (item #12). All figures in Panel B are sample means. Standard deviations are in parentheses. The sample consists of 77,460 firm-year observations. The sample period is from 1976 to 1995.

Panel A: ROA (trimmed at 1% level)

Mean	Median	Minimum	Maximum
0.074	0.104	−1.051	0.417

Panel B: "Eventually BC" states vs. "Never BC" states

	[1] All states	[2] Eventually BC	[3] Never BC
Size	4.450	4.585	3.629
	(2.283)	(2.270)	(2.185)
Age	2.252	2.293	2.002
	(0.918)	(0.924)	(0.837)
HHI	0.225	0.226	0.214
	(0.155)	(0.156)	(0.148)

2.3. Empirical methodology

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

$$y_{ijklt} = \alpha_i + \alpha_t + \beta_1 BC_{kt} + \beta_2 HHI_{jt} + \beta_3 (BC_{kt} \times HHI_{jt}) + \gamma' \mathbf{X}_{ijklt} + \varepsilon_{ijklt}, \quad (2)$$

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

For any given HHI, we can compute the total effect of the BC laws as $\beta_1 + \beta_3 HHI$. The coefficient β_1 on the BC dummy measures the (limit) effect as the HHI goes to zero, implying that it measures the laws' effect on firms in highly competitive industries. The coefficient β_3 measures how the effect varies with the degree of competition. The coefficient β_2 measures the direct effect of competition. In the case where the dependent variable is ROA, the conjecture is that firms in more competitive industries (lower HHI) make fewer profits, implying that the coefficient β_2 should be positive.

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

We estimate Eq. (2) using a difference-in-difference-in-difference (DDD) approach. In the case where the dependent variable is ROA, the first difference compares ROA before and after the passage of the BC laws separately for firms in the control and treatment group. This yields two differences, one for the control group and one for the treatment groups. The second difference takes the difference between these two differences. The result is an estimate of the effect of the BC laws on firms' ROA. The interaction term $BC \times HHI$ estimates a third difference, namely, whether the laws' effect is different for firms in competitive and non-competitive industries. Importantly, the staggered passage of the BC laws implies that the control group is not restricted to firms incorporated in states that never passed a BC law. The control group includes all firms incorporated in states that have not passed a BC law by time *t*. Thus, it includes firms incorporated in states that never passed a BC law as well as firms incorporated in states that passed a law after time *t*.

Our identification strategy benefits from a general lack of congruence between a firm's industry, state of location, and state of incorporation. For instance, the state of incorporation of a firm says little about the firm's industry. Likewise, Table 1 shows that only 37.8% of all firms in our sample are incorporated in their state of location. BC laws, in turn, apply to all firms in a given state of incorporation, regardless of their state of location or industry. Ideally, this lack of congruence should allow us to fully control for any industry shocks and shocks specific to a state of location by including a full set of industry dummies and state of location dummies, each interacted with time dummies. Unfortunately, computational difficulties make it practically infeasible to estimate a specification with so many independent variables. Instead, we follow Bertrand and Mullainathan (2003) and control for local and industry shocks by including a full set of time-varying industry- and state-year controls, which are computed as the mean of the dependent variable in the firm's three-digit SIC industry and state of location, respectively, in a given year, excluding the firm itself.

Controlling for local and industry shocks helps us to separate out the effects of shocks contemporaneous with the BC laws from the effects of the laws themselves. This addresses several important concerns. First, our estimate of the laws' effect could be biased, reflecting in part the effects of contemporaneous shocks. Second, our results could be spurious, coming entirely from contemporaneous shocks. Third, and perhaps most important, economic conditions could influence the passage of the BC laws. For example, poor economic conditions in a particular state might induce local firms to lobby for an anti-takeover law to gain better protection from hostile takeovers. While the inclusion of state- and industry-year controls mitigates concerns that the BC laws are the outcome of lobbying at the local and industry level, respectively, it remains the possibility that lobbying occurs at the state of incorporation level. We will address this issue in detail in Section 3.2.

The HHI is an imperfect measure of competition. The classic example is that in which every city has one cement company. In that case, there would be many cement companies in the industry, but given the high transportation costs for cement, each company would effectively be a local monopoly. Evidently, the HHI would seriously misrepresent the true level of competition in that situation. More generally, this concern applies whenever markets are regionally segmented. However, as long as the resulting measurement error is not systematically related to the passage of the BC laws, which is a reasonable assumption to make, it is unlikely that it will bias our coefficients. Rather, it will only make it harder for us to find any significant results.

In all our regressions, we cluster standard errors at the state of incorporation level. This accounts for arbitrary correlations of the error terms (i) across different firms in a given state of incorporation and year (cross-sectional correlation), (ii) across different firms in a given state of incorporation over time (across-firm serial correlation), and (iii) within the same firm over time (within-firm serial correlation) (see Petersen, 2009). Cross-sectional correlation is a concern because all firms in a given state of incorporation are affected by the same "shock," namely, the passage of the BC law. Serial correlation is a concern because the BC dummy changes little over time, being zero before and one after the passage of the BC law. We will consider alternative ways to account for cross-sectional and serial correlation in robustness checks.

3. Results

3.1. Main results

Panel A of Table 3 contains our main results. Column 1 shows the average effect of the passage of the BC laws across all firms. The coefficient on the BC dummy is −0.006, implying that ROA drops by 0.6 percentage points on average. Given that the average (median) ROA in our sample is about 7.4% (10.4%), this implies a drop in ROA of 8.1% for the average firm and 5.8% for the median firm. The control variables all have the expected signs. The industry- and state-year controls are both positive and significant, which underscores the importance of controlling for industry and local shocks. The coefficients on size and the HHI are both positive, while the coefficient on age is negative.[7] The weak significance of the HHI as a control variable in column 1 is due to the fact that it captures two different effects of competition on profits, which have opposite signs. As we will see below, when we disentangle these two effects, they will both become significant.

[7] We have experimented with squared terms for size, age, and the HHI (both alone and interacted with the BC dummy) to capture possible non-linearities. As is shown in Table 3, the squared term for size is negative and significant, which implies that the relation between size and ROA is concave. The squared term for the HHI had the "right" sign (negative as a control variable and positive when interacted with the BC dummy) but was insignificant. The squared term for age was significant but rendered the coefficient on age itself insignificant with virtually no effect on the other variables. All our results are similar if we include age-squared instead of age.

Table 3

Does corporate governance matter in competitive industries?

BC is a dummy variable that equals one if the firm is incorporated in a state that has passed a BC law. *HHI*(*Low*), *HHI*(*Medium*), and *HHI*(*High*) are dummy variables that equal one if the HHI lies in the bottom, medium, and top tercile, respectively, of its empirical distribution. "Industry-year" and "State-year" are variables that indicate the mean of the dependent variable in the firm's industry and state of location, respectively, excluding the firm itself. *BC Year*(−1) is a dummy variable that equals one if the firm is incorporated in a state that will pass a BC law in one year from now. *BC Year*(0) is a dummy variable that equals one if the firm is incorporated in a state that passes a BC law this year. *BC Year*(1) and *BC Year*(2+) are dummy variables that equal one if the firm is incorporated in a state that passed a BC law one year and two or more years ago, respectively. All other variables are defined in Table 2. The coefficients are estimated using ordinary least squares (OLS). Standard errors are clustered at the state of incorporation level. The sample period is from 1976 to 1995. *t*-Statistics are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A: *Main results*

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

Panel B: *Reverse causality*

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

In column 2, we examine whether the drop in ROA is different for firms in competitive and non-competitive industries. The interaction term between the BC dummy and the HHI has a coefficient of −0.033 (*t*-statistic of 4.95), which implies that the drop in ROA is larger for firms in non-competitive industries.[8] (That these firms have higher profits to begin with is already accounted for by the inclusion of the HHI as a control variable.) As for the economic magnitude of the effect, an increase in the HHI by one standard deviation is associated with a drop in ROA of $-0.033 \times 0.156 = -0.005$, or 0.5 percentage points. We can alternatively divide the sample into HHI quintiles. The mean value of the HHI in the lowest and highest quintile is 0.067 and 0.479, respectively. Hence, while ROA drops by 1.5 percentage points in the highest HHI quintile, it only drops by 0.1 percentage points in the lowest HHI quintile. Of equal interest is the fact that the BC dummy is close to zero and insignificant. Since the BC

[8] Recall that we account for contemporaneous industry shocks by including time-varying industry-year controls, which are computed as the mean ROA in the firm's industry in a given year, excluding the firm itself. As the industry-year controls are computed based on *all* firms in the same three-digit SIC industry (excluding the firm itself), they likely also include firms incorporated in states that have passed a BC law (treatment group), thus potentially picking up some the laws' effect. Not surprisingly, when we compute the industry-year controls using only firms that are in the control group, our results become (slightly) *stronger*: In column 1, the coefficient on the BC dummy becomes −0.007 (*t*-statistic of 2.42), and in column 2, the coefficient on *BC* × *HHI*

(*footnote continued*)
becomes −0.039 (*t*-statistic of 4.46). (The coefficient on the BC dummy in column 2 remains unchanged.) By the same token, the coefficient on the industry-year control becomes smaller in both regressions.

dummy captures the limit effect as the HHI goes to zero, this implies that the passage of the BC laws has no significant effect on firms in highly competitive industries. Finally, the regression in column 2 allows us to disentangle the two opposite effects of competition on profits. The positive coefficient on the HHI as a control variable implies that the direct effect is negative, i.e., firms in more competitive industries make fewer profits. In contrast, the negative coefficient on the interaction term between the BC dummy and the HHI implies that the indirect (or "managerial-slack") effect is positive, i.e., firms in more competitive industries experience a smaller drop in ROA after the laws' passage.

The positive coefficient on the HHI as a control variable also mitigates potential endogeneity concerns related to the HHI. A main concern here is reverse causation. Specifically, a drop in profits, possibly caused by the passage of the BC laws, might lead to firm exits and thus higher industry concentration (higher HHI). As already pointed out by Nickell (1996), reverse causation would thus predict that the HHI as a control variable has a negative sign. However, the coefficient is positive, which is consistent with the (conventional) interpretation that firms in competitive industries make fewer profits.

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

Our results are consistent with the notion that competition mitigates managerial slack. While the *opportunity* for managerial slack increases equally across all industries, managerial slack appears to increase only in non-competitive industries, but not in highly competitive industries, where competitive pressure enforces discipline on management. Importantly, as our results are based on *changes* in ROA, they do not speak to the issue of what is the *level* of managerial slack in competitive industries. In particular, they do not suggest that competitive industries exhibit zero managerial slack. In fact, it is perfectly possible, and indeed quite plausible, that there is some positive "baseline level" of slack in all industries. While firms in competitive industries may naturally operate at this minimum level, firms in non-competitive industries may only operate at this level if there is additionally a credible threat of a disciplinary hostile takeover.

3.2. Reverse causality

While the inclusion of state- and industry-year controls alleviates concerns that the BC laws are the outcome of lobbying at the local and industry level, respectively, it remains the possibility that lobbying occurs at the state of incorporation level. Such lobbying is a concern because it opens up the possibility of reverse causation. Precisely, if a broad coalition of firms incorporated in the same state, which all experience a decline in profitability and, moreover, all operate in non-competitive industries, successfully lobby for an anti-takeover law in their state of incorporation, then causality might be reversed.

Given the anecdotal evidence in Romano (1987), who portrays lobbying for anti-takeover laws as an exclusive political process, the notion of broad-based lobbying seems unlikely. Typically, anti-takeover 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 select firms, the laws were likely exogenous.[9] This notwithstanding, the possibility of reverse causality deserves closer investigation. Following Bertrand and Mullainathan (2003), we replace the BC dummy in Eq. (2) with four dummies: $BC\ Year(-1)$, $BC\ Year(0)$, $BC\ Year(1)$, and $BC\ Year(2+)$, where $BC\ Year(-1)$ is a dummy that equals one if the firm is incorporated in a state that will pass a BC law in one year from now, $BC\ Year(0)$ is a dummy that equals one if the firm is incorporated in a state that passes a BC law this year, and $BC\ Year(1)$ and $BC\ Year(2+)$ are dummies that equal one if the firm is incorporated in a state that passed a BC law one year ago and two or more years ago, respectively. If the BC laws were passed in response to political pressure of a broad coalition of firms, which all experience a decline in profitability and, moreover, all operate in non-competitive industries, then we should see an "effect" of the laws already prior to their passage. In particular, if the coefficient on $BC\ Year(-1) \times HHI$ was negative and significant, then this would be symptomatic of reverse causality.

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

3.3. Change in firms' earnings management?

ROA is an accounting measure that can be manipulated. Accordingly, a drop in ROA after the passage of the BC laws does not necessarily imply a reduction in operating profitability. It could simply reflect a change in the extent to which firms manage their earnings. For example, firms might overstate their earnings to appear more profitable in order to ward off hostile takeovers.

[9] Using newspaper reports (see Section 5), we have identified firms motivating the passage of the BC laws. For example, the Minnesota BC law was adopted under the political pressure of the Dayton Hudson (now Target) Corporation, when it was attacked by the Dart Group Corporation. Similar to other studies (e.g., Garvey and Hanka, 1999), we find that excluding such motivating firms does not affect our results.

Consequently, firms' earnings might drop after the laws' passage not because of a decrease in operating profitability, but simply because the need for earnings overstatement has been reduced. If additionally the threat of being taken over is primarily a concern for firms in non-competitive industries, then this alternative story, based on changes in firms' earnings management, could potentially explain our results.

While it is difficult to completely rule out this alternative story, we can offer some pieces of evidence that are inconsistent with it. First, the likelihood of being taken over is not significantly different in competitive and non-competitive industries: Below we will present a regression predicting the likelihood of being taken over in which the HHI dummies as control variables are all insignificant. (See Table 6 for more details; to avoid perfect multicollinearity, we have dropped one of the HHI dummies, implying that the other two HHI dummies measure the takeover likelihood relative to firms in the lowest HHI tercile.)

Second, we can examine whether the passage of the BC laws has a different effect on ROA depending on whether the laws were passed early or late in the fiscal year. If a BC law is passed only a few months prior to the fiscal year's end, then it would seem hard to imagine that the current year's ROA should drop by much, given that most of the fiscal year is already over. In this case, a significant drop in ROA might be indicative of an earnings management story.

In Panel A of Table 4, we estimate a regression similar to that in Panel B of Table 3, except that the reference point is not the calender year in which the BC law was passed, but the effective month of the law's passage, which is denoted by " 0*m*." Thus, the dummy *BC*(0*m to* 6*m*) indicates that ROA is measured within six months after the law's passage, the dummy *BC*(6*m to* 12*m*) indicates that ROA is measured between six and twelve months after the law's passage, and so forth. For instance, the Delaware BC law was passed on February 8, 1988. A Delaware company whose fiscal year ends in June thus has its fiscal year end within six months after the law's passage. For this company, the dummy *BC*(0*m to* 6*m*) is set to one in 1988. In contrast, a Delaware company whose fiscal year ends in December has its fiscal year end between six and 12 months after the law's passage. For this company, the dummy *BC*(6*m to* 12*m*) is set to one in 1988.[10] The main variable of interest is the interaction term *BC*(0*m to* 6*m*) $\times$ *HHI*, which captures the effect of the BC laws on firms in non-competitive industries when a law is passed late in the fiscal year. If the coefficient on this interaction term was significant, then this might be indicative of an earnings management story. However, as is shown, the coefficient is small and insignificant. Moreover, the coefficients on all subsequent interaction terms are large and significant, implying that it takes about six months until the effect of the BC laws shows up significantly in the ROA number.

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

To test whether firms' earnings management has changed after the passage of the BC laws, we estimate our basic specification using discretionary accruals as the dependent variable. The results are presented in Panel B of Table 4. As is shown in column 1, the coefficients on *BC* and *BC* $\times$ *HHI* are both small and insignificant, suggesting that firms did not change their earnings management after the laws' passage. A related proxy for earnings management are discretionary *current* accruals, as used by Teoh, Welch, and Wong (1998). The authors decompose discretionary accruals into a short-term (or current) component and a long-term component and argue that managers have more discretion over the short-term component. Discretionary current accruals might thus be a less noisy proxy for earnings management. The results, which are shown in column 2, are similar to those in column 1.

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

3.4. Change in firms' asset mix?

An alternative story that is similar to the one above is that in which firms, rather than overstating their earnings, invest in higher risk/higher return (but similar net present value (NPV)) projects to appear more profitable in order to ward off hostile takeovers. As the ROA measure does not adjust for risk, a drop in ROA after the passage of the BC laws does not necessarily imply a reduction in operating profitability. It could simply

[10] Likewise, in 1987, the dummy *BC*($-12m$ *to* $-6m$) is set to one for the first company, while the dummy *BC*($-6m$ *to* 0*m*) is set to one for the second company. In contrast, in Panel B of Table 3, which is based on calender years, the dummy *BC Year*(-1) is set to one for both companies in 1987, the dummy *BC Year*(0) is set to one for both companies in 1988, and so forth.

Table 4

Change in firms' earnings management?

BC(−12*m to* −6*m*) is a dummy variable that equals one if the firm is incorporated in a BC state and the firm's fiscal year end lies between 12 months and six months prior to the month of the law's passage. *BC*(−6*m to* 0*m*), *BC*(0*m to* 6*m*), *BC*(6*m to* 12*m*), and *BC*(12*m*+) are defined analogously. Discretionary accruals are computed as in Dechow, Sloan, and Sweeney (1995). Discretionary current accruals are computed as in Teoh, Welch, and Wong (1998). All other variables are defined in Tables 2 and 3. The coefficients are estimated using OLS. Standard errors are clustered at the state of incorporation level. The sample period is from 1976 to 1995. *t*- statistics are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Fiscal year ends

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

Panel B: Earnings management

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

reflect firms' decisions to change their asset mix towards lower risk/lower return projects, given that the threat of a hostile takeover is now reduced. If additionally the threat of being taken over is primarily a concern for firms in non-competitive industries, then this alternative story, based on changes in firms' asset mix, could potentially explain our results.

To test whether firms' asset mix has become less risky after the passage of the BC laws, we estimate our basic specification using two different measures of asset risk as the dependent variable. The first is cash-flow volatility, as defined in Zhang (2006), which captures both systematic and idiosyncratic asset risk. Cash-flow volatility is computed as the standard deviation of cash flows from operations over the past five years, with a minimum of three years. The second measure is the firm's asset beta, which only captures systematic risk. As is common practice, we compute the asset beta by multiplying the equity beta with one minus the ratio of equity to total assets (e.g., Odders-White and Ready, 2006; Lewellen, 2006). The equity beta is obtained by estimating the market model using five years of monthly stock returns from the Center for Research in Security Prices (CRSP). As is shown in Table 5, regardless of which measure of asset risk we use, the coefficients on *BC* and *BC* × *HHI* are both small and insignificant, suggesting that firms did not change their asset mix after the passage of the BC laws.

While it is difficult to definitely rule out that the drop in ROA is due to a change in firms' asset mix, the evidence presented here is inconsistent with this idea. Additional supporting evidence will be presented in Section 4, where

Table 5
Change in firms' asset mix?

Cash-flow volatility is computed as in Zhang (2006). The asset beta is computed as the equity beta times the market value of equity (Compustat item #24 times item #25) divided by the market value of assets (item #24 times item #25 − item #60 + item #6). The equity beta is obtained by estimating the market model over the previous five years using monthly return data from CRSP. All other variables are defined in Tables 2 and 3. The coefficients are estimated using OLS. Standard errors are clustered at the state of incorporation level. The sample period is from 1976 to 1995. *t*- Statistics are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Dependent variable:	[1] Cash-flow volatility	[2] Asset beta
BC	0.001	0.003
	(0.53)	(0.15)
BC × *HHI*	0.003	−0.007
	(0.61)	(0.30)
Industry-year	0.056***	0.234***
	(4.32)	(18.37)
State-year	0.046**	0.215***
	(2.08)	(6.65)
Size	−0.033***	0.056***
	(13.57)	(4.38)
Size-squared	0.001***	−0.001
	(6.16)	(0.95)
Age	0.005***	−0.058**
	(2.87)	(2.38)
HHI	−0.010	0.109
	(1.53)	(1.56)
Firm fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Observations	54,460	75,831
Adj. *R*-squared	0.63	0.46

we will show that firms did not change their research and development (R&D) activity, capital expenditures, and acquisition activity after the passage of the BC laws.

3.5. Did BC laws reduce the takeover threat?

A key assumption underlying our identification strategy is that BC laws reduce the takeover threat. This assumption may appear in conflict with evidence by Comment and Schwert (1995), which suggests that anti-takeover laws did not significantly lower the takeover likelihood.[11] However, as Garvey and Hanka (1999) point out, as the takeover likelihood is an equilibrium outcome, it is possible that anti-takeover laws are effective, in the sense that they reduce the takeover threat, yet the takeover likelihood remains unchanged. On the one hand, anti-takeover laws increase the costs of mounting a hostile takeover. On the other hand, anti-takeover laws, by reducing the takeover threat, may lead to an increase in managerial slack, which increases the gains from mounting a hostile takeover. Since the two effects go in opposite directions, it is not clear what the overall effect on subsequent takeover activity will be.

While this argument is appealing, it is unlikely to hold for the entire cross section. Given our previous results, we would indeed expect that in non-competitive industries managerial slack increases after the laws' passage, implying that the overall effect on the takeover likelihood is potentially ambiguous. However, we would expect no significant increase in slack in competitive industries, implying that the takeover likelihood in these industries should decline. This latter statement deserves clarification. If it were true that competitive industries leave *zero* room for managerial slack, then we should not observe any disciplinary takeovers in these industries, neither before nor after the passage of the BC laws, and thus, also no change in the takeover likelihood.[12] However, as we argued in Section 3.1, our results do not suggest that competitive industries exhibit zero managerial slack. In fact, it is perfectly possible, and indeed quite plausible, that there is some positive "baseline level" of slack in all industries, and thus, also in competitive industries. In that case, we should observe disciplinary takeovers also in competitive industries, whose frequency we would then expect to decline, absent any offsetting increase in managerial slack, after the passage of the BC laws.

To investigate the effect of the passage of the BC laws on the takeover likelihood, we follow Shumway (2001) and estimate a multiperiod logit model where the dependent variable is a dummy that equals one if the firm is acquired in the following year, and zero otherwise. As Shumway (Proposition 1) shows, this multiperiod logit model is equivalent to a discrete-time hazard model and thus accounts for differences in the time to acquisition. Moreover, the model entails firm-level dependence by construction, since a firm that has survived until time t cannot have been acquired at time $t-1$. In the estimation, we not only account for firm-level dependence but more generally for any arbitrary correlation within a state of incorporation by clustering the logit standard errors at the state of incorporation level.

The takeover data are obtained from the Securities Data Corporation's (SDC) database. Since these data begin in 1979, our sample period is reduced to 1978–1995 (with observed takeovers from 1979–1996). We control for firm age by including age dummies. As Shumway (2001, p. 112) notes, any function of age can be included in the model. Our results are similar if we instead include the

[11] Contrary to his own previous findings, Schwert (2000) finds that hostile takeovers have become significantly less likely after 1989, which he partly attributes to the passage of anti-takeover laws: "This probably reflects the effects of [...] state anti-takeover laws. In contrast, Comment and Schwert (1995) were unable to identify a statistically significant decline in hostile offers based on an analysis of transactions through 1991."

[12] BC laws only affect disciplinary hostile takeovers. They do not impede friendly takeovers, where the target firm's directors can simply approve the business combination. For instance, the Delaware BC law, the most significant of its kind, stipulates that: "Notwithstanding any other provisions of this chapter, a corporation shall not engage in any business combination with any interested stockholder for a period of 3 years following the time that such stockholder became an interested stockholder, unless: (1) Prior to such time the board of directors of the corporation approved either the business combination or the transaction which resulted in the stockholder becoming an interested stockholder [...]" (Del. Gen. Corp. L. Section 203). By implication, any observed change in the takeover frequency after the passage of the BC laws should exclusively come from disciplinary hostile takeovers.

Table 6
Did BC laws reduce the takeover threat?

"Likelihood of being acquired" is a dummy variable that equals one if the firm is acquired in the next calendar year. The acquisition data are from the Securities Data Corporation's (SDC) database. All other variables are defined in Tables 2 and 3. The coefficients are estimated using a multiperiod logit model. Standard errors are clustered at the state of incorporation level. The sample period is from 1978 to 1995. z-Statistics are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Dependent variable: Likelihood of being acquired	[1]	[2]
BC	−0.182	
	(1.59)	
BC × *HHI*(*Low*)		−0.318 *
		(1.82)
BC × *HHI*(*Medium*)		−0.114
		(0.86)
BC × *HHI*(*High*)		−0.029
		(0.23)
Industry-year	3.335 ***	3.347 ***
	(7.35)	(7.20)
State-year	2.189 *	2.170 *
	(1.82)	(1.83)
Size	−0.054 **	−0.054 **
	(2.20)	(2.22)
Size-squared	0.008 ***	0.008 ***
	(3.26)	(3.29)
HHI(*Medium*)	−0.060	−0.196
	(0.63)	(1.06)
HHI(*High*)	−0.060	−0.251
	(0.61)	(1.20)
Age fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Observations	77,142	77,142
Adj. *R*-squared	0.06	0.06

logarithm of age as a control variable, or if we do not control for age at all. The other control variables are the same as in our basic regression. Importantly, we also control for firm size, which, as Schwert (2000) argues, is the only variable that is consistently significant in empirical studies of the takeover likelihood.

The results are presented in Table 6. Column 1 shows the average effect (i.e., across all firms) of the passage of the BC laws on the takeover likelihood. While the coefficient on the BC dummy is negative, it is not significant. Thus, consistent with Comment and Schwert's (1995) findings, BC laws do not significantly reduce the takeover likelihood, *on average*. In column 2, we examine whether the laws' passage has a different effect on the takeover likelihood in competitive and non-competitive industries. We obtain two main results. First, the effect is monotonic in the HHI. Second, and consistent with our hypothesis, we find that while the passage of the BC laws significantly reduces the takeover likelihood in competitive industries (lowest HHI tercile), it has no significant effect on the takeover likelihood in non-competitive industries (medium and highest HHI terciles).[13]

Table 7
Alternative measures of competition (all industries).

HHI (2-digit) and *HHI* (4-digit) are HHIs based on two-digit and four-digit SIC codes, respectively. NPM is operating income before depreciation and amortization (Compustat item #13) divided by sales (item #12). Industry NPM is the median NPM in a given year and three-digit SIC industry. All other variables are defined in Tables 2 and 3. The coefficients are estimated using OLS. Standard errors are clustered at the state of incorporation level. The sample period is from 1976 to 1995. *t*-Statistics are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

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

3.6. Robustness

3.6.1. Alternative competition measures

Our main competition measure is the HHI based on three-digit SIC codes. In Table 7, we use HHIs based on two-digit SIC codes (column 1) and four-digit SIC codes (column 2), respectively. As is shown, the results are similar to those in Table 3. The only difference is that the two-digit HHI as a control variable is not significant, which is due to lack of sufficient "within" variation of this variable. As for the economic magnitude of the "managerial-slack" effect, an increase in the two-digit HHI by one standard deviation is associated with a drop in ROA of $-0.056\times0.076=-0.004$, or 0.4 percentage points, which is close to the estimate in Table 3.

[13] It should be noted that the coefficients associated with the three interaction terms $BC\times HHI(Low)$, $BC\times HHI(Medium)$, and $BC\times HHI(High)$

(*footnote continued*)
are not significantly different from each other. Likewise, if we replace the three interaction terms with a BC dummy and a single interaction term $BC\times HHI$, then neither the BC dummy nor the interaction term is significant.

Table 8
Alternative measures of competition (manufacturing industries).

HHI (Census) is the HHI based on four-digit SIC manufacturing industries (SIC 2000–3999) provided by the U.S. Census Bureau. The index is available for the years 1982, 1987, and 1992 during the sample period. To fill in the missing years, we always use the index value from the latest available year. For the years prior to 1982, we use the index value from 1982. "Import penetration" is a dummy variable that equals one if the import penetration in a given four-digit SIC manufacturing industry lies above the industry mean. Import penetration is defined as imports divided by the sum of total shipments minus exports plus imports. The import data are from Peter Schott's Web page and are described in Feenstra (1996) and Feenstra, Romalis, and Schott (2002). All other variables are defined in Tables 2 and 3. The coefficients are estimated using OLS. Standard errors are clustered at the state of incorporation level. The sample period is from 1976 to 1995. *t*-Statistics are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

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

Likewise, an increase in the four-digit HHI by one standard deviation is associated with a drop in ROA of $-0.022 \times 0.190 = -0.004$, or 0.4 percentage points.

In untabulated regressions, we use two-digit, three-digit, and four-digit HHIs based on firms' assets in place of sales. The idea behind using asset-based HHIs is that sales can be rather volatile, with the effect that changes in the HHI may overstate actual changes in industry concentration (Hou and Robinson, 2006). The results using asset-based HHIs are similar to those in Table 3. An alternative way to address the issue of sales volatility is to use smoothed HHI measures. For instance, using a three-year moving average HHI based on three-digit SIC codes, we find that the interaction term between the BC dummy and the HHI has a coefficient of −0.029 (*t*-statistic of 3.94), which is similar to the estimate in Table 3.

In column 3, we consider a margin-based measure of competition, namely, the median industry net profit margin (NPM) based on three-digit SIC codes. At the firm level, NPM is computed as operating income before depreciation and amortization (Compustat item #13) divided by sales (item # 12). Industry NPM is commonly used in the industrial organization literature as an empirical proxy for the Lerner index, which measures the extent to which firms can set prices above marginal cost. Under the commonly made assumption that marginal cost can be approximated by the average variable cost (Carlton and Perloff, 1989, p. 367), the Lerner index and industry NPM are equivalent. As is shown, the results are similar to our baseline results in Table 3.

In Table 8, we consider competition measures that are only available for manufacturing industries (SIC 2000-3999). In column 1, we use the Census HHI, which is based on all public and private firms. While the Census HHI is broader than the HHI computed from Compustat, it has some limitations. First, the index is only available for the years 1982, 1987, and 1992 during the sample period. To fill in the missing years, we always use the index value from the latest available year. For the years prior to 1982, we use the index value from 1982. Second, the index is only available on the narrow four-digit SIC code level, which implies that it is likely subject to misclassification. Third, the index is only available for manufacturing industries, which implies that the sample is substantially smaller. And yet, the results are similar to those in Table 3. As for the economic magnitude of the "managerial-slack" effect, an increase in the Census HHI by one standard deviation is associated with a drop in ROA of $-0.081 \times 0.046 = -0.004$, or 0.4 percentage points, which is similar to the estimate in Table 3. Note that the HHI as a control variable is omitted in column 1. Except for three "jumps" in 1982, 1987, and 1992, this variable has no "within" variation, implying that the coefficient is not well identified.

Whether we use the Census HHI or the HHI computed from Compustat, we only capture domestic competition. In column 2, we use import penetration as our competition measure. Like the Census HHI, this measure is only available for manufacturing industries, and only on the narrow four-digit SIC code level, which implies that it is likely subject to misclassification. Moreover, it is not clear that import penetration is a suitable measure of competition. For instance, import penetration may be high, yet an industry may be non-competitive because all of the imports come from a single foreign producer. Likewise, import penetration may be low, yet an industry may be highly competitive because domestic competition is fierce. In fact, import penetration may be low *because* domestic competition is fierce. While the results are similar to those in Table 3, they are slightly weaker. This may be partly due to the smaller sample size, but it may also be due to the fact that import penetration is a poor measure of competition. Perhaps the most meaningful way to use import penetration is together with the Census HHI, as is done in column 3. In this regression, the BC dummy measures the (limit) effect of the BC laws on industries with both high domestic competition and high import penetration. As is shown, the results are similar to those in column 1.

3.6.2. *Miscellaneous robustness checks*

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

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

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

Non-Delaware and "Eventually BC" samples. About one-half of the firms in our sample are incorporated in Delaware. Thus, one might be worried that our results are driven by a single BC law. When we exclude Delaware firms from the treatment group, we find that the BC dummy is again close to zero and insignificant, while the interaction term between the BC dummy and the HHI has a coefficient of −0.032 (t-statistic of 2.41), which is almost identical to the estimate in Table 3.[14] Another potential concern is that the control and treatment groups might differ for reasons unrelated to the passage of the BC laws. To address this issue, we restrict the control group to firms incorporated in treatment states that have not yet passed a BC law. The results are again similar to those in Table 3. While the BC dummy is close to zero and insignificant, the interaction term between the BC dummy and the HHI has a coefficient of −0.032 (t-statistic of 4.74).

Alternative performance measures. Our main performance measure is ROA before depreciation. In robustness checks, we use a variety of alternative performance measures: ROA after depreciation, which is defined as operating income after depreciation and amortization (Compustat item # 178) divided by total assets (item #6), net profit margin (NPM), which is defined as operating income before depreciation and amortization (item #13) divided by sales (item #12), and return on equity (ROE), which is defined as net income (item #172) divided by common equity (item #60). Arguably, ROA is the most suited of these measures. NPM is based on firms' sales, which can be rather volatile, while ROE depends on leverage. Regardless of which performance measure we use, however, we obtain results that are similar to those in Table 3. While the BC dummy is always close to zero and insignificant, the interaction term between the BC dummy and the HHI is always negative and significant, with coefficients ranging from −0.031 to −0.035 (t-statistics from 2.74 to 4.73).

Different sample periods. Our sample period is from 1976 to 1995, which is the same period as in Bertrand and Mullainathan (2003). It has been questioned whether the BC laws had any significant effect prior to June 1989, when an appellate court upheld Wisconsin's BC law in *Amanda Acquisition Corp. vs. Universal Foods Corp.*[15] To

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

[15] Already in 1988, the Delaware BC law was held to be constitutional in *RP Acquisition Corp. vs. Staley Continental, Inc*. The Wisconsin ruling in 1989 is viewed as a landmark decision, though, because the Wisconsin law was more stringent than the Delaware law, and because

address this issue, we estimate our basic specification for the truncated period from 1976 to 1988. The results are again similar to our results in Table 3. While the BC dummy is close to zero and insignificant, the interaction term between the BC dummy and the HHI has a coefficient of -0.029 (t-statistic of 4.35). A possible explanation for why the results are similar is that firms may have always believed that the BC laws would be enforced, so they acted accordingly. This conviction may in part stem from earlier rulings on other types of anti-takeover laws, such as the decision in 1987 by the U.S. Supreme Court to uphold Indiana's control share acquisition law (*CTS Corp. vs. Dynamics Corp. of America*).

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

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

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

Heterogeneous time trends and state effects. To allow for heterogeneous time trends and state effects, we interact all covariates with time dummies and treatment-state dummies. The results are again similar to those in Table 3.

Cross-sectional and serial correlation of the error terms. Cross-sectional correlation is a concern because all firms in a given year and state of incorporation are affected by the same "shock," namely, the passage of the BC law (Moulton, 1990). Serial correlation is a concern because the BC dummy changes little over time, being zero before and one after the passage of the BC law (Bertrand, Duflo, and Mullainathan, 2004). Given that the BC dummy is a likely source of both cross-sectional and serial correlation, we cluster standard errors at the state of incorporation level in all our regressions.

In addition to clustering, we consider a number of alternative correction methods. The methods are all described in Bertrand, Duflo, and Mullainathan (2004), which is why we shall be brief here. For instance, we obtain similar results if we use an AR(1) correction method, or if we (block) bootstrap the standard errors using 51 blocks with 200 bootstrap samples. We also obtain similar results if we collapse the data into two periods, before and after the BC law, which is a crude, albeit effective, way to deal with the issue of serial correlation. Since "before" and "after" are different for each treatment state (while for control states "before" and "after" are not even well-defined), the estimation proceeds in two steps. In the first step, we regress ROA on fixed effects and covariates, except for the BC dummy and the interaction term between the BC dummy and the HHI. For treatment states only, we then collect the residuals and compute the average residuals for the pre- and post-BC law periods. This provides us with a two-period panel, where the first period is before the law and the second period is after the law. In the second step, we regress the average residuals on the BC dummy and the interaction term between the BC dummy and the average post-BC HHI. We use robust standard errors to correct for heteroskedasticity. We also obtain similar results if we use a similar two-step procedure to deal with the issue of cross-sectional correlation. Specifically, we collapse the data into state of incorporation-industry-year cells, based on the notion that our main variables of interest, namely, the BC dummy and the HHI, are both at a higher level of aggregation.

4. Empire building or quiet life?

While our results suggest that competition mitigates managerial agency problems, they do not say which agency problem is being mitigated. Does competition curb managerial empire building? Or does it prevent managers from enjoying a "quiet life" by forcing them to undertake "cognitively difficult activities" (Bertrand and Mullainathan, 2003)? In Table 9, we attempt to distinguish between these two hypotheses. For brevity's sake, we only report the coefficients on the BC dummy and the interaction term between the BC dummy and the HHI, which are our main variables of interest.

In Panel A, we estimate our basic specification using various proxies for empire building as the dependent variable. In column 1, we use capital expenditures (Compustat item #30) divided by total assets (item #6). Arguably, capital expenditures are a poor proxy for empire building if most of the activity comes in the form of acquisitions. We address this issue by using total asset

(*footnote continued*)
it was upheld by a federal appellate court and ended up with the U.S. Supreme Court, which sustained the decision of the appellate court.

Table 9
Empire building or quiet life?

In Panel A, capital expenditures (Compustat item #30) are divided by total assets (item #6). Asset growth is the percentage increase in total assets from one year to the next. PPE growth is the percentage increase in property, plant, and equipment (item #8) from one year to the next. "Acquisition ratio" is the sum of the value of all acquisitions made by the firm in a given year divided by the firm's average market capitalization in that year (from CRSP). The acquisition data are from the Securities Data Corporation's (SDC) database. "Likelihood of being acquirer" is a dummy variable that equals one if the firm makes at least one acquisition during the year. In Panel B, Selling, general & admin. expenses are SG&A expenses (item #189) divided by total assets. Advertising expenses (item #45) and Costs of goods sold (item #41) are both divided by sales (item #12). R&D expenses (item #46) are divided by total assets. Wages (real) are the natural logarithm of labor and related expenses (item #42) divided by the number of employees (item #29) and deflated by the consumer price index from the U.S. Bureau of Labor Statistics. All other variables are defined in Tables 2 and 3. For brevity, only the coefficients on *BC* and *BC* × *HHI* are reported. The coefficients are estimated using OLS. Standard errors are clustered at the state of incorporation level. The sample period is from 1976 to 1995, except in columns 4 and 5 of Panel A, where the sample period is from 1979 to 1995. *t*-Statistics are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Empire building					
Dependent variable:	[1] Capital expenditures	[2] Asset growth	[3] PPE growth	[4] Acquisition ratio	[5] Likelihood of being acquirer
BC	−0.000	−0.004	−0.003	0.000	0.003
	(0.27)	(0.70)	(0.53)	(0.02)	(0.42)
BC × *HHI*	0.001	−0.004	−0.000	0.001	−0.010
	(0.18)	(0.39)	(0.04)	(0.43)	(0.48)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	74,435	67,806	64,449	70,248	75,415
Adj. *R*-squared	0.55	0.35	0.22	0.29	0.36

Panel B: Quiet life					
Dependent variable:	[1] Selling, general & admin. expenses	[2] Advertising expenses	[3] R&D expenses	[4] Costs of goods sold	[5] Wages (real)
BC	0.005	0.000	0.001	−0.002	−0.003
	(0.80)	(0.59)	(0.49)	(0.20)	(0.12)
BC × *HHI*	0.029**	0.003	0.007	0.053 **	0.103**
	(2.51)	(1.04)	(1.39)	(2.44)	(2.00)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	68,561	28,389	39,359	74,758	8,651
Adj. *R*-squared	0.81	0.80	0.76	0.60	0.89

growth and PPE growth in columns 2 and 3, respectively. Total asset growth is the percentage increase in total assets, while PPE growth is the percentage increase in property, plant, and equipment (item #8). We also construct more direct proxies for acquisition activity using data from the Securities Data Corporation's (SDC) database. Since the SDC data begin in 1979, the sample period is from 1979 to 1995. In column 4, the dependent variable is the sum of the value of all acquisitions made by a firm in a given year divided by the firm's average market capitalization in that year ("acquisition ratio"). In column 5, the dependent variable is the likelihood of being an acquirer, measured by a dummy variable that equals one if the firm makes at least one acquisition during the year and zero otherwise. As is shown, regardless of which proxy we use, neither the BC dummy nor the interaction term between the BC dummy and the HHI are significant, neither individually nor jointly.

In Panel B, we estimate our basic specification using various proxies for "quiet life" as the dependent variable. In column 1, we use selling, general, and administrative expenses ("overhead costs," item #189) divided by total assets. In column 2, we use advertising expenses (item #45) divided by sales (item #12). In column 3, we use R&D expenses (item #46) divided by total assets. In column 4, we use costs of goods sold ("input costs," item #41) divided by sales. In column 5, we use real wages, which are computed as the natural logarithm of labor and related expenses (item #42) divided by the number of employees (item #29) and deflated by the consumer price index from the U.S. Bureau of Labor Statistics. As is shown, the BC dummy is always close to zero and insignificant. In columns 2 and 3, the interaction term between the BC dummy and the HHI has the right sign but is insignificant. Importantly, however, in columns 1, 4, and 5, the interaction term is positive and significant, implying that overhead costs, input costs, and real wages all increase after the passage of the BC laws, and only so in non-competitive industries. These results are consistent with a "quiet-life" hypothesis, whereby managers insulated from both hostile takeovers and competitive pressure seek to avoid cognitively difficult activities, such as haggling with input suppliers, labor unions, and organizational units within the company demanding bigger overhead budgets.

Let us conclude with two caveats. First, the interaction terms in Panel B have smaller t-statistics than in our regression in Table 3, presumably because the dependent variables are all individual components of ROA. Thus, while the passage of the BC laws has a large and significant effect on ROA overall, it has only a relatively small effect on any individual component of ROA. Second, the wage result in Panel B should be taken with caution. Not only is the sample quite small, as only few firms in Compustat report wage data, but the data are also very noisy (see Bertrand and Mullainathan, 1999). For instance, some firms report wage data only intermittently, while others report no wage data at all. What is more, Compustat only provides aggregate data on labor and related expenses, which also includes pension costs, payroll taxes, and employee benefits. On a positive note, our wage results are similar to those in Bertrand and Mullainathan (1999, 2003), who report wage increases between 1% and 2% after the passage of the BC laws. (In our case, the average wage increase is 1.9%.)

5. Event-study results

Does the stock market anticipate that firms in competitive industries are largely unaffected by the passage of the BC laws? The main difficulty in answering this question lies in the choice of event date. Since the passage date itself is well-anticipated, it is unlikely to contain much new information. Instead, one must find an early date at which significant news about the law is disseminated to the public, e.g., the date of the first newspaper report about the law. For instance, Karpoff and Malatesta (1989), in their event study of anti-takeover laws, find no significant abnormal returns when using either the date of the law's introduction in the state legislature, its final passage, or its signing by the governor as the event date. However, they do find statistically significant abnormal returns when using the first date for which they could find a newspaper report about the law 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, while large out-of-state newspapers, such as the *Wall Street Journal* and the *New York Times*, often provide no coverage, especially if the state in question is small and only few firms are incorporated in it. After a careful search of major newspaper databases (ProQuest, Lexis-Nexis, Factiva, Newsbank America's Newspapers, Google News Archive), we could find newspaper reports for 19 of the 30 BC laws in our sample: Arizona, Connecticut, Delaware, Georgia, Illinois, Kentucky, Maryland, Massachusetts, Minnesota, New Jersey, New York, Ohio, Oklahoma, Pennsylvania, South Carolina, Tennessee, Virginia, Washington, and Wisconsin. Most of the remaining 11 states are small in terms of the number of incorporated firms. In fact, seven of them had fewer than 20 firms—and only one (Nevada) had more than 100 firms—in the merged CRSP-Compustat sample in the year in which the BC law was passed. 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). While this assumption is satisfied in many applications, it is not satisfied in our case. Since all firms incorporated in a given state are affected by the same event, their abnormal returns are likely correlated, leading to biased standard errors. A common way to address this problem is to form portfolios consisting of all firms incorporated in a given state. Since the event dates are different for each state portfolio, the issue of cross-sectional correlation then becomes negligible (MacKinlay, 1997; Karpoff and Malatesta, 1989).

Our empirical methodology follows Karpoff and Malatesta (1989). For each state portfolio j, we estimate the market model using CRSP daily return data from 241 to 41 trading days prior to the event date.[16] That is, we estimate

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

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

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

To obtain cumulative abnormal returns (CAR), we simply sum up the abnormal returns over the desired time interval. We report average CARs based on the 19 state portfolios for the same time intervals as Karpoff and Malatesta (1989): $[-40,-2]$, $[-3,-2]$, $[-1,0]$, [1,2], and [1,10], where $[-1,0]$ is the two-day event window. To see if there is any systematic trend prior to the event date, we additionally report average CARs for the time intervals $[-30,-2]$, $[-20,-2]$, and $[-10,-2]$.

The above methodology yields an estimate of the average impact of the BC laws on stock prices. To examine if the price impact is different for firms in competitive and non-competitive industries, we divide each state portfolio into equal-sized smaller portfolios. For each state j, we form a low- and high-HHI portfolio by sorting firms based on whether their HHI lies below or above the median, respectively. We also do the same with low-, medium-, and high-HHI portfolios by sorting firms based on whether their HHI lies in the lowest, medium, and highest tercile, respectively. The remaining steps are the same as above.

The results are shown in Panel A of Table 10. As is shown in column 1, the average CAR in the two-day event window is -0.32% (z-statistic of -2.58). Moreover, 14 of the 19 individual CARs are negative. Of equal interest is that the average two-day CARs immediately before and after the two-day event window are small and insignificant.

[16] Choosing the estimation window adjacent to the first time interval for which cumulative abnormal returns are computed (here: the time interval $[-40,-2]$) is common practice (MacKinlay, 1997). However, we obtain similar results if we instead estimate the market model for the time interval from 300 to 100 trading days prior to the event date. We also obtain similar results if we use either a three- or four-factor model instead of the market model.

Table 10
Event-study results.
The methodology used to compute cumulative abnormal returns (CARs) is described in Section 5. The event date is the date of the first newspaper report about the BC law. The two-day event window is denoted by $[-1,0]$. The numbers reported in the table are average portfolio CARs based on 19 state portfolios. The 19 states are Arizona, Connecticut, Delaware, Georgia, Illinois, Kentucky, Maryland, Massachusetts, Minnesota, New Jersey, New York, Ohio, Oklahoma, Pennsylvania, Tennessee, South Carolina, Virginia, Washington, and Wisconsin. The construction of the state-HHI portfolios, bottom-HHI portfolios, and top-HHI portfolios is described in Section 5. In Panel B, the hedge portfolio is long in the top-HHI portfolio and short in the bottom-HHI portfolio. z-Statistics are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Event-study results

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

Panel B: Hedge portfolios

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

Columns 2 and 3 show the average CARs for the low- and high-HHI portfolios. The average two-day event CAR for the low-HHI portfolio is small and insignificant, while the average two-day event CAR for the high-HHI portfolio is −0.54% (z-statistic of −2.36). Thus, while firms in competitive industries experience no significant stock price impact around the date of the first newspaper report about the BC law, firms in non-competitive industries experience a significant abnormal stock price decline.

Columns 4–6 show the average CARs for the low-, medium-, and high-HHI portfolios. The results are again similar. While firms in competitive industries experience no significant stock price impact, firms in less competitive industries experience a significant abnormal stock price decline. Moreover, and consistent with the results in column 3 of Table 3, the stock price decline is monotonic in the HHI. While the average two-day event CAR for the low-HHI portfolio is small and insignificant, the average two-day event CARs for the medium- and high-HHI portfolios are −0.44% (z-statistic of −1.67) and −0.67% (z-statistic of −2.31), respectively.[17]

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

[17] We obtain a similar monotonic pattern using median CARs. The median two-day event CAR for the low-, medium-, and high-HHI portfolio is 0.06%, −0.46%, and −0.67%, respectively. The corresponding ratio of positive to negative CARs in the two-day event window is 10:9, 4:15, and 5:14, respectively.

In both cases, the difference between the bottom- and top-HHI group is now significant at the 5% level (z-statistics of 2.02 and 2.06, respectively).

Let us conclude with a word of caution. To a certain extent, the event-study evidence alleviates concerns that our main results are obtained using accounting variables, which can be manipulated (see Section 3.3). That said, it does not convey definitive evidence that firms in non-competitive industries experience a larger drop in profitability. It could be the case that, for a variety of reasons, the value gains from (hostile) takeovers are higher in non-competitive industries, in which case the larger stock price decline in these industries might merely reflect the capitalized value of higher forgone value gains.

6. Conclusion

Using the passage of business combination (BC) laws as a source of exogenous variation, we examine if these laws have a different effect on firms in competitive and non-competitive industries. While firms in non-competitive industries experience a significant drop in operating performance after the laws' passage, firms in competitive industries experience virtually no effect, which is consistent with the notion that competition mitigates managerial slack. When we examine which agency problem competition mitigates, we find evidence in support of a " quiet-life" hypothesis. Input costs, wages, and overhead costs all increase after the laws' passage, and only so in non-competitive industries. We obtain a similar picture when conducting event studies around the dates of the first newspaper reports about the BC laws. While firms in non-competitive industries experience a significant stock price decline, firms in competitive industries experience a small and insignificant stock price impact.

Our results have implications both for policy and research. For instance, our results suggest that policy efforts to improve corporate governance could benefit from focusing primarily on firms in non-competitive industries. Moreover, such efforts could be broadened to also include measures aimed at improving an industry's competitiveness, such as deregulation and antitrust laws. Our results also suggest that researchers who want to study the effects of governance could benefit from interacting governance proxies with measures of competition. The empirical relationship might be stronger, both economically and statistically, for firms in non-competitive industries. To give an example, preliminary findings by the authors suggest that the positive alpha generated by the governance hedge portfolio in Gompers, Ishii, and Metrick (2003) comes entirely from non-competitive industries (Giroud and Mueller, 2008b). In contrast, the alpha in competitive industries is small and insignificant. Hence, good governance at the firm level is associated with higher equity prices in non-competitive industries, but not in competitive industries. While these preliminary findings are encouraging, more research is needed before we can conclude that firm-level governance instruments are moot in competitive industries.

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