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Is there discretion in wage setting? a test using takeover legislation

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Anecdotal evidence suggests that uncontrolled managers let wages rise above competitive levels. To test this belief, we examine the wage impact of antitakeover legislation passed throughout the 1980s in many states. Since many view hostile takeovers as an important disciplining device, these laws, by reducing takeover threats, potentially raised managerial discretion. If uncontrolled managers pay higher wages, we expect wages to rise following these laws. Using firm-level data, we find that these laws raised annual wages by 1% to 2%, or about $500 per year. The findings are robust to a battery of specification checks and do not appear to be contaminated by the political economy of the laws or other sources of bias. These results challenge standard theories of wage determination that ignore the role of managerial preferences.

1. Introduction

■ Theorists have long recognized that the separation of ownership and control warps managerial incentives: managers neither bear the full costs of their actions nor reap the full benefits of their efforts. Costly discretion is the result, in which managers' personal concerns interfere with decision making. Although there have been studies of the effects of managerial discretion on financing and product market decisions, its effects on labor market decisions are not well explored. Many anecdotes and newspaper articles hint that poorly controlled managers pay higher wages. When under pressure, bloated corporations trim fat by slashing wages and eliminating jobs. Successful corporate raiders often capture premiums by staring down unions into wage concessions. Even in non-unionized firms, one often hears of takeovers being followed by drastic cuts in wages

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and benefits. These stories depict a world in which workers' wages are influenced by managers' discretion. Of course, we are not arguing that *owners* do not maximize profits, only that managers may not. Even this argument is at odds with existing models of wage determination, including rent sharing and efficiency wage models, because these models require that wages be set in a profit-maximizing manner. In this article, we empirically investigate whether increased managerial discretion raises worker wages.

Since wages are easily observed, what is the source of managerial discretion in setting wages? Certainly, owners know what wages are being paid to workers. On the other hand, they probably do not know what wages should be paid. While they may observe general labor supply conditions, they may not observe many of the firm-specific supply conditions. They will not have the detailed knowledge—such as quality of applicants or ease of filling positions—needed to infer the optimal wage in specific occupation-experience categories. This asymmetry of information means that owners find it difficult to judge whether wages are too high or too low. A moral hazard problem arises and equilibrium wages are distorted.

Managerial preferences determine the direction of this distortion. There are many reasons to believe that managers act as if they "prefer" high wages. First, they might care *more* about having high-quality workers and low turnover than owners do. Second, in a union context, they might dislike putting forth bargaining effort. Finally, they might care more than owners about improving workplace relations, as they are the ones who endure the workers' complaints and enjoy the workers' company. Foulkes (1980) and Milkovich and Newman (1987) provide evidence consistent with a managerial preference for higher wages. They document that managers care about their relative position in the wage distribution and invest a considerable amount of resources in conducting complex surveys to learn about competitors' wages. A conventional wage policy consists of "leading" competitors' wages. When asked why they want to lead the wage distribution, managers sometimes mention the ability to retain workers, the ability to select from a larger applicant pool, or the desire to pay fair wages. Yet, managers very often seem to show an extreme determination to lead the wage distribution without any clear reason provided except for the desire to "be there." While evocative, such evidence can be interpreted in other ways—perhaps managers don't always know why something maximizes profits, they just know that it does—and, hence, motivates formal empirical work.

Testing the hypothesis that managerial discretion raises wages requires independent variation in the extent of the agency problem, which has traditionally been difficult to find. State antitakeover legislation potentially provides the needed variation. Takeovers are traditionally viewed as a mechanism for disciplining wayward managers (Manne, 1965, and Jensen, 1984, 1986, and 1988). Managers recognize that if they fail to maximize profits, they endanger themselves by risking takeover and subsequent job loss. By raising the cost of takeovers, antitakeover laws insulate managers from takeover pressures and potentially dull their incentives to maximize profits.[1]

These laws directly influence moral hazard and, therefore, may better proxy for managerial discretion than measures such as firm size or earnings. If managerial discretion influences wages, we expect affected firms' wages to rise with the passage of antitakeover laws.[2]

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[1] In an optimal contracting model, shareholders might offset *some* of these effects by increasing managerial incentives. Bertrand and Mullainathan (1998) indeed find that these laws raised pay for performance for CEOs. Of course, these increases would not completely offset the laws' distortions. What remains is the source of our variation.

[2] Examining the impact on employment levels would also be interesting. Unfortunately, our data are too noisy to allow this.

Using a differences-in-differences methodology, we find evidence consistent with this hypothesis. Our estimates imply that state antitakeover laws raised average annual wages by 1% to 2% or about $500 per year in affected firms. We include firm fixed-effects and year dummies, control for observable firm characteristics, allow the impact of these characteristics to vary from year to year, and control for preexisting wage trends specific to legislating states. We further investigate the dynamics of the effect, to see whether a law's "effect" appears before the law itself does. Our results are surprisingly robust to all these specifications checks.

Is there an alternative interpretation to our findings? First, compensating differentials can easily be ruled out because they predict that the antitakeover laws should decrease wages, not increase them. Decreased takeover fears increase job security and should lower wages. Second, one might worry that our findings are driven by CEOs increasing their own pay when given more discretion. However, the quantitative size of our estimates combined with the size of the firms in our sample make it very unlikely that the wage increases are restricted only to CEOs. While we can rule out pay increases to executives as the driving force, our results do not discriminate between pay increases to white collar versus blue collar workers, for example. Finally, in Section 7, we argue that political economy or survivorship biases probably do not contaminate our results.

The findings in this article are consistent with a much older institutional literature in labor economics. Both Lester (1952) and Reynolds (1951) argued that wages exhibit a "range of indeterminateness." Based on many plant visits, they concluded that rather than following the simple marginal rule implied by profit maximization, managers choose from a range or band of feasible wage levels. One of the factors that Lester and Reynolds cited as generating this range of indeterminateness was managerial characteristics. They already acknowledged that owners and managers might have different goals and that the classical model can only explain a "portion of reality." To date, support for the range-of-indeterminateness model has been limited to the disputed findings on interindustry wage differentials (Slichter, 1950; Dickens and Katz, 1987; and Krueger and Summers, 1988), the firm size effect (Brown and Medoff, 1989), and more generally firm fixed effects in wages (Abowd and Kramarz, 1997). Our test isolates a specific mechanism—takeover pressure—which determines a firm's position in the range of feasible wage rates.

The rest of the article is organized as follows. Section 2 briefly discusses related empirical research. Section 3 presents the state antitakeover laws. Section 4 describes the data. Section 5 explains the empirical methodology. We present our results in Section 6. In Section 7 we discuss alternative interpretations of our results and also describe the channels through which managerial discretion can increase workers' wages. Finally, we conclude in Section 8.

2. Related empirical work

■ Our work is close in spirit to the expense preference literature. Inspired by Williamson (1964), articles such as Edwards (1977) and Hannan and Mavinga (1980) have tested the idea that a more competitive environment forces management to cut frivolous expenditures. These articles typically study the banking industry, and they generally find that increased product market concentration raises expenses on office space and increases employment levels. They argue that since competition reduces agency problems, this is evidence for increased discretion raising expenses.[3] Heywood (1991) provides an alternative test of the

[3] Hannan and Mavinga (1980) go further by looking at the interaction between concentration and ownership. They argue that managerial discretion requires both the absence of a strong owner and a lack of product market discipline.

expense preference theory relying on international trade. Since imports affect product market concentration, Heywood uses import penetration as a proxy for product market structure. He concentrates on the impact of import penetration on wages and finds that higher import penetration lowers wages. These articles all proxy for managerial discretion with competition, which raises two concerns. First, theory is unclear about the link between product market competition and discretion. Hart (1983) presents a model where competition lowers discretion, and Scharfstein (1988) presents one where competition raises discretion. Second, competition can independently affect expenses by acting through channels other than discretion. For example, Heywood (1991) himself notes that his results are also consistent with decreased union bargaining power or factor price equalization. In the banking context, increased concentration may force banks to compete on quality dimensions rather than on price dimensions (e.g., fees). Therefore, increased expenses may merely reflect this increased profit-maximizing quality competition.

An independent literature has studied the effects of ownership changes. Brown and Medoff (1988) use unemployment insurance data and find mixed effects of ownership changes on employment and wages. Using the Longitudinal Research Database, Lichtenberg (1992) finds that ownership changes affect white collar wages. Rosett (1990) demonstrates that union employees suffer wage losses following takeovers, though the results are not statistically significant. Gokhale, Groshen, and Neumark (1995) find that takeovers flatten the wage-seniority profile and reduce employment of senior workers. While interesting in its own right, this literature has not been able to identify the impact of managerial discretion on wages.[4] Since the firms under study actually change ownership, an endogeneity bias arises that makes the results hard to interpret. Changes in ownership are conditioned on past and future expected firm performance, all of which may influence wages even in the absence of managerial discretion. Similarly, a host of other factors such as quality of management and operating practices may change with ownership, which again may influence the optimal wage.

Krueger (1991) investigates the effect of ownership structure on wages by looking at the difference between franchises and company-owned stores in the fast food industry. He finds that company-owned stores pay higher wages and have steeper wage profiles than franchises do. Since company-owned stores have more difficulties with the separation of ownership and control, this finding can be interpreted as evidence for discretion in wage setting. A complementary interpretation, preferred by Krueger, is efficiency wages: owner-operated stores may have a harder time monitoring workers than franchises do. His data do not allow one to distinguish between these interpretations.

In summary, the literature provides only ambiguous evidence about the effects of managerial discretion on wages because it relies on indirect sources of variation in discretion, a point many of the authors recognize.[5] By explicitly varying the parameters of the agency relationship, antitakeover legislation potentially provides a better estimate of the impact of discretion on wages.

3. State antitakeover laws

■ **Description.** Serious regulation of takeover activity in the United States begins with the Williams Act, a federal statute passed in 1968. The Williams Act provided

[4] A useful summary of this literature (along with effects of ownership changes on other variables) can be found in Bhagat, Shleifer, and Vishny (1990).

[5] Krueger's variation is perhaps the most direct. However, the allocation of company stores between owner-operated and franchises may itself be endogenous to the characteristics of the labor market. For example, a company may prefer to start franchises in areas where the quality of the labor pool is on average poorer and requires more monitoring.

for detailed disclosure requirements, an antifraud system, and other measures to protect target shareholders during the tender offer process. Individual states greatly extended the Williams Act by passing their own statutes in the 1970s. These are known as the "first generation" of state antitakeover laws. The first-generation laws were deemed unconstitutional by the Supreme Court in 1982 (*Edgar* v. *Mite Corp.*) primarily because they applied to corporations not chartered in the state. States were attempting to regulate beyond their jurisdictional reach and to regulate interstate commerce, both of which constituted constitutional violations. In response to this decision, states hesitantly began a second wave of antitakeover statutes that dealt with some of the issues raised by the Court. To the surprise of many, these statutes were declared constitutional by the Supreme Court in 1987 (*CTS* v. *Dynamics Corp.*).[6] This decision triggered a third generation of even more stringent state laws regulating takeovers.

The second- and third-generation statutes are of three general types: control share acquisition (CSA), fair price (FP), and business combination (BC).[7] In this article, we will focus mainly on BC laws because they induced the largest change in the incentive structure faced by management. BCs impose a moratorium (three to five years) on specified transactions between the target and a raider holding a specified threshold percentage of stock, unless the *board* votes otherwise. Specified transactions include sale of assets, mergers, and business relationships between raider and target. Since these transactions are essential to financing highly leveraged takeovers, which rely on sales of the target's assets, BC laws give the board the power to block such takeovers. But since management often has a great deal of control over the board, incumbent management then has increased power to block such takeovers. Therefore, BC laws seem to entrench management by allowing it the right to "veto" a hostile takeover.[8]

The legal rulings also generally reflect the idea that BC laws tip the balance of power toward management. In *Amanda Acquisition Corp.* v. *Universal Food Corp.*, a landmark case on BC legislation, the court ruled that BC laws did indeed violate management-shareholder neutrality, favoring management. But the ruling went on to state that this violation was not grounds for overturning the law. As another example, Justice Schwartz, deciding on the Delaware BC law, concluded that it altered the balance of power between management and raider, "perhaps significantly." (See Sroufe and Gelband (1990).) As one commentator noted, an implication of the Wisconsin decision was that "[t]he Seventh Circuit's Amanda opinion asserts that a law, such as Wisconsin's business combination statute, can be both economic folly and constitutional" (*New York Law Journal*, September 14, 1989). Our focus will be on the shift in power toward management that these laws caused. They give management the power (through the board) to impede takeovers, generating for us variation in the amount of managerial discretion.[9]

□ **Evidence on impact of laws.** Anecdotal evidence on the importance of the state antitakeover laws is plentiful. A mass of cases often followed each law, in which raiders

[6] The second-generation laws were deemed constitutional primarily because they restricted the jurisdiction of the laws to only firms incorporated in that state. With this precedent in place, challenges to third-generation laws never reached the Supreme Court, even though these laws were much more stringent in practice.

[7] Less common types of statutes were passed by a few states, but we do not consider them here.

[8] Two other types of laws are CSAs, which require other shareholders to approve the voting rights of a large shareholder, and FPs, which require paying a "fair price" for all shares acquired by a raider. We decided not to concentrate on these laws because previous empirical evidence showed that they were relatively weak in practice (see Section 3). Appendix Table A2 confirms this for our test as well.

[9] Some of the laws also had other minor clauses, of which the most relevant deal with labor contracts. These specify that raiders who acquire control of a corporation should honor preexisting collective bargaining contracts. Though the effect will be minor, if anything the increased security provided by these laws should decrease wages as a compensating differential, generating a downward bias on our estimates.

attempted to argue against the law.[10] This indicates that target companies understood the laws well enough to use them as defenses and that raiders believed the laws to be a large-enough deterrent to success to challenge them in court. Moreover, these laws received extensive coverage by both the popular press and legal practitioners.

Empirical work on the laws typically falls under two categories: studies of their impact on takeovers, and studies of their impact on stock prices.[11] Hackl and Testani (1988) perform a straightforward differences-in-differences analysis for laws up to 1988 and find that these laws lessen takeover activity. States passing laws experienced approximately a 48% smaller rise in takeover attempts in this period. They also find that the proportion of takeover attempts using tender offers went down, as well as the number of tender offer attempts that were successful.

Several articles have attempted to establish the effect of these laws on stock prices, and almost all of them focus on a single law (Karpoff and Malatesta, 1989; Pound, 1987; Szewczyk and Tsetsekos, 1992; Romano, 1987; Margotta, McWilliams, and McWilliams 1990; Schumann, 1988; and Block, Barton, and Roth, 1986). By and large, these studies tend to find negative effects of the laws on share prices, though some are insignificant or zero.[12] The most relevant study for our purposes is by Karpoff and Malatesta (1989), who examine stock price reactions to all laws passed before 1987 rather than examining an individual law. Their study is useful because they comprehensively analyze each type of law. They find significant negative reactions to the passage of BCs, resulting in a loss of value of approximately .467%. They find less negative (−.274%) and insignificant responses to FPs. Finally, they find no reaction to the adoption of CSAs. Their results conform well to *a priori* reasoning and further support our focus on BC laws.

4. Data

■ To determine which firms are affected by antitakeover legislation, we must know each firm's state of incorporation. We also need to focus on wages in large, publicly traded firms, since these are the ones that realistically face takeover threat. Therefore, we use COMPUSTAT data, one of the few data sources that include both labor data and corporate variables such as state of incorporation.[13] COMPUSTAT reports financial variables for more than 7,500 individual corporations established in the United States (and territories) since 1976. The data are drawn from annual reports, 10-K filings, and 10-Q filings, and the dataset samples large companies with substantial public ownership. We use all data available between 1976 and 1995. Since the laws were passed in the middle and late 1980s, this gives us several years before and after the laws.

Ideally, we would like state of incorporation some time before the laws' passage, but unfortunately the COMPUSTAT tapes only have state of incorporation in 1995 or

[10] New Jersey's law, for example, was tried in *Bilzerian Partners, Ltd.* v. *Singer Co.*, No. 87–4363 (D.N.J. Dec. 2, 1987). Delaware's law was immediately challenged in *Black & Decker Corp.* v. *American Standard Inc.*, 679 F. Supp. 422 (D.Del. 1988) and *CRTF Corp.* v. *Federated Dept. Stores, Inc.*, 683 F. Supp. 422 (S.D.N.Y. 1988). These are only a few of the many cases revolving around these laws. Courts consistently found the laws applicable. See Matheson and Olson (1991) for more details.

[11] Garvey and Hanka (1999) study the impact of these laws on a different variable: firm leverage. They find that affected firms decrease their leverage.

[12] The main trouble comes from choosing an event date, since information about these laws may be incorporated into prices well before formal passage. For us, the problem of choosing a treatment date is less problematic since wages are reported and decided upon annually. Easterbrook and Fischel (1991) summarize the literature on the individual laws and contend that these laws had, roughly, −.5% effect on prices.

[13] These considerations effectively rule out large individual-level datasets such as the Current Population Survey because measuring the "affected" group can be difficult. We would know only state of residence, not the state of incorporation of the employer.

the state of incorporation in the final year if the firm died before then. Company representatives told us that they do not archive the old information. Existing evidence, however, indicates that changes in state of incorporation are very rare, especially in the later years that dominate our sample (Peterson, 1988, and Romano, 1985). To further verify this, we randomly sampled 75 firms from our sample and checked, using Moody's Industrial Manual, whether they'd changed states of incorporation in our sample period. Consistent with previous evidence, we found only three changes in state of incorporation and these were all to Delaware (two in 1985 and one in 1986), predating the 1988 law. This suggests that using the state of incorporation in 1995 will not bias our results.

The labor data provided by COMPUSTAT are aggregate, with no direct wage measure reported. Instead, we compute wages using the labor expenses and employment data. Labor expenses include salaries, wages, pension costs, profit sharing, incentive compensation, payroll taxes, and other employee benefits; they exclude commissions. Employment is defined as the number of company workers as reported to shareholders in annual reports. It is reported by some firms as an average number of employees over the year and by others as the number of employees at the end of the year; it includes part-time and seasonal employees; it excludes contract workers, consultants, and employees of unconsolidated subsidiaries.[14] We build the wage measure by dividing labor expenses by reported employment.

The resulting labor data are extremely spotty both across firms and time. Many firms report no labor data, and some report it only intermittently. More important, there are large outliers when one computes firm-by-firm annual growth rates of wages.[15] Although our results are unaffected by the inclusion of these outliers, we are uncomfortable using these data points. Hence, we exclude from most of our regressions firms that at any point display aberrant wage changes. Specifically, we drop any firm i for which there exists a period t during which the ratio w_{t+1}/w_t is greater than $7/4$ or less than $4/7$, where these numbers were chosen for simplicity: they represent 75% changes and the inverse. Practically, this results in excluding firms for which at any point in time the wage growth rate is unreasonably positive (more than 75%) or unreasonably negative (less than −44%). This exclusion rule leads to a loss of 547 firm-year observations. Our results were insensitive to the use of this specific exclusion rule. In a subsequent table we shall show the results for 50% growth and its inverse. We get similar estimates even if we keep these outliers. An alternative would be to estimate a median regression, but this is computationally intractable because firm fixed effects produce a large number of independent variables. We shall show, however, median regressions without firm fixed effects, where we see qualitatively similar results.

Our final sample consists of 877 firms over the period 1976–1995 and of 9,305 firm-year cells. It consists of *all* the firms ever in existence during the sample period, for which we can compute wage data for every year they are in existence, and whose wage growth does not exhibit outliers.[16] It thus includes firms that disappeared from COMPUSTAT before 1995 as well as firms that appeared in COMPUSTAT after 1976. Over this period there are 49,474 firm-year cells for firms that report state of incorporation and are incorporated in the United States. Our data, therefore, represent 18.8% of this full sample, with the dissipation coming because many firms do not report labor

[14] This is the reason we do not use employment as a dependent variable. Preliminary regressions using it verified this: our point estimates had quite large standard errors. While the employment effect of takeover legislation is extremely interesting, we are not able to examine it using this data.

[15] While the mean growth rate of wages is 2% and the median growth rate is 1%, the bottom one percentile of the distribution has wage growth rates below −30% and the top one percentile has wage growth rates above 46%.

[16] The restriction on having continuous wage data is so that we can identify wage growth outliers.

TABLE 1 **Summary Statistics**

	FULL	SAMPLE	BC	No BC
Log wage	—	3.293	3.312	3.254
		(.429)	(.465)	(.344)
		[29.2]	[30.2]	[27.4]
Log employment	.760	1.681	1.308	1.307
	(2.006)	(1.703)	(1.690)	(1.702)
		[18.6]	[20.0]	[16.02]
Log assets	5.692	7.344	7.554	6.936
	(2.057)	(1.814)	(1.787)	(1.798)
		[6,170]	[7,193]	[4,189]
Log market value	4.816	5.980	6.159	5.630
	(1.904)	(1.693)	(1.651)	(1.721)
		[1,489]	[1,520]	[1,430]
Log sales	5.313	6.420	6.546	6.174
	(2.041)	(1.686)	(1.668)	(1.692)
		[2,474]	[2,570]	[2,286]
Sample size	49,474	9,305	6,139	3,166

Notes: FULL contains all U.S. firms in COMPUSTAT with nonmissing state or country of incorporation and incorporated in the United States. SAMPLE represents our sample. BC and No BC restrict the sample to firms that have and don't have BC laws, respectively. Standard deviations are in parentheses. The means of the variables in levels are listed in brackets.

data. Our sample is a larger proportion of firm-year observations than of firms because larger (and hence longer-lasting) firms are more likely to report labor expenses.

□ **Summary statistics.** Table 1 presents means of the variables of interest. In columns 1 and 2, we compare the full COMPUSTAT sample to our sample. Columns 3 and 4 compare, within our sample, treatment group (firms incorporated in states that pass BC laws at some point) and control group (firms incorporated in states that never pass BC laws). While we present the means of the log variables, the means of the levels are presented in brackets for our sample. Wages are expressed in thousands of dollars, and employment in thousands of employees. Assets is total assets (current assets plus net property, plant, and equipment plus other noncurrent assets) in thousands of dollars. Sales is net sales (gross sales reduced by cash discounts, trade discounts, returned sales, and allowances for which credit is given to customers) in thousands of dollars. Market value is the end-of-year stock market value of the firm in thousands of dollars. Wages, assets, sales, and market value are deflated using the CPI (1983–1984 = 100).

As one can see by comparing columns 1 and 2, the firms in our sample are significantly larger on average than the firms in the full sample.[17] The next two columns allow us to compare firms incorporated in states passing BC legislation and those in states not passing BC legislation. First, one will note that many of the firms in our sample are located in states passing BC laws. However, as we explain more carefully in Section 5, this does not cause our effective control group to be small. Since the states staggered their passage of laws, the effective control group for any given year

[17] Virtually all of this difference arises from dropping firms with no wage data rather than dropping firms with aberrant wage data. This is to be expected, since the aberrant wage changes result in a loss of only 500 or so firm-year observations.

is the set of states not passing laws that year. Second, firms passing BC legislation appear slightly larger on all dimensions. Because we use firm fixed effects, our empirical methodology allows us to deal with any fixed difference between treatment and control firms. Other problems potentially arise from time-varying differences between treatment and control firms that are correlated with the passage of the laws. We deal with this problem by allowing the effect of observables—assets, sales, employment, market value—to vary with state of time in Section 6.

5. Empirical methodology

■ At its heart, our test uses differences-in-differences, comparing states that pass antitakeover legislation (referred to as treatment states)) to states not passing such laws (control states) before and after the law. In the first level of differences, we subtract wages w after the law from wages before the law, giving us two sets of differences: $\Delta^T w$ for the treatment group and $\Delta^C w$ for the control group. By itself, $\Delta^T w$ could be a misleading estimator of the laws' impact, since other changes contemporaneous with the laws affect this estimate. To deal with this, we introduce a second level of differences. If contemporaneous shocks affect treatment and control groups in roughly similar ways, then those shocks should also be contained in $\Delta^C w$. One can therefore subtract $\Delta^C w$ from the first difference $\Delta^T w$ to estimate the effect of the law.

This approach can be easily understood with an example. Suppose we wish to estimate the effect of the Pennsylvania law passed in 1989. We would subtract wages after 1989 from wages before 1989 for the Pennsylvania firms. However, other things in 1989, such as a recession, may have affected Pennsylvania firms. Choosing a control state, for example New Jersey, would help control for changing economic conditions. If New Jersey firms were also subject to this recession, the change in their wages would be a measure of its severity. We would, therefore, compare the difference in wages in Pennsylvania before and after 1989 to the difference in wages in New Jersey before and after 1989. The difference of those two differences would serve as the estimate of the law's effect in Pennsylvania.

We present a rough cut of the data using this approach in Table 2. The main problem with such an approach is that "after the law" is not well defined, since states passed the law at different times. Therefore we define "After" to be after the law for passing states, but after 1985 for states that aren't passing. 1985 was chosen because the first laws were passed then. This simple differences-in-differences produces a 3%

TABLE 2 **Differences-in-Differences Estimate of the Effect of BC Legislation on Wages**
Dependent Variable: Log Deflated Wage

	Before 1985	After 1985	$\Delta\log(w)$
Non-BC states	3.24	3.29	.049
	(.009)	(.0161)	(.018)
BC states	3.28	3.36	.08
	(.007)	(.009)	(.011)
$\Delta\log(w)$	.042	.073	**.031**
	(.011)	(.018)	**(.022)**

Notes: "Before" indicates observation before the law for BC states and before 1985 for non-BC states. "After" indicates after the law for BC states and 1986 and later for non-BC states. Sample includes all firms with contiguous wage data (sample size = 9,852). Standard errors are in parentheses.

increase in wages. We see that BC states increased wages over the sample period much faster than non-BC states did. Because of the staggered nature of the laws, however, this is an imperfect technique.

In practice, therefore, we implement this approach in a regression framework, which has three advantages. First, we can include firm fixed effects, which allows for more precise controls than simply differencing across states and time. Second, as stated, many laws were passed and at different times. By defining an "After Law" dummy firm by firm, we can easily allow for the staggering of laws. Finally, we can control for time-varying observables of the firm. Let i index firms and t years, w_{it} be the log wage, X_{it} be firm characteristics such as size, BC_i be a dummy indicating that firm i is incorporated a state passing a BC law, and $After_{it}$ be a dummy variable for after the law ($After_{it}$ equals one if the law has been passed by time t), which gives a one-year delay in the law's effect. This allows for lags in implementation as well as the fact that the data may reflect values from the previous calendar year. We estimate

$$w_{it} = \alpha_t + \beta_i + \gamma X_{it} + \delta BC_i{*}After_{it} + \epsilon_{it}. \quad (1)$$

Our estimate of the law's effect is δ, the coefficient on the interaction term: change in wages, specific to firms incorporated in a state that coincide with the passage of legislation. One important implication of staggered passage dates is that we no longer need our control group to be states that do not pass laws. The above specification can be estimated even if all states eventually passed a law. It implicitly takes all firms incorporated in states not passing a law at time t as the control group for a law passed at time t, even if they have already passed one or will pass one later. The necessity of this can be seen in Table A1, which lists states and year of enactment for the laws. Even though only 28 states pass a law in our sample period, almost all of the large states eventually pass a BC law.[18]

There are two sources of variation we do *not* use. First, a few of these laws had opt-out provisions so that firms could, if they chose, make themselves exempt from the laws. However, since opting out is endogenous—because managers play a crucial role in deciding to opt-out, firms that do opt-out will have levels of discretion different from those that do not—using this information will corrupt estimates. We are, therefore, estimating the gross effect of having a BC law with an opt-out provision rather than the effect of having a BC law. These opt-out provisions do not change the qualitative implications of managerial discretion on wages, though they may lessen the magnitude of effect. A second, related, source of variation comes from firm changes of antitakeover provisions in response to the law. Again, because of its endogeneity, we do not exploit this variation.

6. Results

■ **Basic results.** Table 3 presents various estimates of equation (1). Each specification contains year dummies and firm fixed effects. In columns 2, 4, 6, and 8, we further control for log assets, log employment, log sales, and log market value. Columns 1 and 2 use the entire set of firm-year cells for which we were able to compute a wage measure. In columns 3 to 6, we exclude *firms* whose wages grow too much or drop too much at any point in time. In columns 3 and 4, we use growth rates of 75% and its inverse as the cutoff: $7/4$ and $4/7$. Columns 5 and 6 use 50% and its inverse: $3/2$ and $2/3$. Finally, columns 7 and 8 use the full sample but estimate a median regression. However,

[18] Texas passed a law in 1997 after our article was written.

since median regression with firm fixed effects is computationally intractable for us, we use state of incorporation fixed effects instead.

Before discussing the effect of the BC laws, let us first briefly review the other determinants of log wage. While log assets and log sales are positively correlated with log wage, log market value does not appear to additionally influence wages. In all specifications, we find a strong negative sign on log employment. The coefficients on those four determinants of firm size are such that a doubling of the size of a firm leads to a 2% to 3% decrease in wages. This seems at odds with the standard firm size-wage effect where larger firms appear to pay higher wages. However, the strong negative correlation between wages and employment is very likely caused by measurement error. Since the wage measure is defined as labor expenses divided by employment, any measurement error in the employment variable will show up in the wage variable and will negatively bias the employment coefficient. In regressions that are not reported here, we have instrumented log employment with the lagged value of log employment in order to reduce measurement error. We have found that the employment coefficient stays negative but is about 50% to 60% smaller in absolute value. The firm size-wage effect implied by the instrumental variables estimation is such that a doubling of the size of a firm increases wages by about 1%. In the next table we also estimate the basic equation without employment as an independent variable.

In all the columns, we find a consistent 1% to 1.7% increase in wages due to the passage of the laws. The estimates become more precise as we exclude more of the aberrant wage data, although the mean effect does not change much. The addition of controls in columns 2, 4, and 6 does not qualitatively alter the estimated coefficient. Finally, the median regression produces an extremely noisy estimate, though essentially the same number. For the remainder of the article we shall restrict ourselves to the 75-46 rule, though the findings are not sensitive to this restriction.

Table A2 in the Appendix briefly examines the impact of the other two major antitakeover statutes on wages: FP and CSA. The results confirm our analysis in Section 3. Column 2 shows that FP statutes have a (small) positive but insignificant impact on wages. CSA statutes do not appear to have any impact on wages (column 3). In addition, the impact of the FP statute and CSA statute on wages is statistically and economically insignificant once we control for the impact of the BC statute (column 4). This reflects the fact that many states passing CSA or FP statutes later passed BC laws.

□ **Robustness checks.** While the basic regressions indicate that these laws affected wages, several concerns may remain. We deal with these in Table 4.

First, an important assumption of the differences-in-differences methodology is that shocks contemporaneous with the laws affect treatment and control groups similarly. This assumption can be problematic if the treatment and control groups are dissimilar on observable (and potentially unobservable) characteristics. As we have seen in Table 1, the set of firms incorporated in states passing BC laws is larger in every size dimension (market value, sales, assets, and, to a lesser extent, employment). Since states pass laws at different times, this is less of a problem. BC states themselves serve as controls for all states that pass laws at different times. Nevertheless, one worries that shocks contemporaneous with the law that differentially affect large firms may still corrupt our estimates.

Column 1 of Table 4 deals with this problem by allowing the returns to assets, employment, sales, and market value to change over time. That is, we allow a different coefficient on these covariates for each single year. Suppose that an aggregate shock raised wages in large firms. Because the treatment firms are larger, if this shock coincided with the passage of some of the laws, our estimate of the effect of the BC

TABLE 3 **Effects of BC Legislation on Wages**
Dependent Variable: Log Deflated Wage

	(1)	(2)	(3)
BC*$After_t^{BC}$	.010 (.007)	.017*** (.006)	.011** (.005)
Log assets	—	.013* (.008)	—
Log employment	—	−.394**** (.007)	—
Log sales	—	.350**** (.007)	—
Log market value	—	−.002 (.003)	—
Year dummies (YD)	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes
State of incorporation dummies	No	No	No
Adjusted R^2	.892	.931	.939
Sample Size	9,852	9,627	9,305

Notes: Sample in columns (1), (2), (7), and (8) contains all the firm-year observations for which wage is computable. Sample in columns (3) and (4) contains all the firms for which the wage growth rate is always below 75% and always above −44%. Sample in columns (5) and (6) contains all the firms for which the wage growth rate is always below 50% and always above −33%. Columns (7) and (8) reflect a median regression. Standard errors are in parentheses.
* Denotes significance at the 10% level; ** at the 5% level; *** at the 1% level; **** at the .1% level.

legislation would be biased. Part of the estimated treatment effect would indeed come from this shock. Allowing covariates to vary by time controls for any such shock. If shocks to any of these observables contemporaneous with the laws corrupt our results, we expect the coefficient on the treatment variable to drop significantly. Comparing column 1 in Table 4 to column 4 in Table 3, we see that our estimate of the effect of the BC laws remains virtually unchanged.

Second, the differences-in-differences estimate will also be corrupted if wages in the BC group were following a different trend than were wages in the non-BC group. This concern is again lessened by the staggering of laws. Nevertheless, we deal with this problem directly in column 2 by allowing for a different trend term for the BC and non-BC firms. As one can see, our results are not affected by this inclusion.[19] In column 3, we cumulate these related specification checks by including a treatment trend and allowing the returns to all covariates to vary over time. Again, the results stay the same.

Third, we might worry that changes in economic conditions of the states lead to passage of these laws. This would corrupt our estimates if these conditions also affected wages. In column 4, we include state unemployment as a regressor and again find no change in the point estimate.

Fourth, we might worry that having log employment as a regressor may bias many of the coefficients, since the left-hand-side variable is defined as log wage bill divided by log employment. Column 5 replicates the basic regression with log employment dropped, and again the coefficient is unchanged.

[19] The coefficient on the preexisting trend term for BC states is small and insignificant.

TABLE 3 *Extended*

(4)	(5)	(6)	(7)	(8)
.012***	.015***	.014***	.016	.011
(.005)	(.005)	(.005)	(.016)	(.012)
.013**	—	.020***	—	−.078****
(.007)		(.007)		(.003)
−.288****	—	−.273****	—	−.357****
(.007)		(.007)		(.005)
.254****	—	.228****	—	.443****
(.007)		(.007)		(.006)
−.003	—	.005*	—	.016****
(.003)		(.003)		(.004)
Yes	Yes	Yes	Yes	Yes
Yes	Yes	Yes	No	No
No	No	No	Yes	Yes
.952	.943	.954	.062	.258
9,108	8,988	8,803	9,852	9,627

Fifth, given the large number of firms incorporated in Delaware, one might worry that our results are driven by this one data point. In column 6, we replicate the basic regression but with Delaware firms excluded from the sample. The estimated treatment effect stays significant, and the point estimate is actually larger than in column 4 of Table 3. This result is in concordance with Romano's extensive analysis of state antitakeover laws. Romano (1993) claims that the BC statute in Delaware was relatively weak compared to other states.

TABLE 4 **Effects of BC Legislation: Robustness Checks**
Dependent Variable: Log Deflated Wage

	(1)	(2)	(3)	(4)	(5)	(6)
BC*$After_t^{BC}$	.013***	.015***	.015***	.013***	.013**	.019****
	(.006)	(.006)	(.005)	(.005)	(.005)	(.005)
Log assets*YD	Yes	No	Yes	No	No	No
Log employment*YD	Yes	No	Yes	No	No	No
Log sales*YD	Yes	No	Yes	No	No	No
Log market value*YD	Yes	No	Yes	No	No	No
BC*Year	No	Yes	Yes	No	No	No
State unemployment	No	No	No	Yes	No	No
Delaware firms	Yes	Yes	Yes	Yes	Yes	No
Adjusted R^2	.959	.952	.959	.953	.952	.959

Notes: Sample in columns (1) to (5) contains all the firms for which the wage growth rate is always below 75% and always above −44% (sample size = 9,108). Column (6) excludes the Delaware firms (sample size = 5,658). All regressions also include year fixed effects and firm fixed effects. See Table 3 for further notes.

□ **Distribution-of-treatment effect.** The results in the previous sections have established the mean effect of BC laws. One is also interested in the distribution of this effect. Table 5 addresses this issue by interacting the $BC_i{*}After_{it}$ term with firm-level characteristics. We also allow the coefficient on the relevant firm characteristic to vary by year and by BC and non-BC firms. To make the coefficient on the direct $BC_i{*}After_{it}$ term easier to interpret, we have demeaned each of the firm-level characteristics. Therefore, the coefficient on $BC_i{*}After_{it}$ represents the effect of BC laws for a firm of average size. As the results in Table 5 indicate, larger firms appear to have bigger wage increases after the passage of the antitakeover laws.

A priori reasoning does not give much assistance in determining whether wages in large firms should be more or less affected by antitakeover legislation than wages in small firms are. On the one hand, larger firms may have more dispersed ownership, making takeovers a more important disciplining device, so the effect on wages should be bigger. On the other hand, the takeover of larger firms may in general be harder since it requires more capital, so the effect on wages should be smaller. Our empirical findings support the first story.

TABLE 5 Effects of BC Legislation: Distribution of Effects
Dependent Variable: Log Deflated Wage

	(1)	(2)	(3)	(4)
BC*$After_t^{BC}$	.004 (.005)	.011** (.005)	.013** (.005)	.009* (.005)
Log assets*BC*$After_t^{BC}$	.009**** (.003)	—	—	—
Log employment*BC*$After_t^{BC}$	—	.004 (.003)	—	—
Log sales*BC*$After_t^{BC}$	—	—	.004 (.003)	—
Log market value*BC*$After_t^{BC}$	—	—	—	.006** (.003)
Log assets*BC	.017** (.007)	—	—	—
Log employment*BC	—	.048**** (.007)	—	—
Log sales*BC	—	—	.023*** (.007)	—
Log market value*BC	—	—	—	.012*** (.004)
Log assets*YD	Yes	No	No	No
Log employment*YD	No	Yes	No	No
Log sales*YD	No	No	Yes	No
Log market value*YD	No	No	No	Yes
Adjusted R^2	.954	.954	.953	.953

Notes: Sample in all columns contains all the firms for which the wage growth rate is always below 75% and always above −44% (sample size = 9,108). All regressions also include year fixed effects and firm fixed effects. See Table 3 for further notes.

Table 6 Effects of BC Legislation: Dynamics
Dependent Variable: Log Deflated Wage

	(1)	(2)	(3)
BC*$Before^{-1}$	—	−.002 (.006)	.003 (.006)
BC*$Before^{0}$	—	.007 (.006)	−.005 (.006)
BC*$After^{1}$	.003 (.006)	—	.003 (.006)
BC*$After^{2}$	.006 (.006)	—	.006 (.007)
BC*$After^{>2}$	.014** (.006)	—	.014** (.006)
BC*$After_t^{BC}$	—	.014*** (.006)	—
Adjusted R^2	.952	.952	.952

Notes: $Before^{-1}$ is a dummy that equals one for the year before the law passed; $Before^{0}$ is a dummy that equals one for the year the law passed; $After^{1}$ is a dummy that equals one for the year after the law passed; $After^{2}$ is a dummy that equals one for two years after the law passed; $After^{>2}$ is a dummy that equals one for strictly more than two years after the law passed. Sample in all columns contains all the firms for which the wage growth rate is always below 75% and always above −44% (sample size = 9,108). All regressions include year and firm fixed effects. See Table 3 for further notes.

□ **Treatment dynamics.** In the previous section we investigated how the effect of the passage of the BC laws was distributed across firms. In Table 6 we investigate how the effect is distributed over time. We estimate the following regression:

$$\log(w_{it}) = \alpha_t + \beta_i + \gamma X_{it} + \delta BC_i{*}After_{it} + \delta_0 BC_i{*}Before_{it}^{0} + \delta_{-1} BC_i{*}Before_{it}^{-1} + \epsilon_{it},$$

which includes two $Before_{it}$ terms to capture leads. $Before_{it}^{0}$ is a dummy for the year the law passed and $Before_{it}^{-1}$ is a dummy for the year before the law passed. In Table 6 we also break apart the $After_{it}$ dummy into dynamic terms, where $After_{it}^{s}$ refers to a dummy for s years after the law and $After_{it}^{>s}$ refers to a dummy that is one if the law was passed strictly more than s years ago.

In column 1, we investigate the effects of the law one year after, two years after, and more than two years after it was passed. We find that most of the law's effects are in the third and later years. In column 2, we look for evidence of an effect in the year the law was passed and in the year before it was passed. This serves as a useful consistency check. It is comforting that we find no significant effect before or contemporaneous with the law's passage. Section 7 further argues that this is evidence against endogeneity of the laws. Finally, in column 3, we estimate the full regression allowing for effects before and year by year after. The results are consistent with our findings in columns 1 and 2.

7. Interpretation

■ What can explain our findings? We can easily dismiss a compensating differentials interpretation. At best, takeovers reduce layoffs, but by reducing the fear of job loss, compensating differentials should rise.[20] The theory of compensating differentials, therefore, predicts that antitakeover statutes should, if anything, increase wages.

A second worry comes from concerns about the endogeneity of the takeover laws themselves. In practice they might be the result of changing economic conditions, and such changes may be correlated with workers' wages. For example, if states with rising wages (or those expecting wages to rise) passed antitakeover legislation, then the estimated effect will be biased upward. The evidence already presented suggests that this is not the case. First, column 4 of Table 4 shows that including controls for state economic conditions does not affect the estimates. Second, if underlying trends in wages give rise to these laws, we expect our estimates to drop when we explicitly allow for such trends in legislating states. As we saw in columns 5 and 6 of Table 4, the estimated treatment effect hardly changes when we include a treatment trend. Second, if short-term changes in economic conditions give rise to the antitakeover laws, we might expect to find some "effect" of the laws prior to passage. In columns 2 and 3 of Table 6, we included lead dummies for the year the law passed and the year before the law passed. Neither of them was ever significant. These three findings suggest that our results are not driven by political economy biases.

A similar but alternative explanation for our results can come from a survivorship bias induced by the raw effect of the laws on the number of takeovers. Suppose takeovers are more likely to occur in high-wage firms. This will not be a problem because we include firm fixed effects that will capture higher wages of taken over firms. For takeovers to drive our findings, they must be more likely in high-wage *growth* firms. Then these laws reduce takeovers and therefore increase the relative number of high-wage growth firms. Therefore, mean wage rises not through the diminished *threat* but through the reduction of the actual number of takeovers. Thus our results might be driven by a reduction in the actual probability of takeover after the passage of the laws. We believe that this explanation is not very relevant because actual takeovers are very rare events. Even if these laws completely eliminated takeovers, wage growth in firms taken over would have to be significantly higher than average wage growth in order to explain the 1–2% wage effect.

We also feel confident in ruling out an efficiency wage interpretation of our result. Under an efficiency wage story, the increase in workers' wages could be regarded as owners' optimal response to a decrease in the level of monitoring by managers after the antitakeover laws. While theoretically possible, this story is totally at odds with the institutional details of human resource practices in large firms. It requires that owners (shareholders) control worker wages, a practice unheard of in large public corporations (Foulkes, 1980, and Milkovich and Newman, 1987). One may wonder why the question of discretion in wage setting is interesting if it is clear that managers have complete control over the wage-setting process. The key empirical question in this article is whether managers exercise this control in a profit-maximizing way or in a self-serving way. Even when managers have complete control, forces (such as incentive schemes) may make them behave for all practical purposes in a profit-maximizing way. Thus, the question of whether discretion distorts wages is an empirical one.

At this point, we think we can reasonably assert that managerial discretion is most likely responsible for the wage increases. However, discretion may operate through several different channels. First, our results give little guidance as to which employees in the firm benefit from the wage raise. One possibility is that all workers get the raise equally. Another is that some subgroups, for example white collar workers, receive a disproportionate share. Such an interpretation would be supported by Lichtenberg's (1992) finding of larger impacts of ownership changes on auxiliary (as opposed to

[20] See Lichtenberg (1992) for evidence on the employment effect of takeovers.

manufacturing) branches of the firm. A simple calculation suggests, though, that these gains could not have been isolated to the top most layer of management alone. The mean wage in our sample is about $30,000 and the mean employment is about 19,000. A (lower bound) 1% pay raise represents $300 per worker or $5,700,000 per year for the mean firm. Using summary statistics in Bertrand and Mullainathan (1997), we see that the average firm CEO earns approximately $1 million. Even if the top five managers together earned $5 million, a 1% (average wage) rise would require approximately a 100% pay rise for top management. In fact, Bertrand and Mullainathan (1997) find that BC laws cause only a 5% increase in the pay of CEOs.

A second dimension where we are unsure of how discretion operates lies in whether or not the pay increases represent pure rent transfers. One way they may not represent pure rent transfers is if the higher wages represent an increase in worker quality. Managers may choose an excessive level of quality, for example to minimize their private cost of providing training. Another possibility, as would happen in gift-exchange models, is that workers increase unobserved effort in response to the higher wages.

Shleifer and Summers (1988) provide another model in which the wage increases will be accompanied by profit increases. In their model, workers make noncontractible investments in firm-specific human capital and managers sign implicit contracts to compensate workers for these investments. The quasi-rents generated by the investments create a time-consistency problem. Since these investments are sunk, wages can be cut *ex post* and workers never remunerated for their investment. They argue that incumbent managers are more likely to honor implicit contracts to repay workers than outsiders or owners are. This assumption is a dynamic equivalent of our (static) assumption that managers prefer to pay higher wages. Thus, in their theory, managerial discretion raises worker wages because managers have a preference for honoring the existing implicit contracts. This preference then induces human capital investments that raise wages. In this case, managerial discretion enhances shareholder wealth because it solves the time-inconsistency problem.

Thus, in a pure rent-seeking model, profits go down dollar for dollar with the wage bill. In these other models, profits decrease only a fraction (or possibly increase) with each extra dollar. We can attempt to discriminate between the pure rent story and these other stories by comparing the quantitative impact of the laws on shareholder value with the quantitative impact on wages. The average stock price reaction to these laws (about −.5%, according to Karpoff and Malatesta, 1989) does not appear large enough to explain a 1% to 2% pure increase in labor cost. Assuming that labor costs are about four times profits, a permanent 1% to 2% increase in wages will imply a 4% to 8% drop in profits, which in turn implies a 4% to 8% drop in firm value. One would have to believe a huge impact of these laws to believe that these wages represent pure rent transfers. Recall that Karpoff and Malatesta (1989) found only a −.5% drop. This suggests that while increased managerial discretion must have caused some rent dissipation, the higher wages it caused also probably increased the worker quality pool, induced higher effort, deterred breach of trust, or in some other way resulted in increased profit.

8. Conclusion

■ State antitakeover legislation provides plausibly exogenous variation in the degree of the agency problem. We have used this variation to test for the effect of managerial discretion on wages. Using a differences-in-differences approach, we have demonstrated that wages rose following these laws. Our results support many previously untested popular perceptions about the preference of managers for paying high wages. Since larger firms are more likely to be publicly owned and in general have more dispersed ownership, these results also suggest that the firm size–wage effect may partly reflect a managerial discretion effect.[21]

Our findings depict a wage-setting process that implies, to use Reynolds' (1951) term, a "range of indeterminateness." Moral hazard between managers and owners means that wages will reflect the diversity of managerial preferences and discretion. In that sense, this article presents a serious challenge to standard models of wage determination, all of which posit managerial profit maximization.

Appendix

TABLE A1 State Antitakeover Legislation: Dates of Enactment of Business Combination Laws

State	Year of Enactment
Arizona	1987
Connecticut	1989
Delaware	1988
Georgia	1988
Idaho	1988
Illinois	1989
Indiana	1986
Kansas	1989
Kentucky	1987
Maine	1988
Maryland	1989
Massachusetts	1989
Michigan	1989
Minnesota	1987
Missouri	1986
Nebraska	1988
New Jersey	1986
New York	1985
Ohio	1990
Pennsylvania	1989
Rhode Island	1990
South Carolina	1988
South Dakota	1990
Tennessee	1988
Virginia	1988
Washington	1990
Wisconsin	1987
Wyoming	1989

Source: *Annoted State Codes,* various states and years.

[21] Various studies have found that larger firms pay higher wages, even after controlling for worker or plant characteristics. See Lester (1967) and Brown and Medoff (1989).

TABLE A2 Effects of State Antitakeover Legislation on Wages
Dependent Variable: Log Wage

	(1)	(2)	(3)	(4)
BC*$After_t^{BC}$	.011**			.011**
	(.005)			(.006)
FP*$After_t^{FP}$	—	.003	—	−.000
		(.005)		(.006)
CSA*$After_t^{CSA}$	—	—	.000	−.000
			(.006)	(.006)
Year dummies	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Adjusted R^2	.939	.939	.939	.939
Sample size	9,305	9,305	9,305	9,305

Notes: Sample in all columns contains all the firms for which the wage growth rate is always below 75% and always above −44%. See Table 3 for further notes.

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