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Labor unemployment insurance and earnings management ☆

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ABSTRACT

We provide new evidence that firms appear to manage long-run earnings upward in order to manage rank and file employees' perceptions of employment security. In particular, we exploit exogenous state-level changes in unemployment insurance benefits and test for partial unwinding of prior upward earnings management when benefits increase. Consistent with the hypothesis, we find a significant reduction in abnormal accruals, increased recognition of special items and write downs, and greater likelihood of net income-reducing restatements, following an increase in state-level unemployment benefits. A number of cross-sectional results are also consistent with the hypothesis.

1. Introduction

Understanding the forces that shape financial reporting outcomes is an important goal in accounting research. A large prior literature provides evidence on how financial reporting choices are affected by explicit and implicit contracts. Explicit debt contracts with firms' creditors (Watts and Zimmerman, 1986; DeFond and Jiambalvo, 1994; Dichev and Skinner, 2002), and compensation contracts with firms' executives (Healy, 1985), have been shown to influence financial reporting choices. Implicit contracts with equity investors as reflected in these investors' expectations have also been shown to influence financial reporting choices (Graham et al., 2005; Badertscher et al., 2012). Relatively less attention has been devoted however to understanding the potential influence of implicit contracts between firms and rank and file employees.

Firms sell employees a package of explicit and implicit claims, with explicit claims expressed through explicit employment contracts and implicit claims expressed through promises about long-run working conditions and employment and advancement opportunities (Cornell and Shapiro, 1987). The value of the ongoing implicit claims depends on employee expectations about the firm's future financial conditions (Bowen et al., 1995). Firms have an incentive to maximize the value of the package of explicit and implicit claims they offer by maximizing employees' perceived value of the ongoing implicit claims, and this incentive is a potentially important determinant of corporate financial and reporting policies

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(Bowen et al., 1995; Agrawal and Matsa, 2013). In this paper we provide new evidence on whether and how financial reporting choices are affected by the implicit claims of rank and file employees.

The prior literature has suggested two ways in which rank and file employees can potentially affect financial reporting. The first is in the context of union negotiations. The hypothesis posited in the literature is that firms are expected to manage earnings *downwards* in the *short-run* during union negotiations in order to justify resistance to union calls for enhanced pay and benefits. This hypothesis predicts firm behavior when shareholder-employee conflicts of interest are pronounced and employees are unionized, and empirical tests of the hypothesis appear to yield mixed results (Liberty and Zimmerman, 1986; DeAngelo and DeAngelo, 1991; D'Souza et al., 2001; Comprix and Muller 2011; Bova et al., 2015).

The second hypothesis posited in the literature, which is the focus of this paper, is that firms are expected to make *long-run* earnings-*increasing* choices in order to project financial security, as this reduces the cost of employee hiring and retention in competitive labor markets. The hypothesis is predicted to hold more generally when shareholder-employee conflicts of interest are not salient, and the hypothesis is not conditioned on the presence of labor unions. This idea is advanced in a number of papers in the accounting literature. Bowen et al. (1995), Burgstahler and Dichev (1997), Matsumoto (2002), and Cheng and Warfield (2005), among others, argue that firms manage their financial statements to make them appear rosier in order to improve their implicit terms of trade with various stakeholders including employees, and Graham et al. (2005) report consistent evidence from a survey of CFOs.

As in the prior literature, by long-run earnings-increasing choices (or "long-run earnings management") we mean management of cumulative earnings over an extended period that results in a sustained wedge between 'true' and reported cumulative earnings. Cumulative earnings can potentially be managed through a combination of long-run income increasing choices such as depreciation, inventory, and bad debt recognition policies, and short-run accrual and real-earnings-management choices when the wedge is in jeopardy. In particular, the upward management of cumulative earnings does not require upward earnings management in every single reporting period, which makes it relatively more challenging to detect empirically. Bowen et al. (1995) provide one test of the long-run-earnings management hypothesis by examining whether firms with high labor intensity are more likely to adopt long-run income-increasing accounting policies, and find supportive results. In this paper we adopt a potentially more powerful approach by testing for changes in earnings management around an exogenous shock to employees' cost of unemployment.

A large labor economics literature documents that employees bear substantial costs of involuntary unemployment (e.g., Gibbons and Katz, 1991; Gruber, 1997). Employees therefore care about the financial security of their employer (Brown and Matsa, 2013), and one way they assess this is through their employer's earnings performance (e.g., Burgstahler and Dichev, 1997; Matsumoto, 2002; Cheng and Warfield, 2005; Graham et al., 2005). Firms bear at least two costs of exposing workers to unemployment risk. First, firms bear ex ante costs of compensating employees for the level of employment risk that exists at the initiation of employment. These "compensating differentials" are substantial and can take the form of higher wages and benefits, and better working conditions (Smith, 1976; Abowd and Ashenfelter, 1981; Topel, 1984; Hamermesh and Wolfe, 1990). The economic significance of compensating wage differentials is suggested by empirical estimates that it is up to 14% of wages (Abowd and Ashenfelter, 1981), and that up to 41% of interindustry variation in wages is explained by unemployment risk (Li, 1986; Hamermesh and Wolfe, 1990). Second, firms bear substantial turnover costs as employees seek more stable employment, and heightened search costs as potential employees shy away (Brown and Matsa, 2013), if there are significant increases in unemployment risk.

We expect firms try to mitigate these costs by managing long-run earnings in order to manage employee perceptions of unemployment risk. This implies firms' long-run-earnings management incentives *change* when employees' unemployment risk *changes*. Employees' unemployment risk (and the firm's cost of exposing employees to unemployment risk) is increasing in (i) the unemployment probability, (ii) employee risk aversion, and (iii) costs borne by workers during unemployment (Agrawal and Matsa, 2013). Our research design exploits exogenous shocks to employees' unemployment risk through a decrease in the costs borne by workers during unemployment. This follows Agrawal and Matsa (2013) who show that the same exogenous shock affects firms' financial policies through increased leverage.

State-level unemployment insurance (UI) programs are intended to lower employees' unemployment costs by providing temporary income when they are involuntarily unemployed and actively seeking new employment, and UI has been shown to have economically significant effects on employee behavior and aggregate labor supply (Topel, 1984; Meyer, 1990, 1995). An increase in UI benefits decreases employees' cost of separation from, and likely therefore their dependence on, their employers. When employees are better insured against unemployment we expect they are relatively less sensitive to their employers' financial performance. This observation forms the basis for the empirical tests: for firms in a given U.S. state, we examine discretionary earnings decreases around large state-level increases in UI benefits as these state-level changes are relatively exogenous to the firm.

One way to view this is that cumulative or long-run earnings management results in a 'bloated' balance sheet. Managers likely look for opportune moments to partially clean up the bloat, and as long as one determinant of this bloat is managing employee perceptions of unemployment risk, we expect a reduction in bloat (or *partial unwinding* of prior upward earnings management) when there is a reduction in unemployment risk. As such, we are effectively testing *changes* in balance sheet bloat, rather than *levels* of bloat in the cross-section, which allows a relatively more powerful test of the hypothesis.

The empirical tests employ a difference-in-differences design by examining earnings management measures before versus after an exogenous increase in UI benefits, for treatment firms versus control firms. The treatment firms are those headquartered in states with a large increase ($>10\%$) in maximum UI benefits, while control firms are those headquartered

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in matched neighboring states without an increase in benefits. We find no differences in macroeconomic conditions between treatment and control states as indicated by state-level GDP growth, unemployment rates, stock returns and stock return volatilities of firms headquartered in those states, and UI payroll taxes.

Of the four earnings management measures employed, one is a model-implied estimate of abnormal accruals, while three are model-free measures. The abnormal accrual estimate is from the Dechow and Dichev (2002) model, and results are robust to a variety of other models. The model-free measures are special items, write downs, and net income-reducing restatements. Across all earnings management measures, results suggest significant unwinding of prior upward earnings management for treatment firms relative to control firms in the year following an increase in UI benefits. This suggests firms are relatively more likely to partially unwind prior upward earnings management when employees are better insured against unemployment, consistent with the main hypothesis. A battery of further tests examining alternative specifications, and the potential influence of state-level political forces and unobserved economic conditions, indicate robustness of the main result.

A number of cross-sectional predictions are tested. We expect upward earnings management is more likely at firms where employees' perceptions of unemployment risk are relatively more important, such as firms that are more labor intensive, have higher layoff propensity, and employ relatively more low-wage workers. Therefore, we expect relatively greater unwinding of earnings management at these firms following an increase in unemployment benefits. Further, we expect partial unwinding is less likely at firms with other strong incentives to maintain the upward earnings management, such as firms that need to tap the capital markets, meet or beat earnings expectations, avoid debt covenant violations, or inflate executive compensation. These tests yield significant and consistent results.

The evidence in this paper contributes to the accounting literature by providing new evidence on the role of employee perceptions in shaping financial reporting choices. The evidence suggests that managing employees' perception of unemployment risk is one determinant of firms' long-run-earnings management. The prior accounting literature has largely examined the influence of organized labor unions negotiating explicit contracts. In contrast, the evidence here sheds light more generally on the role of rank and file employees and their implicit contract with employers. The results contribute to the literature on the determinants of financial reporting choices, and further our understanding of the role therein of a key factor of production.

In what follows, Section 2 describes the hypotheses; Section 3 describes the data; Section 4 presents the main results; Section 5 describes cross-sectional tests; and Section 6 concludes.

2. Hypothesis development

An extensive literature in labor economics supports the idea that workers bear substantial costs of becoming involuntarily unemployed, including significant decreases in consumption (Gruber, 1997) and difficulty in finding another wage-equivalent job (Farber, 2005; Gibbons and Katz, 1991). As such, employees care deeply about unemployment risk. Firms in turn bear significant costs of exposing workers to unemployment risk, including compensating wage differentials (Abowd and Ashenfelter, 1981; Topel, 1984; Hamermesh and Wolfe, 1990), turnover costs as employees seek more stable employment, and heightened search costs as potential employees shy away (Brown and Matsa, 2013).

Reported earnings are useful for employees to assess the likelihood of becoming involuntarily unemployed. This implicit use of accounting numbers likely creates ongoing incentives to manage earnings upward, as recognized in a number of prior papers including Bowen et al. (1995). Burgstahler and Dichev (1997) argue that lowering transactions cost with stakeholders such as employees is one incentive for upward earnings management. Matsumoto (2002) and Cheng and Warfield (2005) hypothesize and find that one incentive for firms to avoid negative earnings surprises stems from the implicit claims of employees as proxied by labor intensity. Choudhary et al. (2009) hypothesize and find that labor intensity is one incentive for firms to manage income even absent cash flow effects. Zechman (2010) hypothesizes that employees' implicit claims also present an incentive for balance sheet management. Finally, Graham et al. (2005) indicate a significant majority of surveyed CFOs agree that managing earnings to manage stakeholder perception is descriptive of actual practice.

Employees do not necessarily have to be direct consumers of financial statements in order for their perceptions to be shaped by reported earnings. As long as earnings are part of the public information set used by the media, analysts, ratings agencies and other parties to generate forecasts and reports, and employees are consumers of these forecasts, reports and news, reported earnings can shape employee perceptions of unemployment risk.

2.1. The U.S. Unemployment Insurance (UI) System

The UI system provides temporary income to eligible workers who are involuntarily unemployed and actively seeking new employment, in order to mitigate the loss of income from unemployment. The basic framework for insurance provisions under the joint federal-state system is common across all states, though individual states have the autonomy to set the program's parameters: Who is eligible, the duration of benefits, and the amount of weekly unemployment benefits paid.

A state's benefit formula typically calculates the earnings realized by a worker in four of the last five quarters and seeks to replace approximately half of those wages through weekly payments, subject to minimum and maximum bounds. Much of the interstate and intertemporal variation in UI benefits stems from changes to the maximum bounds, and the elasticity of

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Table 1
Event-years with a large increase in unemployment insurance benefits.

States	Event years							States	Event years					
Alabama	1984	1989	1993	2003				Missouri	1980	1985	1998	2001	2008	
Arizona	2005							Montana	2004					
Arkansas	1980	1985	2006					Nebraska	1978	1980	1984	1995	1999	2001
California	1980	1982	1990	2002				Nevada	1982					
Colorado	1982							New Hampshire	1980	1982	1999	2003	2008	
Connecticut	1984	2001						New Jersey	1985					
Delaware	1984	1986	1996					New York	1978	1984	1990	1999	2001	
D.C.	1986	1989	1992	2006				North Carolina	1988	2001				
Florida	1980	1984	1986	1988	1991	1998		Oklahoma	1979	2008				
Georgia	1982							Oregon	1982					
Idaho	1977							Pennsylvania	2001					
Illinois	1980	1983	2001					Rhode Island	1986	1988				
Indiana	1977	1981	2001					South Carolina	1989	1994				
Iowa	1986							Tennessee	1978	1981	1988	1997	1999	2010
Kansas	1977	1979	1981					Texas	1978	1980				
Kentucky	1979	1990	2001	2004				Utah	1982	1985	2001			
Louisiana	1998	2001	2009					Vermont	1999	2003				
Maryland	1979	1981	1987	1996	2001	2003	2008	Virginia	1977	1982	1991	2001	2003	
Massachusetts	1983	2001	2007					Washington	1978	1990	1994	2002		
Michigan	1982	1988	2003					West Virginia	1977	1980				
Minnesota	1980	1986	2000					Wisconsin	1990					
Mississippi	1989	1992						Wyoming	1981	1985	1994	2008		

The table shows 128 state-year events that satisfy three conditions: (i) there is a large increase (> 10%) in maximum total benefits; (ii) there is at least one adjacent state without a large increase in maximum total benefits; and, (iii) neither the state nor the adjacent state experiences a large increase in maximum total benefits in the prior year

actual compensation payments to maximum total benefits is approximately one (Agrawal and Matsa, 2013; Hsu et al., 2013). The maximum bound of benefits therefore effectively captures the generosity of UI benefits, and is inversely related to workers' consumption loss during unemployment.

State-level changes in maximum total UI benefits are motivated by both state-level macroeconomic conditions and political forces. Since these underlying macroeconomic conditions could also be associated with the financial reporting outcomes examined here, our empirical tests ensure that treatment and control states have similar economic conditions. This is analogous to achieving covariate balance under a propensity score matching method. The other determinant of changes in the maximum bound, political forces, is considered in Section 4.2.

States finance their UI programs through payroll taxes levied on employers. The firm-level effective tax rates are tailored to the unemployment history of the individual firm through a method referred to as experience rating, and states have the authority to set their own state-level experience rating scheme. Firms with a recent history of layoffs have higher effective unemployment tax rates under this system. We address in two ways any potential concern that changes in UI payroll tax rates concurrent with changes in maximum total UI benefits explain the financial reporting outcomes examined here. First, we examine UI payroll taxes paid around the event year, and find no changes either for treatment or control states. Second, the regressions control for UI payroll taxes.

2.2. Hypotheses

The primary hypothesis is that firms have an incentive to manage long-run-earnings upward in order to manage employee perceptions of unemployment risk, and therefore that *changes* in unemployment risk are expected to be associated with *changes* in earnings management incentives. As such, we expect to empirically observe a partial reversal of prior upward earnings management following an increase in state-level UI benefits, where the earnings management reversal is expected to manifest in negative discretionary accruals, special items, write downs, and income-reducing restatements.

As noted in the prior literature, there are a number of incentives to manage earnings upward, including incentives stemming from the need to tap capital markets, meet or beat earnings expectations, avoid debt covenant violations, and inflate executive compensation. These incentives are likely episodic, while the incentive to manage employee perceptions of unemployment risk is likely ongoing. For example, it is unlikely that a firm taps the capital markets every year, is on the cusp of meeting or beating analyst forecasts every period (it might meet or beat comfortably in many periods), is in danger of violating a debt covenant every period, or that the incremental compensation payoff from earnings management is equally strong every period (the payoff could be non-linear in earnings as in Healy (1985)). In contrast, firms have ongoing needs to retain and attract talent, which implies that the unemployment risk incentive is long-run and therefore less likely to be irrelevant for managerial actions. Our hypothesis is about the incremental effect on earnings management of the unemployment risk incentive, and the tests control for other earnings management incentives.

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A number of cross-sectional predictions are tested. Firms with high labor intensity are likely more vulnerable to employee perception of employment security than firms with low labor intensity, and are therefore more likely to manage earnings upward on an ongoing basis. As such, we expect a larger reversal of prior upward earnings management at more labor-intensive firms following an increase in UI benefits.

Reported earnings are likely more important to employees' perception of unemployment risk at firms with higher layoff propensity. Such firms likely have greater incentive to manage earnings upward in order to minimize their own costs of exposing workers to unemployment risk. As such we expect a larger reversal of prior upward earnings management at firms with high layoff propensity, following an increase in UI benefits.

Low-wage workers are likely to benefit relatively more from an increase in UI benefits since they have lower savings at the time of job loss (Browning and Crossley, 2001; Bloemen and Stancanelli, 2005). As such, increases in UI benefits likely affect relatively more the earnings management incentives of firms with a higher fraction of low wage workers. We therefore expect such firms to have a larger reversal of prior upward earnings management following an increase in UI benefits.

Finally, when other upward earnings management incentives are strong (weak), an increase in UI benefits is less (more) likely to lead to partial unwinding of prior upward earnings management. As such, we expect a significant (insignificant) partial unwinding of prior upward earnings management when incentives to tap capital markets, meet or beat earnings expectations, avoid debt covenant violations, and inflate executive compensation, are weak (strong).

3. Data

We employ a matched sample for testing, where the sample is developed as follows. First, state-years with increases in the upper bound of unemployment benefits exceeding 10% ("the event") are identified, yielding 128 such state-years. The 10% cutoff lies at the 85th percentile of the distribution of annual changes in the upper bound of unemployment benefits, and allows us to balance the magnitude of the change versus sample size considerations. Results are robust to using other cutoffs such as 5% or 15% increases in the upper bound of unemployment benefits. Second, each event-year is matched to at least one adjacent state that does not experience a large increase in maximum unemployment benefits in the same year and the prior year. This yields 290 matching state-years for the 128 event-years, since some states are matched to more than one state in a given year. Table 1 shows the sample of 128 event-years between 1976 and 2010 with an increase in maximum total benefits exceeding 10%, and without a large increase in unemployment benefits in the pre-event year. As the table shows, event-years are not clustered in any particular state.

The economic significance of the Treatment effect (the reduction in employees' unemployment risk resulting from a 10% increase in unemployment benefits) is suggested by two sets of evidence. First, the evidence in Agrawal and Matsa (2013) suggests a 10% increase in unemployment benefits leads to a 0.5 percentage point increase in financial leverage (debt/assets), which appears economically significant. Second, we calibrate the dollar magnitude of implied wage savings as a result of the reduction in unemployment risk from a 10% increase in unemployment benefits (derivation details are presented in Appendix A). The analysis suggests the reduction in employees' unemployment risk resulting from a 10% increase in unemployment benefits is, ceteris paribus, associated with an implied wage reduction of 8% of earnings for Treatment sample firms. This appears to be an economically meaningful shock for firms.

The tests use data from all firms headquartered in these state-years. Financial statement and stock price data are sourced from Compustat and CRSP, restatement data from Audit Analytics, analyst forecasts from IBES, debt covenant data is from Dealscan, executive compensation data is from Execucomp, unemployment benefit data from the United States Bureau of Labor Statistics, and state-level GDP growth data from the Census Bureau. Political condition variables are obtained from the U.S. Bureau of the Census Statistical Abstracts. The final sample is from 1992 to 2010, which is the longest period for which we have all necessary data for the empirical tests.[1] All variable definitions are presented in Appendix B.

Compustat reports a firm's current rather than historical headquarter location, and ignoring this source of measurement error could introduce bias. To remedy this we extract historical headquarter locations from other data sources. For the period between 1992 and 1995, we locate a firm's headquarter by searching Compact Disclosure. For each year from 1996 onwards, we search each firm's 10-K report via EDGAR for its headquarter address. Overall, Compustat's reported current location is different from the historical location for 17% of firm-year observations.

3.1. Earnings management measures

The main tests use two broad categories of earnings management measures: model-implied estimates of abnormal accruals, and model-free measures. The accrual model is the Dechow-Dichev (2002) model as modified in McNichols (2002). Following Ball and Shivakumar (2006) this model is estimated for each three-digit SIC industry with more than 30 observations (we exclude financials and utilities in estimating the accruals models) as follows:

$$Accruals_t = \alpha_1 + \alpha_2 CFO_{t-1} + \alpha_3 CFO_t + \alpha_4 CFO_{t+1} + \alpha_5 \Delta Sales_t + \alpha_6 PPE_t + \varepsilon_t, \quad (1)$$

[1] Dealscan (the source of debt covenant data) is only available from 1987, and Execucomp (for compensation data) is only available from 1992 and for S&P 1500 firms only. All results hold in the larger sample as well if we do not use covenant and compensation incentive variables.

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Table 2
Unemployment insurance benefit increases and state-level economic characteristics.

	Treatment states			Control adjacent states			
	Pre-event year mean (1)	Event year mean (2)	Diff. (2)–(1)	Pre-event year mean (3)	Event year mean (4)	Diff. (4)–(3)	Diff in Diff [(2)–(1)]–[(4)–(3)]
Benefit	8.451	8.598	0.147	8.637	8.672	0.035	0.109
t-Stat.	.	.	2.928	.	.	1.161	1.742
GDP growth	1.079	1.070	−0.010	1.070	1.064	−0.007	−0.003
t-Stat.	.	.	−1.982	.	.	−2.038	−0.518
Unemp rate	5.605	5.808	0.202	5.788	5.920	0.131	0.073
t-Stat.	.	.	1.025	.	.	0.944	0.282
State stock rets$_t$	0.270	0.260	−0.010	0.301	0.227	−0.074	0.065
t-Stat.	.	.	−0.184	.	.	−1.417	0.751
State stock rets$_{t-1}$	0.276	0.246	−0.031	0.267	0.255	−0.012	−0.019
t-Stat.	.	.	−0.698	.	.	−0.444	−0.367
State stock ret volatility	0.035	0.035	0.001	0.034	0.035	0.001	0.000
t-Stat.	.	.	0.510	.	.	0.763	0.077
UI payroll tax	4.978	4.977	−0.001	5.082	5.084	0.002	−0.013
t-Stat.	.	.	−0.033	.	.	0.404	−0.234

The table presents means of state-level economic characteristics of treatment and control states in the event year and the pre-event year. There are 128 treatment state-years as identified in Table 1, matched to 290 state-years using the matching procedure described in Table 1. An event year is one with an increase in benefit exceeding 10%. *Benefit* is the log of maximum total unemployment benefits. *GDP growth* is the state-level GDP growth. *Unemp rate* is the state-level unemployment rate. *State stock rets* is the value-weighted average annual stock return of firms headquartered in a state. *State stock ret volatility* is the average across firms headquartered in a state of the firm-level standard deviation of daily returns within a year. *UI payroll tax* is the log of average UI tax rates multiplied by taxable wages.

where *t* indexes the year; *Accruals* is income before extraordinary items minus operating cash flows; *CFO* is operating cash flows; Δ*Sales* is the change in sales; and *PPE* is the end of year property, plant and equipment. All variables are scaled by lagged total assets. The residual from eq. (1) is the first measure of earnings management (*Abnormal accruals*). Results are robust to estimation by industry-year, and the use of three other accrual models: (i) the modified cross-sectional Jones (1991) model of Dechow et al. (1995) as used in Dechow et al. (2003) and Cohen et al. (2008); (ii) the performance-matched approach in Kothari et al. (2005); and (iii) the piecewise-linear model from Ball and Shivakumar (2006).

The three model-free measures of earnings management are special items (*Special items*), total write down (*Write down*) and an indicator equal to one for firms that have income-reducing restatements (*Restatement*). The data for write downs and restatements are available for 2000–2010 only.

4. Empirical tests and results

In this section we test the hypothesis that prior upward earnings management reverses around large unemployment benefit increases. Section 4.1 examines the primary relation (Hypothesis 1), while Section 4.2 describes tests intended to enhance identification and test robustness.

4.1. Earnings management reversal around large unemployment benefit changes

We begin by examining macroeconomic conditions in the treatment and control samples in order to shed light on whether differences in the underlying macroeconomic conditions, rather than differences in treatment, have an effect on the outcome of interest. Table 2 shows means of state-level macroeconomic indicators in the event-year and pre- event-year in both the treatment and control samples, along with a test of differences. The macroeconomic indicators include state-level GDP growth and unemployment rates, stock returns and stock return volatilities of all firms headquartered in these states as summary indicators of state-level economic conditions, and UI payroll taxes calculated as the average UI tax rate multiplied by average taxable wages in each state.

The last column of Table 2 shows a test of differences across the treatment and control samples in macroeconomic changes between the event-year and pre- event-year. As that column shows, treatment and control samples differ only in the change in maximum unemployment benefits between the event-year and pre- event-year. This suggests underlying macroeconomic conditions are unlikely to explain differences in examined outcomes across the treatment and control samples.

The subsequent tests are conducted on firm-year observations, where the firms are those headquartered in treatment and control states. Table 3 shows descriptive statistics for the 8,272 firm-years in the sample. The mean of *Abnormal accruals is* −0.022, the mean of *Special items* is −1% of assets, mean *Write down* is −0.1% of assets, and 4.1% of firm-years report income-reducing restatements. *Benefit* equals the log of maximum total unemployment benefits, and is calculated as the

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Table 3
Descriptive statistics.

Variables	*N*	Mean	Std. dev	*Q*1	Median	*Q*3
Abnormal accruals	8272	−0.022	0.077	−0.058	−0.012	0.022
Special items	8272	−0.010	0.015	−0.014	0.000	0.000
Write down	5038	−0.001	0.003	0.000	0.000	0.000
Restatement	5038	0.041	0.198	0.000	0.000	0.000
Benefit	8272	9.094	0.328	8.875	9.075	9.262
Tap capital market	8272	−0.047	1.020	−0.623	−0.425	0.029
Meet or beat	8272	0.226	0.418	0.000	0.000	0.000
Tight covenant	8272	0.190	0.392	0.000	0.000	0.000
Compensation	8272	0.000	1.393	−0.874	−0.032	0.857
Assets	8272	7.172	1.517	6.038	7.024	8.175
Age	8272	24.697	16.011	11.000	19.000	38.000
Industry growth	8272	1.072	0.203	0.972	1.058	1.137
Cash flow	8272	0.102	0.099	0.058	0.103	0.151
Big N	8272	0.963	0.188	1.000	1.000	1.000
Tenure	8272	11.645	8.683	5.000	9.000	17.000
Leverage	8272	0.231	0.186	0.074	0.220	0.342
Book to market	8272	0.517	0.543	0.247	0.415	0.654
GDP growth	8272	1.047	0.027	1.035	1.045	1.064
Unemp rate	8272	5.071	1.410	4.400	4.900	5.700
State stock rets	8272	0.206	0.292	0.026	0.195	0.324
Prior state stock rets	8272	0.235	0.259	0.081	0.225	0.396
State stock ret volatility	8272	0.042	0.011	0.032	0.042	0.051
UI payroll tax	8272	5.189	0.581	4.762	5.218	5.655

The table presents descriptive statistics of all variables used in the study. *Abnormal accruals* is from the Dechow-Dichev (2002) Model as modified in McNichols (2002, 65). *Special items* are special items divided by total assets. *Write down* is the total write-down divided by total assets. *Restatement* is an indicator equal to one for income-reducing restatements. *Benefit* is the log of maximum total benefits. *Tap capital market* is the principal component of future equity issuance and debt issuance. *Meet or beat* is an indicator equal to one if net income before extraordinary items scaled by total assets lies in [0, 0.005), change in net income before extraordinary items scaled by total assets lies in [0, 0.005), or EPS beats analyst forecasts by one cent per share or less. *Tight covenant* is an indicator set to one if the slack of a firm's tightest covenant falls into the lowest decile of slack. *Compensation* is the principal component of log of one plus mean cash compensation paid to top five executives, log of one plus mean delta of the top five executives, log of one plus mean vega of the top five executives. *Assets* is the log of total assets. *Age* is the number of years since firm IPO. *Industry growth* is the sum of sales in a two-digit SIC industry divided by the lagged sum of sales. *Cash flow* is the operating cash flow. *Big N* is an indicator equal to one if the auditor is a Big N auditor, and set to zero otherwise. *Tenure* is the number of consecutive years that the firm has retained the auditor. *Leverage* is debt in current liabilities plus long-term debt scaled by total assets. *Book to market* is the book value of equity divided by the market value of equity. *GDP growth* is the state-level GDP growth. *Unemp rate* is the state-level unemployment rate. *State stock rets* is the value-weighted average annual stock returns of firms headquartered in a state. *State stock ret volatility* is the average across firms headquartered in a state of the firm-level standard deviation of daily stock returns within a year. *UI payroll tax* is the log of average UI tax rates multiplied by taxable wages. Detailed variable definitions are presented in Appendix B.

product of the maximum weekly benefit amount and the maximum duration allowed. The mean and standard deviation of *Benefit* are 9.094 and 0.328, respectively, close to values reported in Agrawal and Matsa (2013).

The regression specification takes the form:

$$DepVar_{i,t+1} = \theta_1 + \theta_2 Benefit_{i,t} + \sum \theta j Control_{j,i,t+1} + v_{i,t+1} \quad (2)$$

where *DepVar* is the dependent variable that equals any one of the four earnings management measures described earlier; "*i*" indexes the firm, "*t*" indexes the year, and "*j*" indexes the *j*th control for $j > 2$. The panel regressions include firm- and year-fixed effects, and standard errors are clustered by state (Petersen, 2009). The inclusion of firm- and year-fixed effects represents a generalization of the difference-in-differences design that allows causal inference in a regression setting (Bertrand and Mullainathan, 2003; Angrist and Pischke, 2009; Armstrong et al., 2012). The controls include lagged dependent variables[2]; other earnings management incentives including the need to tap capital markets in the future, meet or beat analyst expectations, avoid debt covenant violations, and inflate executive compensation; firm characteristics such as total assets, operating cash flows, asset growth, leverage, and cash flow volatility that are likely associated with the dependent variables (Myers et al., 2003; Ashbaugh et al., 2003; Butler et al., 2004); as well as state-level macroeconomic indicators such as GDP growth, unemployment rate, stock returns and stocks return volatilities of firms headquartered in the state, and UI payroll taxes.

Panel A of Table 4 shows coefficients, and *t*-statistics in parentheses, from pooled regression estimation of Eq. (1). The dependent variable in each specification is indicated in the column header. The primary independent variable of interest is *Benefit*, and given the inclusion of firm fixed effects in the regression, results can be interpreted as showing that an increase in *Benefit* significantly predicts changes in the dependent variable.

[2] Angrist and Pischke (2009) note that a model with both fixed effects (FE) and lagged dependent variables (LDV) is challenging to estimate and researchers should evaluate robustness separately to inclusion of FE (without LDV) and inclusion of LDV (without FE). We find robust results under both specifications.

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Table 4
Unemployment insurance benefit increases and earnings management reversal.

Panel A: Fixed effects specification

	Modified Dechow-Dichev Abnormal accruals$_{t+1}$ (1)	*Special items*$_{t+1}$ (2)	*Write down*$_{t+1}$ (3)	*Restatement*$_{t+1}$ (4)
Benefit$_t$	−0.012**	−0.005**	−0.002***	0.117***
	(2.02)	(2.26)	(3.92)	(3.46)
Abnormal accruals$_t$	0.057**			
	(2.30)			
Special items$_t$		−0.003		
		(0.83)		
Write down$_t$			−0.010	
			(1.18)	
Restatement$_t$				−0.151***
				(4.81)
Tap capital market$_{t+1}$	0.003**	0.000	0.000	−0.004
	(2.41)	(1.25)	(0.54)	(1.57)
Meet or beat$_{t+1}$	0.006***	0.001***	0.000***	−0.002
	(4.44)	(3.19)	(2.95)	(0.24)
Tight covenant$_{t+1}$	0.004	−0.000	−0.000	0.003
	(1.52)	(0.41)	(0.46)	(0.26)
Compensation$_{t+1}$	0.007***	0.002***	0.000***	−0.005
	(4.02)	(4.73)	(3.02)	(0.68)
Assets$_{t+1}$	−0.004**	−0.001	−0.000	0.001
	(2.22)	(0.86)	(1.01)	(0.04)
Age$_{t+1}$	0.000	−0.000	−0.000	−0.004
	(0.29)	(0.95)	(0.09)	(0.45)
Industry growth$_{t+1}$	0.009**	0.002*	0.001	−0.048*
	(2.36)	(1.92)	(1.13)	(1.71)
Cash flow$_{t+1}$	−0.333***	0.013***	0.002	−0.050
	(11.89)	(3.73)	(1.17)	(1.05)
Big N$_{t+1}$	0.000	0.002	0.000	0.013
	(0.04)	(0.79)	(0.53)	(0.39)
Tenure$_{t+1}$	0.000	0.000	−0.000	−0.000
	(0.69)	(0.00)	(0.31)	(0.08)
Leverage$_{t+1}$	−0.059***	−0.007***	−0.000	0.040
	(5.33)	(2.94)	(0.38)	(1.04)
Book to market$_{t+1}$	−0.009**	−0.002***	0.000	0.008
	(2.27)	(2.69)	(0.30)	(0.80)
GDP growth$_{t+1}$	0.062*	0.001	0.005	−0.254
	(1.69)	(0.12)	(1.18)	(1.55)
Unemp rate$_{t+1}$	0.000	0.000	0.000	0.010
	(0.50)	(0.69)	(0.31)	(1.46)
State stock rets$_{t+1}$	0.007	0.001	0.000	0.054**
	(1.48)	(0.78)	(0.10)	(2.20)
State stock rets$_t$	−0.002	0.001	0.000	0.032*
	(0.39)	(0.78)	(0.15)	(1.90)
State stock ret volatility$_{t+1}$	−0.246	−0.084*	0.025	−2.220
	(1.37)	(1.97)	(1.06)	(1.60)
UI payroll tax$_{t+1}$	−0.005	−0.000	0.000	−0.012
	(1.21)	(0.55)	(0.15)	(0.60)
Constant	0.126*	0.046*	0.017*	−0.629
	(1.94)	(1.85)	(1.92)	(1.19)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	8272	8272	5038	5038
Adj. R^2	0.526	0.254	0.228	0.094

Panel B: Difference-in-differences specification

	Modified Dechow-Dichev Abnormal accruals$_{t+1}$ (1)	*Special items*$_{t+1}$ (2)	*Write down*$_{t+1}$ (3)	*Restatement*$_{t+1}$ (4)
Treat$_t$	0.002	0.001	0.000	−0.022

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Table 4 (*continued*)

	(0.54)	(0.66)	(0.84)	(1.60)
$Post_t$	0.003*	0.000	−0.000	−0.007
	(1.73)	(1.21)	(1.55)	(0.96)
$Treat_t \times Post_t$	−0.011**	−0.003***	−0.00048***	0.040*
	(2.16)	(2.93)	(2.70)	(1.99)
$Abnormal\ accruals_t$	0.279***			
	(14.37)			
$Special\ items_t$		0.011***		
		(4.37)		
$Write\ down_t$			0.007**	
			(2.61)	
$Restatement_t$				0.058**
				(2.62)
$Tap\ capital\ market_{t+1}$	0.001	0.001***	0.000*	−0.006**
	(1.58)	(2.85)	(1.83)	(2.46)
$Meet\ or\ beat_{t+1}$	0.003*	0.002***	0.000***	−0.008
	(1.78)	(6.48)	(4.70)	(1.24)
$Tight\ covenant_{t+1}$	0.006***	−0.000	−0.000	0.012
	(2.76)	(0.85)	(0.17)	(1.19)
$Compensation_{t+1}$	0.002	0.001***	0.000***	−0.001
	(1.64)	(4.22)	(2.70)	(0.27)
$Assets_{t+1}$	0.001	−0.001***	−0.000	−0.003
	(0.64)	(2.72)	(1.49)	(1.08)
Age_{t+1}	0.000**	0.000*	0.000	0.000
	(2.64)	(1.95)	(1.07)	(0.57)
$Industry\ growth_{t+1}$	0.015***	0.003***	0.002***	−0.020
	(2.84)	(2.77)	(4.24)	(0.98)
$Cash\ flow_{t+1}$	−0.233***	0.017***	0.003***	−0.075**
	(13.76)	(5.67)	(3.80)	(2.09)
$Big\ N_{t+1}$	0.003	−0.000	–0.000*	−0.032
	(0.67)	(0.13)	(1.87)	(0.81)
$Tenure_{t+1}$	0.000	−0.000	–0.000	−0.000
	(0.86)	(1.03)	(0.20)	(1.21)
$Leverage_{t+1}$	−0.018**	−0.001	0.000	−0.010
	(2.19)	(0.39)	(0.98)	(0.60)
$Book\ to\ market_{t+1}$	−0.004**	−0.001	−0.000	−0.001
	(2.17)	(1.44)	(0.70)	(0.24)
$GDP\ growth_{t+1}$	0.119***	0.026**	−0.001	0.114
	(2.80)	(2.19)	(0.38)	(0.93)
$Unemp\ rate_{t+1}$	0.001	0.000**	0.000	−0.001
	(1.19)	(2.10)	(1.36)	(0.62)
$State\ stock\ rets_{t+1}$	0.013***	0.002***	0.000	0.038***
	(4.16)	(3.01)	(0.87)	(2.81)
$State\ stock\ rets_t$	0.004	0.001	−0.000	0.016
	(1.01)	(1.33)	(0.74)	(0.76)
$State\ stock\ ret\ volatility_{t+1}$	−0.509***	−0.159***	−0.037***	−0.082
	(5.81)	(5.99)	(5.29)	(0.21)
$UI\ payroll\ tax_{t+1}$	−0.007***	−0.002***	−0.000	0.001
	(3.16)	(3.37)	(0.62)	(0.17)
Constant	−0.100**	−0.023	0.001	0.005
	(2.27)	(1.67)	(0.21)	(0.03)
Observations	8272	8272	5038	5043
Adj. R^2	0.331	0.074	0.057	0.010

Table 4 presents coefficients, and *t*-statistics in parentheses, from pooled regressions of the dependent variables shown in each column header on the independent variables listed. Panel A presents results for fixed effects specifications, while Panel B presents results for difference-in-differences specifications. *Abnormal accruals* is from the Dechow-Dichev (2002) Model as modified in McNichols (2002, 65); *Special items* are special items divided by total assets; *Write down* is the total write-down divided by total assets; and, *Restatement* is an indicator equal to one for income-reducing restatements. *Benefit* is the log of maximum total benefits. *Treat* is an indicator equal to 1 for firm-years in the treatment sample, and equal to 0 otherwise. *Post* is an indicator equal to 1 for the period after an increase in unemployment benefits, and equal to 0 otherwise. *Tap capital market* is the principal component of future equity issuance and debt issuance. *Meet or beat* is an indicator equal to one if net income before extraordinary items scaled by total assets lies in [0, 0.005), change in net income before extraordinary items scaled by total assets lies in [0, 0.005), or EPS beats analyst forecasts by one cent per share or less. *Tight covenant* is an indicator set to one if the slack of a firm's tightest covenant falls into the lowest decile of slack. *Compensation* is the principal component of log of one plus mean cash compensation paid to top five executives, log of one plus mean delta of the top five executives, log of one plus mean vega of the top five executives. *Assets* is the log of total assets. *Age* is the number of years since firm IPO. *Industry growth* is the sum of sales in a two-digit SIC industry divided by the sum of sales last year. *Cash flow* is the operating cash flow. *Big N* is an indicator set to one if the auditor is a Big N auditor, and set to zero otherwise. *Tenure* is the number of consecutive years that the firm has retained the auditor. *Leverage* is debt in current liabilities plus long-term debt scaled by total assets. *Book to market* is the book value of equity divided by the market value. *GDP growth* is the state-level GDP growth. *Unemp rate* is the state-level unemployment rate. *State stock rets* is the value-weighted average annual stock returns of firms headquartered in a state. *State stock ret volatility* is the average across firms headquartered in a state of the firm-level standard deviation of daily stock returns within a year. *UI payroll tax* is the log of average UI tax rates multiplied by taxable wages. Detailed variable definitions are presented in Appendix B. Standard errors are clustered by state. *, (**), and [***] denote two-tailed statistical significance at 10%, (5%), and [1%] levels, respectively.

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As the table shows, an increase in *Benefit* significantly predicts a reduction in *Abnormal accruals* (two-tailed p-value < 0.05), more negative *Special items* (two-tailed p-value < 0.05) and *Write downs* (two-tailed p-value < 0.01), and an increased likelihood of income-reducing *Restatements* (two-tailed p-value < 0.01). In terms of economic significance, Table 4 suggests a 10% increase in *Benefit* predicts a change in:[3]

(i) *Abnormal accruals* of –0.0012, which is 4% of earnings (given the mean ROA of 3% for our treatment sample firms).
(ii) *Special items* of -0.0005, which is 1.7% of earnings.
(iii) *Write down* of -0.0002, which is 0.7% of earnings.
(iv) *Restatement* probability of 1.17 percentage points, which is about a 29% increase from the mean probability of income-reducing *Restatements*.

The firm-specific independent variables in Table 4 are motivated by the prior literature (Myers et al., 2003; Ashbaugh et al., 2003; Butler et al., 2004), and the signs of their coefficients are consistent with the prior literature. In particular, the signs of the coefficients of other earnings management incentives are consistent with expectation, providing some comfort about proxy validity and regression specification: When the *Tap capital market*, *Meet or beat*, *Tight covenant*, and *Compensation* incentives are stronger, generally abnormal accruals are higher, special items and write downs are lower in magnitude (less negative), and the probability of income-reducing restatements is lower (considering both Panels A and B). In an untabulated falsification test we examine whether non- income-reducing restatements, which constitute 0.8% of all firm-years and 16.3% of the restatement firm-years in our sample, are higher following an increase in unemployment benefits. We re-estimate the regression specification in Panel A with non- income-reducing restatements as the dependent variable, and find the coefficient of *Benefit* is insignificant (t-statistic$=0.26$) as expected.

Panel B of Table 4 shows results when a more 'traditional' difference-in-differences specification is estimated. *Treat* is an indicator equal to 1 for the treatment sample, and zero otherwise, while *Post* is an indicator equal to 1 in the year following the increase in unemployment benefits, and zero otherwise. The main independent variable of interest is *Treat* × *Post*, and it loads significantly negatively in all specifications consistent with the result in Panel A.

Collectively the results in Panels A and B of Table 4 are consistent with the hypothesis that firms manage earnings upward in order to manage employee perceptions of unemployment risk, and this incentive is incremental to other known earnings management incentives.

4.2. Further tests

In this section we describe a number of tests intended to enhance identification and examine robustness. Given the large number of dependent variables examined above, all subsequent results are tabulated using *Abnormal accruals* as the dependent variable.

4.2.1. Extended windows around the event

The main tests in Table 4 compare observations for years t and $t+1$ for firms in treatment and control states. We next examine robustness with respect to expanded windows around the event, ensuring that these windows do not contain other large changes in *Benefit* (so each window only contains one event for a given treatment state). As shown in Panel A of Table 5, *Benefit* loads significantly negatively in samples of three years (two-tailed p-value < 0.10), and five years (two-tailed p-value < 0.10), around the event year, indicating robustness of the main result in Table 4.

4.2.2. All states, all years

The main tests use a matched sample approach and therefore only include firms in treatment and control states. An alternative as in Agrawal and Matsa (2013) is to estimate a pooled regression across all available states and years, without matching treatment state-years to control state-years. As shown in Panel B of Table 5, results are robust to this test approach as *Benefit* loads significantly negatively (two-tailed p-value < 0.10).

4.2.3. State-level political forces and discretionary accruals

Benefit changes can be associated with political conditions (Agrawal and Matsa, 2013), and political conditions can influence discretionary accruals (Jones, 1991; Ramanna and Roychowdhury, 2010). To ensure that the effect of *Benefit* is not due to omitted political forces in the main tests, we control for state-level political conditions in a number of ways. First, gubernatorial election years could be associated with downward earnings management in order to avoid public scrutiny and becoming a political scapegoat, if the election involves economic issues that would adversely affect businesses. Downward earnings management could also assist affiliated politicians hoping to unseat incumbents, since being able to point to lower profitability of area businesses likely helps political contenders challenge the economic record of incumbents. As such we *entertain these possibilities*[4] by controlling for gubernatorial elections held in the year of the observed decrease in

[3] The coefficient of *Benefit* in the first three columns of Table 4 can be interpreted as the effect of a 100% change in benefit.
[4] By "entertaining these possibilities" we mean that we are not testing formal predictions related to political forces.

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Table 5
Additional analyses.

Panel A: Extended windows

	Three years around the event year (1)	*Five years around the event year* (2)
$Benefit_t$	−0.012*	−0.008*
	(1.76)	(1.98)
Controls	Yes	Yes
Firm FE	Yes	Yes
Year FE	Yes	Yes
Observations	11141	12017
Adj. R^2	0.527	0.522

Panel B: All states, All years

	All firm-years 1992–2010 (1)
$Benefit_t$	−0.007*
	(1.82)
Controls	Yes
Firm FE	Yes
Year FE	Yes
Observations	21907
Adj. R^2	0.366

Panel C: Controlling for political events

	(1)	(2)	(3)	(4)
$Benefit_t$	−0.012**	−0.012**	−0.012*	−0.012*
	(2.03)	(2.04)	(1.99)	(1.92)
$Election\ year_{t+1}$	−0.003			
	(1.58)			
$Election\ year\ tight\ margin_{t+1}$		0.002		
		(0.42)		
$Election\ year\ Democrat\ governor_{t+1}$			0.000	
			(0.12)	
$Election\ year\ Democrat\ both\ houses_{t+1}$				−0.000
				(0.12)
Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	8272	8272	8272	8272
Adj. R^2	0.526	0.526	0.526	0.526

The table presents coefficients, and *t*-statistics in parentheses, from pooled regressions of $Abnormal\ accruals_{t+1}$ on the independent variables listed. *Abnormal accruals* is from the Dechow-Dichev (2002) Model as modified in McNichols (2002, 65). *Controls* include all controls listed in Table 4. In Panel A, we expand the event window to three years and five years around the event year, as long as the window has only one event per treatment state. In Panel B, we estimate pooled regressions using data for all available states, years, and firms between 1992 and 2010 (i.e., Panel B does not use a matched sample approach). In Panel C, *Election year* is an indicator equal to one if there is a gubernatorial election in that year, and equal to zero otherwise. *Election year tight margin* is an indicator equal to one if there is a gubernatorial election and the winner vote percentage minus second vote percentage is below the sample median, and equal to zero otherwise. *Election year Democrat governor* is an indicator equal to one if there is a gubernatorial election and the incumbent governor is a Democrat, and equal to zero otherwise. *Election year Democrat both houses* is an indicator equal to one if there is a gubernatorial election and Democrats hold the majority of seats in both the lower and upper house. All the political force data are collected from the US Bureau of the Census Statistical Abstracts.

discretionary accruals ('*Election year*'). Second, these issues are likely more pronounced when the race is tightly contested, and so we entertain this possibility by controlling for the margin of victory in the election outcome ('*Election year tight margin*').

Third, Democrats are often perceived to be less business-friendly than Republicans, so unseating a Democrat incumbent and helping a more business-friendly challenger could be an incentive for downward earnings management. As such we entertain this possibility by controlling for election years in which the incumbent is a Democrat governor ('*Election year*

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Table 6
Testing for omitted variables and dispersed workforce.

	Control for Benefits in $t+1$ and $t+2$ (1)	*Interstate Sales > 70%* (2)	*Exclude Dispersed Industries* (3)
$Benefit_t$	-0.020*	−0.015**	−0.015**
	(1.79)	(2.03)	(2.18)
$Benefit_{t+1}$	0.018		
	(0.96)		
$Benefit_{t+2}$	−0.001		
	(0.07)		
$Abnormal\ accruals_t$	0.057**	0.063**	0.049*
	(2.30)	(2.57)	(1.88)
$Tap\ capital\ market_{t+1}$	0.003**	0.002*	0.003*
	(2.41)	(1.99)	(2.01)
$Meet\ or\ beat_{t+1}$	0.006***	0.004**	0.006***
	(4.49)	(2.50)	(3.42)
$Tight\ covenant_{t+1}$	0.004	0.003	0.003
	(1.56)	(1.40)	(0.88)
$Compensation_{t+1}$	0.007***	0.006***	0.006***
	(3.99)	(3.82)	(3.21)
$Assets_{t+1}$	−0.004**	−0.005*	−0.003
	(2.29)	(1.74)	(1.31)
Age_{t+1}	−0.000	0.000	0.000
	(0.04)	(0.45)	(0.41)
$Industry\ growth_{t+1}$	0.009**	0.011**	0.010**
	(2.35)	(2.52)	(2.47)
$Cash\ flow_{t+1}$	−0.333***	−0.330***	−0.348***
	(11.90)	(10.71)	(11.07)
$Big\ N_{t+1}$	0.000	0.003	−0.004
	(0.04)	(0.30)	(0.44)
$Tenure_{t+1}$	0.000	0.000	0.000
	(0.72)	(0.50)	(0.35)
$Leverage_{t+1}$	−0.059***	−0.062***	−0.058***
	(5.33)	(4.74)	(4.54)
$Book\ to\ market_{t+1}$	−0.009**	−0.009**	−0.008*
	(2.28)	(2.25)	(1.93)
$GDP\ growth_{t+1}$	0.067*	0.051	0.065
	(1.84)	(1.38)	(1.59)
$Unemp\ rate_{t+1}$	0.001	0.001	0.001
	(0.57)	(0.50)	(0.79)
$State\ stock\ rets_{t+1}$	0.006	0.003	0.008
	(1.46)	(0.76)	(1.58)
$State\ stock\ rets_t$	−0.002	−0.003	−0.002
	(0.53)	(0.60)	(0.47)
$State\ stock\ ret\ volatility_{t+1}$	−0.259	−0.225	−0.318
	(1.41)	(1.12)	(1.61)
$UI\ payroll\ tax_{t+1}$	−0.005	−0.004	−0.006
	(1.29)	(0.95)	(1.52)
Constant	0.047	0.155**	0.152**
	(0.51)	(2.30)	(2.05)
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	8272	7476	6893
Adj. R^2	0.526	0.530	0.526

The table presents coefficients, and *t*-statistics in parentheses, from pooled regressions of $Abnormal\ accruals_{t+1}$ on the independent variables listed. *Abnormal accruals* is from the Dechow-Dichev (2002) Model as modified in McNichols (2002, 65). Three specifications are estimated, as identified in the column header. In column 1, *Benefit* in year $t+1$ and $t+2$ are included. In column 2, firms with interstate sales less than 70% are excluded. Interstate sales are measured at the industry-state level as the percent of product shipments to non-neighboring states, based on the 2007 Commodity Flow Survey. In column 3, firms in the retail, wholesale, and transport industries, in which a large percentage of the work force is likely geographically dispersed, are excluded. Standard errors are clustered by state. *, (**), and [***] denote two-tailed statistical significance at 10%, (5%), and [1%] levels, respectively. Variable definitions are presented in Appendix B.

Democrat governor'). Fourth, if Democrats control both legislative houses, they are likely able to pass their policies more easily, which could provide a greater incentive for businesses to assist more business-friendly challengers in winning the (state) executive branch. As such we entertain this possibility by controlling for election years in which Democrats hold the majority of seats in both houses ('*Election year Democrat both houses*'). The results are shown in Panel C of Table 5. The main variable of interest in this study, *Benefit*, loads significantly in all specifications consistent with the hypothesis.

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Table 7
Cross-sectional tests on the importance of unemployment risk for wages.

	Labor Intensity		*Layoff Propensity*		*Percentage of Low-wage worker*	
	High (1)	*Low* (2)	*High* (3)	*Low* (4)	*High* (5)	*Low* (6)
$Benefit_t$	−0.025*	0.005	−0.019*	0.005	−0.027**	0.007
	(1.71)	(0.44)	(1.74)	(0.43)	(2.11)	(0.62)
$Abnormal\ accruals_t$	0.047**	0.046	0.048	0.067	0.035	0.067***
	(2.11)	(1.16)	(1.59)	(1.66)	(0.83)	(3.11)
$Tap\ capital\ market_{t+1}$	0.002	0.003	0.002*	0.004	0.004**	0.002
	(1.59)	(1.06)	(1.75)	(1.65)	(2.26)	(0.96)
$Meet\ or\ beat_{t+1}$	0.007***	0.005***	0.008***	0.005***	0.005*	0.007***
	(3.76)	(2.86)	(3.31)	(2.51)	(1.91)	(2.94)
$Tight\ covenant_{t+1}$	0.002	0.007*	0.004	0.004	0.005	0.002
	(0.73)	(1.91)	(1.02)	(0.97)	(1.40)	(0.64)
$Compensation_{t+1}$	0.008***	0.006***	0.008***	0.006***	0.008***	0.007***
	(3.74)	(2.75)	(4.38)	(2.54)	(2.73)	(2.83)
$Assets_{t+1}$	−0.011**	−0.001	−0.010***	0.003	−0.004	−0.004
	(2.60)	(0.50)	(3.32)	(0.74)	(0.80)	(1.18)
Age_{t+1}	0.001	−0.001	0.001	−0.001	0.001	−0.001
	(0.86)	(0.69)	(1.49)	(1.42)	(1.02)	(0.83)
$Industry\ growth_{t+1}$	0.007	0.009	0.001	0.015***	0.013**	0.006
	(1.40)	(1.49)	(0.13)	(3.36)	(2.28)	(1.00)
$Cash\ flow_{t+1}$	−0.314***	−0.371***	−0.343***	−0.322***	−0.282***	−0.382***
	(8.27)	(10.49)	(8.36)	(10.50)	(10.14)	(7.32)
$Big\ N_{t+1}$	0.024	−0.017*	−0.002	0.001	0.001	−0.009
	(1.03)	(1.92)	(0.13)	(0.11)	(0.10)	(0.64)
$Tenure_{t+1}$	0.000	0.000	0.000	0.000	0.000	−0.000
	(1.37)	(0.32)	(0.27)	(0.69)	(1.02)	(0.02)
$Leverage_{t+1}$	−0.082***	−0.041***	−0.043**	−0.089***	−0.054***	−0.079***
	(4.18)	(2.72)	(2.40)	(4.81)	(3.04)	(4.51)
$Book\ to\ market_{t+1}$	−0.005	−0.012**	−0.009*	−0.009	−0.016**	−0.002
	(1.10)	(2.19)	(1.82)	(1.57)	(2.52)	(0.51)
$GDP\ growth_{t+1}$	0.071	0.018	0.116**	0.006	0.016	0.110**
	(1.35)	(0.37)	(2.18)	(0.09)	(0.22)	(2.34)
$Unemp\ rate_{t+1}$	0.002	−0.000	0.001	0.001	−0.000	0.002
	(1.15)	(0.16)	(0.95)	(0.68)	(0.10)	(0.97)
$State\ stock\ rets_{t+1}$	−0.002	0.009	0.004	0.006	0.007	0.005
	(0.22)	(1.49)	(0.77)	(0.93)	(0.87)	(0.98)
$State\ stock\ rets_t$	−0.001	−0.003	−0.002	−0.002	−0.006	0.001
	(0.24)	(0.53)	(0.42)	(0.32)	(1.06)	(0.29)
$State\ stock\ ret\ volatility_{t+1}$	0.138	−0.712**	−0.176	−0.274	−0.181	−0.222
	(0.56)	(2.30)	(0.68)	(1.03)	(0.63)	(0.75)
$UI\ payroll\ tax_{t+1}$	−0.004	−0.001	−0.005	−0.005	−0.005	−0.005
	(0.59)	(0.35)	(0.86)	(1.23)	(0.93)	(1.02)
Constant	0.223	0.039	0.158	0.013	0.284**	−0.063
	(1.47)	(0.40)	(1.52)	(0.11)	(2.21)	(0.65)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4047	4225	4170	4102	3971	4301
Adj. R−squared	0.567	0.511	0.554	0.508	0.522	0.546

The table presents coefficients, and *t*-statistics in parentheses, from pooled regressions of $Abnormal\ accruals_{t+1}$ on the independent variables listed. *Abnormal accruals* is from the Dechow-Dichev (2002) Model as modified in McNichols (2002, 65). Six specifications are estimated, as identified in the column header. Columns 1 and 2 present results for samples stratified on the median of labor intensity, Columns 3 and 4 on the median of layoff propensity, and Columns 5 and 6 on the median of the fraction of low-wage workers. Labor intensity is calculated as the average ratio of total labor compensation to the total value of production at the three-digit NAICS level, using 1992–2010 data from the Bureau of Labor Statistics. Layoff propensity, obtained from Agrawal and Matsa (2013), is the average annual fraction of workers separated from work as part of a mass layoff, estimated at the three-digit NAICS level. The percentage of low-wage workers, obtained from David Matsa, is the fraction of workers who earn less than $50,000 in annual income, based on three-digit NAICS and the 2000 US population census. Standard errors are clustered by state. (**), and [***] denote two-tailed statistical significance at 10%, (5%), and [1%] levels, respectively. Variable definitions are presented in Appendix B.

4.2.4. Correlated omitted state-level economic conditions

A potential concern is that *unobserved* state and regional economic conditions that are correlated with *Benefit* subsume its relation with the reporting outcomes examined in the main tests. We address this concern in three ways. First, the research design in the main tests matches firms in treatment states with firms in contiguous states, thereby minimizing regional economic differences across firms.

Second, if unobserved state-level economic conditions have some persistence, they are likely to muddy the relation between lagged unemployment benefit changes and downward earnings management. In the first column of results

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in Table 6, we add $Benefit_{t+1}$ and $Benefit_{t+2}$, but observe that only $Benefit_t$ continues to load significantly (two-tailed p-value < 0.10) while future benefits do not load. This suggests it is unlikely $Benefit_t$ is simply capturing unobserved correlated economic conditions if these economic conditions have some persistence.

Third, firms with a high fraction of revenues from sales to out-of-state customers likely have financial reporting outcomes that are less sensitive to economic conditions in their headquarter state. Using the 2007 Commodity Flow Survey from the Bureau of Transportation Statistics, we measure interstate sales at the industry-state level as the percent of the value of product shipments from one state to non-neighboring states. Sales to neighboring states are excluded because these states are likely susceptible to similar economic conditions. The second column of results in Table 6 shows *Benefit* loads significantly (two-tailed p-value < 0.05) even among firms with interstate sales greater than 70%. The use of the 70% cutoff follows Agrawal and Matsa (2013), but results are robust to using 65% or 75% as the cutoff.

Collectively these tests mitigate concerns about correlated omitted state-level economic conditions driving the main result.

4.2.5. Geographically dispersed employees

Firms in the treatment sample are those headquartered in states with a large increase in unemployment benefits, and the main hypothesis is that these firms reverse prior upward earnings management when unemployment insurance becomes more generous. However, employees are covered by the unemployment benefit program in the state in which they work rather than the state in which their employer is headquartered, and many firms have geographically dispersed employees. While we expect this weakens the ability to reject the null hypothesis of no relation between unemployment benefit increases and earnings management reversal, we conduct a further test to address the issue. In particular, we exclude from the sample firms in retail, wholesale, and transport industries that likely have more geographically dispersed employees. As the third column of results in Table 6 shows, *Benefit* continues to load significantly (two-tailed p-value < 0.05) after the exclusion.

4.2.6. Abnormal accrual estimates

Owens et al. (2014) examine discretionary accrual estimates and suggest they might be affected by business model shocks. While all our results are robust to the use of model-free earnings management measures as described earlier, we follow the suggestion in Owens et al. (2014) and exclude firm-years with unsigned market-adjusted stock returns greater than 20% in any month within the last three years to mitigate concerns about business model shocks. We then re-estimate the regression specification in Table 4 with abnormal accruals as the dependent variable and find in untabulated results that *Benefit* loads significantly negatively (two-tailed p-value < 0.10) as predicted.

4.2.7. Loss incidence

The hypothesis tested in this section is that a reduction in earnings (through negative discretionary accruals) becomes more likely when unemployment benefits increase. An implication is that if the earnings reduction happens to cross the zero earnings threshold, reported losses become more likely as above. We re-estimate the regression specification in Table 4 with $Loss_{t+1}$ as the dependent variable, which is an indicator equal to 1 if the firm reports a loss and equal to 0 otherwise, and find in untabulated results that *Benefit* loads significantly positively (two-tailed p-value < 0.10). This indicates that after the increase in *Benefit* (when unemployment risk is lower), the frequency of reported losses increases more for treatment firms relative to control firms, as predicted.

5. Cross-sectional tests

In this section we test a number of cross-sectional predictions derived from the main hypothesis. These tests invoke variation in the relative importance of unemployment risk as a determinant of firms' labor cost, and variation in other earnings management incentives.

5.1. Labor intensity

This hypothesis is motivated by the idea that upward earnings management to manage employee perceptions of employment security is more likely at firms that are more labor intensive. As such, we expect greater earnings management reversal at more labor intensive firms following unemployment benefit increases. Labor intensity is calculated as the average ratio of total labor compensation to the total value of production at the three-digit NAICS level, using data from 1992 to 2010 from the United States Bureau of Labor Statistics. As the first pair of columns in Table 7 shows, *Benefit* loads significantly negatively (two-tailed p-value < 0.10) for firms with high (above median) labor intensity but does not load for firms with low (below median) labor intensity. This suggests greater reversal of prior upward earnings management at more labor intensive firms, as hypothesized.

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Table 8
Cross-sectional tests on the effect of other earnings management incentives.

	Tap capital market$_{t+1}$		*Meet or beat*$_{t+1}$		*Tight covenant*$_{t+1}$		*Compensation*$_{t+1}$		*Composite*$_{t+1}$	
	High (1)	*Low* (2)	*Yes* (3)	*No* (4)	*Yes* (5)	*No* (6)	*High* (7)	*Low* (8)	*High* (9)	*Low* (10)
Benefit$_t$	−0.004	−0.008*	−0.005	−0.020**	−0.018	−0.012*	−0.006	−0.019	−0.004	−0.028*
	(0.28)	(1.86)	(0.21)	(2.40)	(0.47)	(1.69)	(0.36)	(1.38)	(0.32)	(1.92)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4136	4136	1870	6402	1569	6703	4136	4136	4136	4136
Adj. R^2	0.549	0.383	0.720	0.326	0.506	0.539	0.451	0.685	0.466	0.637

The table presents coefficients, and t-statistics in parentheses, from pooled regressions of *Abnormal accruals*$_{t+1}$ on the independent variables listed. *Abnormal accruals* is from the Dechow-Dichev (2002) Model as modified in McNichols (2002, 65). *Controls* include all controls listed in Table 4. *Benefit* is the log of maximum total benefits. *Tap capital market* is the principal component of future equity issuance and debt issuance. *Meet or beat* is an indicator equal to one if net income before extraordinary items scaled by total assets lies in [0, 0.005), change in net income before extraordinary items scaled by total assets lies in [0, 0.005), or EPS beats analyst forecasts by one cent per share or less. *Tight covenant* is an indicator set to one if the slack of a firm's tightest covenant falls into the lowest decile of slack. *Compensation* is the principal component of log of one plus mean cash compensation paid to top five executives, log of one plus mean delta of the top five executives, log of one plus mean vega of the top five executives. *Composite* is a combined measure of the strength of *Tap capital market*, *Meet or beat*, *Tight covenant*, and *Compensation*. Results are reported for ten separate regressions estimated on subsamples identified in the column headers. The sample is split (*High* or *Low*) on the median of the variable in the column header for continuous variables, and on the presence of the indicator (*Yes* or *No*) for indicator variables in the column header. Standard errors are clustered by state. * and (**) denote two-tailed statistical significance at 10% and (5%) levels, respectively. Variable definitions are presented in Appendix B.

5.2. Layoff propensity

This hypothesis is motivated by the idea that unemployment insurance is likely more important to employees in industries with high layoff propensity. As such, firms in these industries are predicted to engage in greater upward earnings management when unemployment benefits are low. We therefore predict greater reversal of prior upward earnings management at such firms when there is an increase in unemployment benefits. Layoff propensity at the three-digit NAICS level is the average annual fraction of workers separated from work as part of a mass layoff, and is obtained from Agrawal and Matsa (2013). As the second pair of columns in Table 7 shows, *Benefit* loads significantly negatively (two-tailed p-value < 0.10) for firms in industries with high (above median) layoff propensity, but does not load for firms in industries with low (below median) layoff propensity. This suggests greater reversal of prior upward earnings management at firms with higher layoff propensity, as hypothesized.

5.3. Percentage of low-wage workers

This hypothesis is motivated by the idea that unemployment insurance generosity is likely more important to low-wage workers because these workers have lower savings at the time of job loss (Browning and Crossley, 2001; Bloemen and Stancanelli, 2005). As such, firms with a higher proportion of low-wage workers are more likely to engage in upward earnings management when unemployment benefits are less generous, and we therefore predict a larger reversal of prior upward earnings management for such firms when unemployment benefits become more generous. The fraction of low wage workers is calculated at the three-digit NAICS level and is defined as the fraction of workers earning less than $50,000 annually based on the 2000 Census. This data is obtained from Professor David Matsa. As the third pair of columns in Table 7 shows, *Benefit* loads significantly negatively (two-tailed p-value < 0.05) for firms with a high (above median) fraction of low-wage workers, but does not load for firms with a low (below median) fraction of low-wage workers. This suggests greater reversal of prior upward earnings management at firms with more low-wage workers, as hypothesized.

5.4. Other earnings management incentives

Other upward earnings management incentives known from the prior literature could offset the incentive to partially unwind prior upward earnings management resulting from an exogenous decrease in unemployment risk. In such a case we would not observe the predicted effect. In particular, firms expecting to raise equity or debt in the upcoming year likely have an incentive to manage earnings upward in order to appear more financially robust, and we capture this incentive through the variable *Tap capital market*. Firms on the cusp of meeting or beating analyst forecasts likely have an incentive to manage earnings upward in order to avoid adverse, or generate positive, stock price reaction, and we capture this incentive through the variable *Meet or beat*. Firms on the verge of violating a debt covenant likely have an incentive to manage earnings upward, and we capture this incentive through the variable *Tight covenant*. Finally, managers at firms with executive compensation structures that are more sensitive to earnings likely have an incentive to manage earnings upward, and we

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capture this incentive through the variable *Compensation*. We further combine these incentives into a composite incentive variable, *Composite*. Detailed variable definitions are presented in Appendix B. We expect that *Benefit* loads significantly when these incentives to maintain upward earnings management are weak.

The sample is partitioned based on whether *Tap capital market* is above the median, whether the indicator variable *Meet or beat* is equal to one, whether the indicator variable *Tight covenant* is equal to one, whether *Compensation* is above the median, and whether *Composite* is above the median. We then estimate a regression of abnormal accruals on *Benefit* (our independent variable of interest) including as controls all variables listed in column 1 of Table 4, Panel A. The regressions are estimated in subsamples with high versus low incentives, and results are shown in Table 8. As the table shows, *Benefit* loads significantly when other earnings management incentives are weak, but not when they are strong, in all cases except *Compensation* (in the case of the compensation incentive the sign of the *Benefit* coefficient is in the predicted direction and marginally significant at 10% in a one-tailed test when *Compensation* is low).

Collectively the cross-sectional tests shed further light on the relation between unemployment benefits and earnings management, and reinforce the main results.

6. Conclusion

A long-standing interest in accounting research has been to identify and explain the forces that shape managers' financial reporting choices. The literature has provided evidence of the role of explicit contracts such as debt (Watts and Zimmerman, 1986; DeFond and Jiambalvo, 1994; Dichev and Skinner, 2002) and executive compensation contracts (Healy, 1985), and the role of implicit contracts such as those with shareholders (Graham et al., 2005; Badertscher et al., 2012) and the general public (Watts and Zimmerman, 1986). To date however there is less evidence on the potential role of rank and file employees in influencing financial reporting choices. We contribute to the literature by providing new evidence in this regard.

Employees bear substantial costs in the event of involuntary unemployment and therefore require an unemployment risk wage premium in competitive labor markets, as documented by a large labor economics literature. Firms bear substantial costs of exposing workers to unemployment risk, and these costs can take the form of compensating wage differentials, turnover costs, and search costs. As such, we expect firms have an incentive to engage in ongoing upward earnings management in order to project an appearance of security and mitigate their costs of exposing workers to unemployment risk. The ongoing nature of this hypothesized earnings management makes it more difficult to test empirically. Our contribution is to identify a setting that facilitates a more powerful test.

Employees' unemployment risk (and the firm's cost of exposing employees to unemployment risk) is increasing in (i) the unemployment probability, (ii) employee risk aversion, and (iii) costs borne by workers during unemployment. These determinants of unemployment risk motivate our empirical identification strategy – we expect firms' earnings management incentives *change* when there is an exogenous *shock* to employees' unemployment risk through a decrease in the costs borne by workers during unemployment.

In particular, we test for partial reversals of prior upward earnings management around increases in state-level unemployment benefits. The notion is that when employees have better insurance against unemployment (i.e., when they are better protected in the event of unemployment), they likely become less sensitive to their employer's performance. This likely reduces firms' incentive to engage in upward earnings management, leading to the prediction of a reversal of prior upward earnings management when unemployment benefits increase. Changes in unemployment benefits occur at the state level and are therefore likely exogenous to individual firms.

Four measures of earnings management are examined: a model-implied estimate of abnormal accruals; and three model-free measures including special items, write downs, and income-reducing restatements. The empirical tests provide consistent evidence of a significant reduction in abnormal accruals, and an increase in negative special items, write downs, and restatements. A number of further tests enhance confidence in identification of the main effect and indicate robustness. Cross-sectional tests suggest greater upward earnings management at firms that are more labor intensive, have higher layoff propensity, have a higher fraction of low-wage workers, and have weaker incentives for upward earnings management stemming from the need to tap capital markets, meet or beat earnings expectations, avoid debt covenant violations, or inflate executive compensation, as predicted.

Collectively the results provide new evidence on the role of rank and file employees in shaping managers' financial reporting choices, and help round out our understanding of the forces that shape financial reporting.

Appendix A. Economic significance of the benefit increase

We calibrate the dollar magnitude of implied wage savings as a result of the reduction in unemployment risk from a large 10% increase in unemployment benefits (*Benefit*) as follows.

As in Agrawal and Matsa (2013), we use the model in Topel (1984):

$$\textit{Ln weekly wage} = 2.532 \times \textit{Unemployment rate} - 2.583 \times \textit{Unemployment rate} \times \textit{UI replacement rate} + \textit{Controls} \quad \text{(A1)}$$

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Table B1
Variable definitions.

Variable	Definition	Source
Earnings management variables		
Abnormal accruals	The residuals from the modified Dechow-Dichev model. The model is estimated for each three-digit SIC industry with more than 30 observations as follows: $Accruals_t = \alpha + \beta_1 CFO_{t-1} + \beta_2 CFO_t + \beta_3 CFO_{t+1} + \beta_4 \Delta Sales_t + \beta_5 PPE_t + \varepsilon_t$, (1) where $Accruals_t$ is income before extraordinary items (ib) minus operating cash flows (oancf after 1988 or ib − Δact + Δche + Δlct − Δdlc + dp before 1988). CFO_t is operating cash flows in year t. $\Delta Sales_t$ is the change in sales (sale) from the previous year to the current year, and PPE_t is the end of year property, plant and equipment (ppegt). All variables are scaled by lagged total assets (at).	Compustat
Special items	Special items (spi) divided by total assets (at).	Compustat
Write down	Total write-down (wda) divided by total assets (at).	Compustat
Restatement	An indicator equal to one for restatements that reduce net income.	Audit Analytics
Test variable		
Benefit	Log of maximum total benefits in year $t-1$ calculated as the product of the maximum weekly benefit amount and the maximum duration allowed in year t-1.	Bureau of Labor Statistics
Other incentive variables		
Tap capital market	The first principal component of *ISSUE_EQ* (future increase in equity capital in next year scaled by total assets, 0 otherwise) and *ISSUE_DEBT* (future increase in debt capital in next year scaled by total assets, 0 otherwise), following Carter et al. (2007, Table 1).	Compustat
Meet or beat	An indicator equal to one if net income before extraordinary items scaled by total assets lies in [0, 0.005), change in net income before extraordinary items scaled by total assets lies in [0, 0.005), or EPS beats analyst forecasts by one cent per share or less, following Cohen et al. (2008, Table 5).	Compustat I/B/E/S
Tight covenant	An indicator equal to one if the slack of a firm's tightest covenant falls into the lowest decile of slack. We follow Demerjian and Owens (2014, Table 4) to calculate each covenant variable. For max. threshold covenants, slack = (max. threshold − actual)/max threshold. For min. threshold covenants, slack = (actual − min. threshold)/min. threshold. Negative slack is deleted.	Compustat Dealscan
Compensation	The first principal component of log of one plus mean cash compensation paid to the top five executives, log of one plus mean delta of the top five executives (Core and Guay 2002), and log of one plus mean vega of the top five executives (Core and Guay 2002). The three variables are defined following Armstrong et al. (2013).	Execucomp
Composite	A composite index of other earnings management incentives, calculated as follows. We rescale the continuous variables *Tap capital market* and *Compensation* to lie between 0 and 1, to make their scales comparable to the indicator variables *Meet or beat* and *Tight covenant*. We then take the average of the four variables as the composite index.	Compustat I/B/E/S Dealscan Execucomp
Firm variables		
Assets	Log of total assets (at).	Compustat
Age	The number of years since firm IPO	Compustat
Industry growth	The sum of sales ($sale_t$) in a two-digit SIC industry divided by the sum of sales last year. ($sale_{t-1}$)	Compustat
Cash flow	Operating cash flow (oancf after 1987 or ib − Δact + Δche + Δlct − Δdlc + dp before 1987) scaled by total assets (at).	Compustat
Big N	An indicator set to one if the auditor is a Big N auditor, and set to zero otherwise	Compustat
Tenure	The number of consecutive years that the firm has retained the auditor	Compustat
Leverage	Debt in current liabilities (dlc) plus long-term debt (dltt) scaled by total assets (at).	Compustat
Book to market	The book value of equity (ceq) divided by the market value (prcc_f*csho).	Compustat
State variables		
GDP growth	State-level GDP growth rates.	Census Bureau
Unemp rate	State-level unemployment rates.	Bureau of Labor Statistics
State stock rets	Value-weighted average annual stock returns of firms headquartered in a state.	Compustat/CRSP
State stock ret volatility	The standard deviation of annual stock returns of firms headquartered in a state.	Compustat/CRSP
UI payroll tax	Log of average UI payroll tax rate multiplied by taxable wage	Bureau of Labor Statistics

UI replacement rate is defined as $\frac{b_i}{w_i(1-t_i)}$, where b_i is weekly benefit amount for which the individual qualifies, w_i is his average weekly wage, and t_i is the relevant marginal tax rate on earned income.

Our objective here is to evaluate the implied reduction in weekly wages due to a reduction in unemployment risk that results from a 10% increase in unemployment benefits, at the mean unemployment rate. We proceed as follows:

(i) We estimate the change in the UI replacement rate (=UI replacement rate in the event year minus that in the pre-event year) for firms in our Treatment sample using the replacement rate formula above. We collect wage data from the Current Population Survey of the Bureau of Labor Statistics, impute UI benefits for individuals based on the provisions of the UI law in the person's state of residence, and estimate marginal tax rates by using federal and state tax tables, marital status, standard deductions and personal exemptions. This leads us to estimate a mean change in the UI replacement rate of 0.042.

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(ii) The mean unemployment rate for our Treatment sample, using data we collect from the Bureau of Labor Statistics, is 0.058 (or 5.8%). Recall our Table 2 shows no significant change in the unemployment rate for the Treatment sample in the event year over the pre-event year.
(iii) Using Eq. (A1) above in which the dependent variable is in logs, the Treatment sample experiences an implied wage reduction of $100 \times 2.583 \times (0.058 \times 0.042) = 0.63\%$.
(iv) The mean ratio of wage expense to sales using data from the Bureau of Labor Statistics (BLS) is 0.41, and the mean sales for our Treatment sample firms is $1725 million. Therefore, the implied wage reduction for our Treatment sample after the benefit increase is $0.63\% \times 0.41 \times 1725 = \$4.5$ million (wage expense is reported by only 10% of Compustat firms, hence the need to infer wages through the wage to sales ratio. In fact we find that the wage to sales ratio is 0.40 for our Treatment sample firms with reported wage expense in Compustat, confirming the BLS figure);
(v) The mean earnings in our Treatment sample is $56.7 million, so the implied wage savings equals $4.5/56.7 = 8\%$ of earnings.

Appendix B

See Table B1.

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