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Employee turnover likelihood and earnings management:

evidence from the inevitable disclosure doctrine

Huasheng Gao, Huai Zhang, and Jin Zhang*

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Abstract

We present evidence that managers consider employee turnover likelihood in their accounting choices. Our tests exploit U.S. state courts' staggered recognition of the inevitable disclosure doctrine (IDD), which reduces employees' ability to switch employers. We find a significant decrease in upward earnings management for firms headquartered in states that recognize the IDD, relative to firms headquartered elsewhere. The effect of the IDD is stronger for firms relying more on human capital and for firms whose employees have higher ex-ante turnover likelihood, confirming the employee retention channel. Overall, our results support the view that retaining employees is an important motive for corporate earnings management.

Keywords: Earnings management, employee turnover likelihood, inevitable disclosure doctrine.

JEL Classification: M41; J01

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*Huasheng Gao is from Fanhai International School of Finance, Fudan University (Email: huashenggao@fudan.edu.cn). Huai Zhang is from Nanyang Business School, Nanyang Technological University (Email: huaizhang@ntu.edu.sg). Jin Zhang is from Monash Business School, Monash University (Email: jin.zhang@monash.edu).

1. Introduction

Firms provide employees with a package of explicit and implicit claims. Explicit claims refer to explicit employment contracts, while implicit claims represent tacit promises about long-run working conditions, continued employment, and career advancement opportunities (Cornell and Shapiro 1987). Research (Bowen et al. 1995; Burgstahler and Dichev 1997; Matsumoto 2002; Cheng and Warfield 2005) offers evidence that firms make long-term income-increasing accounting choices to project financial security, which reduces the expected cost of employee hiring and retention by elevating the value of employees' implicit claims. This evidence implies that firms' earnings management incentives vary with the expected cost of employee hiring and retention changes. Since the expected cost increases with employee turnover likelihood, we hypothesize that exogenous shocks to employee turnover likelihood affect firms' earnings management. We test this hypothesis through the U.S. state courts' staggered recognition of the inevitable disclosure doctrine (IDD).

The IDD is a legal doctrine adopted by state courts to enhance the legal protection of trade secrets for firms headquartered in each respective state. The IDD maintains that, if the new employment of a departing employee would inevitably lead to the disclosure of the firm's trade secrets and cause the firm irreparable harm, state courts can prevent the employee from accepting a job offer or limit his or her responsibility in the new firm.

Not surprisingly, the adoption of the IDD reduces the turnover likelihood of those employees who have access to trade secrets (Seaman 2015). Such employees play an important role in firms' operations, offering invaluable and sometimes irreplaceable human capital to the firm. Firms are likely to consider these employees' turnover likelihood when determining financial accounting policies. When these employees' outside opportunities are restricted by the IDD, they become less sensitive to their employer's financial performance and their firms have lower incentives to manipulate earnings upwards. This observation forms the basis for our empirical tests: we predict that a state's adoption of the IDD leads to a decrease in discretionary accruals for firms headquartered in the state, relative to control firms (i.e., firms headquartered in states that do not experience any change in the IDD recognition).

Another way to understand our prediction is as follows. Before the adoption of the IDD, firms engage in upward long-term earnings management to attract and retain employees, which results in bloated balance sheets. The adoption of the IDD reduces firms' incentives to do so, and firms take this opportunity to unwind prior earnings management, at least partially, to reduce the bloat.

We analyze the IDD setting for the following two reasons. First, the motivation behind the IDD centers around state courts' determination to improve the protection of trade secrets for firms located in the respective state by reducing the risk that former employees reveal a firm's trade secrets to other firms (Harris 2000; Godfrey 2004). As the court's IDD decision is unrelated to firm-specific characteristics and is not intended to curb earnings management, it offers an arguably exogenous shock in the turnover likelihood of key employees. Second, the staggered adoption of the IDD in several U.S. states enables us to identify the IDD's effect in a difference-in-differences framework. Because multiple shocks affect different firms at different points, we can avoid the alternative explanation applicable to settings with a single shock that a contemporaneous event drives our results (Roberts and Whited, 2013).

Using a sample of 94,912 firm-year observations for the period 1987–2011 and a difference-in-differences approach, we empirically explore the impact of employee turnover likelihood on earnings management. We find that, on average, when a state recognizes the IDD, firms headquartered there experience a reduction of 0.9 percentage points in performance-matched discretionary accruals, relative to control firms. This effect is economically important, considering that the sample average value of discretionary accruals is only -0.77 percentage points.

The IDD offers better protection for firms' trade secrets. Therefore, firms may increase their investment in intellectual assets, either through elevated R&D expenditures or through acquisitions of other firms with such assets. Both R&D expenses and the amortization of goodwill reduce income and possibly result in ostensibly lower discretionary accruals. This concern is alleviated through our research design, since we control for R&D expenses and firm M&A in our regression analyses. Investments in intellectual assets may also increase firms' depreciable capital (e.g., property, plant and equipment).[1] Our discretionary accruals are computed using the modified Jones (1991) model, where property, plant and equipment is considered in our estimation of nondiscretionary accruals. The construction of our discretionary accruals implies that our conclusion is unaffected by firms' investment in depreciable capital.

To draw causal interpretations from our estimation, we need to assume that, absent the adoption of the IDD, earnings manipulations in treated firms (firms located in states that adopted the IDD) would have evolved in the same way as that in control firms. This assumption is inherently untestable, because we don't observe the counterfactual. However, we can obtain peripheral evidence by examining pre-treatment trends. We find that the trends are indistinguishable between treated firms and control firms, which adds to our confidence in the validity of our approach. Moreover, our results show that the impact of the IDD on discretionary accruals mainly occurs after the state policy change takes effect, consistent with the causal effect interpretation.

To increase our ability to make causal inferences, we examine changes in our earnings management measure within a short window around the adoption of the IDD. Our conclusion continues to hold. In addition, we focus on the three states that reject their previously adopted IDD. If our hypothesis is true, we expect that firms in the three states will show higher discretionary accruals after the rejection of the IDD. Consistent with this prediction, we show that a state's rejection of its previously adopted IDD leads to an increase in discretionary accruals, and the economic magnitude of IDD rejection is comparable to that of IDD adoption.

We are concerned that our results are driven by local economic conditions that affect both the adoption of the IDD and the subsequent earnings management. To address this concern, we impose the restriction that the control firm be located within a short distance of the treated firm, essentially focusing on firms located on state borders. Since both types of firms are geographically close and are thus influenced by the same local economic conditions, if the local conditions drive our results, we

[1] For example, firms may increase their investment in laboratory equipment.

expect our results to disappear in this sample. Contrary to this expectation, our results continue to hold, indicating that local economic conditions are unlikely to drive our results.

Our hypothesis is based on the notion that the IDD limits employees' outside job opportunities. If the doctrine indeed increases employees' cost of switching employers, its adoption effectively lowers employees' bargaining power and presumably results in less favorable employment contracts. Since compensation is an important part of the employment contract, we study how the adoption of the IDD affects compensation to employees. Our analysis focuses on CEOs and other top-paid managers (who are likely to have access to trade secrets and thus be affected by the IDD). We show that these employees experience a significant pay cut after the IDD is adopted, consistent with the notion that the doctrine reduces their bargaining power.

We continue to corroborate our results by examining three alternative measures of earnings management. We find that, after the adoption of the IDD in a particular state, firms in that state are more likely to write down their assets, report negative special items, and make income-decreasing restatements. These results are consistent with the notion that firms unwind prior upward earnings management, when the IDD lowers their incentives to attract employees through earnings manipulations.

By restricting employees' mobility, the adoption of the IDD could improve local firms' profitability, reducing their incentives to manipulate earnings upwards. If so, we expect that our results disappear, after we control for future profitability, and that the adoption of the IDD increases firms' earnings. These expectations receive no support from our empirical tests.

Since the adoption of the IDD may change employees' incentives to work for the firm, we also analyze how the IDD influences employees' productivity. While the IDD may encourage employees to work harder by restricting their job opportunities to the current employer, it may also lower their incentives to perform, because it disrupts the labor market and employees' human capital can't fetch a fair price. We use three measures of employee productivity (income per employee, number of patents, and number of patent citations), and our results suggest that the IDD has either no impact or a negative impact on employees' productivity.

To test whether our results are indeed tied to employee retention, we conduct several subsample analyses. The incentive to manipulate earnings to retain employees is stronger when human capital is important to the firm and when employees' ex-ante turnover likelihood is high. Therefore, we expect that an exogenous change in turnover likelihood, prompted by the adoption of the IDD, leads to a more pronounced response for firms relying more on human capital and those whose employees have higher ex-ante turnover likelihood.

Human capital is likely to matter more for firms with more intangible assets, with high knowledge worker intensity, and with high inventor intensity (Bowen et al 1995; Gao and Zhang 2017). Consistent with our expectation, we find that the impact of the IDD is significantly more pronounced in the subsample with high intensities of intangible assets, "knowledge workers" (workers whose occupation is in the category of management, professional, and related occupations), and inventors (workers who produce patents). The recognition of the IDD reduces discretionary accruals by 1.8/0.4 percentage points for firms with high/low intangible assets intensity, by 1.3/0.2 percentage points for firms with high/low knowledge worker intensity and by 1.8/0.1 percentage points for firms with high/low inventor intensity.

We use the firm's pension plan policy and number of industry peer firms as two proxies for turnover likelihood. Employees in firms with a defined benefit pension plan face higher costs of switching employers, because a defined benefit plan is less portable (Ippolito 1985; Poterba et al. 2007). Moreover, it is more difficult for employees to switch jobs when there are few industry peer firms (Deng and Gao 2013; Gao et al. 2015). Consistent with our expectation, we find that the impact of the IDD is significantly more pronounced in the subsample with high ex-ante turnover likelihood. The recognition of the IDD reduces discretionary accruals by 1.1/0.3 percentage points for firms without/with a defined benefit pension plan and by 1.3/0.4 percentage points for firms with a large/small number of industry peers.

We conduct several additional analyses. For the sake of brevity, we choose not to tabulate them but briefly discuss our findings here. We find that our results are robust to alternative discretionary accrual measures based on the Jones model (Jones 1991), the Dechow and Dichev (2002) model, and the performance-matching approach advocated by Kothari et al. (2005). Moreover, the effect of the IDD is arguably more pronounced for firms whose managers have strong incentives to manage earnings to retain employees. Poorly performing firms fit this description, because their employees face higher risk of unemployment and are more likely to pay attention to financial results. Consistent with this argument, we show that the effect of the IDD is greater for firms with lower profitability.

Our study contributes to the literature by offering evidence that firms consider the labor market mobility of key employees when making financial accounting choices. We show that, when the turnover likelihood of these employees is reduced by a state's recognition of the IDD, firms have lower incentives to manipulate earnings upwards. Our study therefore extends the literature on the determinants of financial accounting choices and the literature on the influence of stakeholders (Bowen et al 1995; Burgstahler and Dichev 1997; Matsumoto 2002; Cheng and Warfield 2005; Dou et al. 2016).

Moreover, our paper has important policy implications. Although about 20 of the 50 U.S. states have adopted the IDD, legislators in the remaining states are still debating whether to follow suit, partially because the impacts of the IDD on the economy are still unclear. Our paper adds to the recent work on the economic impact of the IDD. Research shows that the adoption of the IDD reduces employee turnover likelihood and increases firm leverage and acquisition likelihood (Png and Samila 2015; Chen et al. 2018; Klasa et al. 2018). Our study contributes to the line of literature by providing evidence that this legislation (unintentionally) reduces corporate upward earnings management.

Note that our study closely relates to two papers. Ali et al. (2015) examine the impact of the adoption of the IDD. There are, however, two major differences. First, the two papers focus on distinct research questions. We examine how the adoption of the IDD affects earnings management, while Ali et al. (2015) study how it affects the asymmetric disclosure of bad versus good news. In their setting, managers do not manipulate earnings, news is exogenously determined, and managers strategically withhold news according to its nature. Second, we develop different hypotheses. We hypothesize that the adoption of the IDD reduces employees' turnover likelihood and therefore managers have lower incentives to manipulate earnings upwards. Ali et al. (2015) hypothesize that the IDD reduces managers' outside opportunities and therefore provides these managers with greater incentives to portray their performance in a positive light by withholding bad news.

Similar to our paper, Dou et al. (2016) examine earnings management to retain employees. Both papers analyze shocks to the turnover likelihood of employees. Dou et al. (2016) examine how exogenous changes in unemployment benefits affect earnings management, while we analyze how the adoption of IDD shapes financial reporting choices. However, two key differences exist. Employees affected by unemployment benefits are likely ordinary rank-and-file employees who face substantial unemployment risks, while employees affected by the IDD are key employees who have access to firms' trade secrets. Therefore, the results of Dou et al. (2016) do not readily apply to our setting, as the type of employees affected is distinct. Since employees influenced by the IDD are likely to be more valuable to firms than those facing unemployment risks, the adoption of the IDD may have a greater effect on firms' financial accounting choices than the unemployment benefits.

In addition, while both papers examine the likelihood of employees leaving their jobs, the underlying mechanisms are dissimilar. Unemployment benefits matter when employees are laid off. In contrast, the IDD reduces the turnover likelihood by discouraging job hopping. To illustrate the difference, consider two employees. One of them performs poorly and is likely to be fired. The other has a stellar performance and is likely to be lured away by peer firms. Although both have an equally high turnover likelihood, the different reasons render it difficult to treat them equivalently. We therefore contribute to the literature from a novel perspective.

The rest of the paper proceeds as follows. Section 2 describes the institutional details of the IDD. Section 3 details our hypotheses. Section 4 covers methodology, variable measurement, and sample formation. Section 5 discusses our empirical results, and Section 6 concludes.

2. Intuitional details on trade secrets and the IDD

Trade secrets are central to a firm's competitive advantage and are often considered the crown jewels of its intellectual capital developed over many years through myriad interactions and projects (Jorda 2007). In U.S. public firms, trade secrets have been estimated to be worth $5 trillion and account for two-thirds of the value of firms' intangible assets (Bird and Jain 2008; U.S. Chamber of Commerce 2014). A trade secret is broadly defined as valuable business information that is not generally known and is subject to reasonable efforts to preserve confidentiality. Examples of trade secrets include software, techniques, business plans, designs, and details about customers and suppliers.

The IDD is a legal doctrine adopted by state courts to enhance the legal protection of trade secrets for firms located in the state when an employee will inevitably use or disclose knowledge of such trade secrets in her new employment. The IDD maintains that, if the new employment would inevitably lead to the disclosure of the firm's trade secrets to competitors and cause the firm irreparable harm, state courts can prevent the employee from working for the firm's competitor or can limit the worker's responsibility in the new firm. Under the IDD, a firm's suit can be based on the threats of irreparable harm (even though the actual harm has not occurred), as long as the firm can show that (1) the departing employee had access to its trade secrets, (2) the employee's duty in the new firm would inevitably lead to the disclosure of trade secrets, and (3) the disclosure of the trade secrets would lead to irreparable economic harm to the firm. Furthermore, the firm does not need to establish any actual wrongdoing by the employee or disclose the actual details of the underlying trade secrets in the lawsuit. As described by Malsberger (2004) and Garmaise (2011), the relevant jurisdiction for a trade secret-related lawsuit typically resides in the state where the job-hopping employee's former employer is headquartered. As a result, once the IDD is adopted, employees of firms headquartered in the state face limited outside employment opportunities.

The legal case between National Starch and Chemical Corporation (referred to as NSCC) and Vincent Lauria is a classic example in which the court applied the IDD. In 1986, NSCC sought an injunction against its former employee, Vincent Lauria, from working for Parker Chemical Corp. (referred to as PCC). Both companies produce envelope adhesives and are direct competitors. During his nine-year employment at NSCC, Lauria was involved in the development of complex envelope adhesives, and he was hired for a similar position in PCC. NSCC argued that, no matter how hard he tried to avoid it, Lauria could not help but use NSCC's trade secrets (in this case, product formulas) in his new position and that disclosing these trade secrets would give PCC an unfair advantage. Though there was no evidence that Lauria took any physical materials from NSCC, the court found the situation a classic case of inevitable disclosure and prohibited Lauria from working for PCC.

We focus on the IDD in our paper, because it is much more effective in reducing employee turnover likelihood than either a nondisclosure agreement (NDA) or a noncompetition covenant (NCC). First, an NDA or NCC is more likely to be included in contracts with top management than in contracts with non-executive employees. Even among top executives, not everyone agrees to include an NDA or NCC in the contract. For example, Garmaise (2011) shows that 30% of his sample firms do not have an NCC in their contracts with top executives. However, the IDD is applicable, regardless of whether the employee's contract includes an NDA or NCC. Second, an NDA or NCC usually has specific geographic restrictions; the scope of enforceable NDA/NCC is often within a county or a city or within a 10- or 50-mile radius around the place of business. In contrast, the IDD typically can be enforced within a much broader geographic scope. Third, the IDD has greater enforceability, since it allows state courts to grant an injunction if employment at the rival firm is expected to inevitably lead to disclosure of trade secrets (but before the actual disclosure). Enforcing an NDA, on the other hand, requires detecting and proving actual disclosure, which tends to be costly and difficult.

We collect the details of IDD adoptions from Klasa et al. (2018). As shown in Table 1, 21 states adopted the IDD once by the end of our sample period. The literature shows that the adoption of the IDD indeed leads to a significant reduction in labor market mobility of employees. For example, Png and Samila (2015) examine job hopping by engineers and scientists and find that the IDD inhibits rival firms' poaching of these employees. Klasa et al. (2018) employ individual employees' information from the Census Bureau and find that the IDD significantly reduces the probability that an employee job-hops to other companies. Based on a sample of corporate executives in the S&P

1500 firms, Chen et al. (2018) find that the IDD leads to a significant reduction in job hopping among these corporate executives.

Finally, it is also worth pointing out that the change in a state's IDD policy can largely be regarded as exogenous in our context of earnings management tests. When considering the adoption of the IDD, state courts mainly aim to achieve a balance between the companies' interests of stronger protection of trade secrets and the employees' interests of labor market freedom (Harris 2000; Godfrey 2004). In other words, given that the primary purpose of the IDD policy change is either to better protect firms' trade secrets or to better protect employees' employment freedom, the change in firms' earnings management is likely to be an unintended consequence of these policy changes. Moreover, unlike other state laws whose adoption can be influenced greatly by interest groups, such as labor unions and companies, the adoption of the IDD largely depends on judicial decisions based on the merits of the specific case. Further, state judges who make the ruling are deemed to be independent of the state and federal government and largely immune to lobby groups and political pressure (Klasa et al., 2018). In summary, the staggered adoption of the IDD is unlikely to be triggered by factors that drive corporate earnings management.

3. Hypothesis development

Firms can be considered as a nexus of contracts. A large body of literature examines how contracts shape firms' accounting choices. Watts and Zimmerman (1986), DeFond and Jiambalvo (1994), and Dichev and Skinner (2002) study the impact of explicit debt contracts. Healy (1985) analyzes the effect of explicit executive compensation contracts with executives, while Graham et al. (2005) and Badertscher et al. (2012) document the influence of implicit contracts with equity investors.

Numerous studies examine how compensation contracts with employees affect firms' accounting choices. The hypothesis is that firms have incentives to make accounting choices to project higher profitability to reduce the cost of employee hiring and retention. Consistent with this hypothesis, Bowen et al. (1995) show that firms are more likely to make income-increasing accounting choices when the labor intensity is high. Burgstahler and Dichev (1997) argue that lowering transaction costs with stakeholders, including employees, motivates upward earnings management. Matsumoto (2002) and Cheng and Warfield (2005) hypothesize and show that implicit claims of employees incentivize managers to avoid negative earnings surprises. Choudhary et al. (2009) find that labor intensity is one incentive for firms to manipulate earnings. Zechman (2010) shows that employees' implicit claims motivate managers to manage balance sheets. Surveying a large number of CFOs, Graham et al. (2005) document that many of the CFOs agree that firms manage earnings to manipulate stakeholders' perception. Finally, Dou et al. (2016) find that an increase in unemployment benefits reduces managers' incentives to engage in upward earnings management. In sum, studies provide strong evidence that managers consider the expected cost of attracting and retaining employees when they make accounting choices.

We expect that the recognition of the IDD reduces firms' incentives to manage earnings upward, because the IDD lowers the expected cost of attracting and retaining key employees by restricting their outside employability. Prior studies offer strong evidence that the IDD effectively curbs the job-hopping ability of employees who have access to trade secrets (e.g., Klasa et al. 2018; Png and Samila 2015). These employees are likely to represent important human capital to the firm, and it is likely that their turnover likelihood is considered when management makes an accounting choice. Likewise, Chen et al. (2018) show that rivals are likely to launch costly acquisitions of a firm when their ability to hire key employees of that firm is limited by the adoption of the IDD. When these key employees have restricted outside opportunities as a result of the IDD, they become less sensitive to their employer's financial performance, and their firms have lower incentives to make long-term income-increasing choices. Our discussion yields the following hypothesis.

H1: A state's adoption of the IDD leads to a decrease in discretionary accruals for firms headquartered in the state, relative to control firms (firms headquartered in states that do not experience any change in the IDD recognition).

The accounting literature has long recognized that earnings management can be used to signal private information or distort reported earnings (Watts and Zimmerman 1986; Healy and Palepu 1993). We note that our hypothesis does not require earnings management to be distortive. High discretionary accruals can increase the firm's attractiveness to current and future employees, by signalling high future profitability. Our hypothesis also does not require employees to be direct consumers of financial statements. As long as earnings are part of the public information set used by the press, analysts, rating agencies, and other information intermediaries and employees consume the products of the information intermediaries, accounting earnings can influence employees' perception of the firm.

Bowen et al. (1995) argue that employee-related earnings manipulation incentives are higher for firms whose human capital plays a more important role. They show that firms that rely more on human capital are more likely to make earnings-increasing accounting choices. Additional supportive evidence can be found in the work of Choudhary et al. (2009), Zechman (2010), and Dou et al. (2016). If the impact of the adoption of the IDD on earnings management is indeed related to the consideration of attracting and retaining employees, we expect this impact to be more pronounced for firms where human capital is more important. Our discussion gives rise to our second hypothesis (H2).

H2: The impact of the recognition of the IDD on discretionary accruals is more pronounced for firms where human capital is more important.

Finally, we predict that the recognition of the IDD has a greater impact on firms whose employees have higher ex-ante turnover likelihood. The IDD reduces employees' outside employment opportunities. Conceivably, the effect is more substantial for firms whose employees have more alternative job opportunities. Employees of firms that have many industry peers are likely to enjoy higher mobility, because their skills and knowledge are more transferable (Deng and Gao 2013; Gao et al. 2015). In addition, Ippolito (1985) and Dorsey (1995) argue that employees in firms with defined benefit pension plans have lower turnover likelihood, because retirement benefits from these plans are less portable and thus it is more costly for their workers to change employers. We therefore empirically identify higher ex-ante turnover likelihood through a higher number of industry peers and the existence of a defined benefit pension plan. Our third hypothesis is as follows.

H3: The impact of the recognition of the IDD on discretionary accruals is more pronounced for firms whose employees have high ex-ante turnover likelihood.

4. Methodology, variable construction, and sample formation

4.1. Methodology

Several U.S. state courts recognized the IDD in different years during the sample period. Thus we can compare changes in discretionary accruals for firms located in the states that experience a change in the recognition of the IDD with changes in discretionary accruals for firms located in states that do not experience any IDD change. We implement this test through the following OLS regression.

$$DA_{i,t} = \alpha + \beta_1 IDD_{s,t-1} + \beta_2 Firm\ Characteristics_{i,t} + \beta_3 State\ Characteristics_{s,t} + Firm\ FE + Year\ FE + \varepsilon_{i,t}, \tag{1}$$

where *i* indexes firm, *s* indexes the state in which the firm's headquarters is located, and *t* indexes the year. The dependent variable is $DA_{i,t}$, the discretionary accruals for firm *i* in year *t*. The variable $IDD_{s,t-1}$ is an indicator variable that equals one if the IDD is in place in state *s* in year *t-1* and zero otherwise. We include a comprehensive set of firm characteristics that affect a firm's earnings management. We also control for state characteristics, since they may affect the adoption of the IDD and firms' earnings management. We include firm fixed effects to control for time-invariant differences in the earnings management across firms, and we include year fixed effects to control for economy-wide shocks. Given that our treatment is defined at the state level, we cluster standard errors by state (Petersen 2009).

The coefficient of interest in this model is β_1. As explained by Imbens and Wooldridge (2009), the employed-firm fixed effects lead to β_1 being estimated as the *within-firm* differences before and after the policy change, as opposed to similar before-after differences in states that did not experience such a change during the same period. This regression approach is used to draw causal inferences in Bertrand and Mullainathan (2003), Imbens and Wooldridge (2009), and Klasa et al. (2018).

4.2. Measure of discretionary accruals

To obtain our primary measure of discretionary accruals, we run the modified Jones (1991) model, as described by Dechow et al. (1995) for each industry-year combination. Each industry is classified by its two-digit SIC code. We require at least 20 observations in each combination. The model is specified as follows.

$$\frac{ACCRUAL_{it}}{ASSETS_{i,t-1}} = \beta_0 + \beta_1 \frac{1}{ASSETS_{i,t-1}} + \beta_2 \frac{\Delta REV_{it} - \Delta AR_{it}}{ASSETS_{i,t-1}} + \beta_3 \frac{PPE_{it}}{ASSETS_{i,t-1}}, \qquad (2)$$

where *ACCRUAL* is earnings before extraordinary items and discontinued operations minus operating cash flows from continuing operations from the statement of cash flows (Hribar and Collins 2002; Cohen et al. 2008), *ASSET* is total assets, *REV* is total revenue, *AR* is accounts receivable, and *PPE* is gross property, plant, and equipment.

The residual of Model (2) is used as the measure of discretionary accruals. We then match on performance. Specifically, we sort firms in each industry-year combination into deciles according to their prior year's ROA. Following Ashbaugh-Skaife et al. (2008), we calculate firm *i*'s performance-matched discretionary accruals (DA) as firm *i*'s discretionary accruals minus the median discretionary accruals for firms in the same ROA decile.

4.3. Control variables

We control for *Ln(total assets)*, a measure of firm size, as larger firms are less likely to take income-increasing accounting choices (Watts and Zimmerman 1986). We expect *ROA* (operating income before depreciation divided by lagged total assets) to be associated with *DA*, as firm performance affects managers' incentive to manipulate earnings (Chen et al. 2015). We control for *R&D* (research and development expenses divided by lagged total assets), because R&D investments may increase information asymmetry and incentivize firms to signal good earnings quality (Aboody and Lev 2000; Godfrey and Hamilton 2005).

The recognition of the IDD may result in lower labor cost and higher profits, diminishing the need for upwards earnings manipulations. To alleviate this concern, we control for firms' labor cost through *SG&A* (selling, general, and administrative expenses divided by lagged total assets).

We control for *Issuance*, an indicator for external financing, since firms tend to manipulate earnings upwards prior to external financing (Teoh et al. 1998; DuCharme et al. 2004; Carter et al.

2007). We also control for *Acquisition*, an indicator of the firm's involvement in M&A, because acquisitive activities have a significant influence on financial accounting (Ali and Zhang 2015).

Existing literature shows that intuitional ownership and analyst coverage deter earnings management (Matsumoto 2002; Yu 2008). Thus we control for *Institution* (the percentage of shares held by institutional investors) and *Ln(1+Analyst)* (the logged value of one plus the number of analysts following the firm) in the regression.

Prior research also suggests that managers have incentives to avoid violating debt covenants and to meet or beat earnings benchmarks (DeFond and Jiambalvo 1994; Sweeney 1994; Burgstahler and Dichev 1997; Graham et al. 2005). We therefore control for *Tight covenant* (a dummy indicating whether the firm is close to violating debt covenants) and *Meet/Beat* (a dummy representing meeting or beating the earnings benchmarks by a small margin).

We use *Sales growth* and *MB* (the market-to-book ratio) to capture firm growth. Managers in high-growth firms are more likely to manipulate earnings upwards, because the market penalizes these firms more severely for missing earnings targets (Skinner and Sloan 2002).

Barton and Simko (2002) show that firms with bloated balance sheets are less capable of upward earnings manipulation. We therefore control for *Net operating assets* (net operating assets divided by lagged sales), a measure of bloatedness of the balance sheet. We control for *Sales volatility* (standard deviation of total sales divided by total assets in the prior five years) and *Operating cycle* ([Average Inventory/(Cost of Sales/365)]+[Average Accounts Receivable/(Sales/365)]), because it is more difficult for auditors to detect earnings management in firms with high sales volatility and a longer operating cycle.

We control for *Big N* (a dummy variable indicating whether the annual report is audited by a Big N audit firm) and *Leverage* (long-term debt plus debt in current liabilities, divided by lagged total assets), because firms' earnings management is usually curbed by debt holders' monitoring and auditors' scrutiny (Francis and Krishnan 1999; Khan and Watts 2009).

We additionally control for various state-level variables. In particular, we control for *GDP*, *Unemployment rate, Hightech* (the percentage of high-tech companies in a state), and *Education* (the percentage of state labor-force residents who finished four years' college education). The first two state-level variables capture local economic conditions, while the latter two variables are likely to be related to a state's decision to recognize the IDD, because states with many high-tech firms and those with a higher level of education likely have more trade secrets. Details of variable definitions can be found in Appendix A.

4.4. Sample formation

We start with all U.S. public firms in the Compustat database. We only include companies that are incorporated and headquartered in the United States. We exclude firms in financial industries (SIC codes 6000–6999) or utility industries (SIC codes 4900–4999), as they face different regulatory oversight. We require at least 20 observations in each industry-year combination (industry is based on the two-digit SIC code). We require that all the firm-year observations have available information for the dependent and control variables described in Sections 4.2 and 4.3. We obtain the firm's headquarters information from Compustat and Compact Disclosure (which records any changes in a firm's headquarters) and manually check any missing information.[2]

We obtain debt covenant data from the Dealscan database and institutional shareholding data from Thomson Reuters Institutional (13f) Holdings. We collect analyst coverage, analyst forecast and actual earnings per share data from the I/B/E/S unadjusted detail file.

Data on each state's GDP are from the Bureau of Economic Analysis and data for each state's unemployment rate are from the U.S. Bureau of Labor Statistics Local Area Unemployment Statistics Series. The percentage of high-tech firms is computed based on the firms in the Compustat U.S. universe. State education information is collected from Integrated Public Use Microdata Series

[2] An extensive literature shows that firms usually locate their core business activities and R&D facilities close to their headquarters (e.g., Howells 1990; Pirinsky and Wang 2006; Breschi 2008). Therefore it is reasonable to assume that a significant part of the firm's key employees, who know its trade secrets, work in the firm's headquarters state.

(IPUMS) (Flood et al. 2015).

Hribar and Collins (2002) point out that cash flow from operations should be used to compute total accrual, and this item is available only from 1987; for this reason, we start our sample in 1987. Since we control for debt covenant and WRDS provides the Dealscan-Compustat linking table for loan deals initiated until the middle of 2012 (Chava and Roberts 2008), our sample ends in 2011. We require all variables included in Equation (1) to be nonmissing. Our final sample consists of 94,912 firm-year observations from 1987 to 2011.

5. Empirical results

5.1. Summary statistics

Table 2 provides summary statistics. The mean value of discretionary accruals is -0.77%, and its median value is 0.01%. The mean value of the IDD shows that 47% of our sample observations are in the states that recognize the IDD. The average firm in our sample has total assets of $1.129 billion, its ROA is 2.06%, its R&D is 6.74% of lagged total assets, and its SG&A expense is about 41% of lagged total assets. About 16% (29%) of sample observations are involved in mergers and acquisitions (debt or equity issuance). On average, institutional investors hold about 31% of sample firms' shares, and our sample firms are followed by five analysts. About 8% of our sample firms face tight debt covenants, and 11% of our sample firms meet or beat earnings benchmarks by a small margin. The mean market-to-book ratio is 2.69, and the net operating assets average about 86% of lagged sales. The mean value of sales volatility is about 28%, and the operating cycle on average is 148 days. The mean value of Big N shows that 76% of sample firms are audited by Big N auditors. The states where our sample firms are headquartered have a mean GDP of $537 billion and an unemployment rate of 5.77%. About 15% of firms headquartered in these states are high-tech firms, and about 27% of labor-force residents in these states have finished four years of college education.

5.2. Testing H1

Table 3 presents the results from estimating Model (1). Column (1) reports the regression results where we include only *IDD*, year-fixed effects, and firm-fixed effects. The coefficient on the

IDD indicator is -0.008, significant at the 1% level, suggesting a negative effect of the IDD adoption on the firm's discretionary accruals. The economic magnitude is sizeable: the adoption of the IDD leads to a decrease in the firm's discretionary accruals by approximately 0.8 percentage points, while the average discretionary accrual in our sample is -0.77 percentage points. Column (2) reports the regression results after we include various control variables. Consistent with the literature (e.g., DuCharme et al. 2004; Ali and Zhang 2015; Chen et al. 2015), we find that discretionary accruals are negatively associated with *total assets*, *R&D*, *Net operating assets*, and *Acquisition*, while they are positively associated with *ROA*, *Issuance*, *Tight covenant*, *Meet/Beat*, *Sales growth*, *MB*, and *Ln(operating cycle)*. More importantly, the coefficient on IDD is -0.009, significant at the 1% level.

The validity of difference-in-differences estimation depends on the parallel trends assumption: absent the IDD, treated firms' earnings management would have evolved in the same way as that of control firms. This assumption is inherently untestable. However, we can shed some light on this assumption by testing whether the time-series trend differed prior to the IDD recognition. If the pre-treatment trends are similar for treatment and control firms, this adds to our confidence about the validity of our approach. We conduct relevant analyses. Specifically, we re-estimate column (1) after replacing *IDD* with the five indicator variables: IDD^{-2}, IDD^{-1}, IDD^{0}, IDD^{1}, and IDD^{2+}. These variables indicate the years relative to the adoption of the IDD. In particular, IDD^{-2} indicates that it is two years before the IDD adoption; IDD^{-1} indicates that it is one year before the IDD adoption; IDD^{0} indicates the year in which the IDD is adopted; IDD^{1} indicates that it is the year after the IDD adoption; and IDD^{2+} indicates that it is two or more years after the IDD adoption.

We focus on IDD^{-2} and IDD^{-1} indicators, because their significance and magnitude indicate whether there is any difference between the treatment group and the control group prior to the adoption of the IDD.

Our results are reported in column (3). We find that the coefficients on these two indicators are not significantly different from zero, suggesting that the parallel trend assumption is not violated. The impact of the IDD starts to appear after the adoption: the coefficients on IDD^{1} and IDD^{2+} are negative and significant.

Overall, the results indicate that a state's recognition of the IDD leads to a significant decrease in discretionary accruals, which supports the view that attracting employees is an important motive for corporate earnings management. Moreover, the treated and control groups share a similar trend in earnings management prior to the IDD adoption; the difference in earnings management arises only *after* the IDD is recognized, which suggests a causal effect. In summary, the results in Table 3 support H1.

5.3. Robustness checks and additional investigation

5.3.1. Shorter window

To draw strong inferences on causality, we examine changes in discretionary accruals in a shorter window around the adoption of the IDD. For each treated firm, we match it to a control firm that is in the same industry, in a state without adopting IDD, and is closest in performance-matched discretionary accrual in the year of IDD adoption. For each pair of the treated and control firms, we keep the observations during [t −1, t +1] around the IDD adoption. We then re-run Equation (1). The results are reported in column (1) of Table 4. We find that the coefficient on *IDD* is negative and significant at the 5% level, suggesting that the firms manipulate earnings downwards immediately after the adoption of the IDD. In columns (2) and (3) of Table 4, we repeat the analysis in column (1) by focusing on the period [t−3, t +3] and [t −5, t +5] around the IDD adoption, respectively; our inference is unchanged. Overall, our results are robust to various alternative periods.

5.3.2. Unobservable local economic conditions

Although we have controlled for observable local economic conditions in the regression specification of Equation (1), our results could be explained by some unobservable local economic conditions, which are associated with both the adoption of the IDD and corporate earnings management. In this subsection, we address this concern by matching treated firms with control firms located nearby. We then investigate whether our results continue to hold. If our results are indeed driven by local economic conditions, we expect our difference-in-differences results will cease to exist, because both types of firms are affected by the same local economic conditions.

Specifically, we match each treated firm to a control firm in the same industry (based on the two-digit SIC code), located within a short distance (100 miles, 90 miles, 80 miles, and 70 miles) of the treated firm and closest in performance-matched discretionary accrual in the year of IDD adoption. If we cannot find a close-by matched control firm to a given treated firm, we drop this treated firm from the sample. By construction, our sample firms in this test are the ones located close to state borders, and for this reason, the sample size significantly drops. The distance requirement essentially implies that we focus on firms located near state borders. We then re-estimate Equation (1) by using this sample of treated and control firms. Table 5 presents the results.

Column (1) reports the results when we require control firms to be located within 100 miles of treated firms. The coefficient on the indicator *IDD* is negative (-0.017) and significant at the 5% level. Its economic magnitude is comparable to the baseline regression reported in Table 3. In columns (2)–(4), we require the distance between the treated firm and control firm to be less than 90, 80, and 70 miles, respectively, and our inference remains unchanged. Under the assumption that the control firms are exposed to similar local economic conditions and hence the change in discretionary accruals of the treated firms should be no different from that of their control firms, our findings suggest that unobservable confounding local economic conditions do not explain the observed impact of the IDD on corporate earnings management.

5.3.3. Rejection of the IDD

Also shown in Table 1, three states rejected the IDD subsequent to its adoption. H1 postulates that the adoption of the IDD decreases firms' discretionary accruals. If H1 is true, the rejection of the IDD will have the opposite impact on discretionary accruals. We test this prediction in this subsection. In particular, to assess the impact of IDD rejection, we compare firms that experience the rejection of the IDD with firms that continue to be affected by the IDD. Thus our sample in this test includes all post-IDD firm-year observations in states that adopt the IDD. Then we re-run Equation (1) by replacing the independent variable *IDD* with *IDD_Rejection*, an indicator that takes the value of one if the firm-year observation is after the firm's headquarter state rejects its previously adopted IDD and zero otherwise. The coefficient on the indicator variable reflects the impact of the rejection of the IDD on discretionary accruals.

Our results are reported in Table 6. Consistent with our expectation, a state's rejection of its previously adopted IDD leads to a significant increase in discretionary accruals. Taking column (2) as an example, the coefficient on *IDD rejection* is 0.013 and significant at the 1% level, indicating that the rejection of the IDD leads to an increase in the firm's discretionary accruals by 1.3 percentage points. This magnitude is similar to the effect of IDD adoption. (For example, the coefficient on the *IDD* indicator is -0.009 in column (2) of Table 3.)

Taken together, while a state's adoption of the IDD leads to a decrease in discretionary accruals, the rejection of a state's previously adopted IDD has the opposite effect: it results in an increase in discretionary accruals with a similar economic magnitude. These findings are consistent with H1 and provide support to a causal effect of the IDD on corporate earnings management.

5.3.4. Alternative measures of earnings management

We use three model-free measures of earnings management to check the robustness of our results. These three measures are *Down-restate* (an indicator that takes the value of one if the firm restates earnings downwards in a year and zero otherwise), *Write-down* (an indicator that takes the value of one if the firm writes down in a year and zero otherwise), and *Negative special items* (an indicator that takes the value of one if the firm records negative special items in the year and zero otherwise). We obtain restatement data from Audit Analytics Database, which provides data beginning in 2001. Write-down and special items data are from Compustat. Write-down data are available beginning in 2000.

Table 7 reports the logit regression results. The coefficients on *IDD* are significantly positive in all three columns. The related odds ratios suggest that the odds of downward restatements, write-downs, and negative special items are increased by 35%, 15%, and 15%, respectively, after the adoption of the IDD. Our results are consistent with those based on abnormal accruals, suggesting that firms unwind prior period upward earnings management after the adoption of the IDD.

5.3.5. Can an increase in profitability drive our results?

Our evidence so far is consistent with our hypothesis that the adoption of the IDD reduces its firms' upward earnings manipulation by lowering the likelihood of employee departure. However, an alternative explanation is that the IDD reduces firms' incentives for upward earnings management by increasing firms' profitability. We conduct two tests to assess the alternative explanation.

First, we re-estimate our baseline regression by additionally controlling for future profitability. Specifically, we include not only the current ROA but also ROAs for the future two years. By doing so, we mitigate the concern that the estimated effect of the IDD adoption on earnings management is explained by expected changes in future profitability. Our results are reported in column (1) of Table 8. The coefficient on the *IDD* indicator is still negative and significant at the 1% level; its economic magnitude (-0.01) is almost the same as that documented in column (2) of Table 3 (-0.009). This result is inconsistent with the alternative explanation.

Second, we directly examine whether the adoption of the IDD affects a firm's operating performance. The regression specification is similar to Equation (1), except that the dependent variable is ROA/operating cash flow (operating cash flow divided by lagged total assets). We report our results in columns (2) and (3) of Table 8. In both columns, the coefficient on the *IDD* indicator is statistically insignificant. In other words, the adoption of the IDD does not lead to an improvement in firms' operating performance. This finding is not surprising, because the IDD offers better protection for trade secrets but may also discourage employees' effort and investment in human capital (Garmaise 2011), resulting in no impact on firms' operating performance. We note that the same finding is documented by Klasa et al. (2018).

Overall, the evidence in this section is inconsistent with the alternative explanation that our findings are due to the effect of the IDD on firms' profitability.

5.3.6. Employment contract terms

Our main hypothesis is based on the notion that the IDD constrains employees' outside job opportunities. If the IDD indeed increases employees' cost of switching employers, its adoption effectively lowers employees' bargaining power and presumably results in less attractive employment contracts.

We collect executive compensation data from S&P's Capital IQ People Intelligence Database (PID). Unlike the ExecuComp database, which mainly covers S&P 1500 firms, PID has a much more comprehensive coverage: it covers almost all U.S. public firms since 1995. During our sample period of 1995–2011, 74% of our sample firms are covered by PID, while only 25% are covered by ExecuComp. We choose to examine how the IDD affects the compensation of corporate executives, because they typically represent the highest level of corporate hierarchy and obviously have access to their firm's trade secrets. Therefore, their compensation is likely to be affected by the IDD.

We distinguish between fixed (i.e., basic salary) and variable compensation. Fixed compensation is usually less likely to change, while variable compensation (the sum of bonus, restricted stock, option grants, and all other compensation) is more likely to vary with the employees' outside opportunities (Balsam and Miharjo 2007; Gao et al. 2015). The model specification is the same as that in Equation (1), except that we use person fixed effect (instead of firm fixed effect), since our observation is at the person-year level. The results are reported in Table 9. In columns (1) and (2), we only focus on the CEO. While the coefficient on *IDD* is insignificant in column (1) (where the dependent variable is *Ln (Salary)*), it is -0.113 and significant at the 5% level in column (2) (where the dependent variable is *Ln(Variable pay)*). This result indicates that, after the adoption of the IDD, CEOs in treated firms experience a pay cut in variable compensation by $11\% \; (= \mathrm{e}^{-0.113} - 1)$. In columns (3) and (4), we repeat the analysis by focusing on the top five managers in the firm. We find that the adoption of the IDD leads to a pay cut of variable pay by approximately 9%.

Overall, these results are consistent with the notion that the IDD reduces the bargaining power of employees who have access to their firm’s trade secrets.

5.3.7. Employee productivity

A related question is: does IDD change employees’ productivity level? On one hand, we can argue that the IDD reduces outside job opportunities and employees may work harder to keep and excel at their current job. On the other hand, the IDD disrupts the efficiency of the labor market. An inefficient labor market lowers employees’ incentives to perform, because their human capital does not fetch a fair price. To illustrate this point, imagine a regulation that forbids all professors from moving to another university. Since even a star professor can’t get an outside offer to move his pay to the appropriate level, professors’ incentives to perform will be very low. Ex ante, it is difficult to see which force prevails, and we take this issue to the data.

We use three measures of productivity. The first is income before extraordinary items divided by the number of employees, which reflects the amount of profits generated by each individual. While this measure is intuitive, we are concerned that it reflects the overall productivity, rather than the productivity of employees affected by the IDD. To more precisely reflect their productivity, we resort to corporate innovation outputs, because employees who have access to trade secrets are likely to influence corporate innovation. We measure innovation output at year t+1, because the innovation process generally takes longer than one year (Fang et al. 2014). We obtain patent and citation data from the patent database of Kogan et al. (2017), which can be downloaded from https://iu.app.box.com/v/patents. The database covers all patents awarded by the U.S. Patent and Trademark Office over the period of 1976–2010. Since it takes around two years for patents to be granted, patents applied in 2009 and 2010 may not be eventually granted. We thus use the innovation data till 2008.

Our regression specification is the same as Equation (1) in the paper, except that the dependent variables are measures of productivity. The regression results are reported in Table 10. Column (1) reports the results when the dependent variable is income per employee. The coefficient on the IDD indicator is statistically insignificant, suggesting that the IDD has no discernable impact on firms' productivity.

In columns (2) and (3), the dependent variable is, respectively, the natural logarithm of one plus the number of patents applied for (and finally granted) and the natural logarithm of one plus the number of citations received by these patents. In both columns, the coefficient on the IDD indicator is significantly negative, suggesting that the adoption of the IDD impedes corporate innovation output. Overall, our results suggest that the adoption of the IDD has either no impact or a negative impact on employees' productivity.

5.3.8. Accounting policies

We also examine how the IDD affects firms' choice of inventory valuation method and depreciation method. Firms' choice of inventory valuation method is measured by *LIFO*, a dummy that takes the value of 1 if the firm's primary inventory valuation method is LIFO and zero otherwise. *Straight-line* is our measure of firms' choice of the depreciation method. It is a dummy that takes the value of 1 if the firm's depreciation method is the straight-line depreciation method and zero otherwise. We use logistic regressions to regress the two measures (*LIFO* and *Straight-line*) on the *IDD* dummy and the full set of control variables. We don't observe a significant impact of the IDD on the choice of either accounting method. Such an "insignificant" result could be due to the following two reasons.

First, accounting policies are very stable over time. In our sample of 94,912 firm-year observations, there are only 938 cases (less than 1% of sample firm-year observations) in which a firm changes its inventory valuation method from LIFO to other methods in a certain year, and only 1,395 (1.5% of sample firm-year observations) in which a firm changes its depreciation method from the straight-line method to other methods in a certain year. Given that we use the difference-in-differences method to compare the changes in accounting policy in treated firms versus the changes in control firms, it is unsurprising to see little effect of the IDD on accounting policy if accounting polices themselves seldom change.

Second, as pointed out by Francis (2001), the effectiveness of earnings manipulations depends on the other party's inability to unravel such manipulations. Since changes in accounting methods are very salient, they may alert employees. Anticipating this, managers may resort to other ways of manipulating earnings.

5.4. Testing H2

H2 predicts that the impact of the adoption of the IDD on discretionary accruals is more pronounced for firms where human capital is more important. We use three proxies to measure the importance of human capital. Following Bowen et al. (1995) and Matsumoto (2002), our first measure is intangible assets intensity, computed as one minus the ratio of gross property, plant, and equipment normalized by total assets. As explained by Bowen et al. (1995), firms with more intangible assets rely more on human capital rather than tangible assets, such as machines or equipment. A firm is classified as one with high (low) intangible assets intensity if its intangible assets intensity is above (below) the sample median in a specific year. While this measure provides a firm-specific estimate of human capital, it does not speak directly to the target of the IDD: employees who have access to trade secrets. To address this concern, we use a second measure. Since employees who have access to trade secrets are likely to be managers or professionals, we gauge the importance of this particular type of talent through knowledge worker intensity, computed as the number of knowledge workers as a proportion of all workers in the industry. We obtain employment data from the IPUMS. Based on the IPUMS occupational codebook, we define a person as a knowledge worker if his or her occupation code (*occ2010*) is in the category of "management, professional and related occupations."[3] This definition includes occupations such as managers, scientists, engineers, financial specialists, and IT professionals. The IPUMS provides annual data on individual workers' occupational code, industry, state, etc. From the IPUMS data, we calculate the proportion of the total workforce being knowledge

[3] IPUMS database categorizes occupations into seven groups: 1) management, professional, and related occupations; 2) service occupations; 3) sales and office occupations; 4) farming, fishing, and forestry occupations; 5) construction, extraction, and maintenance occupations; 6) production, transportation, and material mobbing occupations; and 7) military-specific occupations.

workers for a given two-digit SIC industry in a given year, and then assign that measure to each firm in the industry. We then classify a firm as relying more on human capital if its proportion of knowledge workers among all workers is above the sample median in a given year. Our third measure is inventor intensity, measured by the number of inventors normalized by the total number of employees in a firm. Inventors produce patents and thus can be regarded as one group of key technicians in a firm (Palomeras and Melero 2010; Chang et al. 2015). We collect individual inventor data from the Harvard Business School Patent Dataverse, which provides information on both inventors (i.e., employees who produce the patents) and assignees (i.e., companies that own the patents). We then classify a firm as relying more on human capital if its inventor intensity is above the sample median in a given year.

To test H2, we re-estimate Model (1) for the two subsamples formed based on each measure of human capital importance. Our results are reported in Table 11.

Panel A reports the results where subsamples are formed based on intangible-asset intensity. In the high intangible-asset-intensity subsample, the coefficient on the IDD indicator is -0.018, significant at the 1% level. In contrast, in the subsample of low intangible-assets intensity (column (2)), the coefficient on the IDD indicator is much smaller in magnitude (only -0.004) and is significant at the 10% level. Based on the z-test developed by Clogg et al. (1995) and Beck and Narayanamoorthy (2013), we test the equality of these two coefficients and find that they are significantly different at the 1% level.

Panel B reports the results based on knowledge worker intensity. The coefficient on the IDD indicator is -0.013 and significant at the 1% level in the subsample of high knowledge-worker intensity (column (1)). In the subsample of low knowledge-worker intensity (column (2)), the coefficient on the IDD indicator is -0.002, which is much smaller in magnitude and not significantly different from zero. The null hypothesis that these two coefficients are the same is rejected at the 1% level.

Panel C reports the results based on inventor intensity. The coefficient on the IDD indicator is -0.018 and significant at the 5% level in the subsample of high inventor intensity (column (1)). However, in the subsample of low inventor intensity (column (2)), the coefficient on the IDD indicator is -0.001, which is much smaller in magnitude and not significantly different from zero. These two coefficients are significantly different at the 5% level.

Overall, Table 11 shows that the effect of the IDD is more pronounced for firms whose human capital is more important, lending support to H2.

5.5. Testing H3

H3 posits that the impact of the recognition of the IDD on discretionary accruals is more pronounced for firms whose employees have high ex-ante turnover likelihood (i.e., firms that do not have a defined benefit pension plan and firms that have a higher number of industry peers). We use the number of firms in the same two-digit SIC industry to measure the number of industry peers.

In Table 12, Panel A, we re-estimate our baseline regression for the subsamples formed based on whether the firm has a defined benefit pension plan. Column (1) shows that the coefficient on the IDD indicator is -0.011 and significant at the 1% level, in the subsample of firms without defined benefit pension plans. In contrast, column (2) reports that the coefficient on the IDD indicator is much smaller in magnitude (-0.003) and is not significant, in the subsample of firms with such plans. The difference between the two coefficients is statistically significant at the 5% level. Our findings indicate that the impact of the IDD on earnings manipulation is more pronounced for firms without defined benefit plans (i.e., firms whose employees are more likely to switch jobs ex-ante).

Panel B reports the results from our baseline regression for subsamples formed based on a median-split of the number of industry peer firms. In the subsample with a large number of industry peers, the coefficient on the IDD indicator is -0.013 and significant at the 1% level. However, in the subsample with a small number of industry peers, the coefficient on the IDD indicator is only -0.004 and not significant. The difference between the two coefficients is statistically significant at the 5% level. This result indicates that the treatment effect is significant when there are many industry peer firms (when it is easier for employees to find new jobs), whereas it is virtually absent when there are only a few industry peer firms.

Overall, Table 12 shows that the effect of the IDD on upward earnings management is stronger for firms whose employees have higher ex-ante mobility. These results suggest that retaining human capital via earnings management is indeed the mechanism through which a state's recognition of the IDD influences its local firms' discretionary accruals.

6. Conclusions

We investigate whether retaining employees is an important determinant for corporate earnings management by exploiting exogenous shocks from the staggered recognition of the inevitable disclosure doctrine by U.S. state courts. The recognition of this doctrine prevents a firm's employees from switching to competing firms and thus exogenously reduces the employee turnover likelihood for firms headquartered in the state.

We find a significant decrease in discretionary accruals for firms in states that adopt the IDD, relative to firms in states that do not. In support of a causal interpretation of our findings, our timing tests indicate that there is no pre-treatment difference in discretionary accruals between the two groups of firms and that the reduction in discretionary accruals occurs after the recognition of the IDD. Our conclusion continues to hold when we use shorter windows around the adoption of the IDD. In addition, we show that, after a state rejects its previously adopted IDD, firms engage more in the upward earnings management.

We examine whether local economic conditions explain our finding. To test this alternative explanation, we impose the restriction that the control firm be located within a short distance of the treated firm. We then repeat our analyses. If our results are driven by local economic conditions, we expect to find no significant results from this sample because both the treated firm and the control firm are affected by the same local conditions. This expectation receives no empirical support: we continue to find that the adoption of the IDD reduces firms' upward earnings management.

We also find that top-paid employees (who are likely to have access to trade secrets) experience pay cuts after the adoption of the IDD. This finding is consistent with the notion that, by limiting outside job opportunities, the IDD reduces employee bargaining power and leads to less favorable employment contracts.

Furthermore, we show that after the adoption of the IDD, firms in the state are more likely to write down their assets, report negative special items, and make income-decreasing restatements. These findings are broadly consistent with the notion that firms unwind prior upward earnings management, when the IDD weakens their incentives to attract employees through earnings management.

Our results could also be explained by that the adoption of the IDD improves firms' profitability, which reduces managerial incentives to conduct upward earnings management. We investigate this possibility by controlling for future profitability in our regression and assessing the impact of the IDD on firms' earnings. If improved profitability is responsible for our results, we expect that our results to disappear, once we consider future profitability in our analyses, and that the adoption of the IDD elevates firms' operating performance. Neither expectation is supported, which is evidence against this alternative explanation.

We next analyze how the IDD influences employees' productivity. Using three measures of employee productivity (income per employee, number of patents, and number of patent citations), we show that the IDD has either no impact or a negative impact on employees' productivity.

Our further subsample analyses show that the impact of the IDD on the firms' earnings management is more pronounced for firms relying more on human capital and for firms whose employees have better employment mobility ex-ante. These results confirm that the effect is through the employee retention channel. Overall, our findings are consistent with the view that retaining employees is an important motivation for corporate earnings manipulation.

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Appendix A: Variable Definitions

Variable	Definition
DA	Discretionary accruals from the modified Jones model (Jones 1991; Dechow et al. 1995) and matched according to Ashbaugh-Skaife et al. (2008).
Salary	An employee's basic salary.
Variable pay	An employee's variable compensation, including bonus, other annual compensation, restricted stock awards, option awards, long-term incentive plan, and all other compensation.
Down-restate	A dummy variable that equals 1 for income-decreasing restatements in a year and 0 otherwise.
Write-down	A dummy variable that equals 1 if the firm writes down in a year and 0 otherwise.
Negative special items	A dummy variable that equals 1 if the firm records negative special items in a year and 0 otherwise.
Operating cash flow	Cash flow from operating activities divided by lagged total assets.
IDD	A dummy variable that equals 1 if the inevitable disclosure doctrine (IDD) is recognized in the firm's headquarters state and 0 otherwise.
IDD_Rejection	A dummy variable that equals 1 if the IDD is rejected in the firm's headquarters state and 0 otherwise.
Total assets	Book value of total assets.
R&D	Research and development expenses divided by lagged total assets. If R&D value is missing, we set it to zero.
SG&A	Selling, general, and administrative expenses divided by lagged total assets. If SG&A value is missing, we set it to zero.
ROA	Operating income before depreciation divided by lagged total assets.
Acquisition	A dummy variable that equals 1 if the company is involved in a merger or acquisition and 0 otherwise.
Issuance	A dummy variable that equals 1 if the value of *Acquisition* is 0 and the number of outstanding shares increased by at least 10 percent or long-term debt increased by at least 20 percent during the year, or the firm first appears on the CRSP monthly returns database in the year and 0 otherwise.
Institution	The percentage of shares held by institutional investors by the quarter-end preceding the fiscal year-end.
Analyst	Total number of analysts that make at least one one-year-ahead earnings forecast for the company from the beginning of the fiscal year to the date when the actual earning is released.
Tight covenant	A dummy variable that equals 1 if the tightest slack of a company is smaller than sample median in the year and equals 0 if the tightest slack of a company is larger than sample median in the year, or if the company is not limited by debt covenant in the year, or if the company's tightest slack is negative. We measure slack as [(maximum threshold-actual) / maximum threshold] for maximum threshold covenants, and [(actual-minimum threshold)/ absolute value of minimum threshold] for min threshold covenants (Dou et al.,2016). We adopt the definitions of covenants of Demerjian and Owens (2016) and only consider accounting-based covenants.
Meet/Beat	A dummy variable that equals 1 if the net income before extraordinary items scaled by total assets lies in $[0,0.005)$ or the 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, and 0 otherwise (Cohen et al. 2008).
Sales growth	Annual sales growth rate from year t-1 to year t.
MB	Market value of equity divided by book value of equity.
Net operating assets	Shareholder's equity minus cash and short-term investments plus total debt at the beginning of the year, divided by lagged sales.
Sales volatility	Standard deviation of total sales divided by total assets in the prior five years.
Operating cycle	[Average Inventory/(Cost of Sales/365)] +[Average Accounts Receivable/(Sales/365)].
Big N	A dummy variable that equals 1 if the annual report is audited by a Big N audit firm and 0 otherwise.
Leverage	Long-term debt plus debt in current liabilities, divided by lagged total assets.
GDP	Annual GDP of a given state.

Unemployment rate	The unemployment rate of a state, calculated as the average unemployment rate over the 12 months in a year.
Hightech	The percentage of high-tech companies in a state, measured as the number of high-tech companies divided by the total number of companies in the state, as recorded by Compustat. Following Ljungqvist and Wilhelm (2003), we treat a company as a high-tech company, if its SIC code is 3571, 3572, 3575, 3577, 3578, 3661, 3663, 3669, 3674, 3812, 3823, 3825, 3826, 3827, 3829, 4899, 7370, 7371, 7372, 7373, 7374, 7375, 7378, or 7379.
Education	The percentage of state labor-force residents having finished four years of college education.
Income/Emp	Income before extraordinary items (in millions) divided by the number of employees.
Pat	Number of patents that are applied for (and subsequently awarded) by a firm in a year
Cite	Number of citations received by a firm's patents filed in a year. Following Hall, Jaffe, and Trajtenberg (2005), we use the fixed-effects approach to adjust for the truncation bias of citations. Specifically, we normalize the number of citations received by each patent by dividing it by the average number of citations received by all the patents granted in the same year.

Table 1. Precedent-Setting Legal Cases Adopting or Rejecting the Inevitable Disclosure Doctrine

The table lists the precedent-setting legal cases in which state courts adopted the inevitable disclosure doctrine (IDD), collected from Klasa et al. (2018). The states omitted from the table did not consider the IDD or considered but rejected the IDD.

state	Date	Decision	Reference
New York	12/5/1919	Adopt	Eastman Kodak Co. v. Powers Film Prod., 189 A.D. 556 (N.Y.A.D. 1919)
Florida	7/11/1960	Adopt	Fountain v. Hudson Cush-N-Foam Corp., 122 So. 2d 232 (Fla. Dist. Ct. App. 1960)
	5/21/2001	Reject	Del Monte Fresh Produce Co. v. Dole Food Co. Inc., 148 F. Supp. 2d 1326 (S.D. Fla. 2001)
Delaware	5/5/1964	Adopt	E.I. duPont de Nemours & Co. v. American Potash & Chem. Corp., 200 A.2d 428 (Del. Ch. 1964)
Michigan	2/17/1966	Adopt	Allis-Chalmers Manuf. Co. v. Continental Aviation & Eng. Corp., 255 F. Supp. 645 (E.D. Mich. 1966)
	4/30/2002	Reject	CMI Int’l, Inc. v. Intermet Int’l Corp., 649 N.W.2d 808 (Mich. Ct. App. 2002)
North Carolina	6/17/1976	Adopt	Travenol Laboratories Inc. v. Turner, 228 S.E.2d 478 (N.C. Ct. App. 1976)
Pennsylvania	2/19/1982	Adopt	Air Products & Chemical Inc. v. Johnson, 442 A.2d 1114 (Pa. Super. Ct. 1982)
Minnesota	10/10/1986	Adopt	Surgidev Corp. v. Eye Technology Inc., 648 F. Supp. 661 (D. Minn. 1986)
New Jersey	4/27/1987	Adopt	Nat’l Starch & Chem. Corp. v. Parker Chem. Corp., 530 A.2d 31 (N.J. Super. Ct. 1987)
Illinois	2/9/1989	Adopt	Teradyne Inc. v. Clear Communications Corp., 707 F. Supp. 353 (N.D. 111. 1989)
Texas	5/28/1993	Adopt	Rugen v. Interactive Business Systems Inc., 864 S.W.2d 548 (Tex. App. 1993)
	4/3/2003	Reject	Cardinal Health Staffing Network Inc. v. Bowen, 106 S.W.3d 230 (Tex. App. 2003)
Massachusetts	10/13/1994	Adopt	Bard v. Intoccia, 1994 U.S. Dist. LEXIS 15368 (D. Mass. 1994)
Indiana	7/12/1995	Adopt	Ackerman v. Kimball Int’l Inc., 652 N.E.2d 507 (Ind. 1995)
Connecticut	2/28/1996	Adopt	Branson Ultrasonics Corp. v. Stratman, 921 F. Supp. 909 (D. Conn. 1996)
Iowa	4/1/1996	Adopt	Uncle B’s Bakery v. O’Rourke, 920 F. Supp. 1405 (N.D. Iowa 1996)
Arkansas	3/18/1997	Adopt	Southwestern Energy Co. v. Eickenhorst, 955 F. Supp. 1078 (W.D. Ark. 1997)
Washington	12/30/1997	Adopt	Solutec Corp. Inc. v. Agnew, 88 Wash. App. 1067 (Wash. Ct. App. 1997)
Utah	1/30/1998	Adopt	Novell Inc. v. Timpanogos Research Group Inc., 46 U.S.P.Q.2d 1197 (Utah D.C. 1998)
Georgia	6/29/1998	Adopt	Essex Group Inc. v. Southwire Co., 501 S.E.2d 501 (Ga. 1998)
Missouri	11/2/2000	Adopt	H&R Block Eastern Tax Servs. Inc. v. Enchura, 122 F. Supp. 2d 1067 (W.D. Mo. 2000)
Ohio	9/29/2000	Adopt	Procter & Gamble Co. v. Stoneham, 747 N.E.2d 268 (Ohio Ct. App. 2000)
Kansas	2/2/2006	Adopt	Bradbury Co. v. Teissier-duCros, 413 F. Supp. 2d 1203 (D. Kan. 2006)

Table 2. Descriptive Statistics

The sample consists of 94,912 firm-year observations during the 1987−2011 period, obtained from Compustat. All sample firms are U.S. public firms, excluding financial and utility firms. Variable definitions are provided in Appendix A. All continuous variables are winsorized at the first and 99th percentiles.

	N	Mean	Std. Dev	P25	Median	P75
DA	94,912	-0.77%	22.12%	-6.94%	0.01%	6.76%
IDD	94,912	0.47	0.50	0	0	1
Total assets ($million)	94,912	1128.60	3626.72	22.22	103.13	501.62
Ln(total assets)	94,912	4.69	2.27	3.10	4.64	6.22
ROA	94,912	2.06%	38.58%	-0.99%	10.78%	18.95%
R&D	94,912	6.74%	13.83%	0.00%	0.23%	7.62%
SG&A	94,912	41.42%	44.76%	13.66%	30.37%	53.55%
Acquisition	94,912	0.16	0.36	0	0	0
Issuance	94,912	0.29	0.45	0	0	1
Institution	94,912	30.54%	30.90%	0.25%	21.03%	55.21%
Analyst	94,912	4.81	7.37	0	1	7
Ln(1+Analyst)	94,912	1.09	1.13	0.00	0.69	2.08
Tight covenant	94,912	0.08	0.27	0	0	0
Meet/Beat	94,912	0.11	0.32	0	0	0
Sales growth	94,912	24.67%	82.97%	-3.86%	8.65%	26.37%
MB	94,912	2.69	5.81	0.94	1.80	3.34
Net operating assets	94,912	0.86	1.74	0.28	0.48	0.81
Sales volatility	94,912	27.73%	31.80%	9.33%	17.61%	32.87%
Operating cycle (days)	94,912	148.22	143.22	70.90	116.09	177.53
Ln(operating cycle)	94,912	4.68	0.82	4.26	4.75	5.18
Big N	94,912	0.76	0.43	1	1	1
Leverage	94,912	29.99%	39.95%	3.00%	20.09%	40.29%
GDP($million)	94,912	537374.29	477657.41	192948	370912	773460
Ln(GDP)	94,912	12.79	0.95	12.17	12.82	13.56
Unemployment rate	94,912	5.77%	1.75%	4.60%	5.41%	6.63%
Hightech	94,912	15.24%	8.22%	8.85%	13.15%	21.43%
Education	94,912	27.13%	5.17%	23.30%	26.63%	30.48%

Table 3. Testing H1

This table reports the difference-in-differences tests that examine the impacts of the inevitable disclosure doctrine (IDD) on the firm's discretionary accruals. The dependent variable is *DA*. The indicator variable *IDD* takes the value of one if the IDD is recognized in a state and zero otherwise. In column (1), we report the regression results where we include only *IDD*, year-fixed effects, and firm-fixed effects. In column (2), we report the regression results after we include various control variables. In column (3), we replace *IDD* with the IDD^{-2}, IDD^{-1}, IDD^{0}, IDD^{1}, and IDD^{2+} indicators. These five indicators flag the year, relative to the state adoption of the IDD. The sample consists of 94,912 firm-year observations. Variable definitions are provided in Appendix A. All continuous variables are winsorized at the first and 99th percentiles. *T*-statistics based on robust standard errors clustered by state are in parentheses. The superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1) *DA*	(2) *DA*	(3) *DA*
IDD	-0.008***	-0.009***	
	(-2.907)	(-5.093)	
IDD^{-2}			0.001
			(0.346)
IDD^{-1}			0.007
			(1.290)
IDD^{0}			-0.005
			(-1.293)
IDD^{1}			-0.011*
			(-1.849)
IDD^{2+}			-0.009**
			(-2.591)
Ln (total assets)		-0.021***	-0.021***
		(-8.299)	(-8.111)
ROA		0.214***	0.214***
		(20.962)	(20.970)
R&D		-0.106***	-0.106***
		(-3.596)	(-3.585)
SG&A		0.033***	0.033***
		(3.519)	(3.511)
Acquisition		-0.012***	-0.013***
		(-3.725)	(-3.730)
Issuance		0.018***	0.019***
		(8.333)	(8.359)
Institution		0.012**	0.011*
		(2.014)	(1.987)
Ln(1+Analyst)		-0.000	-0.000
		(-0.071)	(-0.092)
Tight covenant		0.008***	0.008***
		(3.604)	(3.625)
Meet/Beat		0.016***	0.016***
		(11.849)	(11.777)
Sales growth		0.011***	0.011***
		(4.826)	(4.827)
MB		0.001**	0.001**
		(2.591)	(2.601)

Net operating assets		-0.007***	-0.007***
		(-6.003)	(-6.003)
Sales volatility		0.002	0.003
		(0.385)	(0.402)
Ln (operating cycle)		0.009***	0.009***
		(2.729)	(2.734)
Big N		-0.008	-0.008
		(-1.295)	(-1.297)
Leverage		-0.008	-0.008*
		(-1.670)	(-1.681)
Ln(GDP)		0.002	0.003
		(0.648)	(0.821)
Unemployment rate		-0.266***	-0.285***
		(-2.824)	(-2.961)
Hightech		-0.002	-0.003
		(-0.060)	(-0.074)
Education		-0.003	-0.011
		(-0.069)	(-0.249)
Year Fixed Effects	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes
Intercept	-0.004	0.010	0.004
	(-0.515)	(0.201)	(0.072)
N	94,912	94,912	94,912
Adj_R^2	0.151	0.208	0.208

Table 4. Shorter Windows

This table reports the robustness checks of the impacts of the IDD on the firm's discretionary accruals, based on shorter windows. For each treated firm, we match it to a control firm that is in the same industry, in a state without adopting IDD, and closest in performance-matched discretionary accrual in the year of IDD adoption. For each pair of the treated and control firms, we keep the observations during [t−1, t +1], [t−3, t +3], and [t−5, t +5] around the IDD adoption in columns (1)–(3), respectively. Variable definitions are provided in Appendix A. All continuous variables are winsorized at the first and 99th percentiles. *T*-statistics based on robust standard errors clustered by state are in parentheses. The superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1) *DA*	(2) *DA*	(3) *DA*
	[t−1, t +1]	[t−3, t +3]	[t−5, t +5]
IDD	-0.011**	-0.010**	-0.011***
	(-2.019)	(-2.212)	(-2.744)
Ln (total assets)	-0.008***	-0.009***	-0.008***
	(-3.960)	(-6.998)	(-8.544)
ROA	0.135***	0.147***	0.139***
	(10.323)	(18.194)	(13.256)
R&D	-0.026	-0.049***	-0.031
	(-1.106)	(-2.813)	(-1.613)
SG&A	0.012	0.019**	0.019**
	(1.068)	(2.533)	(2.304)
Acquisition	-0.008	-0.005	-0.005
	(-1.210)	(-1.217)	(-1.361)
Issuance	0.020***	0.020***	0.019***
	(3.331)	(3.968)	(5.499)
Institution	0.029***	0.007	0.012
	(2.809)	(0.641)	(1.482)
Ln(1+Analyst)	-0.008**	-0.004	-0.004**
	(-2.378)	(-1.571)	(-2.225)
Tight covenant	-0.010	-0.006	-0.003
	(-1.279)	(-1.181)	(-0.501)
Meet/Beat	0.016***	0.012***	0.013***
	(3.397)	(5.015)	(6.241)
Sales growth	0.020***	0.019***	0.020***
	(3.418)	(5.375)	(4.790)
MB	-0.000	0.000	0.001*
	(-0.280)	(0.673)	(1.687)
Net operating assets	-0.007***	-0.007***	-0.010***
	(-3.613)	(-4.314)	(-5.305)
Sales volatility	-0.018*	-0.002	0.005
	(-1.861)	(-0.185)	(0.437)
Ln (operating cycle)	0.013***	0.021***	0.019***
	(6.277)	(6.609)	(5.494)
Big N	-0.000	-0.001	-0.004
	(-0.007)	(-0.073)	(-0.530)
Leverage	-0.014	-0.008	-0.007
	(-1.203)	(-0.777)	(-0.645)
Ln(GDP)	-0.002	-0.002	-0.001
	(-0.527)	(-0.937)	(-0.811)
Unemployment rate	-0.084	-0.000	0.039
	(-0.396)	(-0.001)	(0.280)
Hightech	-0.012	-0.010	-0.017

	(-0.308)	(-0.377)	(-0.800)
Education	0.002	-0.007	0.019
	(0.060)	(-0.181)	(0.622)
Year Fixed Effects	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes
Intercept	-0.024	-0.066**	-0.071**
	(-0.640)	(-2.098)	(-2.169)
N	5,442	11,352	15,924
Adj_R^2	0.240	0.152	0.122

Table 5. Controlling for Unobservable Local Economic Conditions

This table examines whether the observed effects are driven by unobservable changes in local economic conditions using a sample of treated firms and close-by control firms (located in nonlegislating states). For each treated firm, we match it to a control firm that is in the same industry, in a neighboring state without adopting IDD, and closest in performance-matched discretionary accrual in the year of IDD adoption. In columns (1)–(4), we further require the distance between the treated and matched control firms to be within 100, 90, 80, and 70 miles, respectively. If we cannot find a close-by matched control firm to a given treated firm, we drop this treated firm from the sample. By construction, our sample firms in this table are the ones located close to state borders. Variable definitions are provided in Appendix A. All continuous variables are winsorized at the first and 99th percentiles. *T*-statistics based on robust standard errors clustered by state are in parentheses. The superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1) *DA*	(2) *DA*	(3) *DA*	(4) *DA*
	100 miles	90 miles	80 miles	70 miles
IDD	-0.017**	-0.021***	-0.020***	-0.014*
	(-2.109)	(-3.277)	(-3.277)	(-1.692)
Ln (total assets)	-0.008**	-0.011***	-0.009**	-0.014***
	(-2.485)	(-3.450)	(-2.569)	(-3.981)
ROA	0.041***	0.033***	0.041***	0.038***
	(6.396)	(6.184)	(3.401)	(3.898)
R&D	-0.143**	-0.190***	-0.120	-0.127*
	(-2.667)	(-3.063)	(-1.465)	(-1.866)
SG&A	0.027	0.048**	0.029	0.040
	(1.479)	(2.447)	(1.097)	(1.641)
Acquisition	-0.008	-0.007	-0.004	-0.013
	(-1.118)	(-1.130)	(-0.531)	(-1.668)
Issuance	0.018***	0.019***	0.015***	0.013**
	(4.915)	(3.848)	(3.246)	(2.109)
Institution	-0.006	0.002	-0.015	-0.004
	(-0.443)	(0.127)	(-1.005)	(-0.312)
Ln(1+Analyst)	0.005	0.005	0.004	0.004
	(1.627)	(1.338)	(0.791)	(0.927)
Tight covenant	0.012	0.012	0.013	0.009
	(1.410)	(1.589)	(1.424)	(0.935)
Meet/Beat	0.018***	0.016***	0.018***	0.026***
	(3.742)	(3.005)	(2.959)	(4.927)
Sales growth	0.017***	0.018***	0.021***	0.016*
	(2.864)	(3.114)	(3.180)	(1.971)
MB	-0.001	0.000	0.001	0.002
	(-0.589)	(0.309)	(0.938)	(1.350)
Net operating assets	-0.009**	-0.006*	-0.006	-0.008*
	(-2.308)	(-1.854)	(-1.675)	(-1.802)
Sales volatility	-0.016	-0.028	-0.019	-0.036
	(-0.618)	(-1.100)	(-0.697)	(-1.630)
Ln (operating cycle)	-0.007	-0.004	-0.004	-0.009
	(-1.102)	(-0.613)	(-0.585)	(-1.010)
Big N	-0.015	-0.005	-0.007	-0.004
	(-1.553)	(-0.560)	(-0.623)	(-0.415)
Leverage	-0.034*	-0.038**	-0.045**	-0.009
	(-1.903)	(-2.083)	(-2.251)	(-0.626)
Ln(GDP)	0.005	0.006	0.003	-0.002
	(1.213)	(1.418)	(0.888)	(-0.378)
Unemployment rate	0.081	0.229	0.327	0.391
	(0.276)	(0.735)	(1.151)	(1.268)
Hightech	0.021	-0.079	-0.165***	-0.086
	(0.379)	(-1.549)	(-2.954)	(-1.078)

Education	-0.043	-0.025	0.031	0.060
	(-0.626)	(-0.345)	(0.371)	(0.626)
Year Fixed Effects	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes
Intercept	-0.013	-0.067	-0.083	0.021
	(-0.193)	(-0.920)	(-1.104)	(0.248)
N	5,550	4,860	3,845	3,026
Adj_R^2	0.102	0.115	0.118	0.111

Table 6. Rejection of Previously Adopted IDD

This table reports the impacts of the rejection of previously adopted IDD on the firm's discretionary accruals. The sample includes all post-IDD firm-year observations in states that adopt IDD. The dependent variable is *DA*. The indicator variable *IDD_Rejection* takes the value of one if the firm-year observation is after the firm's headquarter state rejects its previously adopted IDD and zero otherwise. Variable definitions are provided in Appendix A. All continuous variables are winsorized at the first and 99th percentiles. *T*-statistics based on robust standard errors clustered by state are in parentheses. The superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1) *DA*	(2) *DA*
IDD_Rejection	0.011**	0.013***
	(2.317)	(3.313)
Ln (total assets)		-0.019***
		(-4.806)
ROA		0.239***
		(16.408)
R&D		-0.060*
		(-1.919)
SG&A		0.052***
		(3.598)
Acquisition		-0.012***
		(-3.235)
Issuance		0.018***
		(6.191)
Institution		0.015
		(1.654)
Ln(1+Analyst)		0.001
		(0.711)
Tight covenant		0.015***
		(5.124)
Meet/Beat		0.015***
		(6.588)
Sales growth		0.010**
		(2.211)
MB		0.001
		(1.708)
Net operating assets		-0.007***
		(-3.090)
Sales volatility		-0.004
		(-0.402)
Ln (operating cycle)		0.009**
		(2.137)
Big N		-0.014**
		(-2.262)
Leverage		-0.012
		(-1.585)
Ln(GDP)		0.002
		(0.232)
Unemployment rate		-0.302

		(-1.120)
Hightech		-0.012
		(-0.154)
Education		-0.017
		(-0.231)
Year Fixed Effects	Yes	Yes
Firm Fixed Effects	Yes	Yes
Intercept	0.030	0.048
	(1.575)	(0.361)
N	49,506	49,506
Adj_R^2	0.163	0.221

Table 7. Alternative Earnings Management Measures

This table reports the logistic tests that examine the impacts of the IDD on alternative earnings management measures. The sample consists of 94,912 firm-year observations. In column (1), the dependent variable *Down-restate* is an indicator that takes the value of 1 for income-decreasing restatements in a year and zero otherwise. In column (2), the dependent variable *Write-down* is an indicator that takes the value of 1 if the firm writes down in a year and zero otherwise. In column (3), the dependent variable *Negative special items* is an indicator that takes the value of 1 if the firm records negative special items in a year and zero otherwise. The indicator variable *IDD* takes the value of one if the IDD is recognized in a state and zero otherwise. Variable definitions are provided in Appendix A. All continuous variables are winsorized at the first and 99th percentiles. *T*-statistics based on robust standard errors clustered by state are in parentheses. The superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1) *Down-restate*	(2) *Write-down*	(3) *Negative special items*
IDD	0.299**	0.139**	0.137**
	(1.983)	(2.077)	(2.569)
Ln (total assets)	0.410*	-0.014	0.225***
	(1.675)	(-0.165)	(5.886)
ROA	-0.362**	-0.572***	-0.907***
	(-2.350)	(-6.157)	(-14.561)
R&D	-0.699	-0.727***	0.102
	(-1.300)	(-3.181)	(0.546)
SG&A	-0.111	-0.525***	-0.653***
	(-0.453)	(-5.688)	(-10.633)
Acquisition	-0.107	0.033	0.356***
	(-0.959)	(0.472)	(4.787)
Issuance	-0.157***	-0.108***	-0.054***
	(-2.612)	(-2.593)	(-2.726)
Institution	0.061	-0.396**	-0.213**
	(0.235)	(-2.262)	(-2.382)
Ln(1+Analyst)	-0.181*	0.073*	-0.025
	(-1.846)	(1.921)	(-0.821)
Tight covenant	-0.218*	-0.126**	0.005
	(-1.861)	(-2.150)	(0.179)
Meet/Beat	-0.186*	-0.184***	-0.219***
	(-1.953)	(-3.984)	(-8.194)
Sales growth	-0.056	-0.126***	-0.092***
	(-1.156)	(-4.439)	(-5.433)
MB	-0.002	-0.007**	-0.007***
	(-0.578)	(-2.059)	(-3.104)
Net operating assets	0.051*	0.077***	0.060***
	(1.714)	(7.016)	(7.958)
Sales volatility	0.520	0.004	0.093
	(1.488)	(0.035)	(1.311)
Ln (operating cycle)	0.061	0.039	0.055
	(0.207)	(0.294)	(0.714)
Big N	0.239	0.132	0.281***
	(1.161)	(1.641)	(4.412)
Leverage	-0.293*	-0.003	0.014
	(-1.868)	(-0.086)	(0.484)
Ln(GDP)	0.099	-0.009	-0.033
	(0.064)	(-0.010)	(-0.070)
Unemployment rate	-2.139	-1.745	4.867***
	(-0.250)	(-0.424)	(2.685)
Hightech	-1.107	2.570**	0.386

	(-0.460)	(2.208)	(0.416)
Education	-3.943	-0.218	0.220
	(-0.575)	(-0.053)	(0.085)
Year Fixed Effects	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes
Pseudo R^2	0.091	0.088	0.062

Table 8. Profitability-based Alternative Explanation

This table examines whether the effect of IDD on upward earnings manipulation is due to IDD increasing firms' future profitability. In column (1), we re-estimate the baseline regression of column (2) of Table 3 by additionally controlling for one-year-ahead and two-year-ahead ROA. In column (2), we examine the effect of the adoption of IDD on ROA; the regression specification is the same as that in column (2) of Table 3, except that we use ROA as the dependent variable. In column (3), we examine the effect of the adoption of IDD on operating cash flow (cash flow from operating activities normalized by lagged total assets); the regression specification is the same as that in column (2), except that we use operating cash flow as the dependent variable. The indicator variable *IDD* takes the value of one if the IDD is recognized in a state and zero otherwise. Variable definitions are provided in Appendix A. All continuous variables are winsorized at the first and 99th percentiles. *T*-statistics based on robust standard errors clustered by state are in parentheses. The superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1) *DA*	(2) *ROA*	(3) *Operating cash flow*
IDD	-0.010***	-0.004	0.001
	(-5.369)	(-0.752)	(0.218)
Ln (total assets)	-0.021***	0.036***	0.026***
	(-11.095)	(9.311)	(8.668)
ROA_t	0.224***		
	(24.312)		
ROA_{t+1}	-0.033***		
	(-4.078)		
ROA_{t+2}	-0.016*		
	(-1.955)		
R&D	-0.118***	-0.853***	-0.622***
	(-2.985)	(-14.200)	(-14.091)
SG&A	0.025**	-0.227***	-0.143***
	(2.581)	(-9.909)	(-12.610)
Acquisition	-0.009**	0.035***	0.009***
	(-2.624)	(16.193)	(3.480)
Issuance	0.017***	0.002	-0.023***
	(7.415)	(0.809)	(-8.660)
Institution	0.004	0.039***	0.020**
	(0.508)	(2.725)	(2.152)
Ln(1+Analyst)	-0.000	-0.001	-0.001
	(-0.251)	(-0.385)	(-0.554)
Tight covenant	0.008***	0.010***	0.002
	(3.415)	(3.358)	(0.652)
Meet/Beat	0.014***	0.011***	0.004***
	(9.156)	(8.035)	(3.047)
Sales growth	0.010***	0.023***	-0.002
	(4.778)	(5.704)	(-0.715)
MB	0.000*	0.002***	0.001**
	(1.803)	(5.342)	(2.578)
Net operating assets	-0.007***	-0.012***	-0.005***
	(-5.354)	(-7.638)	(-4.084)
Sales volatility	0.002	0.019	0.007
	(0.398)	(1.315)	(0.930)
Ln (operating cycle)	0.007**	-0.066***	-0.053***
	(2.177)	(-12.439)	(-16.303)
Big N	-0.009	-0.023***	-0.024***
	(-1.484)	(-6.505)	(-6.001)
Leverage	-0.006	-0.078***	-0.101***
	(-1.181)	(-8.578)	(-13.166)

Ln(GDP)	-0.000	-0.005	-0.004
	(-0.014)	(-1.005)	(-1.087)
Unemployment rate	-0.232**	0.136	0.120
	(-2.216)	(0.684)	(1.086)
Hightech	0.005	-0.098	-0.053
	(0.152)	(-1.254)	(-0.908)
Education	0.000	0.004	-0.021
	(0.012)	(0.052)	(-0.395)
Year Fixed Effects	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes
Intercept	0.052	0.450***	0.341***
	(0.897)	(7.028)	(7.110)
N	80,417	94,912	94,912
Adj_R2	0.201	0.718	0.655

Table 9. Employment Contract Terms

This table reports the difference-in-differences tests that examine the impacts of the IDD on the employees' compensation. *Ln(Salary)* is the natural logarithm of basic salary. *Ln(Variable pay)* is the natural logarithm of variable compensation, including bonus, restricted stock awards, option awards, long-term incentive plan, and all other compensation. In columns (1) and (2), we focus on CEO. In columns (3) and (4), we focus on the top five highest paid managers. The sample period is 1995–2011. The indicator variable *IDD* takes the value of one if the IDD is recognized in a state and zero otherwise. Variable definitions are provided in Appendix A. All continuous variables are winsorized at the first and 99th percentiles. *T*-statistics based on robust standard errors clustered by state are in parentheses. The superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1) Ln(Salary)	(2) Ln(Variable pay)	(3) Ln(Salary)	(4) Ln(Variable pay)
	CEO		Top 5 Managers	
IDD	0.010	-0.113**	0.004	-0.095**
	(0.749)	(-2.540)	(0.519)	(-2.190)
Ln (total assets)	0.114***	0.393***	0.093***	0.288***
	(8.425)	(17.028)	(13.241)	(17.717)
ROA	0.060***	0.511***	0.032***	0.495***
	(4.954)	(5.818)	(2.973)	(6.390)
R&D	0.058	0.310*	0.004	0.257**
	(1.578)	(1.974)	(0.208)	(2.473)
SG&A	0.009	0.260***	-0.006	0.230***
	(0.901)	(4.070)	(-0.999)	(7.132)
Acquisition	-0.016**	0.002	-0.020***	-0.012
	(-2.270)	(0.101)	(-5.317)	(-0.689)
Issuance	-0.007	0.047	-0.005	0.051***
	(-1.651)	(1.551)	(-1.572)	(4.563)
Institution	0.101***	0.596***	0.059***	0.481***
	(4.749)	(7.817)	(6.108)	(7.676)
Ln(1+Analyst)	0.012**	0.005	0.004	-0.014
	(2.043)	(0.223)	(1.143)	(-0.926)
Tight covenant	0.017**	0.028	0.007	0.024
	(2.220)	(1.060)	(1.660)	(1.483)
Meet/Beat	0.017***	-0.025	0.003	-0.003
	(3.697)	(-1.347)	(1.153)	(-0.197)
Sales growth	-0.012*	0.035**	-0.017***	0.047***
	(-1.974)	(2.433)	(-7.072)	(4.197)
MB	0.000	0.004**	-0.001**	0.005***
	(0.539)	(2.240)	(-2.465)	(3.219)
Net operating assets	-0.003	-0.031**	-0.002	-0.031***
	(-1.487)	(-2.474)	(-1.221)	(-4.034)
Sales volatility	-0.004	0.120	-0.019**	0.076
	(-0.187)	(1.501)	(-2.093)	(1.644)
Ln (operating cycle)	-0.008	-0.071***	-0.014***	-0.058***
	(-0.987)	(-3.295)	(-4.776)	(-5.469)
Big N	0.036**	0.061	0.051***	0.090**
	(2.568)	(1.025)	(5.041)	(2.450)
Leverage	-0.026*	-0.104**	-0.032**	-0.102***
	(-1.730)	(-2.602)	(-2.397)	(-3.753)
Ln(GDP)	-0.019	0.014	0.005	0.033
	(-1.036)	(0.282)	(0.584)	(1.187)
Unemployment rate	-0.172	2.986	-0.483	0.992
	(-0.314)	(0.953)	(-1.131)	(0.400)
Hightech	0.056	-1.194**	0.024	-1.137***

	(0.327)	(-2.373)	(0.256)	(-3.330)
Education	0.198	0.580	-0.083	0.604
	(0.989)	(0.950)	(-0.814)	(1.380)
Year Fixed Effects	Yes	Yes	Yes	Yes
Person Fixed Effects	Yes	Yes	Yes	Yes
Intercept	-2.043***	-4.252***	-2.605***	-5.019***
	(-7.140)	(-6.102)	(-18.516)	(-13.732)
N	42,901	39,062	196,547	181,995
Adj_R^2	0.808	0.673	0.797	0.675

Table 10. Employee Productivity

This table examines the effect of the IDD on employee productivity. In column (1), the dependent variable is income before extraordinary items (in millions) divided by the number of employees. In column (2), the dependent variable is the natural logarithm of one plus the number of patents applied (and finally granted). In column (3), the dependent variable is the natural logarithm of one plus the number of citations received by these patents filed. The indicator variable *IDD* takes the value of one if the IDD is recognized in a state and zero otherwise. Variable definitions are provided in Appendix A. All continuous variables are winsorized at the first and 99th percentiles. *T*-statistics based on robust standard errors clustered by state are in parentheses. The superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1) *Income/Emp*	(2) *Ln(1+Pat)*	(3) *Ln(1+Cite)*
IDD	-0.001	-0.034*	-0.042**
	(-1.273)	(-1.706)	(-2.595)
Ln (total assets)	0.013***	0.089***	0.060***
	(7.152)	(4.968)	(4.569)
ROA	0.118***	-0.019	0.001
	(19.491)	(-1.220)	(0.070)
R&D	-0.003	0.252***	0.325***
	(-0.332)	(5.458)	(6.638)
SG&A	0.031***	0.003	0.006
	(7.687)	(0.378)	(0.688)
Acquisition	-0.010***	-0.005	-0.004
	(-5.853)	(-0.693)	(-0.346)
Issuance	-0.004***	-0.005	-0.000
	(-4.421)	(-0.865)	(-0.059)
Institution	0.038***	-0.015	-0.061**
	(5.649)	(-0.624)	(-2.152)
Ln(1+Analyst)	-0.009***	0.046***	0.054***
	(-5.017)	(4.669)	(6.259)
Tight covenant	0.006**	-0.012	0.003
	(2.497)	(-1.112)	(0.225)
Meet/Beat	0.006***	0.002	-0.000
	(6.816)	(0.249)	(-0.024)
Sales growth	0.014***	-0.006**	0.003
	(8.862)	(-2.403)	(1.012)
MB	0.000***	0.001***	0.002***
	(5.222)	(3.657)	(2.704)
Net operating assets	-0.011***	0.004***	0.002
	(-11.218)	(2.826)	(0.857)
Sales volatility	-0.003	-0.008	0.003
	(-0.988)	(-0.901)	(0.184)
Ln (operating cycle)	-0.010***	-0.026***	-0.016*
	(-4.545)	(-3.592)	(-1.820)
Big N	-0.001	-0.038***	-0.056***
	(-0.351)	(-2.781)	(-3.770)
Leverage	-0.012***	-0.028***	-0.023***
	(-5.112)	(-3.114)	(-2.773)
Ln(GDP)	-0.003***	-0.002	-0.021
	(-3.003)	(-0.069)	(-0.874)
Unemployment rate	0.170**	0.361	1.179
	(2.236)	(0.455)	(1.195)
Hightech	0.002	0.241	0.221
	(0.170)	(1.021)	(1.101)
Education	-0.006	-0.654***	-0.714***

	(-0.225)	(-3.977)	(-4.099)
Year Fixed Effects	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes
Intercept	0.006	0.301	0.685***
	(0.339)	(1.621)	(2.965)
N	94,912	83,229	83,229
Adj_ R^2	0.556	0.827	0.757

Table 11. Testing H2

This table reports the subsample analysis based on the importance of human capital to the firm. In Panel A, we use intangible assets intensity to measure the importance of human capital. A firm is classified as the one with high (low) intangible-assets intensity, if its intangible-assets intensity is above (below) the sample median in a given year. In Panel B, we use percentage of knowledge workers among all workers to measure the importance of human capital. A firm is classified as the one with high (low) knowledge-worker intensity, if its percentage of knowledge workers among all workers is above (below) the sample median in a given year. In Panel C, we use percentage of inventors (i.e., the employees who produce patents) among all workers to measure the importance of human capital. A firm is classified as the one with high (low) inventor intensity, if its percentage of inventors among all workers is above (below) the sample median in a given year. The dependent variable is *DA*. The indicator variable *IDD* takes the value of one if the IDD is recognized in a state and zero otherwise. Variable definitions are provided in Appendix A. All continuous variables are winsorized at the first and 99th percentiles. *T*-statistics based on robust standard errors clustered by state are in parentheses. The superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A Intangible-Assets Intensity

	(1) *DA*	(2) *DA*
	High Intangible-Assets Intensity	Low Intangible-Assets Intensity
IDD	-0.018***	-0.004*
	(-4.172)	(-1.700)
Ln (total assets)	-0.024***	-0.020***
	(-7.975)	(-5.238)
ROA	0.196***	0.238***
	(13.617)	(15.245)
R&D	-0.195***	0.007
	(-6.314)	(0.182)
SG&A	0.036***	0.053***
	(3.981)	(3.271)
Acquisition	-0.014***	-0.008**
	(-3.045)	(-2.201)
Issuance	0.024***	0.014***
	(7.373)	(6.124)
Institution	0.029***	0.006
	(3.371)	(0.513)
Ln(1+Analyst)	-0.002	0.000
	(-0.680)	(0.136)
Tight covenant	0.007	0.008***
	(1.526)	(2.788)
Meet/Beat	0.020***	0.012***
	(6.331)	(9.899)
Sales growth	0.005**	0.015**
	(2.283)	(2.161)
MB	0.001	0.000
	(1.309)	(1.220)
Net operating assets	-0.006***	-0.009***
	(-3.803)	(-2.923)
Sales volatility	0.007	-0.009
	(0.852)	(-0.870)

Ln (operating cycle)	0.003	0.012***
	(0.565)	(3.151)
Big N	-0.003	-0.016***
	(-0.294)	(-2.815)
Leverage	-0.013	0.002
	(-1.324)	(0.236)
Ln(GDP)	-0.002	0.006
	(-0.616)	(1.262)
Unemployment rate	-0.220	-0.287*
	(-1.401)	(-1.746)
Hightech	0.032	-0.024
	(0.834)	(-0.441)
Education	-0.032	0.000
	(-0.438)	(0.004)
Year Fixed Effects	Yes	Yes
Firm Fixed Effects	Yes	Yes
Intercept	0.117**	-0.072
	(2.018)	(-1.090)
P value of test of equal coefficients on IDD between (1) and (2)	0.003***	
N	47,461	47,451
Adj_R^2	0.233	0.260

Panel B Knowledge-Worker Intensity

	(1) DA	(2) DA
	High Knowledge-Worker Intensity	Low Knowledge-Worker Intensity
IDD	-0.013***	-0.002
	(-4.219)	(-1.225)
Ln (total assets)	-0.025***	-0.013***
	(-12.263)	(-3.236)
ROA	0.206***	0.247***
	(18.358)	(23.429)
R&D	-0.094**	-0.174***
	(-2.347)	(-2.773)
SG&A	0.031***	0.020
	(2.830)	(1.237)
Acquisition	-0.016***	-0.008**
	(-3.700)	(-2.329)
Issuance	0.018***	0.017***
	(6.764)	(8.573)
Institution	0.014**	0.014
	(2.097)	(1.462)
Ln(1+Analyst)	-0.002	0.001
	(-0.530)	(0.383)
Tight covenant	0.006***	0.009**
	(2.763)	(2.588)
Meet/Beat	0.018***	0.014***
	(9.356)	(6.291)
Sales growth	0.012***	0.006*
	(3.494)	(1.677)
MB	0.001**	0.001
	(2.028)	(1.475)
Net operating assets	-0.006***	-0.009***
	(-3.580)	(-3.200)
Sales volatility	0.017**	-0.005
	(2.270)	(-0.560)
Ln (operating cycle)	0.007*	0.018***
	(1.693)	(3.349)
Big N	-0.013*	0.004
	(-1.885)	(0.369)
Leverage	-0.017***	0.005
	(-2.761)	(0.665)
Ln(GDP)	0.002	0.002
	(0.551)	(0.520)
Unemployment rate	-0.389**	-0.059
	(-2.600)	(-0.457)
Hightech	-0.052	0.084
	(-1.064)	(1.631)
Education	0.024	0.016
	(0.387)	(0.255)
Year Fixed Effects	Yes	Yes
Firm Fixed Effects	Yes	Yes
Intercept	0.045	-0.110
	(0.737)	(-1.455)
P value of test of equal coefficients on IDD between (1) and (2)	0.003***	
N	50,771	42,496
Adj_R^2	0.245	0.180

Panel C Inventor Intensity

	(1) DA	(2) DA
	High Inventor Intensity	Low Inventor Intensity
IDD	-0.018**	-0.001
	(-2.429)	(-0.273)
Ln (total assets)	-0.029***	-0.005
	(-9.537)	(-1.494)
ROA	0.154***	0.168***
	(12.201)	(12.899)
R&D	-0.157***	-0.257***
	(-4.801)	(-4.130)
SG&A	0.049***	-0.011
	(4.660)	(-0.862)
Acquisition	-0.028***	0.000
	(-4.244)	(0.150)
Issuance	0.023***	0.013***
	(9.139)	(5.776)
Institution	0.027**	0.024**
	(2.242)	(2.375)
Ln(1+Analyst)	-0.004*	-0.003
	(-1.876)	(-1.559)
Tight covenant	0.003	0.013***
	(0.653)	(5.307)
Meet/Beat	0.016***	0.012***
	(5.455)	(4.547)
Sales growth	0.009***	0.022***
	(4.057)	(2.743)
MB	0.000	-0.000
	(0.776)	(-0.643)
Net operating assets	-0.004***	-0.045***
	(-2.916)	(-3.341)
Sales volatility	0.010	-0.020
	(1.027)	(-1.339)
Ln (operating cycle)	0.004	0.041***
	(0.699)	(5.946)
Big N	-0.010	0.002
	(-1.164)	(0.366)
Leverage	-0.037***	-0.001
	(-3.917)	(-0.153)
Ln(GDP)	-0.000	0.010***
	(-0.021)	(2.895)
Unemployment rate	-0.185	-0.126
	(-1.060)	(-0.923)
Hightech	0.090	0.042
	(1.501)	(1.343)
Education	-0.112	-0.004
	(-1.528)	(-0.104)
Year Fixed Effects	Yes	Yes
Firm Fixed Effects	Yes	Yes
Intercept	0.126*	-0.286***
	(1.917)	(-5.016)
P value of test of equal coefficients on IDD between (1) and (2)	0.027**	
N	23,280	23,258
Adj_R^2	0.150	0.122

Table 12. Testing H3

This table reports the subsample analysis based on ex-ante labor mobility. In Panel A, we divide the sample based on whether the firm has a defined benefit pension plan. In Panel B, we divide the sample based on the number of companies in the same two-digit SIC industry. A firm is classified as the one with a large (small) number of industry peers if its number of industry peer firms is above (below) the sample median in a given year. The dependent variable is *DA*. The indicator variable *IDD* takes the value of one if the IDD is recognized in a state and zero otherwise. Variable definitions are provided in Appendix A. All continuous variables are winsorized at the first and 99th percentiles. *T*-statistics based on robust standard errors clustered by state are in parentheses. The superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A Defined Benefit Pension plan

	(1) *DA*	(2) *DA*
	Without Defined Benefit Pension Plan	With Defined Benefit Pension Plan
IDD	-0.011***	-0.003
	(-4.759)	(-1.185)
Ln (total assets)	-0.023***	-0.002
	(-8.628)	(-0.669)
ROA	0.216***	0.107***
	(20.077)	(3.515)
R&D	-0.103***	-0.501***
	(-3.455)	(-5.682)
SG&A	0.036***	0.003
	(3.401)	(0.147)
Acquisition	-0.015***	0.001
	(-4.159)	(0.252)
Issuance	0.021***	0.009***
	(7.912)	(6.001)
Institution	0.009	0.028***
	(1.408)	(3.367)
Ln(1+Analyst)	0.001	-0.004
	(0.377)	(-1.180)
Tight covenant	0.007***	0.009**
	(2.851)	(2.511)
Meet/Beat	0.018***	0.009***
	(12.182)	(3.634)
Sales growth	0.010***	-0.001
	(4.008)	(-0.087)
MB	0.001**	0.000
	(2.568)	(0.696)
Net operating assets	-0.006***	-0.039***
	(-5.278)	(-6.943)
Sales volatility	0.005	-0.011
	(0.775)	(-0.959)
Ln (operating cycle)	0.005	0.041***
	(1.638)	(4.842)
Big N	-0.008	-0.003
	(-1.032)	(-0.456)
Leverage	-0.004	-0.001
	(-0.746)	(-0.072)

Ln(GDP)	0.001	0.001
	(0.284)	(0.397)
Unemployment rate	-0.256**	-0.192
	(-2.243)	(-1.580)
Hightech	0.009	-0.021
	(0.215)	(-0.650)
Education	0.001	-0.004
	(0.025)	(-0.108)
Year Fixed Effects	Yes	Yes
Firm Fixed Effects	Yes	Yes
Intercept	0.038	-0.182***
	(0.746)	(-3.205)
P value of test of equal coefficients on IDD between (1) and (2)	0.034**	
N	79,306	15,606
Adj_R^2	0.213	0.106

Panel B Number of Industry Peer Firms

	(1) *DA*	(2) *DA*
	Large Number of Industry Peers	Small Number of Industry Peers
IDD	-0.013***	-0.004
	(-4.179)	(-1.245)
Ln (total assets)	-0.027***	-0.012**
	(-8.649)	(-2.638)
ROA	0.204***	0.237***
	(16.169)	(11.214)
R&D	-0.112***	0.009
	(-3.397)	(0.095)
SG&A	0.035***	0.009
	(2.703)	(0.784)
Acquisition	-0.022***	-0.000
	(-3.586)	(-0.107)
Issuance	0.019***	0.018***
	(6.778)	(8.494)
Institution	0.018**	0.012
	(2.327)	(1.309)
Ln(1+Analyst)	-0.001	0.001
	(-0.330)	(0.294)
Tight covenant	0.007***	0.008**
	(3.140)	(2.263)
Meet/Beat	0.018***	0.014***
	(10.183)	(5.873)
Sales growth	0.008**	0.013***
	(2.525)	(3.777)
MB	0.001*	0.000
	(1.677)	(0.982)
Net operating assets	-0.006***	-0.009***
	(-3.077)	(-4.259)
Sales volatility	0.012	0.002
	(1.563)	(0.233)
Ln (operating cycle)	0.000	0.019***
	(0.096)	(3.089)
Big N	-0.008	-0.004
	(-1.306)	(-0.502)
Leverage	-0.020***	0.005
	(-2.739)	(0.763)
Ln(GDP)	0.001	0.003
	(0.184)	(0.646)
Unemployment rate	-0.466***	0.015
	(-3.081)	(0.092)
Hightech	-0.030	0.040
	(-0.614)	(0.829)
Education	-0.077	0.059
	(-1.176)	(0.847)
Year Fixed Effects	Yes	Yes
Firm Fixed Effects	Yes	Yes
Intercept	0.127**	-0.135
	(2.146)	(-1.577)
P value of test of equal coefficients on IDD between (1) and (2)	0.027**	
N	50,219	44,693
Adj_R^2	0.249	0.166