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Managerial labor mobility and banks' financial reporting quality

Saeyeon Oh, Kwangwoo Park

Korea Advanced Institute of Science and Technology, South Korea

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ABSTRACT

This paper explores the relationship between managers' labor mobility and the financial reporting quality of banks. Using the state-level adoption of the Inevitable Disclosure Doctrine (IDD) as an exogenous shock discouraging labor mobility, we show that adoption of the IDD is associated with a decline in financial reporting quality, as measured by discretionary loan loss provisions. The effect is larger for banks with managers who have limited outside job opportunities and smaller for banks with tight regulatory oversight. Our results support the view from the career concern hypothesis that bank managers facing restrictions on mobility have greater incentives to engage in discretionary accounting.

1. Introduction

Since the 2008 global financial crisis, accounting and finance researchers have shown considerable interest in bank transparency. The crisis highlighted the economic impact of bank opacity and gave rise to the notion that a lack of information on bank asset quality can harm financial stability. The opacity of banks can exacerbate agency problems by hindering market discipline, and increase the risks of panic and contagion during a crisis (Nier and Baumann, 2006; Morgan, 2002). During the recent global financial crisis, investors sought information on banks' exposure to risks, but this was not readily available. Along with explicit government backing of the banking system, the public disclosure of stress test results helped to relieve the panic by providing investors with the needed information (Jungherr, 2018).[1] Since then, extensive literature has discussed how much transparency in banking should be required, and how transparency affects financial stability (see Acharya and Ryan (2016) for a survey). There is, however, still a paucity of research identifying the determinants of bank transparency, despite the necessity of effective monitoring and regulations. We aim to fill this gap by examining the effect of bank managers' labor mobility on financial reporting quality.

In this paper, we define labor mobility as the degree of friction in the labor market, in terms of how easily individuals can transfer from one job to another. Job switching is an important channel through which individuals increase their compensation or find better-suited jobs (Topel and Ward, 1992). Therefore, the possibility of job switching is used as leverage in negotiating compensation levels and working conditions both explicitly and implicitly. This is well demonstrated by recent retention awards to bank executives, that were granted as competition for talent intensified among banks.[2] The observed efforts made by banks to retain executives imply that a restriction on labor mobility would lower managers' bargaining power in the job market. Accordingly, managers' career concerns would increase. In response, to secure bargaining power, managers may have higher incentives to window-dress their performance by distorting information disclosed to the public. Consequently, the financial reporting quality would be compromised (hereinafter, we refer to this proposed effect as the career concern

E-mail addresses: saeyeonoh@kaist.ac.kr (S. Oh), kpark3@kaist.ac.kr (K. Park)

[1] See details of this assessment in the speech by Ben Bernanke, the former chair of the Federal Reserve, at https://www.federalreserve.gov/newsevents/speech/bernanke20130408a.htm.

[2] For a detailed description of this issue, see "In a Hot Job Market, Banks Dish out Pay Awards to Retain Executives," American Banker, June 7, 2022.

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hypothesis). There are a number of studies on industrial firms testing and supporting this hypothesis (Ali et al., 2019; Chen et al., 2018; Tang et al., 2021), but not yet for banks.

Examining the career concern hypothesis in the banking industry is important for the following reasons. First, understanding the factors that affect banks' financial reporting quality is crucial, considering its potential impact on financial stability. This is well demonstrated by the 2008 financial crisis. For this reason, the literature investigating bank opacity as an endogenously determined variable is growing (Babus and Farboodi, 2020; Jungherr, 2018; Kanagaretnam et al., 2014; Jiang et al., 2016; Jin et al., 2018; Jin et al., 2019).

Second, banks appear to be much more opaque than industrial firms (Morgan, 2002; Iannotta, 2006; Flannery et al., 2013; Sarkar et al., 2019), thus the role of managerial incentives in opacity is large. Sarkar et al. (2019) argue that the lack of information on individual loans and banks' ability to hide problems can significantly contribute to bank opacity. Due to this opacity, bond rating agencies tend to disagree more about the ratings of bank bonds than about the ratings of other firms' bonds (Morgan, 2002; Iannotta, 2006). The greater opacity of banks compared with that of industrial firms makes managerial incentives a more important mechanism for ensuring transparent financial reporting, underlining the need for a separate analysis of banks.

Finally, banks and industrial firms differ considerably in terms of the role and composition of employees, which leads to differences in earnings management incentives. Research argues that industrial firms manage earnings to convince employees of the financial health of the firm, as a strategy to retain employees (Gao et al., 2018). However, this explanation is unlikely to hold for banks. Loan officers, a representative group of bank employees, have expertise in financial analysis (Beatty et al., 2019). Therefore, it would be generally difficult for managers to impress employees with earnings manipulation in the banking sector. The differences in managerial incentives between banks and industrial firms can cause different reactions to the same shock. Therefore, results based on the analysis of industrial firms are not readily applicable to banks.

To explore the relationship between labor mobility and bank financial reporting quality, we use the state-level adoption of the Inevitable Disclosure Doctrine (IDD) as the quasi-experimental setting that discourages labor mobility of bank managers. This identification strategy is necessary for two reasons. First, there is no generally accepted measure of labor mobility for bank managers. Second, a simple OLS regression can lead to inconsistent estimates because banks' information disclosure is associated with the likelihood of managerial turnover which may affect labor mobility. To prevent this reverse causality issue, IDD adoption by state courts provides relevant and exogenous shocks to banks.[3] It is unlikely that banks can predict the adoption or rejection of the IDD. We define the adoption and rejection based on the precedent-setting legal cases, and courts tend to focus on specific characteristics of the case in hand, setting new precedents. However, none of the cases defining the adoption and rejection of the IDD (presented in Appendix B) involves banks. Furthermore, courts tend to make rapid decisions on cases involving trade secrets considering the potential loss to the relevant firms (Klasa et al., 2018), leaving little room for attempts to predict or influence the decisions. Based on these arguments, we take the difference-in-differences approach that compares banks in states adopting or rejecting the IDD and the control banks in states that do not change their view on the IDD.

Our results show that adoption of the IDD lowers the financial reporting quality of banks, measured by discretionary loan loss provisions (*DLLP*).[4] We find that discretionary provisioning increases by 13 percent after the state courts adopt the IDD, which restricts the labor mobility of managers. We also show that the rejection of the IDD has the opposite effect from the adoption - it decreases discretionary provisioning. Our findings imply that restrictions on managers' labor mobility impair the financial reporting quality of banks, a view consistent with the career concern hypothesis.

We further assess whether the relationship between the IDD and the financial reporting quality varies across banks consistent to the career concern hypothesis. First, we find that the effect of increasing discretionary provisioning is larger for banks that are less monitored by regulators and investors. We compare small (private) banks versus large (public) banks assuming that monitoring by regulators and investors deters discretionary accounting at large, public banks. Large banks are more likely to have standardized operations and rely heavily on hard information in the screening process (Park and Pennacchi, 2009; Cole et al., 2004; Berger et al., 2005), which can lower the monitoring costs. We also compare banks in concentrated markets to those in competitive markets, relying on the idea that competition enhances governance, peer-firm comparison, and monitoring efficiency of banks (Shleifer and Vishny, 1997; Jiang et al., 2016). The subsample regression results are as expected. Small banks, private banks, and banks in less competitive markets engage in significantly more discretionary provisioning after the adoption of the IDD. This effect is smaller for banks under intensive monitoring.

Next, we compare banks with small and large numbers of peers in the local market, using the number of peers as a proxy for the outside options for the managers following Yonker (2017). If there is only a small number of banks in the local market, bank managers' outside options are already limited without the adoption of the IDD. In addition, banks are more likely to be considered as direct rivals to each other. In such markets, the labor mobility would significantly drop after the adoption of the IDD. On the contrary, if there are many banks in the local market, bank managers' outside job opportunities would not be strictly limited by the IDD. Consistent with this view, we find that the effect of IDD adoption on discretionary provisioning is

[3] The IDD states that firms can stop a former employee from getting a job at a competitor firm when leaking trade secrets seems inevitable under the new employment. In a way to protect intellectual property of firms, the IDD effectively lowers the mobility of firm managers (Klasa et al., 2018), including those in the banking industry (Agarwal et al., 2019).

[4] *DLLP* is the amount of loan loss provisions (*LLP*) that is reported excessively more or excessively less, compared to the necessary amount to cover loan losses. We use *DLLP* as the measure of financial reporting quality as it is one of the most common measures in the previous literature (Beatty and Liao, 2014; Bushman and Williams, 2012; Beck and Narayanamoorthy, 2013; Kim and Kross, 1998; Jiang et al., 2016).

larger for banks with a small number of peers nearby. The results further support the hypothesis that labor mobility affects banks' financial reporting quality via its effect on the managers' career concerns.

We use alternative measures of financial reporting quality to corroborate our results. We study how the IDD affects the relationship between loan loss provisions and actual loan charge-offs. A higher association between current provisions and subsequent loan charge-offs indicates that banks' financial reporting conforms to the OCC's and SEC's guidelines on provisioning, and that the provision figures covey accurate information on the loan portfolio risks (Altamuro and Beatty, 2010; Kanagaretnam et al., 2014; Beatty et al., 2019). We find that the IDD lowers the association, consistent with the analysis based on *DLLP*.

We also explore whether bank managers use provisioning discretion to inflate earnings. If managers' career concerns are the driving force of the results, managers are more likely to use accounting discretion to increase reported earnings. We find that the IDD leads to an increase in *DLLP* in cases that it leads to higher reported earnings, but the effect is not significant for the income-decreasing cases. Moreover, we provide evidence that IDD adoption increases the probability of banks reporting a slight increase in annual ROA, which corresponds to a case of just-meeting-or-beating an earnings benchmark. According to Graham et al. (2005), managers with career concerns care about meeting earnings targets because missing the targets signals incompetence in the labor market. Our findings suggest that the IDD enhances such incentives, and thereby impairs financial reporting quality.

Our study contributes to the literature on banks' incentives to adjust financial reporting quality. To the best of our knowledge, this paper is the first to provide empirical evidence that a restriction on bank managers' labor mobility leads to an increase in earnings management. Our results also add to the larger body of literature on agency problems in the banking industry. Earnings management is an agency problem that occurs due to the inability of shareholders to monitor managers around the clock. In this vein, our findings are pertinent to bank board members designing an employment contract for managers. Restricting their mobility to retain human capital or trade secrets may have an unintended side effect of elevated information asymmetry and monitoring costs.

Finally, we extend the literature on the relationship between labor mobility and corporate information disclosure. The existing studies mainly focus on industrial firms (Ali et al., 2019; Chen et al., 2018; Tang et al., 2021; Gao et al., 2018), and we add to the literature by focusing on a substantially different setting for banks. Our results highlight the similarities and differences between banks and industrial firms with respect to their incentives to manage earnings. The similarity lies in the managers' incentives to window-dress their performance in the labor market (Ali et al., 2019; Chen et al., 2018; Tang et al., 2021), while the difference is manifested in their efforts and ability to impress employees with earnings management (Gao et al., 2018). Understanding such differences can help regulators in improving the monitoring effectiveness of banks.

The rest of this paper is organized as follows. Section 2 develops our hypotheses based on the previous literature. Section 3 discusses the institutional background of the IDD, and the empirical strategy of the study. Section 4 describes the data, and Section 5 presents the empirical results on the relationship between managers' labor mobility and financial reporting quality of banks. Section 6 provides concluding remarks.

2. Literature review and hypothesis development

2.1. Banks' discretionary accounting

Literature has been pointing out that banks use financial reporting discretion strategically. Beatty and Liao (2014) discuss that bank managers influence reported earnings or regulatory capital with their accounting discretion. Garcia et al. (2021) find that banks in the European Union seasonally shrink their balance sheets to avoid selection as global systemically important banks (G-SIBs) and the regulatory capital surcharge. Berry et al. (2021) find a similar pattern in U.S. G-SIBs. These studies highlight the fact that bank managers use accounting discretion to their advantage, even in cases when such behavior is costly to bank investors.

This is an agency problem based on the bank managers' informational advantage over outside investors. In this vein, our study relates to the larger body of literature on agency problems and corporate governance at banks. Previous studies find that managerial incentives influence bank performance (Minnick et al., 2011), risk-taking (Pathan, 2009), payout policy (Srivastav et al., 2014), and earnings management (Cornett et al., 2009) in the financial services industry. Our focus on a labor market event that directly affects managerial incentives is motivated by these results.

2.2. Labor mobility and information disclosure

Our study also closely relates to the literature on how labor mobility affects firms' information disclosure, especially the ones that analyze the impact of the IDD as a labor mobility restriction. The existing results are mixed using data from industrial firms. For example, Ali et al. (2019) find an increase in the withholding of negative information after the adoption of the IDD, consistent with the analysis of Kothari et al. (2009) and Baginski et al. (2018). Using enforceability of non-compete covenants as the key variable of interest, Chen et al. (2018) and Tang et al. (2021) find supporting evidence that information quality provided by firms is compromised when managers' mobility is restricted. Both papers attribute the results to the increase in managers' career concerns.

Conversely, Gao et al. (2018) find that industrial firms' earnings management decreases with the adoption of the IDD. They explain that firm managers engage in upward earnings management to retain employees, and the IDD reduces such incentives by lowering the likelihood of employee turnover. However, we conjecture that this explanation is unlikely to hold for banks. Bank employees with access to trade secrets are financial experts who take part in both loan underwriting and provisioning decisions themselves (Beatty et al., 2019; Treacy and Carey, 2000). It will be difficult to change bank employees' perception of their employers' financial health by manipulating earnings. Meanwhile, there are studies focusing on proprietary costs of disclosure in examining the effect of the IDD on information disclosure (Li et al., 2018; Kim et al., 2021; Li and Li, 2020). Kim et al. (2021) use stock price synchronicity as the summary measure of the amount of information in the stock market, and find a negative relationship between the IDD and information disclosure.

This paper adds to the literature by extending the analysis on the relationship between labor mobility and information disclosure from industrial firms to the banking industry. We also seek to provide empirical evidence that better aligns with the career concern hypothesis than the proprietary cost explanation. Our hypotheses below highlight the similarities and differences between banks' and industrial firms' managers with respect to their incentives to engage in earnings management.

2.3. Hypothesis development

We hypothesize that a labor mobility restriction increases bank managers' career concerns and incentivizes them to manage earnings. Job switching is an important instrument for individuals to find a job that better fits their competence and preference (Topel and Ward, 1992). Accordingly, the possibility of job switching is used as leverage in negotiating compensation levels and working conditions both explicitly and implicitly. Therefore, a restriction on labor mobility leads to a decrease in the bargaining power of managers in the job market. This argument is further supported by both the recent anecdotal evidence on retention awards to bank executives and findings from the previous literature that a restriction on job switching decreases the compensation level of corporate executives (Gao et al., 2018; Garmaise, 2011). Subsequently, managers are more likely to have career concerns.

Career concerns generally incentivize managers to use financial reporting discretion to window-dress their performance. This is a feasible strategy for the managers as they have an informational advantage over outside shareholders. As described above, it is commonly observed in the literature that managers use their discretion to put a positive spin on the situation and make it more favorable to them (Beatty and Liao, 2014; Garcia et al., 2021; Ali et al., 2019; Chen et al., 2018; Tang et al., 2021). We conjecture that such incentives of managers trigger earnings management at banks, decreasing the financial reporting quality. These arguments are summarized in the first hypothesis below:

H1: A restriction on managers' labor mobility is negatively associated with financial reporting quality at banks.

Earnings management is an agency problem from the perspective of investors (more generally, stakeholders). Therefore, effective monitoring of bank managers can deter attempts to manage earnings to their advantage. Altamuro and Beatty (2010) provide evidence that effective internal control monitoring elevates financial reporting quality at banks. Kanagaretnam et al. (2014) highlight the role of institutional factors in shaping financial reporting practices at banks. They find that the earnings quality of banks is substantially higher in countries with institutional factors that effectively mitigate agency problems and protect outside investors. Based on the findings from Altamuro and Beatty (2010) and Kanagaretnam et al. (2014), we further test for the existence of the moderating role from investors' and regulators' monitoring of banks. As large and public banks are heavily monitored by regulators and investors, we develop our second hypothesis below:

H2: The impact of the IDD in lowering the quality of financial reporting is smaller for large banks and public banks.

3. Empirical strategy

3.1. Inevitable disclosure doctrine

Studying the relationship between managers' labor mobility and banks' financial reporting quality is challenging because unobservable traits of banks and the bank managers can have confounding effects. For example, risk-averse managers might report earnings more transparently, and care more about job stability. In addition, financial reporting can affect the turnover likelihood of the managers.

To address these concerns, we rely on adoption and rejection of the IDD by state courts as the quasi-experimental settings that change the labor mobility. The IDD states that a firm can stop a former employee from getting a job at a competitor firm when the leaking of the trade secrets seems inevitable under the new employment (Klasa et al., 2018). The purpose of the doctrine is to protect firms' trade secrets, which would promote investment in intellectual property. In the process, the doctrine effectively discourages mobility of employees, especially for the ones in managerial positions. Using Census Bureau's Survey of Income and Program Participation (SIPP), Klasa et al. (2018) show that labor mobility of individuals with managerial occupations significantly decreases in states that adopt the IDD. Fig. 1 presents the pattern of IDD adoption by the state courts. Panel A shows the timing of the adoption. New York is the first state to adopt the IDD (in 1919) and Kansas is the last (in 2006) before the end of our sample period. Panel A shows that there is substantial variation in the timing and geography

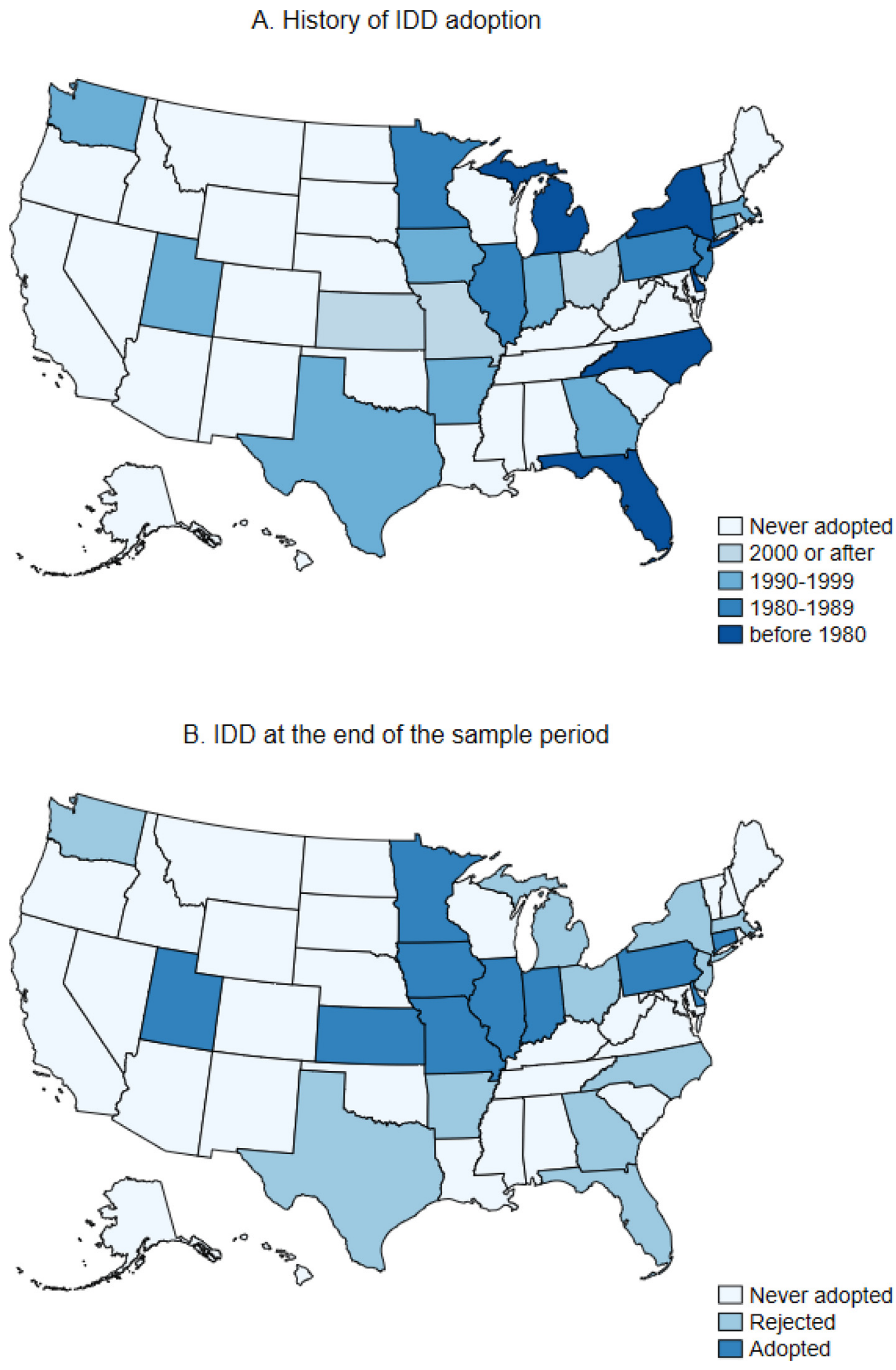

Fig. 1. State courts' view on the Inevitable Disclosure Doctrine. Panel A shows the history of the adoption of the Inevitable Disclosure Doctrine by state courts. The precedent-setting legal cases and the exact adoption dates are listed in Appendix B. Panel B shows the state courts' view on the IDD at the end of the sample period. Twenty-one states adopted the IDD, and 11 among them later repealed it.

of the adoption. Panel B presents the state courts' view on the IDD at the end of the sample period. Twenty-one states adopted the IDD, and 11 of them rejected it later.

For banks, the customer list and the customers' proprietary information are the important trade secrets, and the IDD can help banks keep their secrets from competitors by discouraging labor mobility. Previous studies on the effect of the IDD in the banking industry show that IDD adoption and rejection are relevant shocks to banks. Agarwal et al. (2019) find that IDD adoption leads to a decrease in loan officers' job switching using data from LinkedIn, and that the decrease in labor mobility improves screening and monitoring in the mortgage market. Moreover, Lin et al. (2016) find that banks offer more favorable loan terms to borrowers after the adoption of the IDD, as they value lending relationships more when the trade secrets are better protected. These findings support the identification strategy to use IDD adoption as a relevant shock to the mobility of bank managers.

In addition, the adoption and rejection of the IDD are exogenous events for banks. In this study, adoption and rejection are defined by precedent-setting legal cases. When the courts set a new precedent on the applicability of the IDD, they tend to focus on specific characteristics of the case in hand. None of the precedent-setting legal cases, presented in Appendix B, involves banks. Furthermore, the courts tend to make rapid decisions on cases involving trade secrets (Klasa et al., 2018), which makes it difficult for banks to predict or influence the decisions. Based on these arguments, we take a difference-in-differences approach that compares the financial reporting quality of banks in states adopting or rejecting the IDD and that of control banks in the other states.

3.2. Difference-in-differences approach

We estimate the following generalized difference-in-differences regression models.

$$lnDLLP_{i,s,t} = \beta IDD_{s,t} + \gamma X_{i,s,t-1} + \alpha_i + \eta_t + u_{i,s,t} \tag{1}$$

$$lnDLLP_{i,s,t} = \beta_1 IDDAdoption_{s,t} + \beta_2 IDDRejection_{s,t} + \gamma X_{i,s,t-1} + \alpha_i + \eta_t + u_{i,s,t} \tag{2}$$

In Model (1), i, s, and t indicate bank, state, and quarter respectively. The outcome variable, $lnDLLP_{i,s,t}$, is the measure of banks' financial reporting quality, which we elucidate in the next section. $IDD_{s,t}$ is the key variable of interest. It is an indicator variable that equals one for the quarters of IDD adoption by state courts and afterwards. For states that never adopted the IDD, it equals zero for the whole sample period. For states where the courts decided to reverse their view and reject the IDD during the sample period, $IDD_{s,t}$ reverts back to zero for the period of rejection and afterwards. Bank fixed effects (α_i) and quarter fixed effects (η_t) allow difference-in-differences interpretation of the results.

X is the vector of control variables that include the natural logarithm of total assets ($SIZE_{i,s,t}$), capital ratio ($CAP_{i,s,t-1}$), an indicator for negative net income ($LOSS_{i,s,t}$), and degree of market competition measured with Herfindahl–Hirschman Index ($HHI_{i,s,t}$). We also include earnings before taxes and provisions divided by lagged loans ($EBTP_{i,s,t-1}$), the proportion of commercial and industrial ($Ciloans_{i,s,t-1}$), real estate ($Reloans_{i,s,t-1}$), agriculture ($Agloans_{i,s,t-1}$), and personal loans ($Persloans_{i,s,t-1}$), and one quarter lagged LLP ($LLP_{i,s,t-1}$) following the previous literature (Jiang et al., 2016).

In Model (2), we replace $IDD_{s,t}$ with two indicator variables, $IDDAdoption_{s,t}$ and $IDDRejection_{s,t}$. $IDDAdoption_{s,t}$ ($IDDRejection_{s,t}$) equals 1 for the quarters in and after which the IDD is adopted (rejected), and 0 otherwise.[5] By including these variables, we can compare the size and direction of the effects of IDD adoption and rejection. As adoption of the IDD restricts the labor mobility of bank managers, whereas rejection removes this restriction, we expect these variables to have opposite effects on financial reporting quality.

3.3. Measure of financial reporting quality

We use discretionary loan loss provisions as the measure of financial reporting quality of banks. Numerous previous studies focus on this measure because loan loss provisions are important accruals that can be subject to manipulation, and the measure is available for the vast majority of banks (Beatty and Liao, 2014; Bushman and Williams, 2012; Beck and Narayanamoorthy, 2013; Kim and Kross, 1998; Jiang et al., 2016; Dal Maso et al., 2018; Jin et al., 2019; Gebhardt and Novotny-Farkas, 2011). Following Beatty and Liao (2014) and Jiang et al. (2016), we define discretionary loan loss provisions ($DLLP$) as the absolute value of the estimated residuals from the following model.

$$\begin{aligned} LLP_{i,s,t} = {} & \alpha_1 dNPL_{i,s,t+1} + \alpha_2 dNPL_{i,s,t} + \alpha_3 dNPL_{i,s,t-1} + \alpha_4 dNPL_{i,s,t-2} + \alpha_5 SIZE_{i,s,t-1} + \alpha_6 dLOAN_{i,s,t} + \alpha_7 CSRET_t \\ & + \alpha_8 dGDP_t + \alpha_9 dUNEMP_{s,t} + \beta_0 IDD_{s,t} + (\beta_1 dNPL_{i,s,t+1} + \beta_2 dNPL_{i,s,t} + \beta_3 dNPL_{i,s,t-1} + \beta_4 dNPL_{i,s,t-2} \\ & + \beta_5 SIZE_{i,s,t-1} + \beta_6 dLOAN_{i,s,t} + \beta_7 CSRET_t + \beta_8 dGDP_t + \beta_9 dUNEMP_{s,t}) \times IDD_{s,t} + \delta_s + \epsilon_{i,s,t} \end{aligned} \tag{3}$$

$LLP_{i,s,t}$ is the loan loss provisions scaled by one quarter lagged loans. LLP is considered non-discretionary for the part that is explained by changes in nonperforming loans ($dNPL_{i,s,t+1}, dNPL_{i,s,t}, dNPL_{i,s,t-1}, dNPL_{i,s,t-2}$), bank size ($SIZE_{i,s,t-1}$), loan growth ($dLOAN_{i,s,t}$), and macroeconomic conditions ($CSRET_t, dGDP_t, dUNEMP_{s,t}$). $dNPL$ is defined as the change in nonperforming loans over the quarter scaled by lagged loans. $SIZE$ is the natural logarithm of total assets, and $dLOAN$ is the growth rate of loans over the quarter. $CSRET$ is the return on Case-Shiller Home Price Index. $dGDP$ is the quarterly growth in per capita GDP, and $dUNEMP$ is the change in the state level unemployment rate.

Following Jiang et al. (2016), we include $IDD_{s,t}$ and interaction terms of $IDD_{s,t}$ and the control variables in the model to allow for the possibility that IDD adoption changes how banks accrue for the non-discretionary part of LLP. Previous literature suggests that recognition of the IDD leads to changes in banks' lending behavior, and loan pricing (Lin et al., 2016;

[5] Unlike $IDD_{s,t}$, $IDDAdoption_{s,t}$ does not revert to 0 when a state rejects the IDD.

Agarwal et al., 2019). If so, the IDD can affect banks' ability to predict loan losses or willingness to take relevant risks, which would change the non-discretionary provisioning. In addition, state fixed effects (δ_s) are also included to control for the time invariant state characteristics. We define the absolute value of the estimated residuals as *DLLP* and employ the log transformation of *DLLP* as the outcome variable.

3.4. Alternative measure of the information quality of LLP

To enrich our analysis, we employ additional measures of financial reporting quality with different merits. First, we use the association between the current year's provisions and the next year's loan charge-offs, following Altamuro and Beatty (2010),Kanagaretnam et al. (2014), and Beatty et al. (2019). The OCC's and SEC's guidelines clarify that estimated losses (provisions) should closely predict the subsequent loan charge-offs (Beatty et al., 2019).[6] A close association between the current loan loss provision and the future charge-off indicates that banks' financial reporting practice conforms to the regulatory guidelines, and the reported provision figures convey accurate information on banks' loan portfolio quality. In this vein, we define a new variable, *LLPvalidity*, following Beatty et al. (2019).

$$LLPvalidity_{i,s,t} = -1 \times \left| \frac{CO_{i,s,t+1}}{PROV_{i,s,t}} - 1 \right| \tag{4}$$

$$LLPvalidity_{i,s,t} = \beta_1 IDDAdoption_{s,t} + \beta_2 IDDRejection_{s,t} + \gamma X_{i,s,t-1} + \alpha_i + \eta_t + u_{i,s,t} \tag{5}$$

i, s, and t indicate bank, state, and year, respectively.[7] *LLPvalidity* reflects the distance between the current year's loan loss provisions ($PROV_{i,s,t}$) and the next year's charge-offs ($CO_{i,s,t+1}$). We multiply the distance by -1 so that higher values of *LLPvalidity* indicate higher information quality of loan loss provisions. We use both gross charge-offs and net charge-offs to calculate *LLPvalidity* and check the robustness of the results.[8] Using *LLPvalidity* as the dependent variable, we estimate a difference-in-differences regression model similar to Model (1). We include size ($SIZE_{t-1}$), deposits (DEP_{t-1}), the proportions of commercial and industrial ($Ciloans_{t-1}$), real estate ($Reloans_{t-1}$), agriculture ($Agloans_{t-1}$), and personal ($Persloans_{t-1}$) loans, and a dummy variable for listed banks (*Public*) as control variables, following Kanagaretnam et al. (2014).

3.5. Just-meeting-or-beating earnings benchmarks

We also study the probability of just-meeting-or-beating an earnings benchmark, as an alternative measure of financial reporting quality focused primarily on earnings management incentives. Based on a survey of CFOs, Graham et al. (2005) argue that managers are motivated to meet earnings targets to improve their own reputation and investors' expectation of their firms. Managers can use financial reporting discretion to inflate earnings and meet the targets, which we expect to increase the probability of beating the benchmarks by a narrow margin. To test this hypothesis, we conduct the just-meeting-or-beating earnings benchmarks test following Kanagaretnam et al. (2014) and Altamuro and Beatty (2010). We estimate the following model.

$$SmallPos\Delta ROA_{i,s,t} = \beta_1 IDDAdoption_{s,t} + \beta_2 IDDRejection_{s,t} + \gamma X_{i,s,t-1} + \alpha_i + \eta_t + u_{i,s,t} \tag{6}$$

i, s, and t indicate bank, state, and year, respectively. Following the methodology of Kanagaretnam et al. (2014) and Altamuro and Beatty (2010), we use the previous year's earnings as the earnings benchmark that managers aim to beat. We define an indicator variable, *SmallPosΔROA*, for reporting a small increase in earnings from the previous year. *SmallPosΔROA* equals 1 if a change in *ROA* from the previous year is in the range of 0 to 0.001 ($0 \leqslant ROA_t - ROA_{t-1} \leqslant 0.001$). While *DLLP* and *LLPvalidity* measure the information quality of loan loss provisions, *SmallPosΔROA* is more directly focused on managers' incentive to inflate earnings. In Model (6), in addition to *IDDAdoption*, *IDDRejection*, and the fixed effects, we control for bank size ($SIZE_{t-1}$), non-performing loans (*NPL*), growth in assets (*dAssets*), loans ($LOAN_{t-1}$), capital ratio (CAP_{t-1}), change in cash flows (*dCF*), and an indicator of listed status (*Public*), following the literature (Kanagaretnam et al., 2014; Altamuro and Beatty, 2010). The career concern hypothesis predicts a positive (negative) estimate of β_1 (β_2) in Model (6).

4. Data

The main sample of this study includes 657,307 bank-quarter observations on 12,643 commercial banks from 1994Q1 to 2017Q4. All commercial banks (charter type 200 in Call Reports) with or without foreign offices are included. Banks' financial

[6] For the detailed guidelines by SEC, see https://www.sec.gov/interps/account/sab102.htm.

[7] We use bank-year level observations, instead of bank-quarter level observations, following the previous literature (Beatty et al., 2019; Altamuro and Beatty, 2010; Kanagaretnam et al., 2014).

[8] While both *DLLP* and *LLPvalidity* measure the information quality of *LLP*, *LLPvalidity* has the merit that it does not require an estimation model. Previous research finds that the IDD affects banks' screening and monitoring of mortgage loans (Agarwal et al., 2019) and loan contract terms (Lin et al., 2016), which may lead to changes in the loan portfolio risk. It is important to control such effects in estimating *DLLP*, and thus we carefully include variables measuring loan portfolio risks on the right-hand side of Model (3), following the literature. However, if we omit any important factors, the estimated *DLLP* may falsely include some non-discretionary part of *LLP*. We attenuate such concerns around the estimation problem by using *LLPvalidity* as an alternative measure.

statement variables are from Call Reports. Data on bank branch locations and deposit amounts are from Summary of Deposits (SOD). Case-Shiller Home Price Index, per capita GDP, and state level unemployment rate are retrieved from the Federal Reserve Bank of St.Louis, and the original data sources are S&P Dow Jones Indices LLC, U.S. Bureau of Economic Analysis, and U.S. Bureau of Labor Statistics, respectively.

The adoption and rejection dates of the IDD are obtained from Klasa et al. (2018), Flammer and Kacperczyk (2019), Chen et al. (2022), and Qiu and Wang (2018).[9] During the sample period, 11 states adopted the IDD (CT, IA, IN, KS, MO, UT, AR, GA, MA, OH, and WA), among which five subsequently repealed it before the end of the sample period (AR, GA, MA, OH, and WA). In addition, six states that adopted the IDD before the sample period rejected it during the sample period (FL, MI, NC, NJ, NY, and TX). The treatment group is composed of banks in the 17 states in which the adoption/rejection status of the IDD changed during the sample period. The control group comprises banks in states that never adopted the IDD and states that adopted the IDD before the beginning of the sample period and did not repeal it.

The sample used in the main regression analysis includes banks that have branches in only one state. Banks with branches in multiple states are excluded from the main sample for clear interpretation. Such banks are under multiple jurisdictions for the laws on protection of trade secrets, which makes it less clear when measuring how much the banks' labor markets are restricted by the IDD. More importantly, branching decisions can be endogenous. Banks that are concerned about trade secret protection or employee retention may consider the state courts' view on the IDD when they decide which market (state) to enter. In the similar context, banks that moved across state lines are also excluded.[10]

However, the exclusion of banks with branches in multiple states and banks that moved can limit the external validity of the results. To assuage the concern, Section 5.4 provides empirical results based on the expanded sample including the multi-state banks. Table 1, Panel A presents summary statistics of the main sample used in the analysis on the IDD and discretionary provisioning.[11] Panel B reports the summary statistics on the bank-year level observations used in the tests on provision validity (Section 5.5) and just-meeting-or-beating earnings benchmarks (Section 5.6). Continuous variables are winsorized at 1% and 99%, except for the macroeconomic variables.

5. Empirical results

This section presents the empirical results. We first present baseline regression results, on the relationship between the IDD and *DLLP*, and then explore the timing of the treatment effect to test the parallel trends assumption. Further test results on heterogeneous effects across banks are reported to eliminate alternative explanations and study the channel of the effect. We also provide the results on robustness tests using a matched sample of banks to compare the banks with the most similar traits, and the expanded sample including banks with branches in multiple states to attenuate concerns on external validity. Finally, we present the results on the impact of the IDD on *LLPvalidity* and upward earnings management.

5.1. Baseline results

Table 2 presents the main regression results of the study. In Column 1, the natural logarithm of discretionary loan loss provisions (*lnDLLP*) are regressed on *IDD* after controlling for bank fixed effects and quarter fixed effects. The estimated coefficient on *IDD* is positive, and statistically and economically significant. The coefficient estimate of *IDD* is 0.142, implying that adoption of the IDD leads to a 14.2 percent increase in discretionary provisioning. One standard deviation increase in the variable *IDD* is associated with a 7 percent (0.142×0.496) increase in discretionary provisions. This result is consistent with the hypothesis that reduction in labor mobility impairs banks' financial reporting quality by influencing managers' career concerns and incentives for discretionary accounting.

The results are similar when different model specifications are used. In Column 2, bank size, capital ratio, an indicator for negative net income (*LOSS*), and Herfindahl–Hirschman index are included. Column 3 further controls for profitability (*EBTP*) and loan portfolio variables (*Ciloans*, *Reloans*, *Agloans*, and *Persloans*). The sizes of the coefficients on *IDD* (0.140 in Column 2 and 0.138 in Column 3) are similar to that of Column 1. The model in Column 4 adds lagged *LLP* as another covariate.[12] The result shows that controlling the lagged *LLP* barely changes the estimated effect of the IDD on discretionary provisioning.

In Column 5 of Table 2, we estimate the effects of IDD adoption and rejection separately. As IDD rejection lifts the restriction on labor mobility imposed by IDD adoption, this presumably relaxes managers' career concerns and reduces discretionary provisioning, in opposition to the effect of IDD adoption. To test this prediction, we define *IDDAdoption*

[9] See Appendix B for the IDD adoption and rejection dates of the state courts. We augment the list in Chen et al. (2022) of IDD adoptions and rejections by adding information on North Carolina's rejection in 2014Q4, which we obtain from Qiu and Wang (2018). The list of Chen et al. (2022) is based on Klasa et al. (2018) and Flammer and Kacperczyk (2019).

[10] Banks that moved across states from 1991Q4 to 2017Q4 are excluded. The cutoff period, 1991Q4 is three years before Massachusetts adopted the IDD, which is the first state to change its view on the IDD during the sample period. 2017Q4 corresponds to the last quarter of the sample period. If a bank's physical state code reported in Call Reports changed during this period, the bank is considered to have moved.

[11] Appendix D presents the summary statistics of the sample including the multi-state banks. See Han, Park, and Pennacchi (2015) for the detailed discussion on single-state banks.

[12] While having a lagged outcome variable as a covariate and controlling for fixed effects can be problematic, it is common to control for the effect of lagged *LLP* in studying discretionary provisioning (Jiang et al., 2016; Kanagaretnam et al., 2010). In addition, to be more accurate, lagged *LLP* is not the same, but relates to the lagged outcome variable, *lnDLLP*.

Table 1
Summary statistics

Panel A: Variables used in the analysis on DLLP

Variable	Obs	Mean	Std. Dev.	P25	P50	P75
IDD	657,307	.437	.496	0	0	1
IDDAdoption	657,307	.541	.498	0	1	1
IDDRejection	657,307	.104	.306	0	0	0
LLP(%)	657,307	.107	.218	0	.045	.11
dNPL(%)	657,307	.018	.75	−.182	0	.182
Assets(*USDmil.*)	657,307	346.852	2,819.262	46.634	95.516	206.848
SIZE	657,307	4.642	1.153	3.842	4.559	5.332
dLoan(%)	657,307	2.395	6.236	−.911	1.672	4.624
CSRET(%)	657,307	1.004	1.941	.011	1.165	2.173
GDPgrowth(%)	657,307	.42	.583	.134	.493	.718
dUNEMP(%*p*)	657,307	−.017	.327	−.2	−.1	.1
lnDLLP	657,307	−2.762	1.159	−3.265	−2.623	−2.146
CAP_{t-1}	657,307	.106	.035	.083	.098	.119
LOSS	657,307	.085	.279	0	0	0
HHI	657,307	.22	.133	.133	.186	.265
$EBTP_{t-1}$(%)	657,307	.678	.516	.432	.653	.895
$Ciloans_{t-1}$	657,307	.155	.101	.085	.134	.202
$Reloans_{t-1}$	657,307	.619	.197	.49	.645	.771
$Agloans_{t-1}$	657,307	.093	.141	0	.021	.132
$Persloans_{t-1}$	657,307	.11	.103	.037	.081	.15
Private	657,307	.9	.299	1	1	1

Panel B: Variables for the tests on LLPvalidity and meeting earnings benchmarks

Variable	Obs	Mean	Std. Dev.	P25	P50	P75
IDD	163,634	.437	.496	0	0	1
IDDAdoption	163,634	.546	.498	0	1	1
IDDRejection	163,634	.109	.312	0	0	0
$NCO_{t+1}/PROV_t$	132,149	1.079	2.126	.14	.557	1.217
LLPvalid (*NCO*)	132,149	−1.205	2.107	−1.012	−.727	−.379
$GCO_{t+1}/PROV_t$	132,149	1.501	2.617	.309	.801	1.605
LLPvalid (*GCO*)	132,149	−1.336	2.64	−1	−.668	−.331
$SIZE_{t-1}$	163,634	4.59	1.157	3.789	4.513	5.285
DEP_{t-1}	132,149	.842	.078	.818	.862	.892
$Ciloans_{t-1}$	132,149	.159	.103	.087	.137	.206
$Reloans_{t-1}$	132,149	.625	.193	.502	.651	.772
$Agloans_{t-1}$	132,149	.082	.131	0	.016	.108
$Persloans_{t-1}$	132,149	.113	.105	.038	.083	.154
SmallPosΔROA	163,632	.116	.321	0	0	0
NPL	163,632	.831	1.14	.126	.442	1.045
dAssets	163,632	.095	.184	.008	.054	.119
$LOAN_{t-1}$	163,632	.605	.158	.512	.624	.719
CAP_{t-1}	163,632	.108	.042	.083	.097	.119
dCF	163,632	.001	.006	−.001	.001	.004
Public	163,634	.1	.3	0	0	0

This table presents the summary statistics of the variables used in the empirical analyses. Panel A reports the summary statistics of bank-quarter level observations used in the main analysis on *DLLP*. Panel B presents the summary statistics of bank-year level observations for the tests on provision validity (Section 5.5) and just-meeting-or-beating earnings benchmarks (Section 5.6). Banks with branches in multiple states and banks that moved across state lines are excluded from the sample for clear interpretation of the results. All the continuous variables are winsorized at 1% and 99% except for the macroeconomic variables. Appendix A provides detailed definitions of the variables.

(*IDDRejection*) as an indicator variable that equals 1 for the quarters in and after which the IDD is adopted (rejected), and 0 otherwise. We include both *IDDAdoption* and *IDDRejection* in our regression model, and report the estimation results in Column 5 of Table 2. As expected, IDD adoption leads to an increase in *DLLP*, while IDD rejection reverses this effect. The opposite effects of IDD adoption and rejection reaffirm our findings in Columns 1–4 of Table 2, and support the causal interpretation of the results.[13]

[13] In Appendix C, Column 1, we further restrict the sample to exclude states that enforced the IDD throughout the sample period (i.e., the states that adopted the IDD before the sample period and never repealed it). In this case, the control group is composed of banks in states that never adopted the IDD. The result that IDD adoption (rejection) is positively (negatively) associated with discretionary provisioning does not change in this alternative specification. In Columns 2 and 3 of Appendix C, we estimate the effect of *IDDAdoption* and *IDDRejection* in separate regressions. In Column 2, we use banks in states that adopted the IDD during the sample period (CT, IA, IN, KS, MO, and UT) as the treated banks. Banks in states that never adopted IDD are included as the control group to estimate the effect of *IDDAdoption*. Similarly, Column 3 uses banks in states that rejected the IDD during the sample period (FL, MI, NC, NJ, NY, and TX) as the treated banks. Banks in states that enforced the IDD throughout the sample period are used as the control banks. The results are largely similar to those reported in Column 5 of Table 2.

Table 2
The effect of IDD on discretionary loan loss provisions.

	(1) *lnDLLP*	(2) *lnDLLP*	(3) *lnDLLP*	(4) *lnDLLP*	(5) *lnDLLP*
IDD	0.142***	0.140***	0.138***	0.134***	
	(3.214)	(3.339)	(3.812)	(3.806)	
IDDAdoption					0.115***
					(4.540)
IDDRejection					−0.153***
					(−2.883)
SIZE		0.115***	0.111***	0.104***	0.104***
		(6.683)	(7.531)	(6.896)	(6.881)
CAP_{t-1}		1.271***	1.215***	1.145***	1.145***
		(5.460)	(5.348)	(5.027)	(5.015)
LOSS		0.987***	0.997***	0.958***	0.958***
		(30.488)	(31.843)	(30.362)	(30.389)
HHI		0.069	0.015	0.020	0.017
		(1.075)	(0.265)	(0.353)	(0.310)
$EBTP_{t-1}$			0.032***	0.041***	0.041***
			(3.363)	(4.528)	(4.578)
$Ciloans_{t-1}$			−0.405***	−0.410***	−0.408***
			(−3.640)	(−3.980)	(−4.011)
$Reloans_{t-1}$			−0.317***	−0.290***	−0.288***
			(−2.783)	(−2.752)	(−2.752)
$Agloans_{t-1}$			0.150	0.163	0.165
			(1.218)	(1.413)	(1.415)
$Persloans_{t-1}$			−0.032	−0.070	−0.072
			(−0.247)	(−0.562)	(−0.583)
LLP_{t-1}				0.298***	0.298***
				(12.782)	(12.821)
Bank FE	Yes	Yes	Yes	Yes	Yes
Year-quarter FE	Yes	Yes	Yes	Yes	Yes
Observations	657,307	657,307	657,307	657,307	657,307
Adjusted R-squared	0.199	0.243	0.243	0.246	0.246

This table presents the results of the difference-in-differences regression of discretionary loan loss provisions (*lnDLLP*) on *IDD* and other covariates. The dependent variable, *lnDLLP*, is the natural logarithm of discretionary loan loss provisions. Higher value discretionary loan loss provisions (*DLLP*) indicate lower financial reporting quality. *IDD* is an indicator variable that equals one if the bank's home state adopted the Inevitable Disclosure Doctrine, and zero otherwise. For the states that repealed previously adopted IDD, the variable *IDD* reverts back to zero after the repeal. *IDDAdoption* (*IDDRejection*) is an indicator variable that equals one if the bank's state adopted (repealed previously adopted) IDD, and zero otherwise. Standard errors are corrected for state-level clustering, and t-statistics are reported in the parentheses. ***, **, and * indicate 1%, 5%, and 10% significance, respectively.

Our results are consistent with the analyses of industrial firms by Ali et al. (2019),Chen et al. (2018), and Tang et al. (2021). They provide evidence that a restriction on labor mobility reduces managers' incentive to transparently disclose information by increasing their career concerns. However, our results contradict the finding of Gao et al. (2018) that the IDD reduces industrial firms' incentives to use earnings management to portray their firms in a better light to employees.

A potential explanation for these contradictory results is that banks might find accrual manipulation less effective than industrial firms do in impressing employees, regardless of the adoption of the IDD. Loan officers, as representative bank employees, have access to banks' most important trade secrets (the customer list and their proprietary information). In the context of our analysis, it is important that loan officers are financial experts who take responsibility for underwriting loans. They also assess credit risks, which directly affect loan loss provisioning decisions (Beatty et al., 2019; Treacy and Carey, 2000). Given these employees' knowledge and understanding of banks' loan portfolio risks, attempts to change their perceptions using accrual manipulation are likely to be ineffective. In contrast, industrial firms have a larger proportion of employees who, although they have access to trade secrets, do not take part in accrual decisions and whose expertise does not lie in financial analysis (e.g., engineers, scientists, and IT professionals). The difference between the results of our study and those of Gao et al. (2018) is consistent with this heterogeneity in the expertise and roles of employees.

5.2. Timing of the effect

The validity of the difference-in-differences analysis relies on the assumption that the change in financial reporting quality of the treated banks and the untreated banks would have been the same in the absence of the state courts' decision to adopt or reject the IDD. However, if banks predicted state courts' decisions to adopt or reject the IDD, the observed relation between the passage of the IDD and discretionary provisioning might be the result of self-selection.

To mitigate this concern, we investigate the timing of the change in *DLLP* based on the Granger test, following Bertrand and Mullainathan (2003), Klasa et al. (2018), and Agarwal et al. (2019).

$$lnDLLP_{i,s,t} = \beta_1 IDDAdoption^{-2}_{s,t} + \beta_2 IDDAdoption^{-1}_{s,t} + \ldots + \beta_5 IDDAdoption^{2}_{s,t} + \beta_6 IDDAdoption^{3+}_{s,t} + \beta_7 IDDRejection^{-2}_{s,t} + \beta_8 IDDRejection^{-1}_{s,t} + \ldots + \beta_{11} IDDRejection^{2}_{s,t} + \beta_{12} IDDRejection^{3+}_{s,t} + \gamma X_{i,s,t-1} + \alpha_i + \eta_t + u_{i,s,t} \quad (7)$$

In this model, *IDD* in Model (1) is substituted with several dummy variables indicating time periods relative to the IDD adoption and rejection. For $n = 0, 1, 2$, $IDDAdoption^{n}$ ($IDDAdoption^{-n}$) is a dummy variable that equals one if the observation is from n years after (before) the adoption of the IDD, and zero otherwise. $IDDAdoption^{3+}$ equals one for observations from 3 or more years after the adoption. For example, $IDDAdoption^{0}$ equals one from the quarter of the adoption to 3 quarters after the adoption. $IDDAdoption^{-2}$ equals one from 8 to 5 quarters before the adoption. Indicator variables for time periods relative to the rejection are defined analogously.

The results on the timing of the effect are presented in Table 3. Column 1 controls for the time period indicator variables relative to the adoption and rejection, bank fixed effects, and quarter fixed effects. Column 2 includes the full set of controls in Table 2, Column 4. In both Columns 1 and 2, the coefficient estimates for the periods before the adoption are not statistically different from zero. On the contrary, all coefficient estimates on time dummy variables for the periods after the adoption are positive and statistically significant. Similarly, we find no significant difference between the treated and control banks' discretionary provisioning before the rejection of the IDD. The decrease in *DLLP* materializes only after the rejection of the IDD.

5.3. Heterogeneous treatment effects

This section explores whether the treatment effect is larger for banks with certain traits in a way that is predicted by the career concern hypothesis. An alternative reading of the findings is that proprietary costs of information disclosure increase after the IDD adoption (Kim et al., 2021; Li et al., 2018). As the IDD blocks a way for competitors to obtain trade secrets by hiring employees, the competitors would depend more on a bank's public disclosure, such as financial reports, to gather the desired information. This might be the reason why discretionary provisioning increases after the IDD adoption. While it is difficult to completely rule out this explanation, this section provides subsample regression results that better fit the career concern hypothesis.

First, we test whether the treatment effect is smaller under intensive monitoring. Regulatory scrutiny and monitoring by investors reduce agency problems and discourage the attempts of discretionary accounting ex ante. We group banks into two subsamples based on their assets and publicly listed status, assuming that large banks and public banks get more attention from regulators and investors. In addition, we also compare banks in the competitive and concentrated markets. Previous literature implies that competition can enhance governance at banks, which affects discretionary accounting (Jiang et al., 2016; Cornett et al., 2009; Klein, 2002; Warfield et al., 1995). Therefore, the effect of the IDD on discretionary LLP is expected to be larger for banks in more concentrated markets under the career concern hypothesis.

Table 4, Columns 1 and 2 present the regression results for the large and small banks, respectively.[14] Banks are defined as large if their asset size is bigger than the sample median, and small otherwise. The estimated coefficients on *IDD* are positive for both types of banks. However, the effect is larger for small banks. Small banks increase discretionary provisioning by 8.5 percent with a one standard deviation increase in *IDD*. Similarly, Columns 3–6 show that the effect is more significant for the private banks and banks in highly concentrated markets (with *HHI* above the sample median). These results are consistent with the conjecture that discretionary provisioning is discouraged when banks are heavily monitored.

In Columns 7 and 8, we separate banks into two groups based on the managers' outside job opportunities. If recognizing the IDD increases discretionary provisioning through its effect on the managers' career concerns, the effect should be stronger when the managers face more restricted outside job options. Following Yonker (2017), the number of banks in the local market is used as a proxy for outside options for managers. If there are many banks in the local market, there can be several pairs of banks that are not considered as direct rivals. Managers in these markets would be less influenced by the adoption or rejection of the IDD. Contrarily, if there is a small number of banks in the local market, bank managers already face limited outside job opportunities without the IDD. In addition, the banks are more likely to be considered as direct rivals to each other, which makes the managers' mobility more subject to the application of the IDD.

We define a new variable *Number of peers* as the average of the number of banks in the counties in which the bank has branches. Table 4, Column 7 reports the estimated effect of the IDD on discretionary provisioning for banks with *Number of peers* above the sample median. Column 8 reports the result on the banks with fewer peers (with *Number of peers* below the sample median). As expected, the point estimate of the effect of *IDD* is significant and large for banks with fewer peers. The size of the coefficient (0.164) in Column 8 suggests that a one standard deviation increase in *IDD* leads to an 8 percent increase in discretionary provisioning for the banks with fewer peers. On the contrary, the effect is much smaller for banks with a larger number of peers (Column 7).

[14] The regression models in Table 4 control for all the explanatory variables included in the baseline model in Table 2, Column 4, but the estimated coefficients on the control variables are omitted from the table for brevity.

Table 3
Timing of the effect.

	(1) *lnDLLP*	(2) *lnDLLP*
$IDDAdoption^{-2}$	0.003	0.004
	(0.089)	(0.139)
$IDDAdoption^{-1}$	c0.008	−0.001
	(−0.201)	(−0.036)
$IDDAdoption^{0}$	0.135***	0.142***
	(3.834)	(5.085)
$IDDAdoption^{1}$	0.132***	0.142***
	(3.035)	(3.788)
$IDDAdoption^{2}$	0.145***	0.150***
	(2.999)	(3.821)
$IDDAdoption^{3+}$	0.093*	0.092**
	(1.921)	(2.522)
$IDDRejection^{-2}$	0.056	0.039
	(1.430)	(1.167)
$IDDRejection^{-1}$	0.025	0.022
	(0.663)	(0.713)
$IDDRejection^{0}$	−0.134***	−0.109***
	(−5.320)	(−3.196)
$IDDRejection^{1}$	−0.149***	−0.121***
	(−5.166)	(−3.549)
$IDDRejection^{2}$	−0.139***	−0.112***
	(−4.758)	(−3.087)
$IDDRejection^{3+}$	−0.166	−0.161**
	(−1.612)	(−2.130)
Other controls		Yes
Bank FE	Yes	Yes
Year-quarter FE	Yes	Yes
Observations	657,307	657,307
Adjusted R-squared	0.199	0.246

This table examines the timing of the effect of the IDD on the financial reporting quality of banks. The dependent variable is the natural logarithm of discretionary loan loss provisions (*lnDLLP*). For $n = 0, 1, 2$, $IDDAdoption^{n}$ ($IDDAdoption^{-n}$) is a dummy variable that equals one if the observation is from n years after (before) the adoption of the IDD, and zero otherwise. $IDDAdoption^{3+}$ equals one for observations from 3 or more years after the adoption. $IDDRejection^{-2}$, $IDDRejection^{-1}$, . . ., and $IDDRejection^{3+}$ are defined analogously. The model in Column 2 includes the other control variables in the baseline model (Table 2, Column 4), but the coefficient estimates are omitted from the table for brevity. Standard errors are corrected for state-level clustering, and t-statistics are reported in parentheses. ***, **, and * indicate 1%, 5%, and 10% significance, respectively.

Table 4
Heterogeneous treatment effects.

	(1) Large	(2) Small	(3) Public	(4) Private	(5) LowHHI	(6) HighHHI	(7) MorePeers	(8) FewerPeers
IDD	0.126***	0.171***	0.110***	0.139***	0.109***	0.155***	0.092**	0.164***
	(3.564)	(6.512)	(3.005)	(4.071)	(2.728)	(5.254)	(2.596)	(5.294)
Other controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	328,535	328,499	65,307	591,835	328,627	328,611	328,645	328,641
Adjusted R-squared	0.286	0.230	0.346	0.237	0.257	0.246	0.273	0.221

This table compares subsample regression results. The dependent variable for all the models is the natural logarithm of discretionary loan loss provisions (*lnDLLP*). *IDD* is an indicator variable that equals one if the bank's state adopted the Inevitable Disclosure Doctrine, and zero otherwise. For the states that repealed previously adopted IDD, the variable *IDD* reverts back to zero after the repeal. Columns 1 and 2 compare large and small banks. Banks are defined to be large if their asset size is bigger than the sample median, and small otherwise. Columns 3 and 4 compare the effect on public and private banks. If either the bank or its parent is publicly listed, the bank is considered public. Columns 5 and 6 compare the banks in competitive and concentrated markets. Column 5 uses the sample of banks with *HHI* below the sample median. *HHI* is defined as the average of deposit market Herfindahl–Hirschman Indices of counties the bank's branches are located in. Column 6 studies the banks with *HHI* above the sample median. Columns 7 and 8 separate banks into two groups based on a median split of *Number of peers*. *Number of peers* is the average of the number of banks in the counties that the bank's branches are located in. The models include the other control variables in the baseline model (Table 2, Column 4), but the coefficient estimates are omitted from the table for brevity. Standard errors are corrected for state-level clustering, and t-statistics are reported in the parentheses. ***, **, and * indicate 1%, 5%, and 10% significance, respectively.

5.4. Robustness tests

5.4.1. Analysis of matched sample

This section tests the robustness of the results for different samples of banks. First, we limit the sample using nearest neighbor matching to compare treated and control banks with the most similar characteristics. We match each treated bank with one untreated bank with replacement, based on the Mahalanobis distance on size (*SIZE*), capital ratio (*CAP*), profitability (*EBTP*), and loan loss provisions (*LLP*) four quarters before the treatment.[15]

There are two types of treatments: adoption and rejection of the IDD. For treated banks in states adopting the IDD, banks in states that never adopted the IDD are matched as control banks. For treated banks in states rejecting the IDD, banks in states that enforced the IDD during the entire sample period are matched. The sample is restricted to the periods from 12 quarters before to 12 quarters after the treatment to estimate the more immediate effect of the IDD. Banks with at least one observation before and after the treatment are used.

Table 5, Panel A shows the means of matching covariates of the treated and control banks four quarters before the treatment. The differences in the sample means of all matching covariates are not statistically different from zero. Panel B shows the regression results using the matched sample. The results are consistent with the baseline results in Table 2. The IDD positively affects discretionary provisioning in models with (Column 2) or without (Column 1) the other control variables, suggesting that the restriction on labor mobility lowers the financial reporting quality. Column 3 confirms the finding that IDD rejection is negatively associated with discretionary provisioning, contrary to the adoption. We also repeat the analyses in Section 5.2. The results reaffirm that the change in discretionary LLP materializes only after the adoption or rejection of the IDD, but not before.

5.4.2. Banks with branches in multiple states

The analyses above are based on the sample of banks that have branches in a single state. By limiting the sample this way, we intend to alleviate the concern that banks self-selected into the treatment or control group by opening a branch. However, limiting the sample this way leads to the question on whether the results hold for larger banks that operate branches in multiple states. Therefore, in this section, we expand the sample to include banks with branches in multiple states to mitigate the concern on external validity.

To study the effect of IDD on multi-state banks, we introduce a new variable $IDD(weighted)_{i,t}$, gauging how much multi-state banks' labor markets are restricted by the IDD. $IDD(weighted)_{i,t}$ is defined as the weighted average of $IDD_{s,t}$ in each state where the banks operate branches. The weight for each state is based on the share of deposits from that state. For example, consider a bank that funds 40% of deposits from New York and 60% from Florida in 2005Q1. $IDD_{NY,2005Q1}$ equals one as New York adopted the IDD in 1919 and repealed it in 2009. $IDD_{FL,2005Q1}$ equals zero as Florida rejected the IDD in 2001. Accordingly, $IDD(weighted)$ is 0.4 ($=0.4 \times 1 + 0.6 \times 0$) for this bank in 2005Q1. For banks that operate branches in only one state, $IDD(weighted)_{i,t}$ equals $IDD_{s,t}$.

Table 6 presents the regression results analogous to Table 2 using the expanded sample. The coefficient on *IDD*(*weighted*) is significant and positive in all specifications. In Column 4, the estimated coefficient implies that a one standard deviation increase in *IDD*(*weighted*) leads to a 7.6 (0.154×0.493) percent increase in discretionary provisioning. The results suggest that passage of the IDD increases discretionary provisioning, making it harder for outside investors to monitor banks.

In unreported analyses, we repeat the regression estimations in Section 5.3 that compare the treatment effects across subsamples of banks using the expanded sample. As before, the estimated coefficients are larger for small banks, private banks, banks in concentrated markets, and banks with fewer peers in the local market. We also try alternative weights based on the number of branches in each state in calculating *IDD*(*weighted*) instead of the deposit shares. The results are robust to the alternative measure. Banks' loan loss provisioning becomes more discretionary after the state courts recognize the IDD.

5.5. IDD, loan loss provisions and loan charge-offs

In this section, we study the relationship between the IDD and *LLPvalidity*, an alternative measure of the information quality of *LLP*. Higher values of *LLPvalidity* imply that *LLP* conveys more accurate information about loan portfolio quality and that banks conform more closely to the OCC's and SEC's guidelines, which call for a close correspondence between estimated losses and the actual loss amount. Table 7 reports the regression results using *LLPvalidity* as the dependent variable.

In Column 1, net charge-offs are used to calculate provision validity. We find that provision validity decreases with the adoption of the IDD. The estimated coefficient on *IDDAdoption* is -0.068, and the effect is statistically significant at the 10% level. Using gross charge-offs to measure provision validity in Column 2 does not change the results. Banks' loan loss provisions become less informative about future charge-offs after the adoption of the IDD. The estimated coefficient implies a 0.05 standard deviation decrease ($-0.139/2.64$) in *LLPvalidity* measured with *GCO* in response to the adoption of the IDD. On the other hand, the coefficient estimates on *IDDRejection* are positive in both Columns 1 and 2, but statistically

[15] Massachusetts adopted and rejected the IDD during the sample period. However, for the matching analysis, we only utilize the change in labor mobility caused by the rejection of the IDD for banks in Massachusetts. It is because Massachusetts adopted the IDD in 1994Q4 and our sample period begins after 1993Q4, four quarters before the adoption.

Table 5
Analysis using a matched sample.

Panel A: Comparison of the matching covariates			
	Treatment group (N = 3,791)	Control group (N = 3,791)	Diff. (Treated - Control)
SIZE	4.435	4.426	0.009
CAP	0.105	0.104	0
EBTP	0.731	0.725	0.006
LLP	0.11	0.104	0.005

Panel B: Regression results using the matched sample			
	(1) *lnDLLP*	(2) *lnDLLP*	(3) *lnDLLP*
IDD	0.137***	0.114***	
	(8.644)	(7.226)	
IDDAdoption			0.120***
			(5.348)
IDDRejection			−0.106***
			(−4.530)
SIZE		0.120***	0.119***
		(3.353)	(3.302)
CAP_{t-1}		1.827***	1.827***
		(6.812)	(6.832)
LOSS		1.040***	1.040***
		(21.378)	(21.477)
HHI		0.104	0.106
		(0.645)	(0.654)
$EBTP_{t-1}$		0.002	0.002
		(0.186)	(0.182)
$Ciloans_{t-1}$		0.349*	0.349*
		(1.768)	(1.764)
$Reloans_{t-1}$		0.204	0.204
		(1.036)	(1.033)
$Agloans_{t-1}$		0.381	0.380
		(1.437)	(1.433)
$Persloans_{t-1}$		0.268	0.269
		(1.125)	(1.125)
LLP_{t-1}		0.277***	0.277***
		(5.832)	(5.900)
Bank FE	Yes	Yes	Yes
Year-quarter FE	Yes	Yes	Yes
Observations	176,994	176,994	176,994
Adjusted R-squared	0.222	0.266	0.266

This table examines the impact of the IDD on *DLLP* using a matched sample of treated and control banks. There are two types of treatments: adoption and rejection of the IDD. For banks in states adopting the IDD, banks in states that never adopted the IDD are matched as control banks. For banks in states rejecting the IDD, banks in states that enforced the IDD throughout the entire sample period are used as the control group. The sample is restricted to the periods from 12 quarters before to 12 quarters after the treatment, and banks with at least one observation before and after the treatment are used. Each treated bank is matched to one control bank with the smallest Mahalanobis distance on size (*SIZE*), capital ratio (*CAP*), profitability (*EBTP*), and loan loss provisions (*LLP*) at 4 quarters before the treatment. *SIZE* is the natural logarithm of total assets. *CAP* is book value of equity divided by total assets. *EBTP* equals income before taxes and provisions divided by lagged loans. *LLP* is loan loss provisions divided by lagged loans. Panel A shows the means of matching covariates of the treatment and control group 4 quarters before the treatment. Panel B presents the diff-in-diff regression results using the matched sample. Standard errors are corrected for state-level clustering, and t-statistics are reported in the parentheses. ***, **, and * indicate 1%, 5%, and 10% significance, respectively.

insignificant. This finding implies that bank managers react asymmetrically to career concerns: they show a stronger reaction when their career concerns worsen but are reluctant to reverse the effect when their career concerns are relieved. This is consistent with the managerial myopia view of Chen et al. (2018).

5.6. Upward earnings management

5.6.1. Income-increasing and -decreasing DLLP

So far, our findings suggest that banks in states adopting the IDD increase discretionary provisioning. The question naturally arises as to why banks in states adopting the IDD are more likely to have poorer financial reporting quality. We hypothesize that a reduction in labor mobility induced by the IDD provides incentives for bank managers to engage in window dressing. To study this channel of the effect, this section further explores whether bank managers use financial reporting discretion to inflate earnings, which would be consistent with the career concern hypothesis. Upward earnings management may not be sustainable and may even damage performance in the long run, but career concerns can cause bank

Table 6
Inclusion of banks with branches in multiple states.

	(1) *lnDLLP*	(2) *lnDLLP*	(3) *lnDLLP*	(4) *lnDLLP*
IDD(weighted)	0.162***	0.159***	0.157***	0.154***
	(3.753)	(4.027)	(4.640)	(4.639)
SIZE		0.129***	0.125***	0.117***
		(9.768)	(11.088)	(10.156)
CAP_{t-1}		1.283***	1.219***	1.161***
		(5.470)	(5.302)	(5.032)
LOSS		0.988***	1.000***	0.956***
		(30.017)	(31.436)	(29.868)
HHI		0.034	−0.023	−0.016
		(0.462)	(−0.345)	(−0.236)
$EBTP_{t-1}$			0.036***	0.044***
			(4.070)	(5.225)
$Ciloans_{t-1}$			−0.367***	−0.378***
			(−3.233)	(−3.572)
$Reloans_{t-1}$			−0.302***	−0.280***
			(−2.908)	(−2.899)
$Agloans_{t-1}$			0.194	0.202*
			(1.549)	(1.714)
$Persloans_{t-1}$			−0.034	−0.079
			(−0.248)	(−0.608)
LLP_{t-1}				0.311***
				(13.857)
Bank FE	Yes	Yes	Yes	Yes
Year-quarter FE	Yes	Yes	Yes	Yes
Observations	693,207	693,207	693,207	693,207
Adjusted R-squared	0.201	0.246	0.246	0.249

This table presents the regression results using the sample including multi-state banks. The dependent variable is the natural logarithm of discretionary loan loss provisions (*lnDLLP*). Higher value discretionary loan loss provisions (*DLLP*) indicate lower financial reporting quality. *IDD(weighted)* is the weighted average of the indicator variable $IDD_{s,t}$ of the states the banks have branches in. The weight is given by the share of deposits from each state. For example, consider a bank that funds 40% of deposits from New York and 60% from Florida in 2005Q1. $IDD_{NY,2005Q1} = 1$ as New York adopted IDD in 1919 and repealed it in 2009. $IDD_{FL,2005Q1} = 0$ as Florida rejected IDD in 2001. Accordingly, $IDD(weighted) = 0.4 \times 1 + 0.6 \times 0 = 0.4$. Standard errors are corrected for state-level clustering, and t-statistics are reported in the parentheses. ***, **, and * indicate 1%, 5%, and 10% significance, respectively.

Table 7
IDD and provision validity.

	(1) *LLPValidity (NCO)*	(2) *LLPValidity (GCO)*
IDDAdoption	−0.068*	−0.139**
	(−1.788)	(−2.370)
IDDRejection	0.015	0.020
	(0.197)	(0.189)
$SIZE_{t-1}$	0.154***	0.159***
	(6.293)	(5.258)
DEP_{t-1}	−0.562***	−0.663***
	(−3.309)	(−3.167)
$Ciloans_{t-1}$	−0.123	−0.102
	(−0.506)	(−0.351)
$Reloans_{t-1}$	−0.406**	−0.429*
	(−2.119)	(−1.786)
$Agloans_{t-1}$	−1.244***	−1.488***
	(−4.462)	(−3.730)
$Persloans_{t-1}$	0.549**	0.266
	(2.237)	(0.728)
Public	−0.267***	−0.363***
	(−4.388)	(−4.831)
Bank FE	Yes	Yes
Year FE	Yes	Yes
Observations	132,149	132,149
Adjusted R-squared	0.051	0.054

This table studies IDD's effect on how well banks' loan loss provisions predict future loan losses. The dependent variable is *LLPvalidity*, measuring the closeness between current years' loan loss provisions and future loan charge-offs. *LLPvalidity* is defined as $-|NCO_{t+1}/PROV_t - 1|$ in Column 1, and $-|GCO_{t+1}/PROV_t - 1|$ in Column 2, where *NCO* and *GCO* are net charge-offs and gross charge-offs, respectively. *PROV* is the amount of loan loss provisions. Higher value *LLPvalidity* indicates that banks accrue loan loss provisions that closely predict subsequent loan losses. *IDDAdoption* (*IDDRejection*) equals one for the quarters of IDD adoption (rejection) and afterward, and zero otherwise. Standard errors are corrected for state-level clustering, and t-statistics are reported in the parentheses. ***, **, and * indicate 1%, 5%, and 10% significance, respectively.

managers to behave myopically (Chen et al., 2018). To empirically test this, we take a two-pronged approach. First, we separate the cases of income-increasing and -decreasing *DLLP* and explore the IDD effects in each case. Income-increasing (-decreasing) *DLLP* refers to cases in which the estimated residuals (ϵ) in Model (3) are negative (positive). We expect the effect of the IDD to be more pronounced on the income-increasing *DLLP* than on the income-decreasing *DLLP*, as managers attempt to inflate earnings to overstate their performance.

Table 8 presents the results. Columns 1 and 2 report the regression results for the relationship between the IDD and income-increasing *DLLP*, while Columns 3 and 4 study the cases of income-decreasing *DLLP*. The coefficient estimates on *IDD* are both positive in Columns 1 and 3, but the estimated effect is significant only for the cases of income-increasing *DLLP*. In Columns 2 and 4, we estimate the effects of *IDDAdoption* and *IDDRejection* separately. Confirming the results in Column 1, *IDDAdoption*, which restricts managers' mobility, is associated with an increase in income-increasing *DLLP*, while *IDDRejection*, which lifts the mobility restriction, leads to a decrease in income-increasing *DLLP*. In contrast, neither adoption nor rejection has a significant effect on income-decreasing *DLLP*.

5.6.2. Just-meeting-or-beating earnings benchmarks

Next, we test whether the IDD affects the probability of just-meeting-or-beating an earnings benchmark. Meeting earnings benchmarks is an important goal for managers, helping them to earn credibility among investors and establish a reputation in the labor market (Graham et al., 2005). If the IDD triggers managers' career concerns, managers may use financial reporting discretion to meet the earnings benchmarks, and we expect this practice to increase the probability of beating the benchmarks by a narrow margin. One such important benchmark is the previous year's earnings (Graham et al., 2005; Altamuro and Beatty, 2010; Kanagaretnam et al., 2014), and thus we study how the IDD affects the probability of banks' reporting a small increase in *ROA*.

The regression results of Model (6) are reported in Table 9. We use *SmallPosΔROA* as the dependent variable, which equals 1 if a change in *ROA* from the previous year is in the range of 0 to 0.001, and 0 otherwise. In Column 1, based on a linear probability model, we show that *IDDAdoption* positively affects the probability of just-meeting-or-beating the earnings benchmark, while *IDDRejection* has no significant effect. Adoption of the IDD is associated with a 0.8%p increase in the probability of reporting a slight increase in ROA, which is economically significant given that the sample mean of *SmallPosΔROA* is 0.116. The results of the logistic regression model in Column 2 are consistent with the earlier finding. The coefficient on *IDDAdoption* is positive and statistically significant at the 10% level. Collectively, the results in Table 8 and Table 9 suggest that banks in states recognizing the IDD engage more in upward earnings management.

6. Concluding remarks

The 2008 global financial crisis confirmed a longstanding concern around bank opacity, namely that a lack of information on banks' asset quality can lead to greater systemic risk. Given the economic impact of bank opacity, it is necessary to understand the determinants of banks' financial reporting quality to enhance financial stability. To this end, we evaluate the effect of managerial labor mobility on financial reporting quality at banks. We focus on the degree of labor mobility because it constrains managers' outside job opportunities, and thus affects their career concerns. This is particularly important in the banking industry because of its opaque nature. Despite the existence of accounting rules and regulations, bank managers still have some discretion over financial reporting due to their informational advantages and expertise in bank operation (Beatty and Liao, 2014; Kim and Kross, 1998; Cornett et al., 2009). We conjecture that a restriction on labor mobility motivates earnings management by triggering managers' career concerns, and thereby impairs financial reporting quality at banks.

This paper evaluates the impact of a legal shock discouraging labor mobility (i.e., the IDD) on discretionary provisioning by banks. We find that banks accrue loan loss provisions in a more discretionary manner after their home state adopts the IDD. Rejecting the IDD has the opposite effect, i.e., reducing discretionary provisions. These results suggest that restrictions on labor mobility impair the financial reporting quality of banks, making it harder for outside investors and regulators to understand bank fundamentals. We further perform a number of tests on the heterogeneous effects of the IDD. We find larger effects of the IDD on discretionary provisioning for smaller banks, private banks, and banks with fewer peers in the local market. These results support the view that managers' career concerns are the channel through which labor mobility affects financial reporting quality. We also use alternative measures of financial reporting quality: the validity of provisions in estimating future loan losses, and just-meeting-or-beating earnings benchmarks. Our results using the alternative measures are broadly consistent with the earlier findings. First, after IDD adoption, banks' loan loss provisions become less accurate estimations of actual loans charged off in the subsequent period. Second, we provide evidence that IDD adoption increases bank managers' use of accounting discretion to meet or beat an earnings benchmark.

Our findings have implications for regulators devising policies to enhance financial stability. It is critical to understand the role of labor mobility in shaping managerial incentives and financial reporting practices at banks. Labor market frictions, which exacerbate managers' career concerns, can impair financial reporting quality. The subsequent difficulties in under-

Table 8
Income-increasing and -decreasing discretionary provisions.

	(1)	(2)	(3)	(4)
	Income-increasing		Income-decreasing	
	lnDLLP	*lnDLLP*	*lnDLLP*	*lnDLLP*
IDD	0.160***		0.045	
	(5.711)		(1.025)	
IDDAdoption		0.150***		0.014
		(7.183)		(0.360)
IDDRejection		−0.169***		−0.072
		(−3.864)		(−1.120)
SIZE	0.050***	0.051***	0.137***	0.137***
	(3.138)	(3.139)	(7.150)	(7.059)
CAP_{t-1}	0.537**	0.536**	1.879***	1.882***
	(2.204)	(2.198)	(8.259)	(8.242)
LOSS	−0.018	−0.018	1.571***	1.570***
	(−1.010)	(−1.010)	(61.959)	(61.930)
HHI	0.035	0.034	0.058	0.054
	(0.636)	(0.617)	(0.526)	(0.498)
$EBTP_{t-1}$	−0.017**	−0.017**	0.127***	0.127***
	(−2.154)	(−2.195)	(12.528)	(12.647)
$Ciloans_{t-1}$	−0.395***	−0.394***	−0.020	−0.017
	(−4.396)	(−4.445)	(−0.125)	(−0.111)
$Reloans_{t-1}$	−0.036	−0.035	−0.539***	−0.536***
	(−0.436)	(−0.427)	(−3.282)	(−3.251)
$Agloans_{t-1}$	0.248**	0.249**	0.571**	0.573**
	(2.511)	(2.534)	(2.301)	(2.294)
$Persloans_{t-1}$	−0.390***	−0.391***	0.482**	0.479**
	(−4.449)	(−4.501)	(2.169)	(2.181)
LLP_{t-1}	−0.359***	−0.359***	0.673***	0.672***
	(−22.467)	(−22.420)	(19.951)	(19.993)
Bank FE	Yes	Yes	Yes	Yes
Year-quarter FE	Yes	Yes	Yes	Yes
Observations	468,457	468,457	187,931	187,931
Adjusted R-squared	0.269	0.269	0.360	0.360

This table estimates the effect of IDD on income-increasing and -decreasing discretionary provisions. Columns 1 and 2 study the cases of negative *DLLP* ($\epsilon < 0$ in Model (3)), which leads to an increase in reported earnings. Columns 3 and 4 study the cases of positive *DLLP* ($\epsilon \geqslant 0$ in Model (3)), associated with a decrease in earnings. *IDD* is an indicator variable that equals one if the bank's state adopted the Inevitable Disclosure Doctrine, and zero otherwise. For the states that repealed previously adopted IDD, the variable *IDD* reverts back to zero after the repeal. *IDDAdoption* (*IDDRejection*) equals one for the quarters of IDD adoption (rejection) and afterward, and zero otherwise. Standard errors are corrected for state-level clustering, and t-statistics are reported in the parentheses. ***, **, and * indicate 1%, 5%, and 10% significance, respectively.

standing bank fundamentals can prevent the market from disciplining banks and impede efficient capital allocation. Moreover, insufficient provisioning as a method of inflating earnings can expose banks and their borrowers to higher risks of cyclical downturns (Laeven and Majnoni, 2003). Our results are also pertinent to bank board members designing employment contracts for managers. Restricting managers' mobility to retain talented individuals or protect trade secrets can worsen information asymmetry between managers and shareholders. More generally, our study contributes to the ongoing debate on the necessity of a ban on non-compete agreements and scrutiny of bank mergers.[16] Such regulations would engender substantial changes in the labor mobility and outside job opportunities of bank managers. Our findings, that the IDD negatively affects bank financial reporting quality and that such effects are more pronounced for banks in highly concentrated markets and banks with fewer nearby peers, predict a positive influence of such regulations on bank transparency.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

[16] For detailed discussions on these issues, see "Biden Signs Sweeping Order Aimed at Curbing Power of Big Companies," Financial Times, July 10, 2021, and "Biden Administration Presses to Scrutinize Bank Mergers, Potentially Delaying Deals," Wall Street Journal, December 14, 2021.

Table 9
IDD and just-meeting-or-beating earnings benchmark.

	(1) *SmallPosΔROA* LPM	(2) *SmallPosΔROA* Logit
IDDAdoption	0.008*	0.065*
	(1.869)	(1.758)
IDDRejection	0.005	0.052
	(0.854)	(1.080)
$SIZE_{t-1}$	0.009***	0.109***
	(3.567)	(3.499)
NPL	−0.011***	−0.201***
	(−15.146)	(−16.736)
dAssets	−0.044***	−0.796***
	(−7.444)	(−9.950)
$LOAN_{t-1}$	0.012	0.299***
	(1.333)	(2.815)
CAP_{t-1}	−0.054**	−1.659***
	(−2.187)	(−3.358)
dCF	0.841***	16.432***
	(4.005)	(5.774)
Public	−0.010	−0.097
	(−1.473)	(−1.286)
Bank FE	Yes	Yes
Year FE	Yes	Yes
Observations	163,632	131,086

This table studies the relationship between IDD and the probability of reporting a slight increase in earnings. The dependent variable is a dummy variable that equals one if the ROA change from the previous year is between 0 and 0.001. *IDDAdoption* (*IDDRejection*) equals one for the quarters of IDD adoption (rejection) and afterward, and zero otherwise. Column 1 reports the regression result of the linear probability model, while Column 2 reports the logistic regression result. Standard errors are corrected for state-level clustering, and t-statistics are reported in the parentheses. ***, **, and * indicate 1%, 5%, and 10% significance, respectively.

Appendix A. Variable list

Panel A: Variables in the main analysis	
Variable	Definition
IDD	Indicator variable that equals one for the quarters of IDD adoption and afterwards. For states where the courts decided to change their view and repealed the IDD, the variable *IDD* reverts back to zero for the period of rejection and afterwards.
LLP	Loan loss provisions divided by lagged loans
dNPL	Change in nonperforming loans divided by lagged loans
SIZE	The natural logarithm of total assets
dLOAN	Growth in total loans over the quarter
CSRET	Return on Case-Shiller home price index
dGDP	Growth in GDP per capita over the quarter
dUNEMP(%*p*)	Change in the state unemployment rate over the quarter
CAP	Book value of equity divided by total assets
LOSS	Indicator variable that equals one for negative net income, and zero otherwise.
HHI	Average of deposit market Herfindahl–Hirschman Indices of counties the bank's branches are located in. The weights are given by the shares of deposits the bank raises from the counties.
EBTP	Income before taxes and provisions divided by lagged loans
Ciloans	Share of commercial and industrial loans out of total loans
Reloans	Share of loans secured by real estate out of total loans
Agloans	Share of agricultural loans out of total loans
Persloans	Share of loans to individuals out of total loans
FewerPeers	Indicator variable that equals one if *Number of peers* is less than the sample median, and zero otherwise. *Number of peers* is the weighted average of number of banks in counties that the bank's branches are located in. The weight is given by the share of deposits the bank raises from each county.
Small	Indicator variable that equals one if the bank size is smaller than the sample median, and zero otherwise.
Private	Indicator variable that equals one if neither the bank nor its parent is publicly listed, and zero otherwise. We define a bank as public if it has a CRSP link in the dataset provided by the New York FRB.
HighHHI	Indicator variable that equals one if *HHI* is above the sample median, and zero otherwise.

Panel B: Variables for the tests on LLPvalidity and just-meeting-or-beating earnings benchmarks	
Variable	Definition
LLPvalidity	−1 times the absolute value of the ratio between next year's loan charge-offs and the current year's loan loss provisions minus one. We use both gross charge-offs (*GCO*) and net charge-offs (*NCO*) to calculate *LLPvalidity* and check the robustness of the results.
DEP	Deposits divided by assets
Public	Indicator variable that equals one if either the bank or its parent is publicly listed, and zero otherwise. We define a bank as public if it has a CRSP link in the dataset provided by the New York FRB.
dROA	Change in ROA from the previous year. ROA is defined as pre-tax income divided by lagged assets.
SmallPosΔROA	Indicator variable that equals one if *dROA* is in the range from 0 to 0.001, and zero otherwise
NPL(%)	Nonperforming loans divided by lagged assets
dAssets	Growth in assets over the year
LOAN	Loans divided by assets
dCF	Change in cash flows divided by lagged assets

Appendix B. Dates of adoption and rejection of IDD

This table presents the dates of adoption and rejection of the IDD by state courts. The adoption and rejection are defined by the precedent-setting legal cases. Information in this table is obtained from Klasa et al. (2018), Flammer and Kacperczyk (2019), Chen et al. (2022), and Qiu and Wang (2018).

State	Precedent-setting case	Date	Decision
AR	Southwestern Energy Co. v. Eickenhorst, 955 F. Supp. 1078 (W.D. Ark. 1997)	3/18/1997	Adopt
	Cellco Partnership v. Langston, No. 4:09CV00928 JMM (W.D. Ark. 2009)	12/11/2009	Reject
CT	Branson Ultrasonics Corp. v. Stratman, 921 F. Supp. 909 (D. Conn. 1996)	2/28/1996	Adopt
DE	E.I. duPont de Nemours & Co. v. American Potash & Chem. Corp., 200 A. 2d 428 (Del. Ch. 1964)	5/5/1964	Adopt
FL	Fountain v. Hudson Cush-N-Foam Corp., 122 So. 2d 232 (Fla. Dist. Ct. App. 1960)	7/11/1960	Adopt
	Del Monte Fresh Produce Co. v. Dole Food Co. Inc., 148 F. Supp. 2d 1326 (S.D. Fla. 2001)	5/21/2001	Reject
GA	Essex Group Inc. v. Southwire Co., 501 S.E. 2d 501 (Ga. 1998)	6/29/1998	Adopt
	Holton v. Physician Oncology Services, LP. (Ga. 2013)	5/6/2013	Reject
IA	Uncle B's Bakery v. O'Rourke, 920 F. Supp. 1405 (N.D. Iowa 1996)	4/1/1996	Adopt
IL	Teradyne Inc. v. Clear Communications Corp., 707 F. Supp. 353 (N.D. 111. 1989)	2/9/1989	Adopt
IN	Ackerman v. Kimball Int'l Inc., 652 N.E. 2d 507 (Ind. 1995)	7/12/1995	Adopt
KS	Bradbury Co. v. Teissier-duCros, 413 F. Supp. 2d 1203 (D. Kan. 2006)	2/2/2006	Adopt
MA	Bard v. Intoccia, 1994 U.S. Dist. (D. Mass. 1994)	10/13/1994	Adopt
	U.S. Electrical Services, Inc. v. Schmidt, et al., C.A. No. 12–10845 (D. Mass. 2012)	6/19/2012	Reject
MI	Allis-Chalmers Manuf. Co. v. Continental Aviation & Eng. Corp., 255 F. Supp. 645 (E.D. Mich. 1966)	2/17/1966	Adopt
	CMI Int'l, Inc. v. Intermet Int'l Corp., 649 N.W. 2d 808 (Mich. Ct. App. 2002)	4/30/2002	Reject
MN	Surgidev Corp. v. Eye Technology Inc., 648 F. Supp. 661 (D. Minn. 1986)	10/10/1986	Adopt
MO	H&R Block Eastern Tax Servs. Inc. v. Enchura, 122 F. Supp. 2d 1067 (W.D. Mo. 2000)	11/2/2000	Adopt
NC	Travenol Laboratories Inc. v. Turner, 228 S.E. 2d 478 (N.C. Ct. App. 1976)	6/17/1976	Adopt
	RCR Enters., LLC v. McCall, 14 CVS 3342 (N.C. Sup. Ct. 2014)	12/19/2014	Reject
NJ	Nat'l Starch & Chem. Corp. v. Parker Chem. Corp., 530 A. 2d 31 (N.J. Super. Ct. 1987)	4/27/1987	Adopt
	SCS Healthcare Marketing, LLC v. Allergan U.S., Inc., N.J. Super. Unpub. (N.J. Sup. Ct. Ch. Div. 2012)	12/7/2012	Reject
NY	Eastman Kodak Co. v. Powers Film Prod., 189 A.D. 556 (N.Y.A.D. 1919)	12/5/1919	Adopt
	American Airlines, Inc. v. Imhof, U.S. Dist. (S.D.N.Y. 2009)	6/3/2009	Reject
OH	Procter & Gamble Co. v. Stoneham, 747 N.E. 2d 268 (Ohio Ct. App. 2000)	9/29/2000	Adopt
	Hydrofarm, Inc. v. Orendorff, Ohio App. (Ohio App. Ct. 2008)	12/23/2008	Reject

(*continued on next page*)

Appendix B (*continued*)

State	Precedent-setting case	Date	Decision
PA	Air Products & Chemical Inc. v. Johnson, 442 A. 2d 1114 (Pa. Super. Ct. 1982)	2/19/1982	Adopt
TX	Rugen v. Interactive Business Systems Inc., 864 S.W. 2d 548 (Tex. App. 1993)	5/28/1993	Adopt
	Cardinal Health Staffing Network Inc. v. Bowen, 106 S.W. 3d 230 (Tex. App. 2003)	4/3/2003	Reject
UT	Novell Inc. v. Timpanogos Research Group Inc., 46 U.S.P.Q. 2d 1197 (Utah D.C. 1998)	1/30/1998	Adopt
WA	Solutec Corp. Inc. v. Agnew, 88 Wash. App. 1067 (Wash. Ct. App. 1997)	12/30/1997	Adopt
	Amazon.com, Inc. v. Powers, Case No. C12-1911RAJ (W.D. Wash. 2012)	12/27/2012	Reject

Appendix C. The effects of IDD adoption and rejection

This table presents the regression results estimating the effects of IDD adoption and rejection on discretionary loan loss provisions. The dependent variable, *lnDLLP*, is the natural logarithm of discretionary loan loss provisions. Higher value discretionary loan loss provisions (*DLLP*) indicate lower financial reporting quality. *IDDAdoption* (*IDDRejection*) is an indicator variable that equals one if the bank's state adopted (repealed previously adopted) IDD, and zero otherwise. In Column 1, we use banks in states that adopted or rejected IDD during the sample period as the treated banks. Banks in states that never adopted IDD serve as the control group. Banks in states that adopted IDD before the sample period and kept their view are excluded from the sample. In Column 2, the sample includes banks in states that adopted IDD during the sample period as the treated banks. Banks in the states that never adopted IDD serve as the control group. In Column 3, the sample includes banks in states that rejected IDD during the sample period as the treated banks. Banks in states that adopted IDD before the sample period and never reversed their view are included as the control group. Standard errors are corrected for state-level clustering, and t-statistics are reported in the parentheses. ***, **, and * indicate 1%, 5%, and 10% significance, respectively.

	(1) *lnDLLP*	(2) *lnDLLP*	(3) *lnDLLP*
IDDAdoption	0.123***	0.103***	
	(4.433)	(4.137)	
IDDRejection	−0.149***		−0.202***
	(−2.992)		(−4.064)
SIZE	0.101***	0.114***	0.103***
	(6.277)	(5.362)	(3.911)
CAP_{t-1}	0.976***	1.182***	1.238**
	(4.385)	(3.679)	(2.974)
LOSS	0.951***	0.985***	0.916***
	(27.167)	(22.962)	(14.700)
HHI	0.009	−0.026	0.013
	(0.142)	(−0.297)	(0.114)
$EBTP_{t-1}$	0.044***	0.040***	0.044***
	(5.200)	(3.121)	(3.329)
$Ciloans_{t-1}$	−0.435***	−0.341***	−0.426**
	(−3.875)	(−3.166)	(−2.295)
$Reloans_{t-1}$	−0.308**	−0.179*	−0.355
	(−2.571)	(−1.767)	(−1.740)
$Agloans_{t-1}$	0.200	0.223	0.165
	(1.607)	(1.627)	(0.700)
$Persloans_{t-1}$	0.014	0.097	−0.114
	(0.119)	(0.619)	(−0.486)
LLP_{t-1}	0.288***	0.290***	0.341***
	(11.653)	(9.315)	(9.378)
Bank FE	Yes	Yes	Yes
Year-quarter FE	Yes	Yes	Yes
Observations	544,213	364,189	228,354
Adjusted R-squared	0.243	0.242	0.245

Appendix D. Summary statistics of the sample including multi-state banks

This table presents summary statistics of the expanded sample that includes banks with branches in multiple states. All continuous variables are winsorized at 1% and 99% except for the macroeconomic variables. Appendix A presents the detailed definitions of the variables.

Variable	Obs	Mean	Std. Dev.	P25	P50	P75
$IDD(weighted)$	693,207	.435	.493	0	0	1
$LLP(\%)$	693,207	.11	.225	0	.046	.112
$dNPL(\%)$	693,207	.018	.744	−.18	0	.18
$Assets(USDmil.)$	693,207	983.699	19,751	48.519	101.693	232.453
$SIZE$	693,207	4.756	1.281	3.882	4.622	5.449
$dLoan(\%)$	693,207	2.402	6.284	−.901	1.662	4.593
$CSRET(\%)$	693,207	.997	1.957	−.117	1.165	2.173
$dGDP(\%)$	693,207	.415	.584	.134	.424	.718
$dUNEMP(\%p)$	693,207	−.017	.329	−.2	−.1	.1
$lnDLLP$	693,207	−2.741	1.163	−3.25	−2.604	−2.119
CAP_{t-1}	693,207	.106	.035	.083	.098	.119
$LOSS$	693,207	.085	.279	0	0	0
HHI	693,207	.219	.131	.133	.186	.264
$EBTP_{t-1}(\%)$	693,207	.678	.523	.432	.651	.891
$Ciloans_{t-1}$	693,207	.156	.102	.085	.135	.203
$Reloans_{t-1}$	693,207	.622	.198	.494	.649	.774
$Agloans_{t-1}$	693,207	.09	.139	0	.019	.125
$Persloans_{t-1}$	693,207	.109	.104	.035	.079	.148

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