

Stakeholder Orientation and Bank Earnings Opacity: Evidence from a Quasi-Natural Experiment

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Abstract

Exploiting the staggered adoption of state-level constituency statutes, which allow directors to consider the interests of non-shareholder stakeholders, I examine the effect of stakeholder orientation on bank earnings opacity. Employing a difference-in-differences methodology, I find that banks incorporated in states that adopted constituency statutes significantly increase earnings opacity, primarily through overprovisioning. The effect is concentrated among banks with higher monitoring costs, lower institutional ownership, and lower analyst coverage. Overall, my findings suggest that stakeholder orientation has unintended consequences for bank earnings transparency, with potential implications for market discipline and supervisory oversight.

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1 Introduction

Over the past decades, the traditional corporate model based on shareholder primacy has been increasingly questioned ([Bebchuk and Tallarita, 2020](#)). Critics argue that an excessive emphasis on profit maximisation can conflict with broader stakeholder interests and therefore advocate more stakeholder-oriented approach, under which decision-makers consider the interests of multiple constituencies ([Freeman et al., 2010](#); [Business Roundtable, 2019](#)).

These concerns are particularly relevant in the banking sector, where the global financial crisis highlighted that governance focused on shareholder value maximisation failed to curb excessive risk-taking and generated significant negative externalities for stakeholders and the broader economy ([Laeven, 2013](#)). In response, regulators and policymakers have increasingly emphasised that bank boards should account for the interests of depositors and other non-shareholder constituencies, as safeguarding these interests is important for the stability of single institutions and the whole banking system (e.g., [Basel Committee on Banking Supervision, 2015](#)). This renewed interest in the role of stakeholder-oriented models in banking raises important questions about how stakeholder orientation affects bank behaviour. Despite its policy relevance, empirical evidence on the effects of stakeholder orientation in banking remains scarce. Existing studies focus only on financial stability ([Leung et al., 2019](#)) and payouts ([Chronopoulos et al., 2023](#)). In this paper, I extend this literature by examining the effect of stakeholder orientation on bank earnings opacity.

The quality of banks' financial reporting is crucial for the functioning of the banking system. Greater opacity increases information asymmetries, which in turn reduce the ability of outside stakeholders to assess the value of bank assets and monitor performance and risk ([Bushman and Smith, 2001](#)). Prior research documents that earnings opacity is associated with lower bank stability, loan quality, and market valuations ([Beatty and Liao, 2011](#); [Huizinga and Laeven, 2012](#); [Fosu et al., 2017](#); [Shetabi, 2026](#)). Unsurprisingly, regulatory reforms introduced after the global financial crisis signific-

antly enhanced the role of disclosure and transparency, highlighting the relevance of bank transparency in promoting trust, accountability, and resilience in the banking sector (e.g., [Basel Committee on Banking Supervision, 2011](#)).

Yet, little is known about the effects of stakeholder-oriented governance on banks' financial reporting quality. While [Grougiou et al. \(2014\)](#) analyse the bidirectional relationship between corporate social responsibility (CSR) and bank earnings management, and [Ni \(2020\)](#) investigates the effect of stakeholder orientation on earnings management of non-financial firms, to the best of my knowledge, no previous literature has analysed whether stakeholder orientation affects bank earnings opacity.

From a theoretical perspective, two competing views can explain the relationship between stakeholder orientation and bank earnings opacity. On the one hand, stakeholder orientation may reduce earnings opacity. [Adams and Ferreira \(2007\)](#) show that the divergence between CEOs' and directors' preferences may motivate the former to withhold information from the board to reduce the effectiveness of monitoring and likelihood of board intervention in project selection. Constituency statutes expanded the set of objectives directors are permitted to consider, making board preferences more aligned with those of managers, thereby lowering the cost of information sharing. In line with this argument, [Ni \(2020\)](#) finds that the adoption of constituency statutes improves financial reporting transparency among non-financial firms. I refer to this line of argument as the information-sharing channel.

On the other hand, stakeholder orientation may increase earnings opacity by expanding managerial discretion. While greater alignment between managers and directors may facilitate information sharing, it may also weaken board monitoring ([Adams and Ferreira, 2007](#)). In addition, by broadening the set of objectives directors are expected to consider, stakeholder orientation may make performance evaluation more complex ([Bebchuk and Tallarita, 2020](#); [Cennamo et al., 2009](#)), weakening managerial accountability and in turn widening the scope for opportunistic behaviour (e.g., [Bertrand and Mullainathan, 2003](#)). In banking, this discretion is most likely to manifest in loan loss provi-

sioning, which is forward-looking and relies on substantial managerial judgement. When managers have greater latitude, provisioning choices may deviate from underlying credit conditions, increasing earnings opacity. I term this the managerial discretion channel.

To test these *a priori* ambiguous predictions, I exploit the plausibly exogenous staggered adoption of state-level constituency statutes in the U.S. These statutes allow corporate leaders to consider stakeholder interests in their decision-making, notwithstanding their fiduciary obligations to shareholders (Flammer and Kacperczyk, 2016). Therefore, the enactment of constituency statutes represents a significant shift in corporate governance from the traditional shareholder primacy, where directors are primarily responsible for maximising shareholder value, to a broader stakeholder-oriented approach (Cremers et al., 2019).

Thirty-five states adopted Constituency statutes between 1984 and 2007, primarily as antitakeover defence mechanisms aimed at increasing legal protection for stakeholders. Specifically, these statutes emerged in response to the widespread debate on the limitations of the legal framework in curbing hostile takeovers, which typically impose significant costs on non-shareholder stakeholders (Shleifer and Summers, 1988; Karpoff and Wittry, 2018). Although permissive, constituency statutes are legally enforceable and mark the initial recognition of stakeholder theory in corporate management (Cumming et al., 2021).

To investigate the causal effect of stakeholder orientation on earnings opacity, I exploit the heterogeneity in the timing of these adoption using a difference-in-differences (DiD) approach. This approach offers two key advantages. First, the adoption of the statutes was primarily a response to a wave of hostile takeovers, therefore unlikely to have been driven by bank earnings opacity. This helps rule out the possibility that the estimated effect reflects unobservable bank-specific characteristics that could simultaneously influence stakeholder orientation and bank opacity. Second, because these statutes apply in the state of incorporation rather than the state of headquarters, changes in local economic conditions are unlikely to be the omitted factor driving the relationship between stakeholder

orientation and earnings opacity ([Gao et al., 2021](#)).

Recent research shows that, in the presence of treatment effects that are heterogeneous over time or across units, conventional two-way DiD estimators can yield biased estimates, as early-treated units are inappropriately used as controls for later-treated units (e.g., [Baker et al., 2022](#)). To address this issue, and following prior literature (e.g., [Gormley and Matsa, 2011](#); [Gormley and Matsa, 2016](#); [Cengiz et al., 2019](#)), I employ a stacked DiD design that uses only never-treated banks as the control group. Specifically, for each state-level adoption of constituency statutes, I identify banks incorporated in the adopting state and construct a cohort spanning six quarters before the adoption of constituency statutes and six quarters from the enactment quarter onwards.

Following prior literature, I measure earnings opacity as the discretionary component of loan loss provisions, the main mechanism through which banks manage earnings and capital ([Beatty and Liao, 2014](#)). Specifically, earnings opacity is the absolute value of the residuals obtained by regressing loan loss provisions on a set of bank- and state-specific determinants. These residuals capture the abnormal, or discretionary, portion of loan loss provisions.

In preview, my main results show that, following the adoption of constituency statutes, banks incorporated in adopting states increase earnings opacity relative to banks incorporated in states that did not adopt these statutes. In economic terms, absolute discretionary loan loss provisions increase by 2.4 basis points of lagged loans. This increase is driven by positive discretionary provisions, suggesting that banks use their discretion to overprovision rather than underprovision. These findings are robust to various specifications and robustness tests.

Exploiting cross-sectional heterogeneity, I find that the increase in earnings opacity is concentrated among banks with higher monitoring costs, lower institutional ownership, and lower analyst coverage. Further analysis shows that the increase in discretionary provisions is accompanied by a weaker correspondence between current provisions and future realised credit losses. Overall, these results are consistent with stakeholder orientation increasing managerial discretion, particularly where mon-

itoring is weaker.

This paper makes several contributions to the literature. First, it complements the literature on the effects of stakeholder orientation on corporate outcomes by identifying greater earnings opacity as an unintended consequence of stakeholder orientation in banking. Prior research on non-financial firms shows that stakeholder orientation fosters innovation (Flammer and Kacperczyk, 2016) and firm value (Cremers et al., 2019), while reducing the cost of debt (Gao et al., 2021), earnings management (Ni, 2020), tax avoidance (Cumming et al., 2021; Lin et al., 2025), and share repurchases (Ni et al., 2020). Beyond firm-specific impacts, Bao et al. (2024) show that the consequences of constituency statutes extends to the supply chain, with suppliers of firms in adopting states increasing their relationship-specific investments. Evidence on banks reaches similar conclusions. Leung et al. (2019) show that banks incorporated in adopting states reduce risk-taking by increasing capital and shifting lending towards safer borrowers. Chronopoulos et al. (2023) find that banks reduce share repurchases following adoption, with a stronger effect among banks with sizeable implicit claims, lower transparency, and greater agency conflicts. Taken together, these papers suggest that incorporating stakeholder interests in corporate decision-making can enhance financial stability. This study enriches this literature by documenting an unintended increase in earnings opacity following the adoption of constituency statutes.

Second, this paper contributes to the literature on the determinants of bank earnings opacity. Prior studies have examined the role of bank regulation and competition (Jiang et al., 2016; Dou et al., 2018), asset quality (Huizinga and Laeven, 2012), auditing and internal controls (Altamuro and Beatty, 2010; Kanagaretnam et al., 2010; Nicoletti, 2018), ownership structure (Craig Nichols et al., 2009), corporate social responsibility (Grougiou et al., 2014), debtholder monitoring incentives (Danisewicz et al., 2021), deposit insurance coverage (Rempoutsika et al., 2024; Pugachev et al., 2026), government support (Fan et al., 2020), the distribution of political power (Yue et al., 2022), economic policy uncertainty (Jin et al., 2019) in shaping earnings opacity. Building on this stream of research, I show that

bank earnings opacity is also shaped by legal changes that shift the set of objectives directors are allowed to consider, rather than only through monitoring and regulation, as highlighted in prior work.

In particular, this paper advances the work of [Grougiou et al. \(2014\)](#). Using a simultaneous-equation framework on a sample of U.S. commercial banks, they find that banks that manage earnings more aggressively engage more in CSR, whereas CSR commitment does not affect financial reporting quality. I extend their analysis in two important ways. First, CSR and stakeholder orientation are related but conceptually distinct constructs. CSR refers to the initiatives, practices, and disclosures through which firms address environmental and social concerns, and engage with stakeholders. Empirically, it is typically measured through ratings and scores based on self-reported information and third-party assessments, which may be shaped by disclosure incentives, strategic signalling, or reputational concerns. Such ratings therefore capture observable actions and disclosure rather than the underlying governance orientation. Prior research also documents substantial disagreement across rating providers ([Chatterji et al., 2016](#)), suggesting that CSR ratings are noisy measures of the underlying commitment. Constituency statutes, in contrast, represents a structural shift in corporate governance that weakens shareholder primacy by allowing directors to consider the interests of a broader set of constituencies in their decision-making. CSR can thus be seen as one element of stakeholder-oriented governance, but the two remain distinct: firms may earn high CSR ratings on the basis of observable actions while lacking a governance orientation that accommodates stakeholder interests, and vice versa. Second, the staggered adoption of constituency statutes across US states provides a plausibly exogenous shock to stakeholder orientation, allowing me to identify its causal effect on earnings opacity in banking. Accordingly, this paper identifies stakeholder-oriented governance as an important, unintended, and previously unexplored determinant of bank earnings opacity.

2 Institutional Background and Hypotheses Development

2.1 Institutional Background

The fundamental nature of the corporation has been the subject of a lively debate since the 1930s. The traditional shareholder primacy view posits that managers should act solely to maximise shareholder value and should consider the interests of non-shareholder stakeholders only insofar as doing so is instrumental to that objective ([Berle Jr, 1931](#)). Critics of this view argue instead that firms are a nexus of relationships among different constituencies ([Freeman et al., 2010](#)). On this account, firms do not merely maximise profit but also provide a service to society ([Dodd Jr, 1931](#)). Because stakeholders are affected by corporate actions, managers should balance their interests in decision-making and act as trustees for non-shareholder constituencies.

The wave of hostile takeovers in the 1980s revitalised this debate ([Gao et al., 2021](#)). Takeovers and mergers typically benefit target-firm shareholders, who receive a premium on their shares, at the expense of non-shareholder stakeholders, who often bear substantial costs ([Pontiff et al., 1990](#)). Under traditional corporate law, stakeholders have little protection relative to shareholders, who can bring legal action against directors who fail to meet their fiduciary obligations ([Cumming et al., 2025](#)). In response to the widespread debate on the limitations of the prevailing legal framework in protecting stakeholders from the detrimental effects of hostile takeovers, 35 states enacted constituency statutes between 1984 and 2007, primarily as antitakeover provisions designed to strengthen their legal protection. Table [A.1](#) reports the adoption date for each state.

Constituency statutes allow directors to consider the interests of a broader group of stakeholders, including employees, creditors, suppliers, and local communities, in their corporate decision-making. The first such statute, adopted in Ohio in 1984, states: "A director, in determining what the director reasonably believes to be in the best interests of the corporation, shall consider the interests of the corporation's shareholders and, in the director's discretion, may consider any of the following: (1)

The interests of the corporation's employees, suppliers, creditors, and customers; (2) The economy of the state and nation; (3) Community and societal considerations; (4) The long-term as well as short-term interests of the corporation and its shareholders, including the possibility that these interests may be best served by the continued independence of the corporation."¹

These statutes are largely permissive: they allow directors to weigh stakeholder interests without imposing a corresponding legal obligation (Orts, 1992), thereby preserving directors' fiduciary duties to shareholders and the objective of long-term value maximisation.

Despite their permissive nature, constituency statutes are legally enforceable and represent the earliest recognition of stakeholder theory in US corporate law.² Geczy et al. (2015) examine 47 state and federal court cases relating to constituency statutes between 1983 and 2013 and find only a small number in which courts declined to recognise the expanded fiduciary duties these statutes confer. They conclude that the statutes mark a clear departure from strict shareholder primacy. Consistent with this interpretation, prior research shows that constituency statutes improve corporate stakeholder performance and lead firms to adopt more stakeholder-friendly policies (Flammer and Kacperczyk, 2016; Flammer, 2018; Mayberry and Watson, 2021). Firms also increase stakeholder participation in corporate decision-making following adoption, by appointing more stakeholder representatives to the board, establishing stakeholder committees (Luoma and Goodstein, 1999), and incorporating CSR criteria into executive compensation (Flammer et al., 2019). Overall, this evidence indicates that the adoption of constituency statutes represents a meaningful shift towards stakeholder-oriented governance.

¹OHIO REV. CODE ANN. § 1701.59(F).

²Although intended to reduce takeover threats, the application of constituency statutes is not limited to takeover scenarios in 26 of the 35 adopting states (Geczy et al., 2015).

2.2 Hypotheses Development

Existing literature offers two competing predictions regarding the effect of stakeholder orientation on earnings opacity. Accordingly, I develop two alternative hypotheses. My first hypothesis draws on the framework of [Adams and Ferreira \(2007\)](#). Analysing the board's dual role as monitor and advisor in project selection, they show that CEOs face a trade-off between the costs and benefits of sharing information with the board. This trade-off arises because managers and shareholders often have different project preferences, which gives rise to moral hazard problems. In particular, while CEOs value the board's advisory role in improving project selection, they are averse to being monitored. Greater information sharing therefore improves the quality of the board's advice but, at the same time, strengthens board oversight. When the board's interests are closely aligned with those of shareholders, greater information sharing may increase the likelihood that the board will challenge managerial decisions. In this context, managers may have stronger incentives to withhold information and to engage in activities that increase information asymmetry, such as earnings management, in order to weaken oversight and retain control over project selection.

The enactment of constituency statutes, shifts the board's objective set and, consequently, the CEO's trade-off in sharing information. Stakeholder-oriented governance relaxes strict shareholder primacy and broadens the objectives that boards are expected to balance. Because stakeholder preferences are more closely aligned with those of managers than with those of shareholders ([Avramidis et al., 2018](#)), stakeholder orientation can mitigate the divergence in preferences between managers and boards, bringing board objectives closer to those of the CEO.³ This alignment may reduce board intervention in managerial decision-making and lower the expected cost of information sharing. As a

³Stakeholders and shareholders have conflicting risk preferences and expected payoffs. Shareholders can benefit from higher risk-taking because limited liability makes their payoffs asymmetric and leaves the upside unbounded, whereas stakeholders have concave payoffs as they have fixed claims but significant downside risks ([Jensen and Meckling, 1976](#)). For stakeholders, unlike shareholders, excessive risk raises the probability of losses without a corresponding increase in potential gains. In a similar vein, managers may favour long-term stability and a lower risk of distress, as their human capital and career prospects are tied to a single firm ([Holmström, 1999](#); [Bertrand and Mullainathan, 2003](#); [Gormley et al., 2013](#)).

result, CEOs may have stronger incentives to share information with directors, which reduces opportunistic reporting and ultimately lowers earnings opacity. Consistent with this argument, [Ni \(2020\)](#) finds that non-financial firms incorporated in states adopting constituency statutes engage significantly less in earnings manipulation. Accordingly, I hypothesise the following:

Hypothesis 1a: The adoption of constituency statutes leads to lower earnings opacity.

While the arguments above emphasise the informational benefits of closer alignment between managers and directors, the same mechanism may generate the opposite effect. As [Adams and Ferreira \(2007\)](#) note, governance structures that facilitate information sharing may also weaken the board's propensity to challenge managerial decisions. The resulting increase in managerial discretion may outweigh the benefits associated with improved information flows.

The adoption of constituency statutes expands the set of objectives guiding directors' decision-making and increases the complexity of board oversight. As [Bebchuk and Tallarita \(2020\)](#) and [Cennamo et al. \(2009\)](#) argue, stakeholder governance relies critically on managerial discretion. Considering the interests of multiple stakeholder groups requires directors to balance the interests that are often difficult to measure and may conflict with one another, without establishing corresponding mechanisms through which those stakeholders can hold them accountable. Even where formal control rights remain in place, the presence of heterogeneous objectives complicates performance evaluation and makes monitoring more difficult, thereby widening managerial discretion.

Prior research suggests that weaker oversight of this kind can create incentives for opportunistic rent-seeking and for collusion with stakeholders ([Bertrand and Mullainathan, 2003](#); [Pagano and Volpin, 2005](#); [Cronqvist et al., 2009](#); [Masulis and Reza, 2015](#)). In banking, this latitude is likely to manifest in financial reporting, and particularly in loan loss provisioning. Because provisions are inherently forward-looking and rest on substantial managerial judgement, they represent a relevant margin through which discretion can be exercised, ultimately resulting in higher earnings opacity.

Therefore, I state my second hypothesis as follows:

Hypothesis 1b: The adoption of constituency statutes leads to higher earnings opacity.

3 Research Design

3.1 Data and Sample

To examine the effect of stakeholder orientation on earnings opacity, I construct a sample of listed top-tier U.S. BHCs drawing on multiple sources. Quarterly bank-level financial statements are obtained from FR Y-9C call reports, from 1986q3, the earliest quarter for which these reports are publicly available, to 2007q3, six quarters after the enactment of the last constituency statute among states in which sample banks are incorporated (Texas, 2006q1). Information on state-level adoption dates is from [Karpoff and Wittry \(2018\)](#), and data on banks' states of incorporation are obtained from the National Information Center (NIC) of the Federal Reserve System ([Chronopoulos et al., 2023](#)). Finally, I retrieve state-level macroeconomic data from the Bureau of Economic Analysis, the Bureau of Labor Statistics, and [Klarner \(2013\)](#).

Recent research documents that the conventional two-way fixed-effects DiD estimator can produce biased and inconsistent estimates of the treatment effect when early-treated units are inappropriately used as controls for later-treated units, and vice versa (e.g., [Callaway and Sant'Anna, 2021](#); [Sun and Abraham, 2021](#); [Baker et al., 2022](#)). To address this "bad comparison" problem, I follow prior literature and adopt a cohort-based stacked difference-in-differences design (e.g., [Gormley and Matsa, 2011](#); [Gormley and Matsa, 2016](#); [Cengiz et al., 2019](#)). Specifically, I create a separate cohort for each state's adoption of constituency statutes and stack these cohorts to estimate a two-way fixed-effects DiD with cohort-bank and cohort-time fixed effects. For each cohort, I construct a twelve-quarter event window spanning the six quarters before and six quarters from the adoption quarter onwards. Treated banks

are those incorporated in the state adopting the statute in a given cohort, whereas the control group comprises all banks incorporated in states that never adopted these statutes.

Banks incorporated in states that adopted before 1987q3 cannot form cohorts of their own because of the data requirements of my opacity measure: the first-stage regression includes covariates lagged up to two quarters, so the measure is only available from 1987q2, making 1987q3 the first adoption cohort for which a pre-treatment observation is available. Thus, these banks are excluded from the stacked sample, although they are retained when estimating Equation (1). In addition, I exclude a cohort containing a single treated bank (Tennessee), since a single institution provides too little information to identify a treatment effect and can make the estimated standard errors unreliable. My final stacked sample consists of 4,814 bank-cohort-quarter observations. In total, 46 BHCs are treated at some point during the sample period, while 121 BHCs are never treated. Table A.1 shows treated states included in the stacked sample.

3.2 Measuring Earnings Opacity

Following prior literature, I measure earnings opacity as the discretionary component of loan loss provisions, the main mechanism through which bank managers manage earnings and capital (Huizinga and Laeven, 2012; Beatty and Liao, 2014). This measure is the unexplained portion of loan loss provisions, obtained by regressing them on a set of bank- and state-specific determinants of provisioning, capturing deviations from expected provisioning behaviour (Danisewicz et al., 2021). Following Beatty and Liao (2014), my econometric model takes the following form:

$$\begin{aligned}
LLP_{i,s,h,t} = & \alpha_0 + \alpha_1 dNPA_{i,s,h,t+1} + \alpha_2 dNPA_{i,s,h,t} + \alpha_3 dNPA_{i,s,h,t-1} \\
& + \alpha_4 dNPA_{i,s,h,t-2} + \alpha_5 SIZE_{i,s,h,t-1} + \alpha_6 dLOAN_{i,s,h,t} \\
& + \alpha_7 ALL_{i,s,h,t-1} + \alpha_8 dGSP_{h,t} + \alpha_9 dUNEMP_{h,t} \\
& + \alpha_{10} HPRET_{h,t} + \gamma_s + \varepsilon_{i,s,h,t}
\end{aligned} \tag{1}$$

where the subscripts i , s , h , and t denote bank, state of incorporation, state of headquarters, and quarter, respectively. LLP is loan loss provisions divided by lagged total loans. $dNPA$ is the change in non-performing loans between quarter t and $t - 1$, divided by lagged total loans. [Bushman and Williams \(2012\)](#) find that banks use both forward-looking and past values of non-performing loans in determining provisions. Accordingly, alongside the current change, I include the forward change and the first and second lags of $dNPA$. $SIZE$ is the natural logarithm of total assets, which captures differences in supervisory oversight and public monitoring. $dLOAN$ is the change in total loans between quarter t and $t - 1$, divided by lagged total loans, and accounts for the higher provisioning expected when banks extend lending. ALL is the ratio of the loan loss allowance to lagged total loans, included because adequate coverage built up through past provisioning is likely to affect current provisions.

I also include three state-level macroeconomic variables: the growth rate of gross state product ($dGSP$), the change in the unemployment rate ($dUNEMP$), and the return on the house price index ($HPRET$), all of which prior research shows to be significantly related to loan loss provisions (e.g., [Beatty and Liao, 2014](#)). Equation (1) further includes state-of-incorporation fixed effects (γ_s) to control for unobservable time-invariant state characteristics that may affect provisioning

3.3 Empirical Specification and Descriptive Statistics

To examine the effect of the adoption of constituency statutes on bank earnings opacity, I estimate the following stacked DiD specification:

$$\begin{aligned}
Earnings\ Opacity_{i,s,c,h,t} = & \alpha + \beta_1 CS_{s,t} + \beta_2 SIZE_{i,c,t} + \beta_3 CAP_{i,c,t} + \beta_4 LLP_{i,c,t-1} \\
& + \beta_5 LOSS_{i,c,t} + \beta_6 EBT_{i,c,t} + \beta_7 dGSP_{h,t} + \beta_8 dUNEMP_{h,t} \\
& + \beta_9 HPRET_{h,t} + \delta_{i,c} + \delta_{c,t} + \varepsilon_{i,s,c,h,t}
\end{aligned} \tag{2}$$

where $Earnings\ Opacity_{i,s,c,h,t}$ is the discretionary component of loan loss provisions for bank i , incorporated in state s , belonging to cohort c , headquartered in state h , in quarter t , as defined above. The explanatory variable of interest is $CS_{s,t}$, an indicator equal to one if the bank's state of incorporation has adopted a constituency statute by quarter t , and zero otherwise. Equation (2) further includes a set of bank- and state-level controls that may affect earnings opacity, following [Yue et al. \(2022\)](#). Among the bank-specific variables, I control for the natural logarithm of total assets ($SIZE$), the equity-to-assets ratio (CAP), the one-quarter lagged loan loss provisions ($Lagged\ LLP$), an indicator equal to one in quarters with negative earnings before provisions and taxes ($LOSS$), and the ratio of earnings before provisions and taxes to lagged total loans ($EBTP$).⁴ I further include the quarterly return on the house price index ($HPRET$), the growth rate of gross state product ($dGSP$), and the change in the unemployment rate ($dUNEMP$), which account for time-varying economic conditions in the bank's headquarters state.

$\delta_{i,c}$ and $\delta_{c,t}$ denote cohort-by-bank and cohort-by-quarter fixed effects, which absorb time-invariant bank characteristics within each cohort and shocks common to all banks in a cohort in a given quarter, respectively. Standard errors are clustered at the state-of-incorporation level ([Chronopoulos et al., 2023](#)). Because banks incorporated in never-adopting states enter every cohort while treated banks enter only the cohort corresponding to their state's adoption, unweighted estimation would over-represent control observations and give disproportionate influence to banks replicated across multiple cohorts. I therefore weight observations by the inverse of their frequency in the stacked sample.

Table 1 presents summary statistics for banks incorporated in states that adopted constituency statutes (treated) and banks incorporated in those that did not (non-treated). The mean earnings opacity is 0.0008 for treated banks and 0.0009 for non-treated, with comparable dispersion across groups. The two groups have also similar characteristics. Treated banks are somewhat smaller, hold

⁴The Tier 1 risk-based capital ratio, also included by [Yue et al. \(2022\)](#), is excluded because it is only available in the FR Y-9C call reports from 1993 onwards. Bank capitalisation is captured by the equity-to-assets ratio.

similar levels of capital, have slightly lower provisions, and report marginally higher earnings before provisions.

[Insert Table 1 here]

4 Empirical Results

4.1 Baseline Results

Table 2 presents estimates of Equation (2). Column 1 excludes control variables, while column 2 introduces a set of bank- and state-specific covariates. Both columns include cohort-bank and cohort-time fixed effects. The coefficient of interest, CS , is positive and statistically significant. Estimates in column 2 suggest that banks incorporated in adopting states increase earnings opacity by 2.4 basis points of lagged loans relative to banks incorporated in non-adopting states. This corresponds to approximately 17.6% of average total loan loss provisions. The magnitude is comparable to those reported by [Pugachev et al. \(2026\)](#), who find that banks increase provisions by 3.4 basis points of lagged loans, or 38% of the average loan loss provisions, following the extension of deposit insurance coverage.

These findings are consistent with the managerial discretion channel, which posits that stakeholder governance may make board oversight more complex and costly, thereby widening managerial discretion over financial reporting. Under stakeholder-oriented governance, multiple and potentially conflicting objectives complicate performance evaluation and monitoring (e.g., [Bebchuk and Tallarita, 2020](#)). In such settings, broadening the range of interests directors may consider affords managers greater latitude over provisioning, particularly given the inherently forward-looking and judgemental nature of loan loss provisions, ultimately resulting in greater earnings opacity.

The estimates above, however, do not reveal which form of discretionary provisioning drives the increase in opacity. On the one hand, greater managerial discretion and diffuse accountability

may shield opportunistic rent-seeking behaviour (e.g., [Jensen and Meckling, 1976](#); [Bertrand and Mulainathan, 2003](#)), encouraging underprovisioning aimed at inflating earnings and capital positions ([Ahmed et al., 1999](#); [Laeven and Majnoni, 2003](#)). On the other hand, managers may rationally trade lower current reported earnings for greater future discretion over the timing of future recognition, weakening the mapping between economic performance and reported results ([Beatty and Liao, 2014](#)).

To distinguish between these possibilities, and following prior literature (e.g., [Danisewicz et al., 2021](#)), I re-estimate my baseline model separately for income-increasing and income-decreasing discretionary provisions. Specifically, I split the sample according to the sign of the residuals from Equation (1), into *Underprovisioning* (negative residuals) and *Overprovisioning* (positive residuals). Columns (3) and (4) report the results. The coefficient on *CS* is positive and statistically significant for overprovisioning, while it is small and statistically insignificant for underprovisioning. This indicates that the increase in earnings opacity operates primarily through income-decreasing provisions rather than through provisioning aimed at inflating reported earnings and capital. If the increase reflects greater managerial discretion rather than more accurate provisioning, the effect should be concentrated in banks where monitoring is weakest.

[Insert Table 2 here]

4.2 Dynamic Effects

A key assumption in the DiD framework is that, absent the adoption of constituency statutes, treated and control banks would have followed parallel trends in earnings opacity [Bertrand et al. \(2004\)](#). To assess this assumption and mitigate concerns that my results reflect systematic differences between treated and control banks or reverse causality, I follow [Ni \(2020\)](#) and examine the dynamic change in the earnings opacity around the enactment of constituency statutes. Specifically, I estimate a dynamic version of Equation (2) in which the treatment indicator, *CS*, is replaced by a set of indicators for the

quarters relative to adoption, each equal to one for treated banks in the corresponding event quarter and zero otherwise. The specification includes indicators from six quarters before to five quarters after adoption (CS_{t-6} to CS_{t+5}). The quarter immediately preceding adoption is omitted and serves as the reference period.

Table 3 reports the results. Across all specifications, the coefficients on the indicators for the quarters preceding adoption are small in magnitude and statistically insignificant, supporting the absence of differential pre-treatment trends in earnings opacity. In contrast, the post-adoption coefficients are generally positive and statistically significant, in line with my baseline results. Taken together, these findings show no evidence of pre-existing trends and indicate a positive and persistent increase in earnings opacity following the adoption of constituency statutes, supporting a causal interpretation of the results.

[Insert Table 3 here]

5 Robustness Tests

5.1 Coincident Laws

A potential concern is that the adoption of other state-level laws coinciding with constituency statutes may affect earnings opacity and confound my results. To address this possibility, I collect the adoption dates of several contemporaneous state-level laws and augment Equation (2) with indicator variables equal to one from the adoption quarter of these laws onwards.

First, I control for the coincident adoption of universal demand (UD) laws. Between the late 1980s and the early 2000s, several U.S. states adopted UD laws, which require shareholders to obtain board approval before filing derivative lawsuits. These laws abolished the futility exception, which had previously allowed shareholders to bypass the board and proceed directly with derivative litigation when directors were not independent or faced conflicts of interest (Kinney, 1994). UD laws therefore

shift power from shareholders to directors by bringing derivative litigation under internal control, weakening shareholder litigation rights as a governance mechanism. Prior research shows that UD laws affect corporate policies and governance arrangements (e.g., [Ni and Yin, 2018](#); [Nguyen et al., 2018, 2020](#); [Foroughi et al., 2022](#); [Freund et al., 2023](#)).

Second, I examine whether other coincident antitakeover laws drive the increase in earnings opacity. Prior literature suggests that takeover threats alter managerial reporting incentives, both by reducing the value of an opaque information environment as a takeover defence ([Armstrong et al., 2012](#)) and by aligning the preferences of managers, directors, and shareholders more closely ([Ni, 2020](#)). I therefore include an indicator variable for the adoption of share acquisition, business combination, poison pill, and fair price laws (*ANTI*).

Finally, I consider whether the competitive pressures arising from bank deregulation confound my results. Before the 1970s, most U.S. states imposed strict restrictions on the geographic scope of banks' operations, substantially limiting both within-state and cross-state branching. These intrastate and interstate constraints were progressively relaxed over the following decades, increasing competition and reshaping bank behaviour (e.g., [Jayaratne and Strahan, 1996](#); [Rice and Strahan, 2010](#); [Kroszner and Strahan, 2014](#)). Consistent with this, [Jiang et al. \(2016\)](#) find that deregulation-induced competition improved the quality of financial reporting. Accordingly, I control for both intrastate (*INTRA*) and interstate (*INTER*) deregulation.⁵

Table 4 reports the results after controlling for the passage of coincident state-level laws. The main coefficient of interest, *CS*, remains positive and statistically significant, indicating that my findings are robust to the inclusion of contemporaneous legal and regulatory changes.

[Insert Table 4 here]

⁵Following [Jiang et al. \(2016\)](#), I assume that deregulation takes effect in the last quarter of the year in which a state deregulates. The two indicator variables therefore equal one from the first quarter of the following year.

5.2 Propensity Score Matching

Next, I conduct two additional tests to mitigate concerns that the non-random choice of incorporation and systematic differences in characteristics between treated and control banks may bias the results.

First, following [Ni \(2020\)](#), I employ one-to-one nearest neighbour propensity score matching with common support and without replacement to construct a sample of treated and control banks with comparable characteristics. For each cohort, I retain observations for the quarter preceding the adoption of the constituency statute and estimate the probability of treatment using a logit model based on the continuous bank-level controls from Equation (2). I then match each treated bank to the never-treated bank with the closest propensity score, imposing a caliper of 0.01. This procedure yields a sample of 44 matched pairs, for a total of 574 observations.

Second, I complement this approach with kernel propensity score matching with common support. Unlike nearest neighbour matching, kernel matching constructs the counterfactual by re-weighting control observations, assigning greater weight to banks that are more similar in terms of their estimated propensity score ([Heckman et al., 1998](#)). By drawing on a broader set of untreated observations within the common support region, this approach can reduce the variance of the resulting treatment effect estimates. For each cohort, I estimate propensity scores using a logit model and the continuous bank-level controls from the baseline specification, measured one quarter before treatment. I then apply an Epanechnikov kernel to generate the weights used in the estimation of Equation (2).

Table 5 reports the results for the nearest neighbour-matched sample (column 1) and the kernel-matched sample (column 2). In line with my baseline results, the coefficient on *CS* is positive and statistically significant at the 1% level in both specifications.

[Insert Table 5 here]

5.3 Falsification Tests

To further assess the validity of the identification strategy and ensure that the results are not driven by chance, I conduct a placebo test based on the random reassignment of treatment states and adoption dates, following [Ni et al. \(2020\)](#). More specifically, I randomly assign pseudo-treatment to states without replacement and draw pseudo-adoption dates from the quarters spanning the first to the last actual adoption quarter. I then re-estimate the effects of these pseudo-events using Equation (2) and store the resulting coefficients. I repeat this procedure 5,000 times to obtain an empirical distribution of placebo estimates against which the baseline estimate can be compared.

Figure 1 presents this distribution. The dashed vertical line indicates the actual coefficient estimated in column 2 of Table 2 (0.0002). The absolute value of the placebo coefficient is at least as large as that of the actual estimate in only 10 of the 5,000 simulations, yielding a two-sided empirical p-value of 0.0022 after applying the finite-simulation correction. This evidence suggests that the baseline finding is unlikely to be driven by chance, providing additional support for a causal interpretation of the effect of the adoption of constituency statutes on earnings opacity.

[Insert Figure 1 here]

5.4 Other Robustness Checks

In this section, I carry out a series of supplementary tests to further examine the robustness of my main findings. For brevity, Table 6 reports only the coefficients on *CS* and their corresponding standard errors.

In Panel A, I assess whether my baseline results are sensitive to alternative sample definitions by re-estimating Equation (2) on different subsamples. First, I account for the possibility that my results are influenced by the distinctive characteristics of banks incorporated in Delaware. Delaware is one of the few states that never adopted constituency statutes, although its corporate law allows firms

to consider stakeholder interests where doing so is instrumental to the maximisation of shareholder value (Barzuza, 2009). Since most of the control banks in my sample are incorporated in Delaware, in row (1), I exclude these banks.

Second, Karpoff and Wittry (2018) find that several firms in Arizona, Indiana, Massachusetts, Minnesota, and Pennsylvania lobbied for the adoption of constituency statutes, making these legislative changes for firms incorporated in those states potentially endogenous. To mitigate the concern that my results might be driven by such lobbying activities, in row (2), I therefore exclude banks incorporated in these states.

Finally, following Ni (2020), in row (3) I exclude banks incorporated in the nine states whose constituency statutes apply only in takeover contexts. This test allows me to rule out the possibility that the documented effect is driven solely by takeover provisions.

Across all specifications, my main findings remain qualitatively similar to those reported in Table 2, indicating that they are robust to alternative sample definitions.

Next, I test the sensitivity of my main results to alternative dependent variables using different specifications of Equation (1). In rows (4)–(6), I re-estimate the first-stage regression using the alternative models proposed by Beatty and Liao (2014). Specifically, in row (4), *Earnings Opacity* is obtained by excluding *ALL* from Equation (1). In row (5), *ALL* is replaced by the ratio of net charge-offs to lagged total loans (*CHO*), while in row (6) opacity is estimated from a model that includes both *ALL* and *CHO*. Rows (7) and (8) report estimates based on an adapted version of the Kanagaretnam et al. (2010) specification, which controls for loan portfolio composition through the shares of real estate, commercial, and consumer loans in total loans, and on an adapted version of the Nicoletti (2018) model, which excludes *ALL* and accounts for earnings before loan loss provisions and bank equity capital. In all cases, *CS* remains positive and statistically significant, corroborating my baseline results.

In panel C, I examine the robustness of my results to alternative fixed effects, cohort composition,

and weighting schemes. In row (9), I re-estimate Equation (2) including region-by-quarter-by-cohort fixed effects. This specification controls for broader regional economic conditions, beyond state-level controls, that may affect provisioning. In row (10), I also retain cohorts in which only one bank is treated, which are excluded from the baseline analysis. In row (11), I re-estimate my baseline models without sample weights. In line with results reported in Table 2, CS is positive and statistically significant.

Finally, I re-estimate the baseline model using alternative event windows of $[-4, +4]$, $[-8, +8]$, and $[-10, +10]$ quarters. The results, reported in Panel D, show that the effect of constituency statutes on earnings opacity is qualitatively unchanged across all event windows.

[Insert Table 6 here]

6 Additional Analyses

6.1 Cross-Sectional Tests

In this section, I leverage cross-sectional heterogeneity in my sample to further examine the effect of stakeholder orientation on earnings opacity. If the adoption of constituency statutes increases earnings opacity by widening managerial discretion, its effect should be concentrated in banks where managerial actions are inherently harder to monitor and where external constraints are weaker. I therefore examine three dimensions of the monitoring environment: the difficulty of evaluating a bank's loan portfolio, the presence of institutional investors, and the intensity of analyst coverage. For each conditioning variable, I compute each bank's cohort-specific median value over the pre-treatment period and partition at the sample median.

Information asymmetry and higher monitoring costs impair the ability of directors and outside stakeholders to assess and challenge managerial actions (Diamond, 1984). Bank assets are inherently opaque, as their realised values depend on private information and on the subjective evaluation of

borrowers' repayment capacity. Managers consequently retain substantial discretion over provisioning and the recognition of loan losses (Beatty and Liao, 2014). Where loan portfolios are more difficult to evaluate, governance mechanisms might be less effective in constraining managerial decisions. To the extent that stakeholder orientation widens managerial latitude, its effect should be stronger in banks with higher monitoring costs.

To test this proposition, following Avramidis et al. (2018), I proxy for monitoring costs using the ratio of loans not secured by real estate to total assets. The value of these loans depends more heavily on private information and subjective assessment, making the evaluation by directors and outsiders more difficult. Columns (1) and (2) of Table 7 report the results. *CS* is positive and statistically significant only for banks with high monitoring costs, indicating that the effect of constituency statutes is stronger where managerial actions are inherently harder to monitor.

Next, I examine the role of institutional ownership. Relative to dispersed individual shareholders, institutional investors have stronger incentives and greater capacity to acquire information, monitor managerial decisions, and discipline opportunistic behaviour (Shleifer and Vishny, 1986). This disciplinary role may be particularly valuable in banking, where provisioning rests on private information and managerial judgement. Institutional investors may therefore constrain managers' ability to exploit this informational advantage to manage earnings through loan loss provisions. Consistent with this view, prior research documents that higher institutional ownership is associated with lower earnings management (Elyasiani et al., 2017; Miller et al., 2021). I therefore expect the effect of constituency statutes to be stronger in banks with low institutional ownership, where weaker monitoring by sophisticated investors may afford managers greater latitude over discretionary provisioning.

Columns (3) and (4) of Table 7 report estimates after splitting the sample into banks with high and low institutional ownership, measured as the percentage of shares held by institutional investors.⁶ Consistent with the role of institutional investors as a source of external monitoring, earnings opacity

⁶Institutional ownership is sourced from LSEG 13-F.

increases following the adoption of constituency statutes only among banks with low institutional ownership.

Finally, I exploit heterogeneity in analyst coverage to examine whether my results vary with the quality of the external information environment. Analysts play an important governance role by producing, analysing and disseminating firm information, and by subjecting managerial actions to greater market scrutiny (Yu, 2008; Irani and Oesch, 2016). In reducing managers' informational advantage over outsiders, analysts can discipline managerial behaviour and mitigate agency conflicts. Empirical evidence supports this view: exploiting exogenous reductions in coverage, Chen et al. (2015) show that lower coverage leads to greater managerial expropriation and greater earnings management, while Yu (2008) finds that higher coverage is associated with less earnings management. Where oversight is already weakened by the adoption of constituency statutes, low analyst coverage may afford managers further scope to exercise discretion over financial reporting, amplifying the effect on earnings opacity.

I examine this prediction in Columns (5) and (6), which report separate regressions for banks with high and low analyst coverage, proxied by the number of earnings-per-share estimates.⁷ The coefficient on *CS* is significant only for banks with low coverage, again indicating that the effect of constituency statutes is stronger where external monitoring is weaker.

Collectively, these cross-sectional results indicate that the effect of stakeholder orientation on earnings opacity is concentrated in banks with weaker monitoring, consistent with the managerial discretion channel.

[Insert Table 7 here]

⁷The number of earnings-per-share forecasts is sourced from I/B/E/S.

6.2 Alignment of Loan Loss Provisions with Future Charge-Offs

Next, I examine the implications of the adoption of constituency statutes for the correspondence between loan loss provisions and future realised credit losses. The evidence presented so far is consistent with constituency statutes expanding managerial discretion over provisions. The implications of this additional discretion for the mapping between current provisions and future realised losses are, however, ambiguous *a priori*. As discussed by [Bushman and Williams \(2012\)](#), greater discretion may have opposing effects. On the one hand, it affords managers more scope for opportunistic behaviour, which would weaken the extent to which current provisions reflect future charge-offs. On the other hand, greater discretion may allow managers to incorporate additional information into their estimates of future credit losses, strengthening the association between current provisions and subsequent loan portfolio quality.

To test this, I re-estimate Equation (2), replacing *Earnings Opacity* with *LLP Validity*, a measure of how closely current provisions reflect future realised credit losses.⁸ Following [Beatty et al. \(2019\)](#) and [Oh and Park \(2023\)](#), I define *LLP Validity* as follows:

$$LLP Validity_{i,s,c,t} = -1 \times \left| \frac{CO_{i,s,c,t+1}}{LLP_{i,s,c,t}} - 1 \right| \quad (3)$$

LLP Validity captures the correspondence between current provisions and subsequent net charge-offs, with higher values indicating that provisions more accurately anticipate future realised losses. I construct this variable over both one-quarter and four-quarter horizons. In the one-quarter specification, $LLP_{i,s,c,t}$ denotes provisions recorded in quarter t and $CO_{i,s,c,t+1}$ denotes net charge-offs realised in quarter $t + 1$. In the four-quarter specification, these terms denote, respectively, total provisions recorded from quarter $t - 3$ to t and total net charge-offs realised from quarter $t + 1$ to $t + 4$. The former captures the shorter-horizon mapping between provisions and realised losses, while the latter is more

⁸Lagged *LLP* is excluded from the specification to avoid a mechanical relationship with the dependent variable.

consistent with the OCC's (1997) view that provisions should map into realised credit losses within a one-year window.

The results, reported in Table 8, show that the coefficient on *CS* is negative and statistically significant in both specifications, indicating that provisions become less closely tied to future charge-offs following the enactment of constituency statutes. Together with my baseline results, this evidence suggests that provisions increasingly deviate from both the observable determinants of expected credit losses and subsequently realised losses. The increase in discretionary provisioning therefore does not appear to reflect a more accurate anticipation of future losses. Rather, the change in provisioning behaviour appears to reflect greater managerial discretion.

[Insert Table 8 here]

7 Conclusions

In this paper, I examine the effect of stakeholder orientation on bank earnings opacity. Exploiting the staggered adoption of constituency statutes, I provide evidence that banks incorporated in adopting states significantly increase earnings opacity, with the effect driven primarily by overprovisioning. These results are robust to a range of identification tests, supporting a causal interpretation. Further analyses show that the increase in opacity is concentrated among banks with higher monitoring costs, lower institutional ownership, and lower analyst coverage. In addition, I find that the correspondence between loan loss provisions and future realised credit losses weakens following adoption. Collectively, my findings indicate that stakeholder orientation expands managerial discretion over provisioning and reduces transparency of reported earnings.

This study contributes to the understanding of the effects of stakeholder governance reforms by identifying stakeholder orientation as an unintended source of bank opacity. While prior research has focused on bank stability and payout policies (Leung et al., 2019; Chronopoulos et al., 2023), I provide

new evidence that broadening corporate objectives also affects banks' financial reporting quality. This is particularly relevant in banking, where the discretionary nature of loan loss provisions makes earnings especially susceptible to managerial influence. Although stakeholder orientation can generate benefits for non-shareholder stakeholders, it may also widen managerial discretion and, in doing so, reduce the transparency of reported earnings. Reduced financial reporting transparency may in turn weaken market discipline and complicate supervisory oversight, with potential consequences for bank stability.

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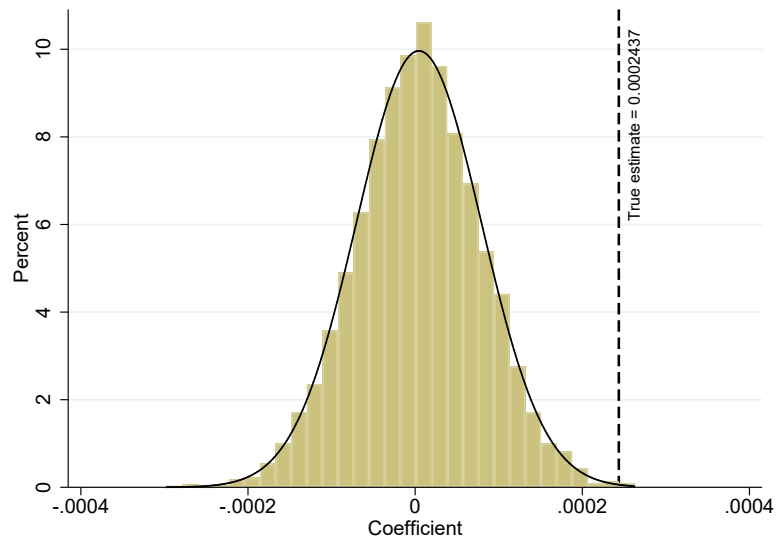
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Figure 1: Falsification Test



Notes: The figure presents the empirical distribution of placebo coefficients obtained from 5,000 simulations in which the adoption states and enactment dates of constituency statutes are randomly assigned. The dashed vertical line indicates the actual estimate reported in Table 2.

Tables

Table 1: Descriptive Statistics for Treated and Non-treated Banks

This table presents the descriptive statistics for the main dependent and independent variables for banks incorporated in states that adopted constituency statutes (Treated) and those incorporated in states that did not (Non-treated). See Table A.2 in the Appendix for variable definitions and construction.

		Treated banks					Non-treated banks				
		N	Mean	SD	P25	P75	N	Mean	SD	P25	P75
Earnings Opacity		473	0.0008	0.0011	0.0002	0.0009	4341	0.0009	0.0012	0.0003	0.0011
LLP Validity		464	-0.0663	1.3588	-0.2215	0.6951	4161	-0.0003	1.3665	-0.0911	0.6720
Four-Quarter Validity	LLP	434	-0.6487	0.8539	-0.7367	-0.2426	3789	-0.6094	0.7685	-0.7279	-0.2200
CS		473	0.5624	0.4966	0.0000	1.0000	4341	0.0000	0.0000	0.0000	0.0000
SIZE		473	14.2629	1.5265	13.1077	15.0945	4341	14.5977	1.5953	13.2839	15.6029
CAP		473	0.0807	0.0198	0.0675	0.0901	4341	0.0802	0.0245	0.0639	0.0902
Lagged LLP		473	0.0012	0.0016	0.0005	0.0014	4341	0.0015	0.0019	0.0006	0.0017
LOSS		473	0.0233	0.1509	0.0000	0.0000	4341	0.0325	0.1773	0.0000	0.0000
EBP		473	0.0080	0.0035	0.0063	0.0094	4341	0.0077	0.0034	0.0056	0.0092
dGSP		473	0.0153	0.0052	0.0120	0.0192	4341	0.0143	0.0057	0.0103	0.0186
dUNEMP		473	-0.0002	0.2756	-0.2000	0.1000	4341	-0.0410	0.2536	-0.2000	0.1000
HPRET		473	0.0101	0.0135	0.0039	0.0148	4341	0.0152	0.0182	0.0042	0.0218
Mon. Costs		473	0.2572	0.1059	0.1731	0.3318	4341	0.2884	0.1240	0.2036	0.3711
Inst. Own.		473	0.4126	0.4148	0.0459	1.0000	4341	0.4631	0.3707	0.1236	1.0000
Analyst Coverage		473	-0.1083	0.6039	-0.3835	0.1467	4341	0.0213	0.6709	-0.3435	0.2936

Table 2: The Impact of Constituency Statutes on Earnings Opacity

This table presents estimates of the effect of Constituency Statutes on *Earnings Opacity* based on Equation (2). Columns (1) and (2) measure earnings opacity using the absolute value of discretionary loan loss provisions from Equation (1). Column (3) examines positive discretionary loan loss provisions, capturing overprovisioning (income-decreasing discretion), while Column (4) examines negative discretionary loan loss provisions, capturing underprovisioning (income-increasing discretion). All columns include cohort-bank and cohort-time fixed effects. See Table A.2 in the Appendix for variable definitions. Standard errors clustered at the state of incorporation level are in parentheses. * Denotes significance at the 10% level, ** denotes significance at the 5% level and *** denotes significance at the 1% level.

	Earnings Opacity		Over- provisioning	Under- provisioning
	(1)	(2)	(3)	(4)
CS	0.0004** (0.0001)	0.0002*** (0.0001)	0.0005*** (0.0001)	0.0000 (0.0000)
SIZE		-0.0004 (0.0003)	-0.0009*** (0.0003)	0.0004** (0.0002)
CAP		-0.0098 (0.0062)	-0.0218** (0.0085)	-0.0018 (0.0027)
Lagged LLP		0.0222 (0.0177)	0.0189 (0.0330)	0.0302*** (0.0108)
LOSS		0.0040*** (0.0003)	0.0047*** (0.0003)	-0.0005 (0.0003)
EBP		0.0570 (0.0383)	0.0996*** (0.0302)	-0.0526 (0.0329)
dGSP		-0.0231*** (0.0063)	-0.0256** (0.0101)	-0.0112*** (0.0031)
dUNEMP		0.0002 (0.0002)	-0.0000 (0.0002)	0.0006*** (0.0001)
HPRET		-0.0021 (0.0016)	-0.0011 (0.0025)	-0.0045*** (0.0016)
Constant	0.0007*** (0.0000)	0.0075 (0.0050)	0.0149*** (0.0044)	-0.0040* (0.0020)
Cohort-bank Fixed Effects	Yes	Yes	Yes	Yes
Cohort-time Fixed Effects	Yes	Yes	Yes	Yes
Observations	4,814	4,814	2,133	2,598
Adj. R-squared	0.349	0.601	0.686	0.583

Table 3: Dynamic Effects of Constituency Statutes on Earnings Opacity

This table presents estimates of the dynamic effect of Constituency Statutes on *Earnings Opacity*. All columns include cohort-bank and cohort-time fixed effects. See Table A.2 in the Appendix for variable definitions. Standard errors clustered at the state of incorporation level are in parentheses. * Denotes significance at the 10% level, ** denotes significance at the 5% level and *** denotes significance at the 1% level.

	Earnings Opacity	
	(1)	(2)
CS_{t-6}	-0.0001 (0.0001)	-0.0001 (0.0002)
CS_{t-5}	-0.0001 (0.0001)	-0.0001 (0.0001)
CS_{t-4}	-0.0001 (0.0001)	-0.0000 (0.0001)
CS_{t-3}	-0.0002** (0.0001)	-0.0002 (0.0001)
CS_{t-2}	0.0000 (0.0001)	0.0001 (0.0001)
CS_t	0.0001 (0.0001)	0.0002 (0.0002)
CS_{t+1}	0.0002 (0.0002)	-0.0000 (0.0002)
CS_{t+2}	0.0003** (0.0001)	0.0003** (0.0001)
CS_{t+3}	0.0002* (0.0001)	0.0002* (0.0001)
CS_{t+4}	0.0004** (0.0002)	0.0004*** (0.0001)
CS_{t+5}	0.0004** (0.0002)	0.0005** (0.0002)
All Controls	No	Yes
Cohort-bank Fixed Effects	Yes	Yes
Cohort-time Fixed Effects	Yes	Yes
Observations	4,814	4,814
Adj. R-squared	0.349	0.576

Table 4: The Impact of Constituency Statutes on Earnings Opacity: Coincident State Laws

This table presents estimates of the effect of Constituency Statutes on *Earnings Opacity* controlling for coincident state laws. All columns include the controls specified in Equation (2), as well as cohort-bank and cohort-time fixed effects. See Table A.2 in the Appendix for variable definitions. Standard errors clustered at the state of incorporation level are in parentheses. * Denotes significance at the 10% level, ** denotes significance at the 5% level and *** denotes significance at the 1% level.

	Earnings Opacity				
	(1)	(2)	(3)	(4)	(5)
CS	0.0002*** (0.0001)	0.0002*** (0.0001)	0.0002*** (0.0001)	0.0002*** (0.0001)	0.0002*** (0.0001)
UD	-0.0005*** (0.0001)				-0.0005*** (0.0001)
ANTI		0.0002 (0.0002)			0.0001 (0.0002)
INTRA			-0.0002 (0.0002)		-0.0002 (0.0002)
INTER				-0.0002 (0.0002)	-0.0003 (0.0002)
All Controls	Yes	Yes	Yes	Yes	Yes
Cohort-bank Fixed Effects	Yes	Yes	Yes	Yes	Yes
Cohort-time Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	4,814	4,814	4,814	4,814	4,814
Adj. R-squared	0.602	0.601	0.601	0.601	0.603

Table 5: The Impact of Constituency Statutes on Earnings Opacity: Matched Samples

This table presents estimates of the effect of Constituency Statutes on *Earnings Opacity*, for samples constructed using nearest-neighbour and kernel propensity-score matching. All columns include the controls specified in Equation (2), as well as cohort-bank and cohort-time fixed effects. See Table A.2 in the Appendix for variable definitions. Standard errors clustered at the state of incorporation level are in parentheses. * Denotes significance at the 10% level, ** denotes significance at the 5% level and *** denotes significance at the 1% level.

	Earnings Opacity	
	Nearest-Neighbour Matching (1)	Kernel Matching (2)
CS	0.0003*** (0.0001)	0.0002*** (0.0001)
All Controls	Yes	Yes
Cohort-bank Fixed Effects	Yes	Yes
Cohort-time Fixed Effects	Yes	Yes
Observations	574	3,593
Adj. R-squared	0.621	0.722

Table 6: Robustness Checks: The Effect of Constituency Statutes on Earnings Opacity

This table presents robustness tests of the estimated effect of *Constituency Statutes* on *Earnings Opacity*. Each row reports the CS coefficient from a separate stacked difference-in-differences regression. All columns include the controls specified in Equation (2), as well as cohort-bank and cohort-time fixed effects. Panel A uses alternative samples. Panel B reports estimates based on alternative measures of earnings opacity. Panel C presents estimates using alternative fixed effects, cohort composition, and weighting schemes. Panel D varies the event window used in the stacked difference-in-differences analysis. See Table A.2 in the Appendix for variable definitions and construction. Standard errors clustered at the state of incorporation level are in parentheses.* Denotes significance at the 10% level, ** denotes significance at the 5% level and *** denotes significance at the 1% level.

Robustness specification	CS coefficient	Standard error
<i>Panel A: Alternative Samples</i>		
(1) Excluding Delaware banks	0.0003***	(0.0001)
(2) Excluding lobbying states	0.0002**	(0.0001)
(3) Excluding states where CS laws apply only in takeover contexts	0.0002**	(0.0001)
<i>Panel B: Alternative Measures of Earnings Opacity</i>		
(4) Earnings Opacity—No ALL	0.0003**	(0.0001)
(5) Earnings Opacity—CHO	0.0002**	(0.0001)
(6) Earnings Opacity—ALL and CHO	0.0002**	(0.0001)
(7) Earnings Opacity—Loan Composition	0.0002**	(0.0001)
(8) Earnings Opacity— Earnings and Capital Controls	0.0003***	(0.0001)
<i>Panel C: Alternative fixed effects, cohort composition, and weighting</i>		
(9) Region $\times$ cohort $\times$ quarter fixed effects	0.0002**	(0.0001)
(10) Including single-bank cohorts	0.0002**	(0.0001)
(11) Unweighted	0.0003**	(0.0001)
<i>Panel D: Alternative Event Windows</i>		
(12) $[-4, +4]$ quarters	0.0002**	(0.0001)
(13) $[-8, +8]$ quarters	0.0003***	(0.0001)
(14) $[-10, +10]$ quarters	0.0003***	(0.0001)
All Controls		Yes
Cohort-bank Fixed Effects		Yes
Cohort-time Fixed Effects		Yes

Table 7: Cross-sectional tests

This table presents cross-sectional tests of the effect of *Constituency Statutes* on *Earnings Opacity*. The high and low subsamples are defined based on the pre-adoption median of each sample-splitting variable. All columns include the controls specified in Equation (2), as well as cohort-bank and cohort-time fixed effects. See Table A.2 in the Appendix for variable definitions. Standard errors clustered at the state of incorporation level are in parentheses. * Denotes significance at the 10% level, ** denotes significance at the 5% level and *** denotes significance at the 1% level.

	Monitoring costs		Institutional ownership		Analyst coverage	
	(1) High	(2) Low	(3) High	(4) Low	(5) High	(6) Low
CS	0.0003*** (0.0001)	0.0000 (0.0001)	0.0001 (0.0001)	0.0003*** (0.0001)	0.0002 (0.0001)	0.0002** (0.0001)
All Controls	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-bank fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,358	2,295	2,263	2,390	2,042	2,611
Adj. R-squared	0.662	0.626	0.645	0.615	0.693	0.594

Table 8: The Impact of Constituency Statutes Adoption on LLP Validity

This table presents estimates of the effect of *Constituency Statutes* on *LLP Validity*. All columns include the controls specified in Equation (2), as well as cohort-bank and cohort-time fixed effects. See Table A.2 in the Appendix for variable definitions. Standard errors clustered at the state of incorporation level are in parentheses. * Denotes significance at the 10% level, ** denotes significance at the 5% level and *** denotes significance at the 1% level.

	LLP Validity	
	(1)	(2)
CS	-0.1751** (0.0722)	-0.1379** (0.0586)
All Controls	Yes	Yes
Cohort-bank Fixed Effects	Yes	Yes
Cohort-time Fixed Effects	Yes	Yes
Observations	4,624	4,210
Adj. R-squared	0.177	0.384

Appendix

Table A.1: Adoption Dates of Constituency Statutes

This table reports the quarter in which each U.S. state adopted a constituency statute. The final column indicates whether a state forms a cohort in the stacked sample. States adopting before 1987q3 are excluded because the earnings opacity measure is only available from 1987q2, so that no pre-treatment observation is available for earlier adoptions. Cohorts containing a single treated bank are also excluded. The remaining states are excluded because no sample bank is incorporated in them.

State	Adoption quarter	In sample
Ohio	1984q4	
Illinois	1985q3	
Maine	1985q3	
Indiana	1986q2	
Missouri	1986q2	
Minnesota	1987q2	
New Mexico	1987q2	
Wisconsin	1987q2	
Arizona	1987q3	
New York	1987q3	Yes
Idaho	1988q1	
Tennessee	1988q1	
Virginia	1988q1	
Connecticut	1988q2	
Kentucky	1988q3	Yes
Louisiana	1988q3	
New Jersey	1989q1	Yes
Oregon	1989q1	
Florida	1989q2	Yes
Hawaii	1989q2	Yes
Georgia	1989q3	Yes
Massachusetts	1989q3	Yes
Iowa	1989q4	
Wyoming	1990q1	
Pennsylvania	1990q2	Yes
Mississippi	1990q3	Yes
Rhode Island	1990q3	Yes
South Dakota	1990q3	
Nevada	1991q4	
North Dakota	1993q3	
North Carolina	1993q4	Yes
Vermont	1998q2	
Maryland	1999q2	Yes
Texas	2006q1	Yes
Nebraska	2007q1	

Table A.2: Variable Definitions

Variable	Definition
<i>Panel A: Dependent Variables</i>	
Earnings Opacity	Discretionary loan loss provisions, estimated as the absolute value of residuals from the loan loss provision model specified in Equation 1, following Beatty and Liao (2014).
Earnings Opacity—No ALL	The absolute value of the residuals from Equation 1, re-estimated without the allowance for loan and lease losses, following Beatty and Liao (2014).
Earnings Opacity—CHO	Absolute value of the residuals from Equation 1, re-estimated by replacing the allowance for loan and lease losses with net charge-offs scaled by lagged total loans, following Beatty and Liao (2014).
Earnings Opacity—ALL and CHO	Absolute value of the residuals from Equation 1, estimated by including both the allowance for loan and lease losses and net charge-offs scaled by lagged total loans, following Beatty and Liao (2014).
Earnings Opacity—Loan Composition	Absolute value of the residuals from an adapted specification of Equation 1 that controls for loan portfolio composition using real estate, commercial and industrial, and consumer loans as shares of total loans, in the spirit of Kanagaretnam et al. (2010).
Earnings Opacity—Earnings and Capital Controls	Absolute value of the residuals from an adapted specification of Equation 1 that excludes the allowance for loan and lease losses and controls for earnings before loan loss provisions and bank capital, following Nicoletti (2018).
LLP Validity	Negative absolute difference between the ratio of net charge-offs realised in quarter $t + 1$ to loan loss provisions recorded in quarter t and one.
LLP Validity (Four-Quarter)	Negative absolute difference between the ratio of total net charge-offs realised from quarters $t + 1$ to $t + 4$ to total loan loss provisions recorded from quarters $t - 3$ to t and one.
<i>Panel B: Policy Indicators</i>	
CS	An indicator variable equal to one if the bank's state of incorporation has adopted constituency statutes by quarter t , and zero otherwise.
UD	An indicator variable equal to one if the bank's state of incorporation has adopted Universal Demand Laws by quarter t , and zero otherwise.
ANTI	An indicator variable equal to one if the bank's state of incorporation has adopted other antitakeover laws by quarter t , and zero otherwise.
INTRA	An indicator variable equal to one if the bank's state of headquarters has implemented intrastate banking deregulation by year t , and zero otherwise.
INTER	An indicator variable equal to one if the bank's state of headquarters has implemented interstate banking deregulation by year t , and zero otherwise.

Table A.2: Variable Definitions (continued)

Variable	Definition
<i>Panel C: Control Variables</i>	
SIZE	Natural logarithm of total assets.
CAP	The ratio of book value of equity to by total assets.
LLP	The ratio of loan loss provisions to lagged total loans.
LOSS	Indicator variable equal to one when the bank reports negative net income during the quarter, and zero otherwise.
EBP	The ratio of earnings before loan loss provisions and taxes to lagged total loans
dGsP	Quarterly percentage change in state-level gross domestic product.
dUNEMP	Quarterly change in the state unemployment rate.
HPRET	Quarterly return on the state house price index.
<i>Panel D: Variables Used in Cross-Sectional Tests</i>	
MON COST	The ratio of loans not secured by real estate to total assets.
OWN	The percentage of shares held by institutional investors.
COV	Residuals from a regression of the natural logarithm of one plus the number of earnings-per-share estimates on bank size, including bank and quarter fixed effects.
<i>Panel E: Variables Used to Construct the Dependent Variables</i>	
NPA	The ratio of non-performing loans to lagged total loans.
dNPA	Quarterly change in non-performing loans, scaled by lagged total loans.
LOAN	The ratio of total loans to total assets.
dLOAN	Quarterly change in total loans, scaled by lagged total loans.
ALL	The ratio of loan loss allowance to lagged total loans.
CHO	The ratio of net charge-offs to lagged total loans.
Real Estate Loans	The ratio of loans secured by real estate to total loans.
Commercial Loans	The ratio of commercial and industrial loans to total loans.
Consumer Loans	The ratio of consumer loans to total loans.