Danisewicz, P., McGowan, D., Onali, E., & Schaeck, K. (Accepted/In press). Debtholder Monitoring Incentives and Bank Earnings Opacity. *Journal of Financial and Quantitative Analysis*.

Debtholder Monitoring Incentives and Bank Earnings Opacity

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Abstract

We exploit exogenous legislative changes that alter the priority structure of different classes of debt to study how debtholder monitoring incentives affect bank earnings opacity. We present novel evidence that exposing nondepositors to greater losses in bankruptcy reduces bank earnings opacity, especially for banks with larger shares of nondeposit funding, listed banks, and independent banks. The reduction in earnings opacity is driven by a lower propensity to overstate earnings and becomes larger during crises, when the incentive to conceal capital shortfalls is stronger. Our findings highlight the importance of creditors' monitoring incentives in improving the quality of information disclosure.

Keywords: debtholder monitoring incentives; bank earnings opacity; earnings management; debt structure

JEL Codes: G21, G28

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Acknowledgements

We greatly appreciate comments and suggestions by an anonymous reviewer, Stefano Colonello, Reint Gropp, and Michael Koetter. We also thank conference and seminar participants at the Royal Economic Society Meeting, at the University of Southampton, at the University of Manchester, and at the IWH Halle.

I. Introduction

The opacity of bank balance sheets impedes market discipline by limiting outsiders' ability to accurately assess banks' value and risk. Information asymmetries arising from opacity can undermine banks' ability to raise capital, dry up interbank markets, and fuel contagion, thus increasing systemic risk. Policymakers therefore aim to mitigate bank opacity by requiring increased disclosure and restricting asset composition.

In this paper, we test whether changes in the priority structure of debt claims that alter different creditors' monitoring incentives affect bank opacity. We focus on earnings opacity because financial reporting systems are important tools to reveal asymmetric information to outsiders (Huizinga and Laeven (2012)) and meet outsiders' expectations to avoid interventions (Acharya and Lambrecht (2015)).[1]

We exploit the staggered introduction of depositor preference laws in 15 U.S. states to estimate the causal effect of debtholder monitoring incentives on bank earnings opacity. Our setting is useful from an econometric perspective. These laws are plausibly exogenous to earnings opacity as their primary objective is to safeguard deposits, and they also only affect state-chartered banks (the treatment group) but not nationally-chartered banks in the same state (the control group). These laws do not affect insured depositors' position in the claim structure, but they make uninsured deposits senior to nondeposits.[2] Nondepositors, who are typically better monitors than depositors (King (2004)), therefore face greater losses in the event of bankruptcy, and are incentivised to exert stronger monitoring to reduce the likelihood of losing their claim.

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[1] Chemmanur et al. (2009) argue that conveying information to outsiders helps reduce information asymmetries, and Bushman and Williams (2012) emphasize that outsiders' ability to exert discipline depends on accounting information, which is crucial for monitoring. Accounting rules that affect availability, timeliness, consistency, and reliability of information about performance and risk matter for the volatility and cyclicality of earnings. Bushman (2016) also stresses that discretionary provisioning affects accounting numbers as an input into regulatory calculations, thus limiting outside monitoring.

[2] Nondepositors are suppliers of Fed funds; providers of other borrowed money; unsecured lenders; holders of debentures; beneficiaries of guarantees; holders of bankers' acceptances; and holders of subordinated debt claims. Nondeposit claims also include general creditor claims, e.g., trade creditors, landlords, and suppliers.

Two studies provide a theoretical underpinning for our research. First, Birchler (2000) predicts that depositor preference leads to more efficient monitoring by assigning greater monitoring incentives to nondepositors. Second, Cordella et al. (2018) posit that bailout guarantees decrease the sensitivity of debt pricing to risk. Since depositor preference laws are akin to a guarantee for deposits, they decrease the risk sensitivity of uninsured depositors, but increase nondepositors' losses in a liquidation, leading to higher price sensitivity to bank risk. For this reason, nondepositors are likely to demand greater transparency. To the extent that the increase in nondepositors' monitoring is greater than the decrease in uninsured depositors' monitoring, depositor preference laws should result in reduced earnings opacity.

Our key result is that incentivizing nondepositor monitoring significantly reduces earnings opacity. A detailed analysis highlights that the reductions in earnings opacity are driven by reductions in earnings overstatements. The economic magnitude is equivalent to an 8.4 percent reduction of understatements of loan loss provisions.[3]

This research matters for two reasons. First, bank regulators are concerned about opacity. Laeven and Majnoni (2003) document that opacity exacerbates the procyclicality of lending, and Huizinga and Laeven (2012) report that banks use discretionary accounting to overstate the value of distressed assets and regulatory capital during crises to conceal problems. Second, our work is timely and policy-relevant because bailouts, blanket guarantees, and other forms of support during the recent crisis weakened the monitoring incentives of bank creditors (Berger and Turk-Ariss (2015)). Our findings highlight the beneficial effects of the introduction of depositor

[3] This figure is based on Column (2) in Panel B of Table 4 and is computed as $\exp^{(-0.8741)}-1 = -0.084$.

priority laws in the European Union that call for more monitoring by sophisticated creditors and require junior debtholders to contribute to bank resolutions, resulting in greater transparency.[4]

We use difference-in-difference estimation to compare earnings opacity within state-chartered banks to that of nationally-chartered banks in the same state-quarter, thereby controlling for local economic conditions. To bias our estimates, variation in omitted variables must coincide with the law changes and differentially affect nationally-chartered and state-chartered banks. Our data contain 15 separate enactments of depositor preference laws which reduces the likelihood that such variables confound the results. We also conduct tests to mitigate concerns that banking crises in New England and Texas, the Savings and Loan (S&L) crisis, regulatory responses to these crises (i.e., the Financial Institutions Reform, Recovery, and Enforcement Act (FIRREA) and the Federal Deposit Insurance Corporation Improvement Act (FDICIA)), or the deregulation of banking markets and the subsequent wave of mergers and acquisitions affect our findings. We also show that using an alternative control group based on a matching strategy leaves our inferences intact.

We employ measures of earnings opacity that are widely used in the finance, accounting, and banking literature (Yu (2008); Hutton et al. (2009); Cornett et al. (2009); Beatty and Liao (2014); Jiang et al. (2016); Chen et al. (2018)). In our main tests, these measures are the natural logarithm of the absolute value of residuals from regressions of loan loss provisions on drivers of changes in these loan loss provisions. Larger absolute values of these residuals indicate greater earnings opacity. In additional tests, we use signed measures to capture whether discretionary provisioning is used to increase or decrease earnings, because managers have incentives to overstate earnings (Huizinga and Laeven (2012); Norden and Stoian (2014); Jiang et al. (2016)).

[4] The view about the adverse consequences of opacity contrasts with Chen and Hasan (2006) and Gorton (2013) who argue that greater transparency can trigger bank runs. Similarly, Dang et al. (2017) stress that opacity minimizes information leakages.

Further tests use measures that are based on the idea that outsiders find it difficult to value the real estate owned by a bank, which is considered to reflect opacity (Flannery et al. (2013). We also calculate the sum of opaque assets consisting of bank premises, fixed assets, investments in unconsolidated subsidiaries, intangible assets, and the balance sheet category "other assets". Flannery et al. (2013) stress that investors find it difficult to value such other assets.

Beyond contributing to the literature on debtholder monitoring, this study is also related to three other strands of literature. Our work is related to research on how legislation affects transparency in financial markets and banking. Benos et al. (forthcoming) show that introducing a centralized trading requirement under the Dodd-Frank Act increased competition and reduced opacity in the market for interest rate swaps, and Jiang et al. (2016) examine how deregulating state banking markets affects bank opacity. They find that greater competition increases the quality of banks' information disclosure which enhances markets' ability to exert discipline. In contrast to these studies, we focus on how changes in monitoring incentives that apply to one important group of creditors and differentially affect state-chartered and nationally-chartered banks influence bank opacity. In addition, our use of state-quarter-fixed effects also allows disentangling the effects of banking deregulation from the effects of depositor preference law enactment on bank opacity.

Second, we contribute to the literature on banks' earnings management. Wahlen (1994) shows that banks exercise discretion to reduce regulatory costs and increase loan loss provisions when cash flows increase. Cohen et al. (2014) report that earnings management exacerbates tail risk during crises, and Bushman and Williams (2015) show that more opaque banks have higher financing costs, engage in more risk shifting, and suffer more from illiquidity during recessions. Our work contributes to this literature by highlighting that legislation can mitigate bank opacity.

Third, we advance the literature on debt priority laws in banking. Hirschhorn and Zervos (1990) and Thomson (1994) propose that nondepositors collateralize claims and shorten the maturity of their claims in response to depositor preference laws to protect themselves. Osterberg (1996) and Osterberg and Thomson (1999; 2003) model the role of depositor preference for the cost of failure, failure rates, resolution types, and the cost of debt. Pages and Santos (2003) focus on the interaction of monitoring incentives of debtholders and regulators when depositors have a priority claim. They argue that regulatory monitoring depends on debtholder monitoring. Our work differs from these studies by showing that reallocating monitoring incentives to junior debtholders triggers reductions in earnings opacity.

Recent work by Danisewicz et al. (2018a) shows that priority for deposits increases nondeposit costs and reduces bank risk.[5] In contrast to their work, we document a direct response of banks' accounting choices to changes in nondepositors' monitoring incentives. This is illustrated by tests that show that banks limit discretionary provisioning more if they are more reliant on nondeposit funding. We also find that banks that use more Fed funds reduce earnings opacity more than those that are less reliant on this type of funding, consistent with Fed funds providers' superior monitoring abilities. Our results therefore highlight a mechanism by which greater nondepositor monitoring incentives result in more effective influencing of bank behavior.

Unlike Danisewicz et al. (2018a), we also examine responses by nondepositors to the subordination of their claims that may undermine monitoring incentives such as nondepositors' propensity to collateralize claims and reduce their maturity. Such actions, albeit present in the data, do not interfere with the disciplining effect for earnings opacity. To avoid misattributing the reductions in opacity to nondepositor monitoring rather than regulatory monitoring, we also

[5] The higher risk of losing the claim triggers greater nondepositor monitoring. This results in higher monitoring costs that nondepositors pass on to the banks by demanding higher interest rates.

control for regulatory monitoring. Moreover, we show that depositor preference laws affect bank behavior heterogeneously in the cross section and over time. Large banks, banks with greater exposure to real estate assets, and unsound banks respond more strongly to depositor preference laws. Further tests highlight that the effects of depositor preference are more pronounced in listed banks than in unlisted banks, and they are also stronger for independent banks than for banks that are members of a bank holding company. Finally, we find that the adjustments in earnings opacity are greater during crisis episodes.

We proceed as follows. Section II reports on the history of depositor preference laws and Section III develops our hypothesis. Section IV describes the identification strategy. Section V presents the main results. Section VI explores the factors that mitigate and amplify the responsiveness of bank opacity to the reallocation of monitoring incentives. Section VII shows sensitivity tests, and Section VIII concludes.

II. Institutional details

Figure 1 shows that thirty states enacted depositor preference between 1909 and 1993. These laws leave the debt priority order within nationally-chartered banks unaffected but change it for state-chartered banks. Table 1 shows that in the absence of these laws, the claim structure follows that specified in the Banking Act of 1935 assigning equal rank to the claims of uninsured depositors and nondepositors. With depositor preference, claims of insured and uninsured depositors are equal, elevating uninsured depositors' claims above those of nondepositors, assigning nondepositors a junior claim in the event of bankruptcy.

[FIGURE 1] [TABLE 1]

Figure 1 highlights that implementation of depositor preference law occurred at different times across states. There are two reasons behind the introduction of the law.[6] First, the laws fix omissions in prior legislation. Kansas introduced depositor preference law because there were no state guidelines for the liquidation of claims. Likewise, Texas and California amended their laws claiming that state law had not contained priority rules for payments to creditors during bank liquidation. A key benefit of priority rules, formalized in a model by Hardy (2013), is to reduce bankruptcy costs because priority rules can avoid costly litigation by creditors to establish the size and priority of their claim via courts.[7]

Second, protecting depositors during liquidation provoked the introduction of depositor preference laws. Missouri banking regulations contain multiple references to policy efforts to protect depositors, one of them being the priority claim for depositors in a liquidation. Similarly, the records of the state bank commissioner in Kansas stress safeguarding deposits as a primary objective. In Rhode Island, priority was assigned to depositors to protect their claims in a failure. The legislative councils' digests stress that the changes allow swifter reorganization and dissolution of state-chartered banks, as in California. However, in several states no reason is given for the amendments in the priority structure. The keyword search of news sources does not suggest much interest by the media in state depositor preference, consistent with the view that

[6] Danisewicz et al. (2018a) show that economic and political considerations do not coincide with the introduction of depositor preference. Banks in Florida, Hawaii, and Minnesota performed well during the adoption of the laws, but banking difficulties arose in Texas, California, Maine, New Hampshire, Connecticut, and Rhode Island. They perform keyword searches in media sources and review assembly laws from the state legislative councils' archives, digests, and the concurrencies of the state amendments to understand the reasons for the adoption of state depositor preference laws. We improve on this search strategy and use additional keyword searches in Business Source Complete, described in Section A.1 of the Online Appendix. We also randomly sample failed state-chartered banks in states with depositor preference law by performing additional media searches. Where possible, we also cross checked this information with the "FDIC's History of the 80s – Lessons for the Future", a resource describing the banking problems in the 1980s. See https://www.fdic.gov/bank/historical/history/.

[7] Creditors often seek settlement of claims in a bank liquidation via courts (e.g. Harvard Law Review (1991), Hardy (2013)).

these laws fix omissions in prior legislation.[8] There is also no evidence that the laws were implemented due to concerns about bank opacity or earnings management.

The differential treatment of depositors was abandoned in the Omnibus Budget Reconciliation Act on August 10, 1993. This law introduced national depositor preference and assigned priority to deposit claims on state-chartered banks in states that did not previously adopt depositor preference. Depositor claims on nationally-chartered banks also gained priority. National depositor preference law was introduced to save money in the federal budget (Thomson (1994), Marino and Bennett (1999)).

III. Hypothesis development

The claims of uninsured depositors and nondepositors receive their pro rata share of the net value of assets that are liquidated because they have equal priority in the absence of depositor preference. Uninsured depositors and nondepositors may consequently incur losses (Marino and Bennett (1999)). Elevating uninsured depositor claims above those of nondepositors via depositor preference asymmetrically affects these claimants' monitoring incentives. With depositor preference, nondepositors face a higher probability of losses in case of liquidation. Therefore, they have greater incentives to monitor and are likely to demand greater transparency. In contrast, the monitoring incentives of uninsured depositors are reduced (Birchler (2000)).

In theory, reallocating monitoring incentives from uninsured depositors to nondepositors via depositor preference could result in either stronger or weaker monitoring in the aggregate, with corresponding effects for banks' earnings management and information disclosure. It is also plausible that the effects of more monitoring by nondepositors and less monitoring by uninsured depositors cancel out, so that depositor preference laws have no effect on bank opacity.

[8] Although news items that discuss resolutions of state-chartered banks in Kansas (e.g., Cedar Vale State Bank) and Texas (e.g., First City Bancorporation of Texas) mention that the FDIC assigned priority claims to depositors, neither these news items nor the FDIC's History of the 80s contain information about the motivation for state depositor preference laws.

Banks' liability structure sheds some light on these issues. Uninsured deposits and nondeposits account for 8 percent and 5 percent of total liabilities, respectively. That is, the monitoring incentives of relatively inefficient monitors (uninsured depositors) are reallocated to a somewhat smaller proportion of efficient monitors (nondepositors) via the laws. Moreover, depositors are typically individuals who cannot influence bank behavior nor are concerned about opacity whereas nondepositors comprise institutional investors such as other banks that can influence bank conduct. These characteristics of the data are suggestive of an increase in overall monitoring.

We therefore hypothesize that the greater nondepositor monitoring incentives motivate banks to limit discretionary provisioning to reduce opacity. This improves safety and soundness by reducing information asymmetries (Acharya and Ryan (2016)).

The more opaque a bank, the more effort nondepositors expend collecting information about its condition. This monitoring activity imposes private costs on nondepositors. The extent of these costs is a function of depositor preference law. For a given level of opacity, nondepositors conduct more monitoring and therefore incur higher private costs following subordination of their claims. To avoid that nondepositors pass on their greater monitoring costs through higher cost of capital, banks may respond to the shift in monitoring incentives toward nondepositors by releasing more information (Easley and O'Hara (2004); Yu (2008)). This idea is also reflected in related findings by Irani and Oesch (2013, 2016). They show that outside monitoring decreases accrual manipulation and increases financial reporting quality. Therefore, depositor preference laws are likely to induce banks to reduce earnings opacity because nondepositors' monitoring imposes costs on banks that can be alleviated by lowering opacity.

This argument is also reinforced by Cordella et al. (2018). They posit that the pricing of debt instruments relies on costly information disclosure, and highlight that banks have greater incentives to disseminate information when debtholders are not protected by government guarantees which decrease the sensitivity of debt pricing to risk. Debtholders that are likely to lose their claim, either because their claim is junior in nature (as is the case for uncollateralized debt) or during a bail in, are likely to penalize opaque banks by requiring a greater return. Provided that the effect of depositor preference is larger for nondepositors' monitoring incentives than for uninsured depositors', greater monitoring incentives will motivate nondepositors to demand more transparency. This should reduce earnings opacity, reflected in less discretionary accounting choices for state-chartered banks.[9]

We state our hypothesis as follows.

Hypothesis: *The increase in debtholder monitoring incentives from depositor preference laws decreases earnings opacity for state-chartered banks, relative to nationally-chartered banks.*

IV. Data description, variable definitions, and identification strategy

We construct a quarterly bank level data set for commercial and savings banks obtained from the Quarterly Report on Condition and Income (Call Report) from the Federal Reserve Bank of Chicago. Our sample covers the period 1983Q1 to 1993Q2 and includes banks from Arizona, California, Colorado, Connecticut, Florida, Hawaii, Kansas, Louisiana, Maine, Minnesota, Missouri, New Hampshire, North Dakota, Rhode Island, and Texas, the fifteen states that enacted depositor preference during this time.[10,11]

[9] This prediction is reinforced by the fact that nondepositor claims include Fed funds, which are crucial to most banks' funding mix. It is vital for banks to maintain access to these funds and allow debtholder monitoring. Shleifer and Vishny (1997) show that large debtholders strongly influence borrower behavior since they lend at short maturities, requiring frequent renegotiation. Consistent with this, Holod and Peek (2007) find that opacity limits banks' ability to raise funds.

[10] On March 31, 1983, all insured commercial banks started reporting Call reports on a quarterly basis.

We choose this timeframe because banks were not required to submit quarterly Call Reports before 1983 and because all banks, irrespective of their charter, were subject to national depositor preference from 1993Q3 as a result of the Omnibus Budget Reconciliation Act. Following Osterberg (1996), we exclude banks in New York State, due to their size and regulatory environment. Only institutions that operate at least four quarters before and after the introduction of state depositor preference are included, ensuring that a sufficient number of observations is available for each bank.[12]

These selection criteria yield a final sample of 205,057 bank quarter observations for 5,524 banks. These banks account for 67 percent of total banking assets in the U.S. The state-chartered banks hold 39 percent of total bank assets in these states.

Call Reports provide information on banks' charters (state or national), location, size, equity capital to total assets, total loans, loan portfolio composition, loan loss provisions, return on assets, liability structure, and the regulatory agency. We approximate collateralization with the sum of the ratio of pledged securities, Federal fund repos, standby letters of credit, and secured pledged deposits to total nondeposit liabilities. We capture nondeposit maturity structure with the ratio of nondeposits with a maturity of one year or less to total nondeposits. We combine this information with the Case Shiller Index and data on the state level per capita income and unemployment rates from the Federal Reserve Bank of St Louis.

Finally, we obtain information about regulatory monitoring and bank soundness. We collect annual information about the distance in miles between the bank's headquarters and the nearest field office of the corresponding regulator (Office of the Comptroller of the Currency, the

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[11] Utah introduced depositor preference on January 1, 1983 while Virginia introduced depositor preference on July 1, 1983. Before 1983 banks reported Call Report information on a less frequent basis. We require four quarters of pre-treatment data to test the parallel trends assumption. As no pre-treatment data is available for Utah, and only two quarters of pre-treatment data are available for Virginia, we exclude observations of banks from both states from the sample. In unreported tests that include observations from both states, our inferences are robust.

[12] Including New York banks or banks that do not satisfy the four-quarter sample screen leaves our results unaffected.

Federal Reserve, FDIC, state regulator) to allow for time variation in this variable. Distance is a good proxy for regulatory monitoring because on site examinations enable regulators to verify the accuracy of the information submitted by banks (Berger et al. (2016)) and budget constraints lead regulators to monitor more intensively firms that are geographically closer to regulatory field offices (Kedia and Rajgopal (2011)).

A. Measuring bank opacity

Our main tests use opacity measures that reflect discretion in loan loss provisioning. Loan loss provisions are the most important bank accrual and are informative about estimated losses on opaque assets. To do so, we regress loan loss provisions on a set of independent variables. The residuals are interpreted as an indicator of the abnormal accrual of loan loss provisions, one of the most common ways banks manipulate earnings and regulatory capital. Higher values are interpreted as evidence of greater earnings management and greater earnings opacity.

Following prior work (Wahlen (1994); Yu (2008); Cornett et al. (2009); Hutton et al. (2009); Jiang et al. (2016)), our tests rely on the absolute value of the residuals; that is, we use an unsigned measure of earnings opacity. Absolute values of the residuals reflect both negative and positive residuals, allowing us to capture income-increasing and income-decreasing earnings management (Jiang et al. (2016)). This avoids that such actions offset each other and lead to low power in our tests. Since the residuals are not normally distributed, we perform a logarithmic transformation. We also use signed earnings opacity measures to investigate whether banks use discretionary provisioning to increase or decrease earnings (Norden and Stoian (2014)).

Beatty and Liao (2014) review and identify different models to measure discretionary provisions. We follow their four main specifications and estimate

$$(1)\quad LLP_{bst} = \alpha_0 + \alpha_1 \Delta NPA_{bst+1} + \alpha_2 \Delta NPA_{bst} + \alpha_3 \Delta NPA_{bst-1} + \alpha_4 \Delta NPA_{bst-2} + \alpha_5 SIZE_{bst-1} + \alpha_6 \Delta Loan_{bst} + \alpha_7 \Delta GSP_{st} + \alpha_8 CSRET_{st} + \alpha_9 \Delta UNEMP_{st} + \varepsilon_{bst},$$

where LLP_{bst} is the ratio of loan loss provisions to lagged total loans in bank *b* in state *s* in quarter *t*. ΔNPA_{bst} is the change in nonperforming assets divided by lagged total loans to capture changes in risk taking and risk culture. As in Jiang et al. (2016), we include lags and leads of ΔNPA_{bst} to capture historical changes in *NPA* and current and forward looking information on *NPA*, when banks choose the current level of LLP_{bst}; this approach also controls for changes in accounting preferences over time. $SIZE_{bst-1}$ is the natural logarithm of total assets in the previous quarter. It reflects differences in the level of monitoring by regulators and private sector stakeholders of banks of different size. This variable is not normally distributed, and so we take the log. $\Delta LOAN_{bst}$ is the change in total loans to lagged total loans. ΔGSP_{st} (the change in state Gross State Product), $CSRET_{st}$ (the state Case-Shiller Index), and $\Delta UNEMP_{st}$ (the change in the state unemployment rate) capture the effects of time-varying macroeconomic state-specific conditions that affect LLP_{bst}.

We estimate equation (1) using OLS. We then calculate ε_{bst}, the abnormal component of loan loss provisions, which proxies for discretionary changes in loan loss provisions and indicates earnings opacity. We take the natural logarithm of the absolute value of the residuals, denoted as EO^1_{bst}.

We also estimate the remaining three models of Beatty and Liao (2014) to construct EO^2_{bst}, EO^3_{bst}, and EO^4_{bst} based on the following three equations, respectively.

(2) $$LLP_{bst} = \alpha_0 + \alpha_1 \Delta NPA_{bst+1} + \alpha_2 \Delta NPA_{bst} + \alpha_3 \Delta NPA_{bst-1} + \alpha_4 \Delta NPA_{bst-2}$$
$$+\alpha_5 SIZE_{bst-1} + \alpha_6 \Delta Loan_{bst} + \alpha_7 \Delta GSP_{st} + \alpha_8 CSRET_t$$
$$+\alpha_9 \Delta UNEMP_{st} + \alpha_{10} ALW_{bst-1} + \varepsilon_{bst},$$

where ALW_{bst-1} denotes the ratio of loan loss allowances to total loans. The next regression includes the additional control variable CO_{bst}, net charge offs divided by lagged total loans. We estimate

(3) $$LLP_{bst} = \alpha_0 + \alpha_1 \Delta NPA_{bst+1} + \alpha_2 \Delta NPA_{bst} + \alpha_3 \Delta NPA_{bst-1} + \alpha_4 \Delta NPA_{bst-2}$$
$$+\alpha_5 SIZE_{bst-1} + \alpha_6 \Delta Loan_{bst} + \alpha_7 \Delta GSP_{st} + \alpha_8 CSRET_t$$
$$+\alpha_9 \Delta UNEMP_{st} + \alpha_{10} CO_{bst} + \varepsilon_{bst}.$$

Our fourth model simultaneously controls for both loan loss allowances and net charge offs. We estimate

(4) $$LLP_{bst} = \alpha_0 + \alpha_1 \Delta NPA_{bst+1} + \alpha_2 \Delta NPA_{bst} + \alpha_3 \Delta NPA_{bst-1} + \alpha_4 \Delta NPA_{bst-2}$$
$$+\alpha_5 SIZE_{bst-1} + \alpha_6 \Delta Loan_{bst} + \alpha_7 \Delta GSP_{st} + \alpha_8 CSRET_t$$
$$+\alpha_9 \Delta UNEMP_{st} + \alpha_{10} ALW_{bst-1} + \alpha_{11} CO_{bst} + \varepsilon_{bst}.$$

We include past allowances because current provisions may be lower if banks have recognized high provisions in the past.[13]

Using equations (1)–(4) we also calculate signed earnings opacity variables, i.e., we use the values of the residuals but do not take their absolute values. We denote the positive signed measures as EO_{bst}^{1+}, EO_{bst}^{2+}, EO_{bst}^{3+}, and EO_{bst}^{4+}, and the negative signed variables as EO_{bst}^{1-}, EO_{bst}^{2-}, EO_{bst}^{3-}, and EO_{bst}^{4-}.

[13] Previous evidence reported by Beatty and Liao (2014) shows that these measures of opacity correlate intuitively with SEC earnings restatements, suggesting that these measures based on discretionary loan loss provisioning capture opacity. In unreported tests, we correlate the opacity measures we calculate in this research with SEC earnings restatements and also with bid-ask spreads as used in Flannery et al. (2004, 2013) during our sample period, and find positively significant correlations. This indicates the earnings-based measures are valid measures of opacity.

To rule out measurement problems, we use one alternative measure for earnings opacity based on Bushman and Williams (2012), which also considers bank profit as an explanatory variable. We denote this variable EO^{BW}

$$(5) \quad LLP_{bst} = \alpha_0 + \alpha_1 \Delta NPA_{bst+1} + \alpha_2 \Delta NPA_{bst} + \alpha_3 \Delta NPA_{bst-1} + \alpha_4 \Delta NPA_{bst-2} + \alpha_5 SIZE_{bst-1} + \alpha_6 \Delta Loan_{bst} + \alpha_7 \Delta GSP_{st} + \alpha_8 CSRET_{st} + \alpha_9 \Delta UNEMP_{st} + \alpha_{10} Profit_{bst-1} + \varepsilon_{bst},$$

where $Profit_{bst-1}$ denotes the ratio of net earnings before loan loss provisions to lagged total loans.

Moreover, we rely on two other measures of opacity. We calculate the ratio of other real estate owned relative to total loans (OREO). The idea behind this measure is that it is difficult for outsiders to value real estate owned by a bank. The higher this ratio, the more opaque is the bank (Flannery et al. (2013)). Further, we follow Flannery et al. (2013) and calculate opaque assets (OPAQUE) which is the sum of the book value of bank premises and fixed assets, investments in unconsolidated subsidiaries, intangible assets (such as mortgage service rights and core deposit intangibles) and the category "other assets" (such as accounts receivable, repossessed autos, boats and other collateral). The intuition is that opacity is positively related to OPAQUE because investors find it difficult to value such assets. All three alternative measures of opacity are not normally distributed, and we therefore take the log.

Table 2 defines the variables used in the analysis and presents summary statistics.

[TABLE 2]

B. Identification Strategy

To tease out the causal effects of changes in debt priority structure on opacity, we employ difference-in-differences estimation. We estimate

(6) $$y_{bst} = \gamma_b + \beta_1 TG_{bst} + \beta_2 TG_{bst} * Post_{st} + \beta_3 X_{bst} + \gamma_{st} + \eta_{bst},$$

where y_{bst} is one of the opacity measures described above for bank *b* in state *s* at quarter *t*. TG_{bst} is a dummy equal to 1 for state-chartered banks (0 for nationally-chartered banks); $Post_{st}$ is a dummy that is equal to 1 if depositor preference is in force in state *s* at time *t* (0 otherwise); X_{bst} is a vector containing the bank time-varying control variables size, capital ratio, the loss dummy, and the one quarter lagged loan loss provision. The latter variable controls for reversals of accruals over time (Kanagaretnam et al. (2010); Jiang et al. (2016)). Bank size, the capital ratio, and the lagged loan loss provision are not normally distributed and enter our estimations in logs. γ_b and γ_{st} are bank and state-quarter-fixed effects, respectively.[14] The error term is η_{bst}.[15] Standard errors are clustered at the state level.

Bank-fixed effects eliminate time-invariant bank-specific heterogeneity. The state-quarter-fixed effects eliminate all time-varying state-specific confounding factors that simultaneously affect the treatment and control groups, such as demand-side effects. The state-quarter-fixed effects ensure that the average treatment effect is estimated based on comparisons between the treatment and control group within the same state-quarter.

There are two assumptions that must be satisfied to draw causal inferences when using difference-in-differences estimation (Roberts and Whited (2013)). First, the treatment must be exogenous with respect to the outcome of interest. Second, the control group must be a valid counterfactual for the treatment group. We now examine the validity of these assumptions.

C. Plausible exogeneity of state depositor preference laws

[14] The post dummy is captured by state-quarter-fixed effects. It does not appear in the estimation equation.

[15] Banks' charter choice could affect our analysis. Banks may try to avoid depositor preference by switching their charter to remain opaque. This concern is unlikely to matter because only 3.3 percent of banks change charter. We model in Table B.1 of our Online Appendix the charter switch in Panel A as a function of the post dummy, bank characteristics, and the opacity measures. Neither the post dummy nor the opacity measures predict the charter choice. Panel B excludes banks that switch charters. Our inferences remain unaffected.

The absence of news items about bank opacity from the keyword searches in Section II suggests that the laws were not implemented due to concerns about opacity. To empirically support the view that opacity and other economic and institutional characteristics do not drive the adoption of state depositor preference laws, we model the adoption of the laws as a function of changes over time and across states in variables that capture such characteristics. We estimate the equation

$$(7) \quad Post_{st} = \alpha_s + \beta Opacity_{st}^{NC} + \gamma Opacity_{st}^{SC} + \delta W_{st} + \pi_t + \mu_{st},$$

where $Post_{st}$ is a dummy equal to 1 if depositor preference is in force in state *s* at time *t* and 0 otherwise. $Opacity_{st}^{NC}$ and $Opacity_{st}^{SC}$ denote the average level of earnings opacity (measured using EO^1) in quarter *t* for state-chartered or nationally-chartered banks in state *s* during quarter *t*, respectively; W_{st} is a vector of state level macroeconomic and banking characteristics that describe economic and institutional variables, such as the share of assets held by state-chartered banks in the state-quarter; a proxy for the S&L crisis (captured by the ratio of assets in failed thrifts to total bank assets), the log of assets in failed banks, the mean bank profitability in the state; and a dummy equal to 1 if a Democrat is state governor (0 otherwise) to capture political parties' willingness to regulate the financial sector.[16] α_s and π_t are state- and quarter-fixed effects, respectively; μ_{st} is the error term.

Our data spans the period 1983Q1 to 1993Q2 for the 15 states that introduced state depositor preference before the introduction of national depositor preference. Standard errors are clustered at the state level. We drop a state from the analysis once depositor preference is introduced.

[16] In Panel A of Table 3, earnings opacity is measured using EO^1, the opacity measure constructed using equation (1). The results in Panel A are unchanged using other opacity measures from Section IV.A.

The results of this test are presented in Panel A of Table 3. In Column (1) we include contemporaneous earnings opacity and the institutional control variables but find no significant relationship between these variables and the adoption of depositor preference. Next, we test whether previous values of earnings opacity affect the adoption of depositor preference law by including the level of earnings opacity in the previous eight quarters as independent variables in equation (7). The coefficients on the lags of earnings opacity reported in Column (2) of the table are all insignificant. The results are consistent with our review of news items and historical sources that show no systematic relationship between earnings opacity and adoption of the laws.

D. Parallel trends between treatment and control group

We next examine the key identifying assumption of parallel trends between state-chartered and nationally-chartered banks to verify whether nationally-chartered banks constitute a valid counterfactual.[17] Meyer (1995) points out that parallel movements between treatment and control groups would be expected in the pre-treatment period if there is no interaction between the treatment and the outcome variables.

[TABLE 3]

We follow Lemmon and Roberts (2010) and Roberts and Whited (2013) by using *t*-tests to examine differences in the growth rate of earnings opacity between state-chartered and nationally-chartered banks. We conduct these tests for the four quarters preceding imposition of depositor preference. The insignificant *t*-statistics in Panel B of Table 3 indicate that equality holds in all cells, and that the groups share parallel trends. Nationally-chartered banks represent a

[17] Differences in levels across treatment and control groups do not compromise the validity of the estimator as they are netted out by bank-fixed effects in equation (6). See Lemmon and Roberts (2010) and Roberts and Whited (2013).

valid counterfactual of how state-chartered banks' earnings opacity would have evolved in the absence of depositor preference.

V. Main results

We focus in Section V.A on the effects of the increase in monitoring incentives for nondepositors on earnings opacity. Section V.B examines alternative opacity measures, Section V.C explores further effects from the law changes, and Section V.D provides additional insights into the magnitude of the effects depending on banks' reliance on nondeposit funds.

A. Baseline results

Table 4 shows the effect of the reallocation of monitoring incentives to nondepositors on earnings opacity. Panel A presents the coefficients for the unsigned measures of earnings opacity. We report specifications with and without bank-specific control variables. Roberts and Whited (2013) highlight that, if a treatment is exogenous, then the coefficient estimates should be unaffected by the inclusion of control variables, because the treatment is unrelated to omitted variables contained in the error term.

Greater monitoring incentives for nondepositors reduce earnings opacity. The key coefficient (the interaction term between the treatment group dummy and the post dummy variable) enters all tests negatively and significantly, and it remains similar, regardless of the inclusion of control variables. The control variables suggest that the lag of loan loss provisions is positively correlated with earnings opacity, but bank size plays no role. Higher capital ratios correlate negatively with opacity, and banks that incur losses tend to be more opaque.

Panel B reports the coefficients for income-increasing (negative) and income-decreasing (positive) residuals. This analysis is important because discretionary accounting choices that

overstate earnings through (income-increasing) negative residuals bolster bank capital. Negative residuals also understate the riskiness of banks' lending.

The reallocation of monitoring incentives does not affect income-decreasing residuals. However, discretionary provisioning that overstates earnings to make banks appear more profitable declines following the change in monitoring incentives. These findings suggest that the declines in the absolute values of residuals are driven by the laws limiting income-increasing discretionary provisioning. In terms of the economic magnitude, these figures suggest that the decline in earnings overstatements is equivalent to a reduction of understatements of loan loss provisions by between 8.2 and 8.7 percent.

[TABLE 4]

B. Alternative measures of bank opacity

We now investigate three alternative opacity measures to rule out that our inferences are driven by measurement issues and a focus on discretionary accounting choices.

Column (1) in Table 5 shows that using the Bushman and Williams (2012) approach to calculate the residuals leaves our inferences unchanged. Likewise, we find in Column (2) that OREO falls significantly within the treatment group. Economically, the treatment effect represents an 8.3 percent reduction in opacity. Consistent with the previous results, we also find significant reductions in opacity when measured using OPAQUE. In Column (3), the interaction coefficient indicates a 2.1 percent decline in OPAQUE within the treatment group relative to the counterfactual.

[TABLE 5]

C. Further effects of the reallocation of monitoring incentives

The increase in monitoring incentives for nondepositors may have additional effects beyond reducing opacity.

First, it may trigger the collateralization of nondeposit claims and a shortening of the maturity structure of these claims (Hirschorn and Zervos (1990); Thomson (1994)). Second, it may alter regulators' monitoring incentives (Pages and Santos (2003)) to the extent that regulatory monitoring, rather than nondepositor monitoring, may drive the documented reduction in opacity.[18] Third, there may be complex interactions between regulators' and nondepositors' monitoring incentives, soundness, changes in nondeposit claims' maturity structure, and the collateralization of such claims (Pages and Santos (2003)). These additional effects pose a key challenge to establishing a causal effect of nondepositor monitoring incentives on opacity because both the collateralization of nondeposit claims and the reductions in maturity structure undermine nondepositor monitoring incentives (Manove et al. (2001)).

We aim to disentangle these effects and control for these interactions. Since Section V.B shows that measurement issues are not driving our inferences, all subsequent tests use opacity measures that focus on earnings opacity, i.e., the measures based on residuals.

Panel A of Table 6 shows in Column (1) that depositor preference laws significantly increase the collateralization of nondeposit claims. Column (2) illustrates that the share of nondeposit claims with a maturity of one year or less also increases significantly. In Column (3), we use the distance in miles (in logs) between the bank's headquarters and the nearest regulator field office as a measure of ex-ante regulatory monitoring as a dependent variable.[19] This test

[18] State-chartered banks that are not members of the Federal Reserve System have two supervisors. The corresponding state supervisor and one federal supervisor. State-chartered banks that are members of the Federal Reserve System have only the FDIC as a supervisor. These potential differences in regulatory intensity are captured in the bank-fixed effects.

[19] Anecdotal evidence supports the view that regulatory monitoring was not affected by the change in debtholders' monitoring incentives. A review of the FDIC's History of the 80s suggests that regulators reduced examiner resources at state and federal agencies until the mid-1980s. Examiner figures declined for the FDIC, the OCC, and for state regulators. There are also decreases in the number of examinations and increases in the interval between examinations. Evidence for Texas (which

shows that the reallocation of monitoring incentives for debtholders does not affect ex-ante regulatory monitoring.[20]

To rule out that changes in collateralization and maturity structure undermine the effect of the switch in monitoring incentives of nondepositors on earnings opacity, Panel B of Table 6 replicates our regressions from Panel A of Table 4 but controls for collateralization and nondeposit maturity structure. To reflect on the interaction of the change in monitoring incentives of nondepositors with the incentives of regulators, our tests now also include the distance in miles (in logs) between the bank's headquarters and the nearest regulator field office as measure of ex-ante regulatory monitoring.

[TABLE 6]

These tests reinforce the view that the change in monitoring incentives for nondepositors affects earnings opacity beyond its effects on collateralization, maturity structure, and regulatory monitoring. The economic magnitudes remain similar to the baseline results.

D. Further evidence for the role of nondepositors for adjustments in earnings opacity

We now investigate the shift in monitoring incentives in more detail. Our first test examines whether earnings opacity responds more strongly in banks that are likely to be monitored more intensively. If nondepositors' monitoring drives our results, banks that use more nondeposit funding, irrespective of who supplies these funds, should face greater scrutiny. Thus, decreases in earnings opacity should be greater in banks that use more nondeposits. This test

adopted depositor preference in 1985), Louisiana (1985), and California (1986) comports with this nationwide pattern. The decline in resources reverses when increasing numbers of problem banks require more examiners and greater examination frequency from 1986 onward. However, while the average number of days between examinations in Texas declined by 1993, the number of examinations remained lower than at the beginning of our sample period.

[20] We also retrieve data on enforcement actions from the regulators' websites as a measure of ex-post regulatory monitoring and use it as a dependent variable. The data are available from 1989Q1 onwards. Enforcement actions are issued if regulators uncover risky behavior and require banks to take corrective action (Danisewicz et al. (2018b)). If regulatory monitoring increases after the law changes, regulators may uncover more unsound behavior, resulting in more enforcement actions. In unreported tests, we find that adoption of depositor preference laws does not lead to significantly more regulatory monitoring as reflected in the number of enforcement actions.

examines the necessary condition that monitoring incentives play a role for the observed reductions in opacity.

We test this in Panel A of Table 7 using the measures of earnings opacity as the dependent variable. We split the sample at the pre-treatment median of the share of nondeposit funding to total liabilities to ensure that the values are not affected by depositor preference. While the significance level is reduced, the coefficients show that banks that are likely to face larger increases in monitoring take greater steps to reduce opacity. Across all measures, the magnitude of the key coefficient is greater for banks with more nondeposits. Chow tests confirm significant differences of the coefficient estimates across all subsamples.

Our second test examines monitoring ability. This is a sufficient condition for the effect on earnings opacity. We focus on banks' dependence on Fed funds purchased because Fed funds are the most important component of nondeposit funds. Moreover, since Fed funds are supplied by banks, these claimants have the best understanding of the conditions in the banking industry. Panel B of Table 7 splits the sample at the pre-treatment median of the ratio of Fed funds purchased to total liabilities.

The earnings adjustments are significantly larger in banks whose ratio of Fed funds purchased to total liabilities lies above the pre-treatment median of this ratio of all banks. This finding is insensitive to the choice of the measure of earnings opacity. The Chow tests show that the coefficients are significantly different across the subsamples. These results suggest that providers of funds with greater monitoring ability have a greater effect on bank earnings opacity.

The third set of tests focuses on nondepositor monitoring. We follow the approach in the market discipline literature that infers debtholder monitoring when debt prices display risk sensitivity (e.g., Flannery and Sorescu (1996)). Panel C of Table 7 shows that the cost of

nondeposit funds, reflected in the ratio of interest expenses on nondeposits to total liabilities, increase significantly in response to depositor preference in Column (1). The sample split at the median level of nondeposits to total liabilities in Column (2) illustrates that the increase in nondeposit funding costs is greater for those banks that use more of this type of funding, in line with our argument about nondepositor monitoring. The final set of tests show that the costs for the two main components of nondeposit funds, Fed funds purchased (Column 3), and subordinated debt funds (Column 4) increase by 19.4 and 23.3 percent, respectively.[21]

[TABLE 7]

VI. Factors that amplify and mitigate the effects of the reallocation of monitoring incentives

Banking crises, the corresponding responses by regulators, and other changes in regulation may play a role for the effect of the reallocation of debtholders' monitoring incentives on opacity. Likewise, bank-specific characteristics such as size, exposure to real estate assets, distress, public listing, and holding company membership may also correlate with opacity.

A. The role of banking turmoil in New England, Texas, and the S&L crisis

Huizinga and Laeven (2012) report that banks were more prone to become opaque during the recent crisis, and Knaup and Wagner (2012) highlight that correctly measuring credit risk, the key driver for loan loss provisions, is particularly difficult during crises.

We therefore ask whether the disciplining effects from the increase in nondepositor monitoring are larger during crises. Panel A of Table 8 splits the sample into crisis and non crisis observations. We classify the observations for banks in Connecticut, Maine, New Hampshire,

[21] Fed funds are provided by other banks that are aware of the changes in regulation and are typically considered to be sophisticated monitors. Likewise, subordinated debt funds are usually held by large institutional investors that have better monitoring technologies than small atomistic depositors.

and Rhode Island for the period 1991Q1–1993Q3 as crisis episodes to reflect the New England banking crisis. To consider the Texas banking crisis, we classify observations from Texas for the period 1986Q1–1988Q4 as a crisis. Our tests show that the economic magnitude of the reductions in earnings opacity is significantly larger during crises. Chow tests confirm this finding.

Unlike the New England and Texas crises, the S&L crisis was not limited to one region but caused failures nationwide between 1986 and 1995. Panel B of Table 8 therefore includes an additional interaction between the treatment group dummy and the S&L crisis variable to capture whether the S&L crisis drives the inferences. The effect of depositor preference law on earnings opacity remains robust. There is no evidence that the S&L crisis differentially affected opacity between state- and nationally-chartered banks.

[TABLE 8]

B. The role of deregulation, bank mergers, and regulatory responses to banking crises

Our sample period is characterized by changes in regulation. The removal of branching restrictions resulted in 38 states lifting obstacles to intrastate and interstate branching that trigger entry of new competitors and a period of mergers and acquisitions. While there is little correlation between the timing of deregulation and the introduction of state depositor preference laws, Jiang et al. (2016) show that tougher competition arising from the removal of branching restrictions reduced earnings opacity.

All our tests contain state-quarter-fixed effects that capture the effects of both types of deregulation on all banks in a given quarter. To investigate differential effects on state-chartered banks and nationally-chartered banks from the reallocation of monitoring incentives to nondepositors, Table 9 contains an interaction between the treatment group dummy and a

dummy that takes on the value of one if a state permits interstate branching (0 otherwise) in Panel A, and an interaction between the treatment group dummy and a dummy that takes the value of one if a state permits intrastate branching (0 otherwise) in Panel B. The findings for the key coefficient of interest are qualitatively similar.

We investigate the effects of consolidation in the banking industry further and collect data on bank mergers and acquisitions from the Federal Reserve Bank of Chicago.[22] We focus first on a subsample of banks involved in mergers and acquisitions in Panel C. In Panel D, we restrict the sample to banks that are not involved in mergers and acquisitions. In both cases we find that introducing state depositor preference laws significantly reduces earnings opacity for the treated banks, and the effect is somewhat more pronounced for banks involved in mergers and acquisitions.[23]

The banking problems described in Section VI.A resulted in a tightening of regulation with the Financial Institutions Reform, Recovery, and Enforcement Act in 1989 (FIRREA), and the Federal Deposit Insurance Corporation Improvement Act (FDICIA) in 1991. FIRREA provided the authorities with more resources to resolve banks, and FDICIA contained further efforts to bolster soundness. While the effects of these two pieces of legislation are captured by the state-quarter-fixed effects, we construct a dummy variable, *FIRREA*, that takes on the value of one in the quarters from 1989Q1 (0 otherwise), and interact this dummy with the treatment group dummy in Panel E. The coefficient estimate on the key interaction term remains similar to the baseline estimates. In Panel F, we generate a dummy variable, *FDICIA*, that equals 1 for the quarters from 1992Q1 (0 otherwise), and interact the *FDICIA* dummy with the treatment group

[22] See https://www.chicagofed.org/banking/financial-institution-reports/merger-data.

[23] In unreported tests, we rerun our regressions and remove banks from the sample that are acquired and banks that are the acquirer. The effects are equally pronounced and do not change our inferences.

dummy. Despite this change, the TG-Post coefficient remains negative and significant. These two pieces of legislation do not interfere with our inferences.

[TABLE 9]

C. The role of size, loan portfolio composition, soundness, listing, and BHC membership

Opacity is also affected by bank-specific characteristics. Huizinga and Laeven (2012) report that banks are particularly prone to opacity if they are large and have more exposure to real estate assets. They also state that bank distress results in greater opacity. Likewise, Beatty et al. (2002) and Beatty and Liao (2014) highlight that bank characteristics such as public listing and holding company membership affect both abilities and incentives to manage earnings.

To examine these cross sectional predictions, we split the sample at the median level of bank size in terms of total assets in Panel A of Table 10, at the median level of the ratio of real estate loans to total loans in Panel B, and at the median level of the probability of bank failure in Panel C.[24] All sample splits use the pre-treatment median values. The key coefficients are significantly greater in magnitude for larger banks. While there are significant opacity-reducing effects for banks below the median of the ratio of real estate loans to total loans, the interaction coefficient is always larger for banks whose exposure is above the median. The findings concerning soundness, approximated with the probability of failure, are also in line with the predictions. The magnitudes for the key coefficient are only significantly negative for the banks whose failure probability is above the median. Chow tests confirm that the coefficients are significantly different across subsamples.

Panel D explores whether the effects differ across listed and unlisted banks. The introduction of depositor preference laws leads to a significant decline in earnings opacity within both groups. However, listed banks respond to a greater extent. These results extend prior work

[24] Online Appendix Table B.2 presents the regression estimates for our failure prediction model.

by Beatty et al. (2002). They show listed banks are more opaque compared to unlisted ones. It is plausible therefore that the marginal effect of depositor preference law on earnings opacity is greater within listed banks because they have greater scope to reduce opacity relative to an unlisted bank that is ex-ante less opaque.

Panel E compares banks that are members of bank holding companies (BHCs) with independent banks. For both groups, earnings opacity within state-chartered banks significantly decreases following implementation of depositor preference law, but the effect is somewhat stronger among independent banks. The unconditional mean for the earnings opacity measure is higher for independent banks relative to BHC member banks, in line with greater autonomy for such banks (Aghion and Tirole (1997)). For these banks, depositor preference introduces greater market discipline, resulting in a greater reduction of earnings opacity.

[TABLE 10]

VII. Other sensitivity checks

This section discusses further sensitivity checks. We first focus on omitted variables, then we replicate our main tests using an alternative control group. Finally, we address concerns regarding anticipation effects, and we also consider an alternative treatment of standard errors.

A. Omitted variables

Recall that our main tests include state-quarter-fixed effects. Therefore, the primary threats to inference are time-varying shocks that differentially affect the treatment and control groups. Furthermore, any omitted variable must coincide with the 15 separate enactments of depositor preference laws to bias our inferences. Omitted variables that satisfy these criteria simultaneously are unlikely to exist. In this subsection, we address three plausible threats to identification.

First, we assuage concerns about differences in the geographical coverage between state-chartered and nationally-chartered banks in terms of the geographical reach of their activities. We use an interaction term in Panel A of Online Appendix Table B.3 between the treatment group dummy and the number of counties each bank operates in. Our inferences endure.

Second, our main tests include state-quarter-fixed effects that sweep out state-specific time-varying shocks. Panel B of Online Appendix Table B.3 takes this issue further because earnings management may be countercyclical. We therefore examine whether macroeconomic fluctuations confound our inferences and include an interaction between the treatment group dummy and the state unemployment rate (*UNEMP*). The effect of depositor preference laws on earnings opacity is robust to this change.

Third, the rotation of regulators documented in Agarwal et al. (2014) may coincide with changes in regulatory monitoring activity. To address this, we interact the treatment group dummy with a dummy that takes on the value of one if the bank is regulated by the FDIC (0 otherwise) in Panel C of Online Appendix Table B.3. Our key inferences are unaffected.[25]

B. Validity of the counterfactual

Our tests for parallel trends suggest that the control group is observationally similar to the treatment group and therefore constitutes a valid counterfactual. However, banking markets are local in nature. A concern may be that the introduction of depositor preference laws results in a reallocation of state-chartered banks' nondeposits to nationally-chartered banks in the neighbourhood, suggesting that the control group may be indirectly affected by the treatment which could bias our coefficient of interest.

[25] The results in Panel B of Table B.1 show that removing banks that switch charter status during the sample, and therefore potentially change supervisor, does not drive our findings.

To mitigate this concern, we use a 1:1 nearest neighbor matching strategy with replacement following Lemmon and Roberts (2010). First, we use a probit model to estimate

$$(8) \quad TG_b = \alpha + \beta_1 LLP_b + \beta_2 Size_b + \beta_3 Capital\ ratio_b + \beta_4 Loss_b + \beta_5 Collateral_b + \beta_6 NDMS_b + \beta_7 Regdistance_b + \varepsilon_b,$$

where TG_b is a dummy equal to 1 if bank b is state-chartered in the pre-treatment period (0 otherwise); LLP_b, $Size_b$, $Capital\ ratio_b$, $Loss_b$, $Collateral_b$, $NDMS_b$, and $Regdist_b$ are the mean loan loss provision, size, capital ratio, collateralization of nondeposit claims, nondeposit maturity structure and distance to the nearest regulator office for bank b in the pre-treatment period.

We then compute propensity scores using the estimates obtained from equation (8). Bank b's nearest neighbor is the bank with the most similar propensity score. We also impose the condition that the propensity score must lie within a 0.01 range of bank b's propensity score. Our matched sample pairs one state-chartered bank with one propensity score matched nationally-chartered bank, resulting in a sample with 77,269 observations. Online Appendix Table B.4 replicates our main tests but uses the alternative matched control group. The average treatment effect is larger than in the baseline models.

C. Anticipation effects and precision of the standard errors

Banks may have expected the introduction of depositor preference and restricted discretionary provisioning before the adoption of these laws. To test this, we use four placebo dummy variables equal to 1 in the first, second, third, and fourth quarter before depositor preference enactment (0 otherwise) and interact these placebo dummies with the treatment group indicator. The placebo interactions in Panel A of Online Appendix Table B.5 remain

insignificant, suggesting that anticipation does not affect our inferences. Another benefit of this test is that it supports the validity of the parallel trends assumption.

Difference-in-differences estimates are sensitive to the treatment of standard errors. So far, all tests cluster the standard errors at the state level. Our test statistics are therefore based on the most conservative standard errors. As an alternative to addressing the question of too few blocks of clusters, we follow Bertrand et al. (2004) and collapse the data on a single pre- and post-treatment mean for each bank and use robust standard errors. Our estimates remain robust in Panel B of Online Appendix Table B.5.

VIII. Conclusion

Opacity is a key aspect in banking because it hinders the ability of outsiders to assess bank value and risk. In recent years, regulatory authorities have taken steps to increase transparency by performing stress tests, releasing the results to the public, and changing legislation to incentivize greater monitoring of bank behavior.

We investigate whether nondepositors' monitoring incentives play a role for bank opacity. To this end, we study whether the introduction of state depositor preference laws that reallocate monitoring incentives from senior to junior debtholders affects banks' discretionary loan loss provisioning, the level of real estate owned, and the level of other types of opaque assets banks hold on the balance sheet.

We document three key results. First, we show that assigning a junior claim to nondepositors reduces bank opacity by economically meaningful magnitudes. This finding is primarily driven by banks restricting discretionary provisioning to increase earnings. Second, we

document that the reallocation of monitoring incentives is particularly effective in reducing earnings opacity during crises. This is beneficial because information asymmetries are particularly pronounced during episodes of turmoil. Third, we find that larger banks, banks with greater exposure to real estate markets, unsound banks, listed banks, and independent banks display a greater responsiveness to the greater monitoring incentives of nondepositors.

Given concerns about the consequences of bank opacity, this research is timely and policy-relevant. Our findings support innovations in regulation, such as the introduction of depositor priority laws in the European Union after the crisis that call for more monitoring by sophisticated creditors. The change in regulation which we study incentivizes stronger monitoring by nondepositors which we show to have potential to reduce bank opacity.

These results generalize beyond our setting. Many small and medium-sized banks in Europe and the U.S. exhibit similar balance sheet characteristics to those in our sample. They also depend on nondeposit funding to finance new projects (Demirgüc-Kunt and Huizinga (2010)). This suggests considerable potential for improving bank transparency by subordinating the claims of nondepositors to those of depositors.

Our findings point to a largely neglected role of nondepositors' monitoring incentives in reducing information asymmetries and improving information quality and accuracy. They also illustrate an efficient way to strengthen nondepositors' monitoring incentives. To this extent, these results highlight a role for depositor priority in the regulatory framework.

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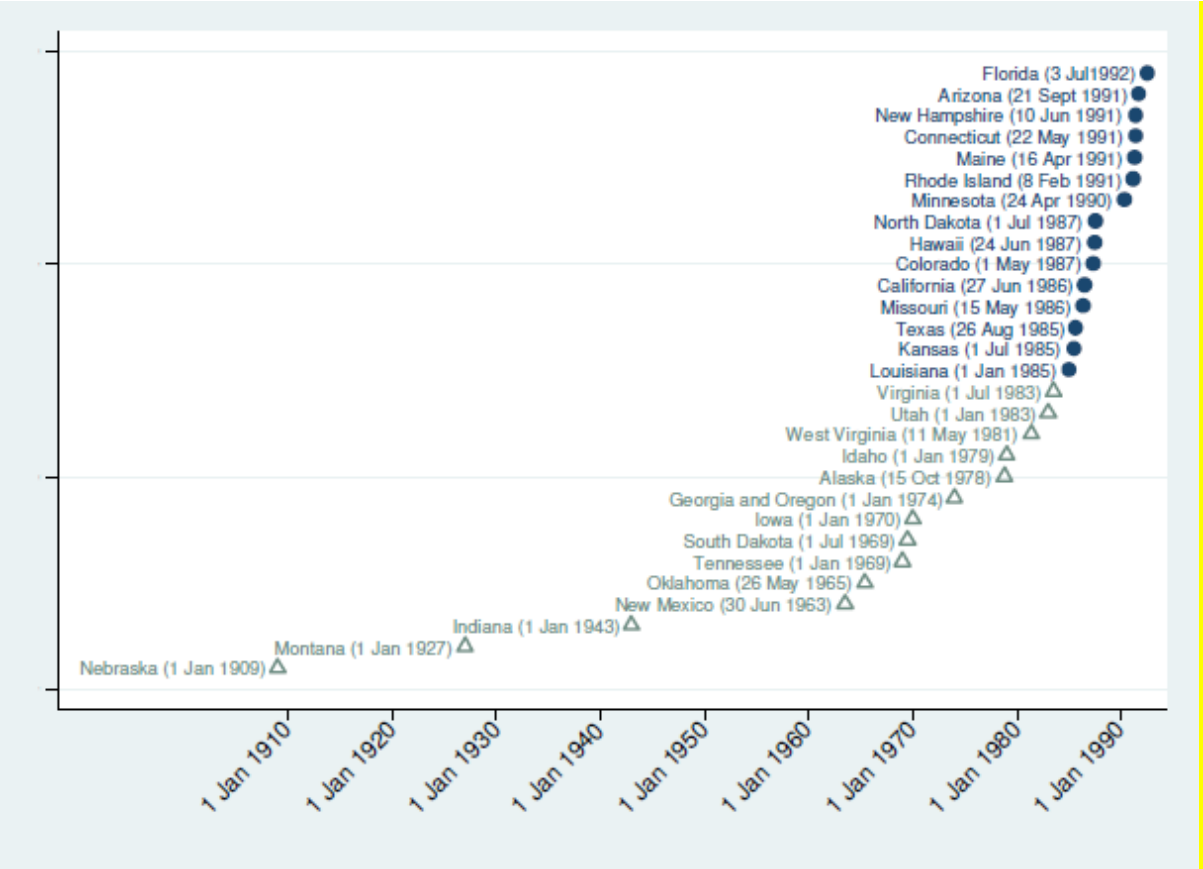
Figure 1
Timing of state depositor preference adoption



Notes: Figure 1 presents the timing of depositor preference adoption based on Marino and Bennett (1999). States that enter our empirical tests are denoted by a circle, while all other states that introduced depositor preference are denoted by a triangle. The date of adoption is presented in parentheses. The adoption date is assumed to be 1 Jan 1974 for Georgia because the adoption date in 1974 is not recorded. Depositor preference passed both houses during 1979 in Idaho, but the enactment date is unclear, and we assume 1 Jan 1979. Texas amended its law in 1993Q2 and did not have depositor preference until national depositor preference was enacted in August 1993.

Table 1
Claim structure with and without state depositor preference law

Claim structure without depositor preference	Claim structure with depositor preference
1. Receiver (FDIC)	1. Receiver (FDIC)
2. Secured creditors	2. Secured creditors
3. Insured depositors	3. Insured and uninsured depositors
4. Uninsured depositors and nondeposit creditors	4. Nondeposit creditors
5. Shareholders	5. Shareholders

Notes: This table provides an overview of debt priority claim structure with and without depositor preference law. In the codified text of the state laws, provisions concerning depositor preference are typically presented under headings titled "Involuntary Liquidation Procedure", "Payment of Claims", or "Distribution of Assets". Although the exposition differs across states, the priority structure for the claims on failed state-chartered banks converges to the structure presented in this table. See also Thomson (1994), Osterberg (1996) and Marino and Bennett (1999).

Table 2
Summary statistics and variable definitions

Panel A: Variable Definitions

Variable	Definition
TG	A dummy variable equal to 1 if bank *b* in state *s* at time *t* is state-chartered, 0 otherwise.
Post	A dummy variable equal to 1 if depositor preference law is in force in state *s* at time *t*, 0 otherwise.
EO^{1}	The natural logarithm of earnings opacity in bank *b* in state *s* at time *t* outlined in Section IV.A, defined as the absolute value of the residual calculated based on equation (1).
EO^{2}	The natural logarithm of earnings opacity in bank *b* in state *s* at time *t* outlined in Section IV.A, defined as the absolute value of the residual calculated based on equation (2).
EO^{3}	The natural logarithm of earnings opacity in bank *b* in state *s* at time *t* outlined in Section IV.A, defined as the absolute value of the residual calculated based on equation (3).
EO^{4}	The natural logarithm of earnings opacity in bank *b* in state *s* at time *t* outlined in Section IV.A, defined as the absolute value of the residual calculated based on equation (4).
EO^{1+}	The natural logarithm of earnings opacity in bank *b* in state *s* at time *t* outlined in Section IV.A, defined as the positive signed value of the residual calculated based on equation (1).
EO^{1-}	The natural logarithm of earnings opacity in bank *b* in state *s* at time *t* outlined in Section IV.A, defined as the negative signed value of the residual calculated based on equation (1).
EO^{2+}	The natural logarithm of earnings opacity in bank *b* in state *s* at time *t* outlined in Section IV.A, defined as the positive signed value of the residual calculated based on equation (2).
EO^{2-}	The natural logarithm of earnings opacity in bank *b* in state *s* at time *t* outlined in Section IV.A, defined as the negative signed value of the residual calculated based on equation (2).
EO^{3+}	The natural logarithm of earnings opacity in bank *b* in state *s* at time *t* outlined in Section IV.A, defined as the positive signed value of the residual calculated based on equation (3).
EO^{3-}	The natural logarithm of earnings opacity in bank *b* in state *s* at time *t* outlined in Section IV.A, defined as the negative signed value of the residual calculated based on equation (3).
EO^{4+}	The natural logarithm of earnings opacity in bank *b* in state *s* at time *t* outlined in Section IV.A, defined as the positive signed value of the residual calculated based on equation (4).
EO^{4-}	The natural logarithm of earnings opacity in bank *b* in state *s* at time *t* outlined in Section IV.A, defined as the negative signed value of the residual calculated based on equation (4).
EO^{BW}	The natural logarithm of earnings opacity in bank *b* in state *s* at time *t* defined as the absolute value of the residual calculated based on equation (5).
OREO	The natural logarithm of the ratio of other real estate owned relative to total loans in bank *b* in state *s* at time *t*.
OPAQUE	The natural logarithm of the sum of the book value of bank premises and fixed assets, investments in unconsolidated subsidiaries, intangible assets and the balance sheet category "other assets".
LLP_{t-1}	The lagged ratio of loan loss provisions to total loans in bank *b* in state *s* at time *t*.
Bank size	The natural logarithm of total assets of bank *b* in state *s* at time *t*.
Capital ratio	The natural logarithm of the ratio of equity capital to total assets in bank *b* in state *s* at time *t*.
Loss	A dummy variable equal to 1 if bank *b* in state *s* at time *t* reports $ROA < 0$, 0 otherwise.
Collateralization	The natural logarithm of the sum of pledged securities, federal fund repos, standby letters of credit, and secured pledged deposits to total nondeposit liabilities for bank *b* in state *s* at time *t*.
ND maturity structure	The natural logarithm of the ratio of nondeposits with a maturity of 1 year or less to total nondeposits for bank *b* in state *s* at time *t*.
Distance to regulator office	The distance in hundreds of miles between the bank's headquarters and the nearest field office of the corresponding regulator (OCC, Federal Reserve, FDIC, or state regulator) for bank *b* in state *s* at time *t*.
ND costs	The natural logarithm of the ratio of nondeposit interest expenses to nondeposit liabilities for bank *b* in state *s* at time *t*.
Fed funds costs	The natural logarithm of the ratio of Fed fund expenses to Fed fund liabilities for bank *b* in state *s* at time *t*.
Subordinated debt costs	The natural logarithm of the ratio of interest expenses on subordinated debt for bank *b* in state *s* at time *t*.
Intrastate	A dummy variable equal to 1 if state *s* permits intrastate bank branching at time *t*, 0 otherwise.
Interstate	A dummy variable equal to 1 if state *s* permits interstate bank branching at time *t*, 0 otherwise.
FIRREA	A dummy variable equal to 1 if an observation is from 1989Q1 onwards, 0 otherwise.
FDICIA	A dummy variable equal to 1 if an observation is from 1992Q2 onwards, 0 otherwise.
Number of counties	Number of counties in which bank *b* has at least one branch at time *t*.
UNEMP	The unemployment rate (%) in state *s* at time *t*.
FDIC	A dummy variable equal to 1 if bank *b* in state *s* at time *t* is regulated by the FDIC, 0 otherwise.
$Placebo_{t-1}$	A dummy variable equal to 1 in the quarter prior to depositor preference law being enacted in state *s*, 0 otherwise.
$Placebo_{t-2}$	A dummy variable equal to 1 in the quarter two quarters prior to depositor preference law being enacted in state *s*, 0 otherwise.
$Placebo_{t-3}$	A dummy variable equal to 1 in the quarter three quarters prior to depositor preference law being enacted in state *s*, 0 otherwise.
$Placebot_{-4}$	A dummy variable equal to 1 in the quarter four quarters prior to depositor preference law being enacted in state *s*, 0 otherwise.
Charter switch	A dummy variable equal to 1 if bank *b* in state *s* at time *t* switches charter, 0 otherwise.
NPA	The ratio of nonperforming assets to total loans in bank *b* in state *s* at time *t*.
Cost income ratio	The ratio of salaries and employee benefits plus expenses on premises to income in bank *b* in state *s* at time *t*.
Cash	The natural logarithm of cash holdings in bank *b* in state *s* at time *t*.
ΔLoan	Change in total loans to lagged total loans in bank *b* in state *s* at time *t*.
ΔGSP	The growth rate of Gross State Product in state *s* at time *t*.

CSRET	The Case-Shiller Index in state *s* at time *t*.
ALW	Is the ratio of loan loss allowances to total loans in bank *b* in state *s* at time *t*.
CO	Net charge-offs divided by lagged total loans in bank *b* in state *s* at time *t*.
Profit	The ratio of net earnings before loan loss provisions to lagged total loans in bank *b* in state *s* at time *t*.
Opacity^{SC}	The mean of EO_{bst}^{1} among state-chartered banks in state *s* at time *t*.
Opacity^{NC}	The mean of EO_{bst}^{1} among nationally-chartered banks in state *s* at time *t*.
State-chartered assets	The ratio of assets held by state-chartered banks in state *s* at time *t* to total bank assets in the state at time *t*.
S&L Crisis	The ratio of assets in failed thrifts to total bank assets in state *s* at time *t*.
Assets in all failed banks	The natural logarithm of total assets in failed banks in state *s* at time *t*.
Bank profitability	The mean of ROA of all banks operating in state *s* at time *t*.
Democrat governor	A dummy variable equal to 1 if the state governor belongs to the Democratic Party in state *s* at time *t*, 0 otherwise.

Panel B: Descriptive Statistics

Variable	Obs.	Mean	St. Dev	5^{th} percentile	95^{th} percentile
TG	205,057	0.6436	0.4789	0	1
Post	205,057	0.5876	0.4923	0	1
EO^{1}	205,057	0.0033	0.0043	0.0003	0.0109
EO^{2}	205,057	0.0033	0.0043	0.0003	0.0109
EO^{3}	205,057	0.0033	0.0043	0.0003	0.0109
EO^{4}	205,057	0.0033	0.0043	0.0003	0.0108
EO^{1+}	58,402	0.0056	0.0070	0.0002	0.0216
EO^{1-}	146,655	-0.0024	0.0019	-0.0049	-0.0004
EO^{2+}	58,275	0.0056	0.0070	0.0002	0.0216
EO^{2-}	146,782	-0.0023	0.0019	-0.0049	-0.0004
EO^{3+}	58,252	0.0056	0.0070	0.0002	0.0216
EO^{3-}	146,805	-0.0023	0.0019	-0.0049	-0.0004
EO^{4+}	58,290	0.0055	0.0070	0.0002	0.0216
EO^{4-}	146,767	-0.0023	0.0019	-0.0049	-0.0004
EO^{BW}	205,057	0.0033	0.0044	0.0003	0.0109
OREO	205,057	0.0167	0.0307	0.0003	0.0667
OPAQUE	205,057	0.0544	0.7209	0.0016	0.0982
LLP_{t-1}	205,057	9.2329	0.1313	9.2103	9.2823
Bank size	205,057	10.6542	1.1706	9.0397	12.7470
Capital ratio	205,057	0.0840	0.0334	0.0433	0.1400
Loss	205,057	0.1480	0.3551	0	1
Collateralization	205,057	0.1479	0.1110	0.0113	0.3490
ND maturity structure	205,057	0.1706	1.4978	0	0.6738
Distance to regulator office	205,057	1.5004	2.0927	0.0387	4.3845
ND costs	205,057	0.0175	0.0719	0	0.0671
Fed Funds costs	205,057	0.0228	0.4091	0	0.0748
Subordinated debt costs	205,057	0.0044	0.1183	0	0.0308
Intrastate	205,057	0.4605	0.4984	0	1
Interstate	205,057	0.2402	0.4272	0	1
S&L crisis	205,057	0.0207	0.0566	0	0.4569
FIRREA	205,057	0.2728	0.4454	0	1
FDICIA	205,057	0.1253	0.3310	0	1
Number of counties	205,057	1.2562	1.814	1	2
UNEMP	205,057	6.4764	1.6362	4.3	9.27
FDIC	205,057	0.2475	0.4316	0	1
$Placebo_{t-1}$	205,057	0.0266	0.1609	0	1
$Placebo_{t-2}$	205,057	0.0265	0.1606	0	1
$Placebo_{t-3}$	205,057	0.0264	0.1603	0	1
$Placebo_{t-4}$	205,057	0.0265	0.1606	0	1
Charter switch	205,057	0.0322	0.1766	0	1
NPA	205,057	0.0103	0.0162	0	0.0393
Cost income ratio	205,057	0.1133	0.1289	0.0001	0.3273
Cash	205,057	0.0558	0.0704	0.0000	0.1875
ΔLoan	205,057	0.0233	0.4148	-0.0856	0.1460
ΔGSP	205,057	0.0085	0.0344	-0.0715	0.0643
CSRET	205,057	66.4035	9.7618	50.75	77.05
ALW	205,057	0.0098	0.0112	0.0028	0.0231
CO	205,057	0.0057	0.0185	0	0.0258
$Opacity^{SC}$	888	0.0018	0.0008	0.0002	0.0126
$Opacity^{NC}$	888	0.0018	0.0010	0.0003	0.0098
State-chartered assets	888	0.6587	0.1171	0.5155	0.8975
S&L Crisis	888	0.0104	0.0358	0	0.0448
Assets in all failed banks	888	0.0098	0.0353	0.0001	0.0441
Bank profitability	888	0.0029	0.0009	0.0013	0.0042
Democrat governor	888	0.5366	0.4989	0	1

Notes: All variables are reported in levels except Bank size and LLP_{t-1} which are reported in natural logarithms.

Table 3
Exogeneity of depositor preference laws with respect to earnings opacity and tests for parallel trends

Panel A: Political economy environment of state depositor preference law adoption

	(1)	(2)
Dependent variable	Post dummy variable (representing the adoption of state depositor preference law)	
$Opacity_t^{SC}$	-0.0015	
	(-0.12)	
$Opacity_t^{NC}$	-0.0045	
	(-0.44)	
$Opacity_{t-1}^{SC}$		0.0071
		(0.61)
$Opacity_{t-1}^{NC}$		-0.0055
		(-0.59)
$Opacity_{t-2}^{SC}$		-0.0036
		(-0.24)
$Opacity_{t-2}^{NC}$		0.0141
		(1.01)
$Opacity_{t-3}^{SC}$		0.0003
		(0.01)
$Opacity_{t-3}^{NC}$		0.0215
		(1.20)
$Opacity_{t-4}^{SC}$		0.0059
		(0.41)
$Opacity_{t-4}^{NC}$		0.0294
		(1.36)
$Opacity_{t-5}^{SC}$		-0.0003
		(-0.02)
$Opacity_{t-5}^{NC}$		-0.0031
		(-0.14)
$Opacity_{t-6}^{SC}$		0.0045
		(0.28)
$Opacity_{t-6}^{NC}$		-0.0045
		(-0.22)
$Opacity_{t-7}^{SC}$		0.0111
		(0.93)
$Opacity_{t-7}^{NC}$		-0.0265
		(-1.62)
$Opacity_{t-8}^{SC}$		0.0075
		(0.85)
$Opacity_{t-8}^{NC}$		0.0098
		(0.41)
State-chartered assets	-0.0336	-0.0675
	(-0.28)	(-0.67)
S&L crisis	-0.1001	-0.1544**
	(-1.68)	(-2.11)
Assets in all failed banks	0.0004	0.0002
	(1.68)	(0.68)
Bank profitability	0.0218	0.0204
	(1.44)	(1.05)
Democrat governor	0.0069	0.0039
	(0.42)	(0.25)
State FE	Yes	Yes
Quarter FE	Yes	Yes
Observations	888	888
R^2	0.1570	0.1721

Panel B: Parallel trends

	EO^1				EO^2				EO^3				EO^4			
Time	State Charter	National Charter	Difference	*t*-statistic	State Charter	National Charter	Difference	*t*-statistic	State Charter	National Charter	Difference	*t*-statistic	State Charter	National Charter	Difference	*t*-statistic
$t-1$	-6.2245	-6.2180	0.0065	0.06	-6.2354	-6.2923	-0.0569	-0.46	-6.2301	-6.2969	-0.0667	-0.56	-6.2253	-6.3012	-0.0759	-0.64
	(0.0863)	(0.0789)	(0.1169)		(0.0934)	(0.0804)	(0.1241)		(0.0920)	(0.0747)	(0.1195)		(0.0911)	(0.0736)	(0.1181)	
$t-2$	-6.2131	-6.1571	0.0560	0.62	-6.1560	-6.1392	0.0168	0.12	-6.2193	-6.1557	0.0636	0.37	-6.1843	-6.1501	0.0341	0.24
	(0.0665)	(0.0614)	(0.0904)		(0.1056)	(0.0938)	(0.1420)		(0.1388)	(0.0935)	(0.1698)		(0.1126)	(0.0922)	(0.1468)	
$t-3$	-6.1900	-6.1240	0.0659	0.84	-6.1694	-6.0955	0.0739	0.41	-6.1555	-6.0974	0.0581	0.35	-6.1526	-6.0960	0.0566	0.34
	(0.0607)	(0.0499)	(0.0788)		(0.1527)	(0.0899)	(0.1807)		(0.1391)	(0.0893)	(0.1682)		(0.1357)	(0.0896)	(0.1653)	
$t-4$	-6.1840	-6.1342	0.0498	0.70	-6.1852	-6.1874	-0.0022	-0.01	-6.1755	-6.1972	-0.0216	-0.12	-6.1857	-6.1895	-0.0038	-0.02
	(0.0510)	(0.0489)	(0.0716)		(0.0991)	(0.1548)	(0.1757)		(0.0957)	(0.1568)	(0.1743)		(0.0983)	(0.1555)	(0.1754)	

Notes: Panel A reports estimates of a linear probability model for equation (7). The results are unchanged when we construct $Opacity_{st}^{SC}$ and $Opacity_{st}^{NC}$ using the alternative measures of opacity described in Section IV.A. Observations from quarters after depositor preference law enactment are excluded. Definitions of the variables are provided in Panel A of Table 2. The standard errors are clustered at the state level and the corresponding heteroscedasticity-robust *t*-statistics are reported in parentheses. Panel B presents results of *t*-tests on the equality of the growth rate of the earnings opacity variables in the pre-treatment periods *t-1, t-2, t-3,* and *t-4* between the treatment and control groups.

Table 4
Effects of nondepositors' monitoring incentives on bank earnings opacity

Panel A: Unsigned measures of earnings opacity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable	EO^1	EO^1	EO^2	EO^2	EO^3	EO^3	EO^4	EO^4
TG	-0.0644	-0.0059	-0.0602	-0.0010	-0.0624	-0.0030	-0.0630	-0.0037
	(-1.21)	(-0.18)	(-1.09)	(-0.03)	(-1.13)	(-0.09)	(-1.15)	(-0.11)
TG * Post	-0.0579***	-0.0641***	-0.0602***	-0.0667***	-0.0587***	-0.0654***	-0.0594***	-0.0659***
	(-3.10)	(-3.68)	(-3.07)	(-3.68)	(-3.01)	(-3.67)	(-3.07)	(-3.70)
LLP_{t-1}	0.6277***	0.2938***	0.5865***	0.2533***	0.6109***	0.2788***	0.5718***	0.2383***
	(4.77)	(6.77)	(3.97)	(4.23)	(5.58)	(9.46)	(4.68)	(7.08)
Bank size		0.0247		0.0166		0.0125		0.0166
		(1.53)		(1.03)		(0.77)		(1.02)
Capital ratio		-0.0930***		-0.0941***		-0.0934***		-0.0939***
		(-4.47)		(-4.52)		(-4.39)		(-4.48)
Loss		0.7636***		0.7649***		0.7647***		0.7656***
		(25.87)		(25.94)		(24.95)		(25.20)
State * Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	205,057	205,057	205,057	205,057	205,057	205,057	205,057	205,057
R^2	0.1704	0.2217	0.1734	0.2247	0.1756	0.2265	0.1747	0.2259

Panel B: Signed measures of earnings opacity

Dependent variable	EO^{1+}	EO^{1-}	EO^{2+}	EO^{2-}	EO^{3+}	EO^{3-}	EO^{4+}	EO^{4-}
TG	-0.0392	-0.0107	-0.0323	-0.0029	-0.0327	-0.0095	-0.0300	-0.0103
	(-0.45)	(-0.30)	(-0.40)	(-0.08)	(-0.42)	(-0.26)	(-0.38)	(-0.29)
TG * Post	-0.0141	-0.0874***	-0.0173	-0.0906***	-0.0157	-0.0876***	-0.0212	-0.0859***
	(-0.31)	(-5.87)	(-0.39)	(-5.83)	(-0.38)	(-5.66)	(-0.49)	(-5.59)
LLP_{t-1}	-0.1048***	0.2214**	-0.1180***	0.1243	-0.2229***	0.4025***	-0.2553***	0.4101***
	(-4.03)	(2.17)	(-4.03)	(1.48)	(-5.83)	(7.68)	(-7.00)	(7.93)
Bank size	0.2559***	-0.1622***	0.2869***	-0.1886***	0.2994***	-0.2029***	0.2960***	-0.1967***
	(7.31)	(-4.72)	(9.36)	(-5.98)	(9.29)	(-6.51)	(9.38)	(-6.36)
Capital ratio	-0.1366***	-0.0611	-0.1362***	-0.0664	-0.1353***	-0.0655	-0.1353***	-0.0642
	(-7.88)	(-1.46)	(-7.88)	(-1.60)	(-7.63)	(-1.61)	(-7.69)	(-1.58)
Loss	1.2837***	0.1241***	1.2776***	0.1282***	1.2785***	0.1231***	1.2817***	0.1226***
	(69.29)	(9.23)	(66.35)	(9.57)	(67.21)	(8.89)	(68.22)	(8.87)
State * Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	58,402	146,655	58,275	146,782	58,252	146,805	58,290	146,767
R^2	0.3884	0.2127	0.3865	0.2230	0.3832	0.2304	0.3833	0.2283

Notes: This table reports estimates of equation (6) using the earnings opacity measures EO^1-EO^4 as the dependent variable. Panel A uses the absolute value of the residuals as measure of earnings opacity. Panel B uses the signed residuals as measures of earnings opacity. Definitions of the variables are provided in Panel A of Table 2. The sample is restricted to banks in states that enacted depositor preference over the period 1983Q1 to 1993Q2. Standard errors are clustered at the state level and the corresponding heteroscedasticity-robust *t*-statistics are reported in parentheses. *** and ** indicate statistical significance at the 1 and 5 percent levels, respectively.

Table 5
Effects of increases in nondepositors' monitoring incentives: Alternative measures of opacity

Dependent variable	(1) EO^{BW}	(2) OREO	(3) OPAQUE
TG	0.0571*	0.3215**	-0.0057
	(1.97)	(2.81)	(-0.47)
TG * Post	-0.0676***	-0.0862*	-0.0217*
	(-3.22)	(-1.74)	(-1.92)
LLP_{t-1}	0.3359***	0.4223***	0.2693***
	(8.14)	(3.15)	(3.35)
Bank size	0.0368	-0.4320***	0.0628***
	(1.62)	(-7.16)	(5.45)
Capital ratio	-0.0946***	-0.2925***	-0.0229***
	(-4.88)	(-7.37)	(-3.18)
Loss	0.8017***	0.4158***	-0.0072*
	(20.71)	(10.79)	(-1.83)
State * Quarter FE	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes
Observations	205,057	205,057	205,057
R^2	0.2040	0.6199	0.8568

Notes: This table reports estimates of equation (6) using EO^{BW}, OREO, and OPAQUE as the dependent variable. Definitions of the variables are provided in Panel A of Table 2. The sample is restricted to banks in states that enacted depositor preference over the period 1983Q1 to 1993Q2. Standard errors are clustered at the state level and the corresponding heteroscedasticity-robust *t*-statistics are reported in parentheses. ***, ** and * indicate statistical significance at the 1 percent, 5 percent and 10 percent levels, respectively.

Table 6
Further effects of nondepositors' monitoring incentives and controlling for additional factors

Panel A: Collateralization, maturity structure, and distance to regulator office

	(1)	(2)	(3)
Dependent variable	Collateralization	ND maturity structure	Distance to regulator office
TG	0.0359	-0.3586	-0.7904**
	(0.84)	(-1.08)	(-2.26)
TG * Post	0.0642**	0.7446***	0.0105
	(2.23)	(3.08)	(0.70)
LLP_{t-1}	-0.5302***	0.0861	0.0024
	(-8.52)	(0.45)	(0.26)
Bank size	0.1080	0.4469***	-0.0090
	(1.59)	(3.58)	(-1.65)
Capital ratio	-0.0097	0.1850***	0.0015
	(-0.67)	(3.56)	(0.38)
Loss	0.0643***	-0.2198***	-0.0031
	(4.45)	(-4.37)	(-1.73)
State * Quarter FE	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes
Observations	205,057	205,057	205,057
R^2	0.4116	0.4439	0.9964

Panel B: Bank earnings opacity with additional controls

	(1)	(2)	(3)	(4)
Dependent variable	EO^1	EO^2	EO^3	EO^4
TG	0.0050	0.0096	0.0077	0.0075
	(0.14)	(0.26)	(0.21)	(0.20)
TG * Post	-0.0630***	-0.0656***	-0.0643***	-0.0649***
	(-3.64)	(-3.64)	(-3.63)	(-3.66)
LLP_{t-1}	0.2942***	0.2536***	0.2791***	0.2387***
	(6.77)	(4.23)	(9.45)	(7.08)
Bank size	0.0255	0.0174	0.0133	0.0175
	(1.56)	(1.07)	(0.81)	(1.06)
Capital ratio	-0.0927***	-0.0938***	-0.0931***	-0.0936***
	(-4.44)	(-4.50)	(-4.36)	(-4.45)
Loss	0.7632***	0.7646***	0.7644***	0.7652***
	(25.82)	(25.90)	(24.90)	(25.16)
Collateralization	0.0006	0.0004	0.0004	0.0005
	(0.39)	(0.31)	(0.30)	(0.35)
ND maturity structure	-0.0017***	-0.0016***	-0.0017***	-0.0017***
	(-3.27)	(-3.25)	(-3.34)	(-3.21)
Distance to regulator office	0.0146	0.0142	0.0142	0.0150
	(0.85)	(0.80)	(0.78)	(0.82)
State * Quarter FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Observations	205,057	205,057	205,057	205,057
R^2	0.2217	0.2248	0.2265	0.2259

Notes: This table reports estimates of equation (6) using collateralization, nondeposit maturity structure, and the distance to the regulator office as the dependent variable in Panel A. Panel B replicates the tests from Panel A of Table 4 but additionally controls for collateralization of nondeposit claims, ND maturity structure, and distance to the nearest regulator office. Definitions of the variables are provided in Panel A of Table 2. The sample is restricted to banks in states that enacted depositor preference over the period 1983Q1 to 1993Q2. The standard errors are clustered at the state level and the corresponding heteroscedasticity-robust *t*-statistics are reported in parentheses. *** and ** indicate statistical significance at the 1 and 5 percent levels, respectively.

Table 7
Further evidence for the responsiveness of bank earnings opacity to increases in nondepositors' monitoring incentives

Panel A: Sample split at the median of Nondeposit funding/Total liabilities

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable	EO^1		EO^2		EO^3		EO^4	
Sample split	< p50 ND/TL	≥ p50 ND/TL	< p50 ND/TL	≥ p50 ND/TL	< p50 ND/TL	≥ p50 ND/TL	< p50 ND/TL	≥ p50 ND/TL
TG	0.0047	-0.0261	0.0071	-0.0185	0.0090	-0.0238	0.0084	-0.0246
	(0.11)	(-0.48)	(0.17)	(-0.33)	(0.22)	(-0.42)	(0.20)	(-0.44)
TG * Post	-0.0497**	-0.0686**	-0.0493**	-0.0748**	-0.0489**	-0.0720**	-0.0490**	-0.0729**
	(-2.69)	(-2.81)	(-2.64)	(-2.84)	(-2.61)	(-2.93)	(-2.63)	(-2.93)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State * Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	102,832	102,225	102,832	102,225	102,832	102,225	102,832	102,225
R^2	0.2347	0.2620	0.2368	0.2658	0.2375	0.2680	0.2373	0.2673
Chow test *F*-statistic	13.61		13.74		13.73		13.86	
p-value	0.00		0.00		0.00		0.00	

Panel B: Sample split at the median of Fed funds purchased/Total liabilities

Dependent variable	EO^1		EO^2		EO^3		EO^4	
Sample split	< p50 Fed funds purchased/TL	≥ p50 Fed funds purchased/TL	< p50 Fed funds purchased/TL	≥ p50 Fed funds purchased/TL	< p50 Fed funds purchased/TL	≥ p50 Fed funds purchased/TL	< p50 Fed funds purchased/TL	≥ p50 Fed funds purchased/TL
TG	0.0015	0.0051	0.0064	0.0107	0.0033	0.0104	0.0016	0.0097
	(0.04)	(0.14)	(0.14)	(0.29)	(0.08)	(0.27)	(0.04)	(0.25)
TG * Post	-0.0549***	-0.0661***	-0.0586**	-0.0680***	-0.0572**	-0.0670***	-0.0583**	-0.0672***
	(-3.05)	(-3.09)	(-2.76)	(-3.22)	(-2.94)	(-3.10)	(-2.96)	(-3.12)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State * Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	102,672	102,385	102,672	102,385	102,672	102,385	102,672	102,385
R^2	0.2517	0.2472	0.2550	0.2497	0.2570	0.2512	0.2563	0.2508
Chow test *F*-statistic	7.42		7.29		7.32		7.24	
p-value	0.00		0.00		0.00		0.00	

Panel C: Depositor preference and the cost of nondeposit funds

Dependent variable	ND costs	ND costs		Fed Funds costs	Subordinated debt costs
Sample split	Full sample	< p50 ND/TL	≥ p50 ND/TL	Full sample	Full sample
TG	0.1357	-0.0283	0.1377	-0.0171	0.2387
	(0.53)	(-0.05)	(1.03)	(-0.08)	(0.76)
TG * Post	0.3410***	0.3262***	0.3827***	0.1772*	0.2092*
	(4.35)	(2.99)	(4.60)	(2.00)	(1.91)
Control variables	Yes	Yes	Yes	Yes	Yes
State * Quarter FE	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes
Observations	205,057	102,826	102,231	205,057	205,057
R^2	0.6230	0.6792	0.5178	0.6046	0.6524
Chow test *F*-statistic	n/a	41.46		n/a	n/a
p-value	n/a	0.00		n/a	n/a

Notes: Panels A and B of this table report estimates of equation (6) using the earnings opacity measures EO^1-EO^4 as the dependent variable. Panel C reports estimates of equation (6) using nondeposit costs, Fed Funds costs, and subordinated debt costs as the dependent variable. The control variables are LLP_{t-1}, Bank size, Capital ratio, and the Loss dummy. We split the sample at the pre-treatment median of the ratio of nondeposit funding to total liabilities in Panel A, and Panel B splits the sample at the pre-treatment median of the ratio of Fed funds purchased to total liabilities which ensures the values are unaffected by the passage of depositor preference laws. Definitions of the variables are provided in Panel A of Table 2. The sample is restricted to banks in states that enacted depositor preference over the period 1983Q1 to 1993Q2. The Chow test *F*-statistic tests for equality between the coefficients in the models split above and below the median. The *p*-value is the *p*-value on the Chow test *F*-statistic. Standard errors are clustered at the state level and the corresponding heteroscedasticity-robust *t*-statistics are reported in parentheses. ***, ** and * indicate statistical significance at the 1 percent, 5 percent and 10 percent levels, respectively.

Table 8
Effects of increases in nondepositors' monitoring incentives during crises and non crisis periods

Panel A: Regional banking crises (New England and Texas)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable	EO^1	EO^1	EO^2	EO^2	EO^3	EO^3	EO^4	EO^4
Sample split	Non Crisis	Crisis	Non Crisis	Crisis	Non Crisis	Crisis	Non Crisis	Crisis
TG	-0.0055	0.1368	-0.0015	0.1257	-0.0035	0.1456	-0.0048	0.1588
	(-0.21)	(1.30)	(-0.06)	(1.20)	(-0.13)	(1.38)	(-0.18)	(1.51)
TG * Post	-0.0639***	-0.4008***	-0.0672***	-0.3611***	-0.0652***	-0.3844***	-0.0649***	-0.4029***
	(-5.79)	(-4.54)	(-6.10)	(-4.10)	(-5.89)	(-4.36)	(-5.87)	(-4.57)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State * Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	179,563	25,494	179,563	25,494	179,563	25,494	179,563	25,494
R^2	0.2195	0.3113	0.2228	0.3144	0.2248	0.3146	0.2242	0.3140
Chow test *F*-statistic	19.98		7.53		12.52		16.41	
p-value	0.00		0.02		0.00		0.00	

Panel B: S&L crisis

	(1)	(2)	(3)	(4)
Dependent variable	EO^1	EO^2	EO^3	EO^4
TG	-0.0058	-0.0009	-0.0029	-0.0036
	(-0.18)	(-0.03)	(-0.08)	(-0.10)
TG * Post	-0.0639***	-0.0663***	-0.0650***	-0.0654***
	(-3.39)	(-3.43)	(-3.37)	(-3.40)
TG * S&L crisis	-0.0039	-0.0101	-0.0089	-0.0137
	(-0.05)	(-0.11)	(-0.11)	(-0.16)
Control variables	Yes	Yes	Yes	Yes
State * Quarter FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Observations	205,057	205,057	205,057	205,057
R^2	0.2217	0.2247	0.2265	0.2259

Notes: This table reports estimates of equation (6) using the earnings opacity measures EO^1-EO^4 as the dependent variable. The control variables are LLP_{t-1}, Bank size, Capital ratio, and the Loss dummy. Panel A focuses on the regional banking crises in New England and in Texas. The sample includes observations from Connecticut, Maine, New Hampshire, Rhode Island and Texas. Panel B focuses on the S&L crisis. As most states were affected by the S&L crisis, there are few observations of non crisis periods. We therefore interact the TG dummy with the S&L crisis variable to identify whether state-chartered banks were differentially affected by the S&L crisis. Definitions of the variables are provided in Panel A of Table 2. The sample is restricted to banks in states that enacted depositor preference over the period 1983Q1 to 1993Q2. The Chow test *F*-statistic in Panel A tests for equality between the coefficients in the models split into non crisis and crisis periods. The *p*-value is the *p*-value on the Chow test *F*-statistic. Standard errors are clustered at the state level and the corresponding heteroscedasticity-robust *t*-statistics are reported in parentheses. *** indicates statistical significance at the 1 percent level.

Table 9
Effects of increases in nondepositors' monitoring incentives, and changes in regulation and mergers

Panel A: Interstate deregulation					**Panel B: Intrastate deregulation**			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Dependent variable	EO^1	EO^2	EO^3	EO^4	EO^1	EO^2	EO^3	EO^4
TG * Post	-0.0354*	-0.0398*	-0.0383**	-0.0385**	-0.0411*	-0.0448**	-0.0435**	-0.0438**
	(-2.03)	(-2.14)	(-2.15)	(-2.16)	(-2.12)	(-2.21)	(-2.28)	(-2.26)
TG * Interstate	-0.0850***	-0.0796**	-0.0805***	-0.0813**				
	(-3.08)	(-2.96)	(-2.98)	(-2.97)				
TG * Intrastate					-0.0565**	-0.0537**	-0.0538**	-0.0544**
					(-2.49)	(-2.40)	(-2.34)	(-2.37)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State * Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	205,057	205,057	205,057	205,057	205,057	205,057	205,057	205,057
R^2	0.2218	0.2249	0.2266	0.2260	0.2217	0.2248	0.2266	0.2260
Panel C: Subsample - banks involved in M&A					**Panel D: Subsample – excluding banks involved in M&A**			
Dependent variable	EO^1	EO^2	EO^3	EO^4	EO^1	EO^2	EO^3	EO^4
TG	-0.0127	-0.0087	-0.0087	-0.0099	0.0095	0.0144	0.0114	0.0109
	(-0.32)	(-0.21)	(-0.21)	(-0.24)	(0.22)	(0.33)	(0.26)	(0.25)
TG * Post	-0.0873***	-0.0920***	-0.0939***	-0.0933***	-0.0491**	-0.0509**	-0.0490**	-0.0501**
	(-4.02)	(-3.75)	(-4.04)	(-4.15)	(-2.28)	(-2.32)	(-2.24)	(-2.28)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State * Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	45,422	45,422	45,422	45,422	159,635	159,635	159,635	159,635
R^2	0.2888	0.2921	0.2938	0.2934	0.1992	0.2023	0.2042	0.2035
Panel E: Regulatory reforms – FIRREA					**Panel F: Regulatory reforms - FDICIA**			
Dependent variable	EO^1	EO^2	EO^3	EO^4	EO^1	EO^2	EO^3	EO^4
TG * Post	-0.0557***	-0.0583***	-0.0577***	-0.0581***	-0.0405**	-0.0444**	-0.0431**	-0.0435**
	(-3.76)	(-3.64)	(-3.71)	(-3.71)	(-2.35)	(-2.40)	(-2.43)	(-2.44)
TG * FIRREA	-0.0276	-0.0275*	-0.0255	-0.0257				
	(-1.69)	(-1.82)	(-1.59)	(-1.56)				
TG * FDICIA					-0.1019***	-0.0959***	-0.0963***	-0.0968***
					(-3.42)	(-3.12)	(-3.32)	(-3.37)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State * Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	205,057	205,057	205,057	205,057	205,057	205,057	205,057	205,057
R^2	0.2217	0.2248	0.2265	0.2259	0.2219	0.2249	0.2267	0.2261

Notes: This table reports estimates of equation (6) using the earnings opacity measures EO^1-EO^4 as the dependent variable. Panel A focuses on interstate regulation and includes an interaction term between a dummy for interstate deregulation and the treatment group dummy. Panel B focuses on intrastate regulation and includes an interaction term between a dummy for intrastate deregulation and the treatment group dummy. Panel C (D) includes only banks that are (not) involved in M&A activity during the sample period. Panel E focuses on FIRREA and includes an interaction term between the FIRREA dummy and the treatment group dummy. Panel F focuses on FDICIA and includes an interaction term between the FDICIA dummy and the treatment group dummy. The control variables are LLP_{t-1}, Bank size, Capital ratio, and the Loss dummy. Definitions of the variables are provided in Panel A of Table 2. The sample is restricted to banks in states that enacted depositor preference over the period 1983Q1 to 1993Q2. Standard errors are clustered at the state level and the corresponding heteroscedasticity-robust *t*-statistics are reported in parentheses. ***, ** and * indicate statistical significance at the 1 percent, 5 percent and 10 percent levels, respectively.

Table 10
Effects of increases in nondepositors' monitoring incentives and bank characteristics

Dependent variable	(1) EO^1	(2) EO^1	(3) EO^2	(4) EO^2	(5) EO^3	(6) EO^3	(7) EO^4	(8) EO^4
Panel A: Bank size								
Sample split	Bank size		Bank size		Bank size		Bank size	
	<p50	≥p50	<p50	≥p50	<p50	≥p50	<p50	≥p50
TG	-0.0476	0.0379	-0.0465	0.0493	-0.0459	0.0455	-0.0462	0.0435
	(-0.91)	(1.29)	(-0.89)	(1.51)	(-0.88)	(1.41)	(-0.88)	(1.36)
TG * Post	-0.0466*	-0.0613**	-0.0456*	-0.0690**	-0.0453*	-0.0665**	-0.0456*	-0.0666**
	(-1.86)	(-2.26)	(-1.83)	(-2.32)	(-1.82)	(-2.37)	(-1.83)	(-2.36)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State * Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	102,314	102,743	102,314	102,743	102,314	102,743	102,314	102,743
R^2	0.2096	0.2450	0.2100	0.2480	0.2098	0.2491	0.2100	0.2491
Chow test *F*-statistic	5.26		5.44		5.38		5.44	
p-value	0.04		0.04		0.04		0.04	
Panel B: Real estate loans								
Sample split	Real estate loans/Total loans		Real estate loans/Total loans		Real estate loans/Total loans		Real estate loans/Total loans	
	<p50	≥p50	<p50	≥p50	<p50	≥p50	<p50	≥p50
TG	-0.0843**	0.0457	-0.0824**	0.0539	-0.0815**	0.0527	-0.0809*	0.0489
	(-2.19)	(1.04)	(-2.18)	(1.19)	(-2.19)	(1.12)	(-2.14)	(1.06)
TG * Post	-0.0399**	-0.0668*	-0.0418**	-0.0710**	-0.0408**	-0.0694**	-0.0418**	-0.0685*
	(-2.40)	(-2.04)	(-2.37)	(-2.23)	(-2.22)	(-2.17)	(-2.30)	(-2.12)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State * Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	102,430	107,772	102,430	107,772	102,430	107,772	102,430	107,772
R^2	0.2706	0.2323	0.2734	0.2345	0.2742	0.2366	0.2738	0.2358
Chow test *F*-statistic	5.94		5.76		5.82		5.81	
p-value	0.01		0.01		0.01		0.01	
Panel C: Failure probability								
Sample split	Failure probability		Failure probability		Failure probability		Failure probability	
	<p50	≥p50	<p50	≥p50	<p50	≥p50	<p50	≥p50
TG	-0.0424	0.0110	-0.0411	0.0191	-0.0407	0.0160	-0.0411	0.0147
	(-0.97)	(0.32)	(-0.95)	(0.52)	(-0.94)	(0.44)	(-0.95)	(0.41)
TG * Post	-0.0510	-0.0608*	-0.0501	-0.0676*	-0.0497	-0.0657**	-0.0499	-0.0658**
	(-1.72)	(-2.08)	(-1.69)	(-2.12)	(-1.67)	(-2.17)	(-1.68)	(-2.16)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State * Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	102,590	102,467	102,590	102,467	102,590	102,467	102,590	102,467
R^2	0.1942	0.2419	0.1948	0.2451	0.1949	0.2465	0.1950	0.2463
Chow test *F*-statistic	12.43		12.35		11.36		11.44	
p-value	0.00		0.00		0.00		0.00	
Panel D: Listed and unlisted banks								
Sample split	Listed banks	Unlisted banks	Listed banks	Unlisted banks	Listed banks	Unlisted banks	Listed banks	Unlisted banks
TG	0.0124	-0.0082	0.0185	-0.0026	0.0209	-0.0058	0.0190	-0.0063
	(0.23)	(-0.26)	(0.34)	(-0.07)	(0.38)	(-0.17)	(0.35)	(-0.19)
TG * Post	-0.0658**	-0.0426*	-0.0664**	-0.0461**	-0.0665**	-0.0440*	-0.0662**	-0.0448*
	(-2.48)	(-1.98)	(-2.45)	(-2.16)	(-2.44)	(-2.05)	(-2.44)	(-2.10)
State * Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	41,268	163,789	41,268	163,789	41,268	163,789	41,268	163,789
R^2	0.2327	0.2315	0.2355	0.2346	0.2365	0.2365	0.2361	0.2359
Chow test *F*-statistic	11.68		14.14		14.52		14.48	
p-value	0.00		0.00		0.00		0.00	
Panel E: Member banks of BHCs and Non BHC banks								
Sample split	BHC members	Non BHC banks	BHC members	Non BHC banks	BHC members	Non BHC banks	BHC members	Non BHC banks
TG	-0.0085	0.0092	-0.0026	0.0141	-0.0058	0.0168	-0.0065	0.0154
	(-0.27)	(0.16)	(-0.08)	(0.25)	(-0.17)	(0.29)	(-0.19)	(0.27)
TG * Post	-0.0428*	-0.0640**	-0.0465**	-0.0644**	-0.0445*	-0.0644**	-0.0451*	-0.0642**
	(-2.00)	(-2.39)	(-2.17)	(-2.36)	(-2.08)	(-2.34)	(-2.12)	(-2.35)
State * Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	137,251	67,806	137,251	67,806	137,251	67,806	137,251	67,806
R^2	0.2316	0.2321	0.2348	0.2347	0.2368	0.2356	0.2362	0.2352
Chow test *F*-statistic	4.2		4.65		4.43		4.56	
p-value	0.05		0.05		0.05		0.05	

Notes: This table reports estimates of equation (6) using the earnings opacity measures EO^1-EO^4 as the dependent variable. Panel A splits the sample at the median level of bank size. Panel B splits the sample at the median of the ratio of real estate loans to total loans. Panel C splits the sample at the median failure probability. Panel D presents results from samples of listed and unlisted banks. Panel E reports results from samples of BHC and non BHC member banks. Except for Panel D and E, the sample splits use the pre-treatment median values to ensure that they are not affected by depositor preference laws. The control variables are LLP_{t-1}, Bank size, Capital ratio, and the Loss dummy. Definitions of the variables are provided in Panel A of Table 2. The sample is restricted to banks in states that enacted depositor preference over the period 1983Q1 to 1993Q2. Standard errors are clustered at the state level and the corresponding heteroscedasticity-robust *t*-statistics are reported in parentheses. ** and * indicate statistical significance at the 5 percent and 10 percent levels, respectively.

Online Appendix

Debtholder Monitoring Incentives and Bank Earnings Opacity

Not for Publication

Appendix A: Details for the introduction of state depositor preference laws

A.1 List of keywords - search strategy for motivation behind state depositor preference laws

The following list of keywords is used in Lexis/Nexis, Factiva, American Banker, Journal State Legislatures, and Business Source Complete.

priority for bank deposits, priority for depositors, depositor priority, depositor preference, priority claim, creditor ranking, bank liquidation, bank failure, liquidation of bank, claim structure for deposits, ranking of depositors, deposit obligation, depositor obligation, claims of depositors, claim structure, priority of claims liquidation priority, liquidation regime, claims to be paid before those of general creditors, pari passu with general creditors, market discipline, enforcement actions, deposit rank, depositor rank, Omnibus Reconciliation Act, earnings opacity, opacity, transparency, opaque.

Our keyword search is constrained to the 12 months prior to the day of the introduction of state depositor preference.

Appendix B: Additional Results and Robustness Tests

Table B.1
Charter Switching

Panel A: Determinants of charter switching

	(1)	(2)	(3)	(4)
Dependent variable	Charter switch	Charter switch	Charter switch	Charter switch
Post	0.0010	0.0010	0.0010	0.0010
	(1.62)	(1.62)	(1.62)	(1.62)
LLP_{t-1}	-0.0000	-0.0000	-0.0000	-0.0000
	(-0.05)	(-0.05)	(-0.05)	(-0.05)
Bank size	0.0005	0.0005	0.0005	0.0005
	(1.08)	(1.08)	(1.08)	(1.08)
Capital ratio	0.0002	0.0002	0.0003	0.0003
	(1.73)	(1.73)	(1.74)	(1.74)
Loss	0.0007*	0.0007*	0.0007*	0.0007*
	(1.87)	(1.88)	(1.87)	(1.87)
EO^1	0.0000			
	(0.31)			
EO^2		0.0000		
		(0.32)		
EO^3			0.0000	
			(0.39)	
EO^4				0.0000
				(0.40)
State FE	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Observations	205,057	205,057	205,057	205,057
R^2	0.0401	0.0401	0.0401	0.0401

Panel B: Excluding banks that switch charter

Dependent variable	EO^1	EO^2	EO^3	EO^4
TG * Post	-0.0737***	-0.0764***	-0.0755***	-0.0761***
	(-3.79)	(-3.86)	(-3.89)	(-3.92)
LLP_{t-1}	0.2916***	0.2489***	0.2760***	0.2334***
	(6.47)	(4.10)	(8.71)	(6.72)
Bank size	0.0178	0.0101	0.0058	0.0098
	(1.06)	(0.60)	(0.34)	(0.59)
Capital ratio	-0.0919***	-0.0931***	-0.0922***	-0.0928***
	(-4.33)	(-4.39)	(-4.25)	(-4.33)
Loss	0.7565***	0.7576***	0.7573***	0.7583***
	(24.75)	(24.76)	(23.86)	(24.09)
State * Quarter FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Observations	197,681	197,681	197,681	197,681
R^2	0.2215	0.2246	0.2263	0.2257

Notes: Panel A reports estimates of equation $s_{bst} = \beta Post_{st} + \gamma X_{bst} + EO^j_{bst} + \delta_b + \delta_s + \delta_t + \varepsilon_{bst}$, where s_{bst} is a dummy variable equal to 1 if bank b in state s switches charter during quarter t, 0 otherwise; $Post_{st}$ is a dummy variable equal to 1 if depositor preference is present in state s during quarter t; X_{bst} is a vector containing the variables LLP_{t-1}, Bank size, Capital ratio, the Loss dummy, δ_b, δ_s and δ_t denote bank-, state- and quarter-fixed effects, respectively; ε_{bst} is the error term. EO^j_{bst} denotes one of the earnings opacity measures, EO^1, EO^2, EO^3 and EO^4. Panel B reports estimates of equation (6) using a sample that excludes banks that switch charter during the sample period. The TG dummy is omitted in this equation because it is captured by the bank-fixed effect. Standard errors are clustered at the state level and the corresponding heteroscedasticity-robust *t*-statistics are reported in parentheses. *** and * indicate statistical significance at the 1 percent and 10 percent levels, respectively.

Table B.2
Failure Model

Dependent variable	(1) Failure dummy
Bank size	-0.0003
	(-1.17)
Capital ratio	-0.0002*
	(-1.92)
NPA	0.0021
	(0.60)
Cost income ratio	-0.0001
	(-1.01)
Cash	0.0001
	(1.10)
State * Quarter FE	Yes
Bank FE	Yes
Observations	205,057
R^2	0.0448

Notes: We report estimates of the equation $fail_{bst} = \gamma_b + \beta_1 Bank\ size_{bst} + \beta_2 Capital\ ratio_{bst} + \beta_3 NPA_{bst} + \beta_4 Cost\ Income\ Ratio_{bst} + \beta_5 Cash_{bst} + \gamma_{st} + \varepsilon_{bst}$, where $fail_{bst}$ is a dummy variable equal to 1 if bank b in state s fails at time t, 0 otherwise; Bank size, Capital ratio, NPA, Cost Income Ratio, and Cash denote bank size, the capital ratio, the nonperforming loans ratio, the cost income ratio, and cash for bank b in state s at time t; γ_b and γ_{st} denote bank and state-quarter-fixed effects, respectively; ε_{bst} is the error term. * indicates statistical significance at the 10 percent level.

Table B.3
Further sensitivity checks: Geographical diversification, macroeconomic shocks, and regulators

Panel A: Geographical diversification				
	(1)	(2)	(3)	(4)
Dependent variable	EO^1	EO^2	EO^3	EO^4
TG * Post	-0.0610***	-0.0644***	-0.0631***	-0.0633***
	(-3.49)	(-3.50)	(-3.51)	(-3.51)
TG * Number of counties	-0.0354***	-0.0289**	-0.0296*	-0.0313**
	(-3.24)	(-2.24)	(-2.11)	(-2.16)
Control variables	Yes	Yes	Yes	Yes
State * Quarter FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Observations	205,057	205,057	205,057	205,057
R^2	0.2218	0.2249	0.2266	0.2261
Panel B: Macroeconomic shocks				
Dependent variable	EO^1	EO^2	EO^3	EO^4
TG * Post	-0.0641***	-0.0666***	-0.0654***	-0.0659***
	(-3.74)	(-3.75)	(-3.70)	(-3.73)
TG * UNEMP	0.0071	0.0081	0.0077	0.0077
	(0.71)	(0.86)	(0.79)	(0.77)
Control variables	Yes	Yes	Yes	Yes
State * Quarter FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Observations	205,057	205,057	205,057	205,057
R^2	0.2217	0.2248	0.2265	0.2259
Panel C: Regulatory agency				
Dependent variable	EO^1	EO^2	EO^3	EO^4
TG * Post	-0.0642***	-0.0668***	-0.0655***	-0.0660***
	(-3.68)	(-3.68)	(-3.68)	(-3.70)
TG * FDIC	-0.1712*	-0.1727*	-0.1713*	-0.1710*
	(-2.01)	(-1.95)	(-1.91)	(-1.94)
Control variables	Yes	Yes	Yes	Yes
State * Quarter FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Observations	205,057	205,057	205,057	205,057
R^2	0.2217	0.2248	0.2265	0.2259

Notes: We report estimates of equation (6) using the earnings opacity measures EO^1-EO^4 as dependent variables. Panel A includes an interaction term between the treatment group dummy and the number of counties a bank operates in. Panel B includes an interaction term between the treatment group dummy and the state unemployment rate, and Panel C includes an interaction term between the treatment group dummy and an FDIC dummy. The control variables are LLP_{t-1}, Bank size, Capital ratio, and the Loss dummy. The sample is restricted to banks in states that enacted depositor preference over the period 1983Q1 to 1993Q2. Standard errors are clustered at the state level, and the corresponding heteroscedasticity-robust *t*-statistics are reported in parentheses. ***, ** and * indicate statistical significance at the 1 percent, 5 percent and 10 percent levels, respectively.

Table B.4
Alternative control group: Matched Sample

Dependent variable	(1) EO^1	(2) EO^2	(3) EO^3	(4) EO^4
TG	0.0105	0.0104	0.0116	0.0120
	(0.11)	(0.11)	(0.12)	(0.13)
TG * Post	-0.1325***	-0.1312***	-0.1317***	-0.1322***
	(-4.18)	(-4.16)	(-4.15)	(-4.16)
LLP_{t-1}	1.3024***	1.3728***	1.3709***	1.3450***
	(5.09)	(5.33)	(5.04)	(4.92)
Bank size	-0.0916	-0.1022	-0.1094	-0.1067
	(-1.41)	(-1.56)	(-1.66)	(-1.63)
Capital ratio	-0.1089***	-0.1084***	-0.1085***	-0.1087***
	(-3.81)	(-3.80)	(-3.78)	(-3.80)
Loss	0.6978***	0.6943***	0.6937***	0.6947***
	(20.35)	(20.65)	(20.72)	(20.61)
State * Quarter FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Observations	77,269	77,269	77,269	77,269
R^2	0.1828	0.1863	0.1879	0.1874

Notes: We report estimates of equation (6) based on a 1:1 nearest neighbor propensity matching strategy using the earnings opacity measures EO^1-EO^4 as dependent variables. Our matched sample pairs one state-chartered bank with one propensity-score matched nationally-chartered bank, resulting in a sample with 77,269 observations. The sample is restricted to banks in states that enacted depositor preference over the period 1983Q1 to 1993Q2. Standard errors are clustered at the state level, and the corresponding heteroscedasticity-robust *t*-statistics are reported in parentheses. *** indicates statistical significance at the 1 percent level.

Table B.5
Tests for anticipation effects and alternative treatment of standard errors

Panel A: Anticipation effects				
	(1)	(2)	(3)	(4)
Dependent variable	EO^1	EO^2	EO^3	EO^4
TG	-0.0007	0.0030	0.0004	0.0002
	(-0.02)	(0.08)	(0.01)	(0.01)
TG * Post	-0.0700***	-0.0712***	-0.0692***	-0.0703***
	(-3.43)	(-3.33)	(-3.33)	(-3.35)
TG * Placebo$_{t-1}$	-0.0257	-0.0203	-0.0184	-0.0193
	(-0.92)	(-0.79)	(-0.72)	(-0.75)
TG * Placebo$_{t-2}$	0.0165	0.0188	0.0182	0.0157
	(0.65)	(0.79)	(0.69)	(0.60)
TG * Placebo$_{t-3}$	-0.0449	-0.0379	-0.0345	-0.0379
	(-0.82)	(-0.69)	(-0.63)	(-0.69)
TG * Placebo$_{t-4}$	-0.0121	-0.0115	-0.0079	-0.0088
	(-0.45)	(-0.41)	(-0.27)	(-0.31)
Control variables	Yes	Yes	Yes	Yes
State * Quarter FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Observations	205,057	205,057	205,057	205,057
R^2	0.2217	0.2248	0.2265	0.2259
Panel B: Bertrand et al. (2004) collapsing technique				
Dependent variable	EO^1	EO^2	EO^3	EO^4
TG	-0.0956	-0.0863	-0.0860	-0.0893
	(-1.47)	(-1.33)	(-1.32)	(-1.38)
TG * Post	-0.0974***	-0.1004***	-0.0995***	-0.0991***
	(-6.21)	(-6.40)	(-6.33)	(-6.32)
Controls	Yes	Yes	Yes	Yes
State * Period FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Observations	11,048	11,048	11,048	11,048
R^2	0.7343	0.7378	0.7407	0.7403

Notes: We report estimates of equation (6) using the earnings opacity measures EO^1-EO^4 as dependent variables. Panel A examines anticipation effects by including placebo dummies at *t-1, t-2, t-3,* and *t-4* interacted with the treatment group dummy, and Panel B uses the collapsing technique described in Bertrand et al. (2004) to mitigate concerns about serial correlation in panels. The data in Panel B contain a before and after period for each bank and we therefore generate period dummy variables, interacted with the state-fixed effects to mirror the state*quarter-fixed effects. The control variables are LLP$_{t-1}$, Bank size, Capital ratio, and the Loss dummy. The sample is restricted to banks in states that enacted depositor preference over the period 1983Q1 to 1993Q2. Standard errors are clustered at the state level, except for Panel B, and the corresponding heteroscedasticity-robust *t*-statistics are reported in parentheses. *** indicates statistical significance at the 1 percent level.