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The Effects of Bank Regulators and External Auditors on Loan Loss Provisions☆

Allison Nicoletti*

The Wharton School, University of Pennsylvania, 3620 Locust Walk, Philadelphia, PA 19104

Abstract

I examine whether bank regulators and external auditors have conflicting effects on loan loss provision timeliness, an accounting choice associated with important economic consequences and a potential conflict between regulators and auditors. In the absence of the other group, auditors and strict regulators are each positively associated with timeliness. However, audits are negatively associated with timeliness when strict regulators are present, consistent with a conflict for which auditors are the dominating group as audited banks attain a similar level of timeliness regardless of the extent of regulatory scrutiny. Collectively, this suggests that regulators and auditors differentially influence loan loss provisions.

Keywords: banks; loan loss provisions; bank supervision; auditors

1. Introduction

This paper investigates how the objectives and incentives of bank regulators and external auditors affect loan loss provisions. Both groups examine financial reports during on-site safety and soundness examinations or financial statements audits and serve an important monitoring role given their access to internal bank information. In particular, regulators and auditors spend considerable time evaluating the loan loss provision, although this focus is driven by their different objectives (Balla, Rose and Romero, 2012). For regulators, the loan loss provision focus stems from safety and soundness concerns and the associated macroeconomic consequences, such as bank lending activity and systemic risk (Beatty and Liao, 2011; Bushman and Williams,

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* *Email address:* anico@wharton.upenn.edu
Tel.: 215-898-2610

2015). The auditor focus is driven by the potential for misstatement given the high inherent risk associated with the loan loss provision estimate (AICPA, 2007). Anecdotal evidence suggests that these distinctions affect bank financial reporting and result in a potential conflict between the objectives of regulators and auditors (Black, 1990; Wall and Koch, 2000; Dugan, 2009), but this has been relatively unexplored by prior literature. This is an important omission given that regulators and auditors are influential in the financial reporting process and that loan loss accounting contributed to the severity of the most recent financial crisis (Barth and Landsman, 2010; Beatty and Liao, 2014; Armstrong, Guay, Mehran and Weber, 2016).

To capture the influence of regulators and auditors on loan loss provisions, I examine differences in loan loss provision timeliness for a sample of commercial banks exhibiting variation in regulatory scrutiny and external audit status. Loan loss provisions are considered to be more timely if provisions are associated with current and future changes in non-performing loans (e.g., Nichols, Wahlen and Wieland, 2009).[1] I focus on this aspect of the loan loss provision for three reasons. First, there is anecdotal evidence suggesting a conflict between regulators and auditors in their influence on the timing of loan loss recognition. Specifically, former Comptroller of the Currency John Dugan argues that auditors restrict the use of judgmental, forward-looking information due to earnings management concerns and a strict interpretation of loan loss accounting standards, which delays recognition until objective information is available (Dugan, 2009). Second, bank regulatory handbooks related to the loan loss provision specifically discuss an application of Generally Accepted Accounting Principles (GAAP) involving the comparison between the loan loss reserve and non-performing loans as well as a loss horizon of one year (OCC, 2012). Third, this is an important accounting choice for banks since delays in recognition are associated with significant economic consequences of particular interest to bank regulators, including more pro-cyclical lending, less discipline from outsiders over risk-taking, and greater contributions to systemic risk (Beatty and Liao, 2011; Bushman and Williams, 2012, 2015).

The effect of regulators and auditors on loan loss provision timeliness depends on each group's objective and incentives. Regulators are charged with protection of consumer and depositor interests as well as the

[1] An important distinction is between recognizing *timely* loan loss provisions and recognizing *larger* loan loss provisions. The former represents the mapping from changes in non-performing loans (that can be positive or negative), which are taken as a relatively non-discretionary measure of underlying loan quality, to the loan loss provision, while the latter represents an increase in the loan loss provision regardless of underlying loan quality. An example of the latter is dynamic provisioning models for which loan loss provisions are built up during good times when loans are not necessarily in non-performing status (Balla and McKenna, 2009). I examine an accounting choice that is more consistent with this notion in Section 6.3.

stability of the financial system (Spong, 2000), suggesting that they would impose more timely loan loss provisions due to the economic effects of delays (e.g., Bushman and Williams, 2015). However, even though the prompt corrective action mandate of the Federal Deposit Insurance Corporation Improvement Act of 1991 (FDICIA) requires timely intervention at problem banks, regulators may not impose more timely loan loss provisions if it does not allow them to engage in forbearance (Gallemore, 2016). Alternatively, auditors express an opinion as to whether the financial statements present fairly in accordance with GAAP. Although auditors are not focused on timeliness *per se*, they do focus on the application of GAAP, which involves determining whether the incurred loss criteria have been met in the current period. Further, auditors may not influence loan loss provision timeliness due to client retention concerns as managers have the ability to change audit firms to obtain more favorable accounting treatment (DeFond and Subramanyam, 1998). They are also subject to reviews during which deficiencies are identified for inappropriate reliance on managerial assumptions (AICPA, 2015), suggesting that they may constrain the use of subjective adjustment factors, which might delay loan loss recognition (Dugan, 2009).

To examine the effects of regulators and auditors on loan loss provision timeliness, I use a sample of commercial banks filing regulatory reports between 1997 and 2005.[2] To obtain variation in audit status, the sample is restricted to private banks with assets below $500 million, the FDICIA mandated audit threshold. This allows for the necessary variation in external audit status while maintaining relative homogeneity across banks in their operations, size, and other regulatory requirements.[3] I match each audited bank to an unaudited bank on key characteristics, including asset size, in an effort to mitigate endogeneity concerns related to audit choice. Importantly, both the audited and unaudited banks in the matched sample also exhibit variation in scrutiny by bank regulators. To measure regulatory scrutiny, I rely on the state-level regulatory index constructed by Agarwal, Lucca, Seru and Trebbi (2014), which uses access to private Federal Reserve Board data and the fact that state-chartered banks are examined in alternating fashion by their federal regulator (Federal Reserve or FDIC) and state regulator. Specifically, this measure identifies state banks located in

[2] The sample period is chosen based on the period for which the Agarwal et al. (2014) regulatory scrutiny index is valid and the timing of the financial crisis. I discuss this in further detail in Section 4.3. It is also important to note that FDICIA requires that regulatory reports follow GAAP, although it grants regulators the power to write alternative accounting guidance provided it is no less stringent than GAAP.

[3] While the focus on relatively small private banks provides the necessary variation for my study, it does potentially limit the generalizability of the results.

states with relatively "strict" state regulators (greater regulatory scrutiny) or "lenient" state regulators (lower regulatory scrutiny).

The design uses national banks as a control group in an effort to address the inherent difficulty of separating discretionary accounting choices from underlying economic conditions. This is particularly important in my setting given that the regulatory scrutiny measure is based on the bank's location. Thus, a simple comparison of banks operating in states with more lenient regulators to those with strict regulators could capture differences in local economic conditions. Further, this would also confound examination of the effect of an audit in lenient states versus strict states if auditors respond differently to varying economic conditions. National banks are a reasonable control group because they are subject to examinations from a consistent regulator, the Office of the Comptroller of the Currency (OCC), and therefore are not affected by the regulatory index. Thus, assuming that national and state banks operating in the same state are similarly affected by local economic conditions and by an external audit, I can distinguish the effects of interest from alternative explanations. I argue that this is a reasonable assumption given that Blair and Kushmeider (2006) discuss how differences between national and state banks are relatively limited in more recent years and that the national and state banks in my sample are similar on observable dimensions.

I present two sets of analyses.[4] The first set focuses on establishing how regulators and auditors each influence loan loss provision timeliness when any conflict between the two groups is likely to be minimal. I find that in the absence of an audit, banks subject to greater regulatory scrutiny are more timely compared to banks subject to lower regulatory scrutiny. This is consistent with broad regulatory objectives given the system-wide benefits of timely loan loss recognition and suggests that other incentives do not fully attenuate this effect. I next document that in the presence of lower regulatory scrutiny, audits are positively associated with loan loss provision timeliness, which indicates that the net effect of auditor objectives and incentives results in more timely loan loss provisions.[5] Collectively, these analyses suggest that in the relative absence of a conflict, the net effect of either regulator or auditor objectives and incentives is to increase loan loss provision timeliness.

The second set of analyses then focuses on the effects of regulators and auditors when any conflict is

[4] See Appendix B for a visual representation of the comparisons.

[5] The positive effect of an audit at lower regulatory scrutiny banks is sensitive to different specifications. I discuss this issue in further detail in Section 5.2.

more likely to be present. As such, this is a joint test of whether a conflict exists as well as which group is the dominating influence in the case of a conflict. I find that an audit is negatively associated with timeliness at banks subject to greater regulatory scrutiny. I also document that there is not a significant effect of greater regulatory scrutiny on loan loss provision timeliness at audited banks. Taken together, these results are consistent with a conflict between regulators and auditors over loan loss provision timeliness and with auditors being the dominating group in the case of a conflict. My focus on an accounting choice may contribute to the ability of auditors to serve as the dominating force. It is important to point out that although these findings are consistent with differences between regulators and auditors with respect to loan loss provision timeliness, they are not interpreted as evidence of either regulators or auditors implementing the incurred loss model improperly.

I perform several additional analyses to corroborate my interpretation. First, I examine variation in loosened credit standards, which is a time when I expect the conflict to heighten, because banks are increasing risk in their loan portfolio (Dell'Ariccia and Marquez, 2006), but objective information likely isn't available to justify the higher loss rates (Dugan, 2009). Second, I perform the analyses utilizing variation within state-chartered banks only to mitigate concerns regarding differences across national and state banks. I rely on the variation across different states documented in Agarwal et al. (2014) as well as the idea that within a state, banks that are located closer to the state bank regulator headquarters likely receive more scrutiny. This latter variation is consistent with prior literature using geographic proximity to capture monitoring intensity (Kedia and Rajgopal, 2011; Berger, Bouwman, Kick and Schaeck, 2016). Finally, I examine a different aspect of loan loss recognition that does not rely on changes in non-performing loans. I focus on how the ratio of loan loss reserves to total loans varies during the credit cycle, which is predicated on the notion that regulator objectives are consistent with larger reserves in "good" times. The results of these additional tests largely corroborate the main findings, although the effect of an audit at lower regulatory scrutiny banks is less robust in these alternative analyses.

The primary contribution of this paper is to examine how regulators and auditors influence loan loss provision timeliness and to empirically test whether a conflict exists regarding this accounting choice. Despite the substantial focus on the consequences of loan loss provision discretion in prior literature, little attention has been paid to the role of the external audit and supervision functions or to the determinants of loan loss

provision timeliness (Beatty and Liao, 2014; Bushman, 2014). This is important given that the different objectives and incentives of regulators and auditors may put banks in a difficult position during the financial reporting process, which is consistent with anecdotal evidence. For example, *Independent Banker* states that some banks struggle to balance the conflicting advice given by regulators and auditors by explaining that, "community bankers feel caught between examiners telling them to pour more money into loan loss reserves and auditors telling them they shouldn't" (Gamble, 2008, p.50). My results suggest that the objectives and incentives of regulators and auditors shape their interpretation of GAAP, which affects the timing of loan loss recognition, and that their influence is not always in agreement. Although I am unable to draw further inferences regarding the consequences of this arrangement for the banking system, this result has implications for managers, regulators, auditors, and groups involved in the oversight of the audit profession in terms of understanding the differing effects of groups involved in bank monitoring.[6]

This paper also contributes to a growing literature investigating the effects of different regulators on bank-level outcomes. Agarwal et al. (2014) identify heterogeneity within bank regulators by showing that federal regulators assign higher (worse) CAMELS ratings relative to state regulators. Costello, Granja and Weber (2016) conclude that stricter state regulators increase the transparency of bank financial reporting by enforcing income-decreasing restatements. More related to the current study, Bischof, Daske, Elfers and Hail (2016) use an international setting to examine how banking and securities regulators differ in the enforcement of risk disclosure requirements under International Financial Reporting Standard (IFRS) 7 and Basel II. I complement these papers by examining the role of bank regulators and auditors in implementing loan loss accounting under GAAP at private U.S. banks. This setting and associated cross-sectional variation allow for examination of separate auditor and regulator effects as well as the interaction between the two monitoring parties. In addition, focusing on U.S. bank loan loss provisions ensures that both groups oversee implementation of the same accounting rules and reduces concerns related to cross-country differences.

[6] In June 2016, the Financial Accounting Standards Board (FASB) issued a new standard implementing the Current Expected Credit Loss (CECL) model, which requires recognition of expected losses over the life of the loan by removing the probable threshold and expanding the information set to consider. This is expected to increase flexibility and discretion (e.g., see `http://www.aba.com/Advocacy/Issues/Documents/CECL-backgrounder.pdf`). Public and private banks are required to adopt the new standard in 2020 and 2021, respectively.

2. Background and Related Literature

2.1. Institutional Background

2.1.1. On-site Safety and Soundness Examinations

Regulators monitor bank financial condition through on-site safety and soundness examinations and off-site monitoring systems, such as the filing of periodic regulatory reports. Commercial banks are required to file the Report of Condition and Income (Call report) on a quarterly basis, regardless of their size, independent audit status, or trading status. The Call report follows GAAP and provides a balance sheet, income statement, and multiple detailed financial schedules, but very limited qualitative disclosure such as footnotes or Management Discussion and Analysis (MD&A) (Badertscher, Burks and Easton, 2018). On-site examinations typically occur on an annual basis although banks meeting certain criteria are permitted to be examined at least once every 18 months.[7] The culmination of the examination is a written report and assignment of CAMELS ratings, which are shared only with bank management.[8]

The type of bank charter determines the agency primarily responsible for supervision and on-site examinations. The Federal Reserve supervises bank holding companies and relies on the examinations performed by the primary supervisor of each subsidiary bank in their evaluation of the consolidated entity (Federal Reserve 2005). National banks are supervised by the Office of the Comptroller of the Currency (OCC). State-chartered banks are supervised both by their state regulator and their federal regulator, which is the Federal Reserve for state-member banks and the Federal Deposit Insurance Corporation (FDIC) for state non-member banks. As specified by the Riegle Community Development and Regulatory Improvement Act of 1994, state-chartered banks are examined on an alternating basis by their respective state and federal regulators.[9] This institutional feature provides the basis for the analysis in Agarwal et al. (2014) and is central

[7] On-site examinations can occur in any of the four fiscal quarters, although the specific dates are not publicly disclosed. During my sample period, banks must meet the following criteria to be eligible for the 18 month cycle: (1) assets below $250 million, (2) CAMELS ratings of 1 or 2, (3) well-capitalized, (4) well-managed, (5) not operating under a formal enforcement action and (6) has not experienced a change in control in the previous 12 months. Although CAMELS ratings are not publicly available, the FDIC notes that between January 1, 1997 and December 31, 2006, approximately 95% of banks were well-capitalized and had a CAMELS rating of 1 or 2 (see `https://www.fdic.gov/deposit/insurance/assessments/priorperiod.html` for more details).

[8] The CAMELS rating refers to capital adequacy (C), asset quality (A), management (M), earnings (E), liquidity (L) and sensitivity to market risk (S). The rating is assigned on a scale from 1 through 5 with 1 representing the lowest regulatory concern and 5 the greatest. Supervisors assign a composite CAMELS rating for the institution as a whole as well as for each of the six components. The composite rating provides the basis for the Agarwal et al. (2014) state-level regulatory index measure.

[9] To be eligible for the alternating exams, banks must have a CAMELS rating of 1 or 2 and not have experienced a change in

to my design. Although banks choose their charter type, differences in operations between national and state banks have decreased significantly in recent years (Blair and Kushmeider, 2006).

2.1.2. Independent Audit Requirements

All publicly traded banks are required to receive an independent audit of the financial statements, but there are different requirements for audits of privately held institutions. The Federal Deposit Insurance Corporation Improvement Act of 1991 (FDICIA) and subsequently issued inter-agency regulatory guidance require banks with assets greater than $500 million to receive an independent audit of the financial statements. Banks that are subsidiaries of a holding company can meet this requirement at the holding company level. Newly-insured or newly-chartered banks are typically required to receive an external audit for the first three years. For these requirements, the appropriate regulatory agency can grant exemptions for subsidiaries of bank holding companies that receive an audit. Regulators also have the ability to require audits for any safety and soundness reason (Dahl, O'Keefe, and Hanweck, 1998).

The above discussion suggests that the majority of audited private banks with assets below the $500 million threshold are receiving an audit on a voluntary basis. Various factors potentially affect the audit choice, but prior literature primarily discusses demand for an audit arising from external parties such as debtholders or other shareholders in more widely held firms or from the desire for accounting expertise (Lo, 2015; Barton, Hodder and Shepardson, 2015). These papers also identify bank size as the most significant determinant of audit choice. Thus, I create a size-matched sample of banks prior to conducting the main tests in order to mitigate observable differences between audited and unaudited banks.

2.2. Loan Loss Accounting

The loan loss provision is the largest accrual, is an important indicator of performance, and requires significant managerial discretion in arriving at an appropriate estimate (Beatty and Liao 2014; FSF 2009). Current U.S. GAAP requires recognition of loan losses when, based on current information and events, it is probable that the bank will be unable to collect all contractual cash flows per the loan agreement. Loans

control in the past 12 months. State non-member banks with assets greater than $250 million are also subject to the alternating examinations but the state and federal regulator typically conduct joint examinations with the lead agency alternating each year (Agarwal et al., 2014). Approximately 6% of the sample observations are state non-member banks exceeding this threshold and results are robust to excluding these observations.

reserved for at the pool level are governed by Statement of Financial Accounting Standards (SFAS) 5, which provides guidance for all loss contingencies.[10] The standard specifies that losses are not recognized unless the loss is probable as of the financial statement date using information available prior to financial statement issuance and the loss can be reasonably estimated. SFAS 114 provides guidance for loans reserved for individually and requires recognition when it is probable that all interest and principal payments will not be received. The accounting for loan losses is referred to as the "incurred loss model" because losses are not recognized unless they have been incurred as of the financial statement date. The standards do not provide guidance on how to assess whether a loan is impaired, leading to the need for discretion in determining whether a loss should be recognized (FSF, 2009).

An extensive literature focuses on the loan loss provision due to its role as the largest accrual and the extent of managerial discretion afforded under the incurred loss model (Beatty and Liao, 2014). Several prior papers examine whether bank managers use discretion in the loan loss provision for earnings management, capital management, or signaling (Kanagaretnam, Lobo and Yang, 2004; Ahmed, Thomas and Takeda, 1999; Kim and Kross, 1998; Collins, Shackelford and Wahlen, 1995; Beatty, Chamberlain and Magliolo, 1995). Collectively, this literature suggests that managers may use discretion to achieve different reporting benchmarks. The focus of this paper and a more recent stream of papers is loan loss provision timeliness, where provisions are considered more timely if losses are recognized concurrently with or in advance of loans becoming non-performing (e.g., Nichols et al., 2009). This literature finds that delayed loan loss recognition is associated with greater opacity, pro-cyclical lending, and contributions to systemic risk (Iannotta and Kwan, 2014; Beatty and Liao, 2011; Bushman and Williams, 2012, 2015).

2.3. Related Literature

Prior literature provides some insight into the effects of auditors and regulators on financial reporting although relatively few studies consider both groups simultaneously. One stream of papers investigates more extreme outcomes of the auditing or examination process. Gunther and Moore (2003) find that supervisory examinations and external audits are positively associated with Call report restatements that result in an upward revision of the loan loss provision. In a related study, Costello et al. (2016) investigate the effect of

[10] The current standards are codified under Accounting Standards Codification (ASC) 450-20 (SFAS 5) and ASC 310-10-35 (SFAS 114).

regulatory leniency, as measured by the regulatory index generated in Agarwal et al. (2014), on the likelihood of an income-decreasing restatement. They find that greater regulatory leniency is associated with lower likelihood of an income-decreasing restatement, particularly in periods leading up to economic downturns. In contrast to these studies, I examine the effects of both regulators and auditors as well as an accounting choice that occurs on a more continuous basis rather than a relatively infrequent outcome of the reporting process.

More related to the current study are papers that examine the ongoing effects of regulators and auditors on financial reporting through their influence on loan loss provisions. Kanagaretnam, Krishnan and Lobo (2010) document a negative relation between abnormal audit fees and discretionary loan loss provisions but only for banks not subject to the internal control provisions of FDICIA or the Sarbanes-Oxley Act (SOX). They interpret these findings as evidence that auditor independence is compromised when auditors receive large unexpected fees and banks are not subject to the internal control provisions of SOX or FDICIA. Conversely, I examine how auditor presence affects the timing of loan loss recognition, rather than loan loss provision outcomes conditional on receiving an audit, as well as the interaction between regulators and auditors.

Rezende and Wu (2014) investigate changes in on-site examination frequency and find that more frequent regulatory exams are associated with lower loan loss provisions, fewer non-performing loans, and lower charge-offs, which they interpret as bank supervision disciplining risk-taking. Dahl et al. (1998) conclude that regulators increase commercial loan charge-offs but do not significantly affect loan loss provisions, while auditors increase loan loss provisions. The differing results of these studies may be attributable to differences in the sample period or identification strategy. Altamuro and Beatty (2010) examine the effects of the internal control provisions of FDICIA on bank accounting quality. They find that banks with assets greater than $500 million experience increased loan loss provision validity and earnings persistence as well as decreased benchmark-beating and conservatism relative to banks not subject to the internal control provisions. My paper contributes to this literature by considering the interaction between regulators and auditors, separating the effects of these two groups from regulation as a whole, and investigating a different accounting choice.

3. Hypothesis Development

3.1. Regulator and auditor effects in relative absence of conflict

The first two hypotheses examine the effect of regulators and auditors individually in the relative absence of a conflict. I examine the effect of greater regulatory scrutiny at unaudited banks as well as the effect of an audit when regulatory scrutiny is lower. An assumption in these predictions is that any conflict between regulators and auditors is likely to be weaker in the presence of lower regulatory scrutiny as I do not observe the effect of an audit in the complete absence of regulatory scrutiny given that all banks are regulated. The ultimate effect of regulators and auditors, respectively, on loan loss provision timeliness in this context depends on each group's objectives and incentives as discussed below.

The first hypothesis focuses on the effect of greater regulatory scrutiny on loan loss provision timeliness at unaudited banks. Through the supervision function, regulators perform on-site examinations during which they determine whether banks are in compliance with regulations, including those targeted at meeting credit demands of the local community, and evaluate whether loans have been properly reserved for based on available information (Gilbert, 1993; OCC, 2012). Timely loan loss recognition is consistent with both the guidance provided in regulatory handbooks and the system-wide benefits documented in prior literature including less pro-cyclical lending and lower contributions to systemic risk (OCC, 2012; Beatty and Liao, 2011; Bushman and Williams, 2015). Further, Bushman and Williams (2012) argue that more timely loan loss provisions allow for greater discipline over bank risk-taking from outside parties compared to other forward-looking provisioning practices.

However, regulators have some flexibility in conducting examinations that may allow them to maximize their own utility (Rosen, 2003). Mishkin (2000) argues that despite the prompt corrective action (PCA) mandate of FDICIA, regulators still have flexibility to engage in forbearance and may do so in the hopes of avoiding blame for bank failure by allowing the bank time to recover. Gallemore (2016) argues that delayed loss recognition allows regulators to engage in forbearance as the bank is more opaque to outsiders, suggesting that regulators may not impose timely loss recognition in order to conceal poor bank performance. Given that the empirical tests are capturing the net effect of the previously discussed objective and incentives, the first hypothesis is stated in null form as follows:

Hypothesis 1 *At unaudited banks, loan loss provision timeliness is not different for banks subject to greater regulatory scrutiny compared to lower regulatory scrutiny.*

The second hypothesis examines the effect of an external audit on loan loss provision timeliness in the presence of lower regulatory scrutiny. Auditors also spend considerable time investigating the loan loss reserve given that it is associated with high inherent risk for misstatement, but they approach this estimate from a different perspective (AICPA, 2007; Balla et al., 2012; Wall and Koch, 2000). The objective of auditors is to determine whether financial statements are presented fairly, in all material respects, in accordance with GAAP, which includes determining whether the financial statements appropriately reflect the economics of the underlying transactions (DeFond and Zhang, 2014). Thus, the effect of an external audit on loan loss provision timeliness is partially driven by reputation or litigation concerns resulting from an audit failure over financial statements that do not present fairly within GAAP. This suggests that an audit may have either a positive effect on timeliness to the extent that more timely loan loss provisions appropriately reflect the losses or a limited effect if timely loan losses are inconsistent with the auditor GAAP interpretation (Dugan, 2009).

Auditor behavior is also affected by continuing client concerns (e.g., Reynolds and Francis, 2001). Thus, an external audit may not affect loan loss recognition as firms can change auditors to obtain more favorable outcomes (DeFond and Subramanyam, 1998). In addition, the American Institute of Certified Public Accountants (AICPA) states that insufficient testing of subjective components of the loan loss provision is a common problem area in bank audits (AICPA, 2015). Negative consequences occur from peer review findings (e.g., Hilary and Lennox, 2005), indicating that auditors may be hesitant to allow the use of subjective adjustment factors in supporting the loan loss provision estimate.[11] This is also consistent with Dugan (2009) who argues that auditors delay loss recognition until objective information is available due to a strict interpretation of the incurred loss model and earnings management concerns. This leads to the next hypothesis, which relates to the net effect of the above described objective and incentives in the presence of lower regulatory scrutiny and is stated in null form as follows:

Hypothesis 2 *At banks subject to lower regulatory scrutiny, loan loss provision timeliness is not different for audited banks compared to unaudited banks.*

[11] Historical loss experience is the typical starting point in determining the loan loss provision, but banks are required to adjust the historical loss rate for subjective factors. Specific examples of subjective adjustment factors include changes in bank lending practices, changes in national or local economic conditions, and changes in the trend of volume or severity of past due loans (Grant Thornton, 2012).

3.2. Regulator and auditor effects in presence of potential conflict

The second set of hypotheses examines the effect of regulators and auditors on loan loss provision timeliness when any conflict is more likely to be present. I investigate this by examining the effect of greater regulatory scrutiny at audited banks and the effect of an audit in the presence of greater regulatory scrutiny. I focus on these comparisons because loan loss provision timeliness at banks subject to greater regulatory scrutiny and an external audit is a function of both groups' influence, making it difficult to disentangle the extent to which each group contributes to the observed outcome. Therefore, these comparisons allow me to draw inferences regarding whether a conflict exists.

There are two important aspects to point out with respect to the following predictions. First, each individual hypothesis has multiple interpretations. Therefore, I rely on the interpretation of both hypotheses collectively in order to draw inferences about any conflict between regulators and auditors. Second, the observed associations involve a joint test of whether a conflict exists and which group's influence dominates. The extent to which a conflict exists depends on the different objectives and incentives faced by each group as discussed in the previous section. In terms of which group dominates, it is plausible that either group may serve as the dominating force. On one hand, the assessment of the loan loss reserve is an important part of the on-site examination and regulators have the ability to influence bank decisions (OCC, 2012; Mishkin, 2000). On the other hand, I focus on accounting discretion, indicating that auditors are charged with attesting to the fair presentation of the financial statements, which does not apply to regulators.

I first examine the effect of greater regulatory scrutiny on loan loss provision timeliness at audited banks. One possibility is that greater regulatory scrutiny at audited banks is associated with a significant incremental effect (positive or negative) on loan loss provision timeliness, which is consistent with a conflict for which the regulator influence dominates that of the auditor. A second possibility is that there is not a significant effect of greater regulatory scrutiny on timeliness at audited banks, which is consistent with either a conflict where auditor objectives and incentives for timeliness dominate those of the regulator or with the lack of a conflict. I next investigate the effect of an audit at banks subject to greater regulatory scrutiny. Documenting an incremental effect of an audit is consistent with a conflict for which the auditor influence dominates that of the regulator. In the case of an insignificant effect of an audit, I am unable to differentiate between a conflict for which the regulator dominates the auditor or with the lack of a conflict. Given these possibilities, the

second set of hypotheses is stated in null form as follows:

Hypothesis 3a *At audited banks, loan loss provision timeliness is not different for banks subject to greater regulatory scrutiny compared to lower regulatory scrutiny.*

Hypothesis 3b *At banks subject to greater regulatory scrutiny, loan loss provision timeliness is not different for audited banks compared to unaudited banks.*

I interpret the above hypotheses as follows. A significant finding for Hypothesis 3a but an insignificant finding for Hypothesis 3b is consistent with the regulator influence over timeliness dominating that of the auditor. An insignificant finding for Hypothesis 3a but a significant finding for Hypothesis 3b is consistent with the auditor influence over timeliness dominating that of the regulator. Finally, an insignificant finding for both Hypothesis 3a and 3b is consistent with a lack of conflict between auditors and regulators.

4. Research Design and Sample

4.1. Main Empirical Measures

A critical aspect of the incurred loss model is determining when a loan is impaired and should be provided for in the loan loss reserve. Bank regulatory handbooks provide guidance regarding loan impairment through the classification of nonaccrual loans, which are typically viewed as a relatively non-discretionary measure of loan portfolio quality. More specifically, the OCC (2012) states that: "...some banks consider a loan impaired if it would be reported as a nonaccrual loan on the report of condition and income. This is a reasonable and appropriate application of the standard."[12] The guide also states: "Many banks consider coverage of one year's losses an appropriate benchmark of an adequate reserve for most pools of loans." This guidance is consistent with prior papers that consider banks to be more timely if losses are recognized concurrently with or in advance of loans becoming non-performing (Nichols et al., 2009; Beatty and Liao, 2011; Bushman and Williams, 2015). Based on both the discussion in bank regulatory handbooks and prior literature, I use the recognition of concurrent (year t) and future (year $t+1$) non-performing loans in the loan loss provision to capture timeliness.[13]

[12] Based on this guidance, I use nonaccrual loans to capture future impaired loans but note that the main results are qualitatively similar if loans 90 days past due and still accruing are also included.

[13] Regulators and auditors have access to information on non-performing loans for the current time period (year t), suggesting that disagreements between them are more likely to arise over recognition of future non-performing loans (year $t+1$). However, even if both groups have access to non-performing loan information for year t, differences may still arise regarding the amount of loss to accrue. Further, results are largely similar if I perform inferences using only the change in non-performing loans in year $t+1$,

Prior literature utilizes either a pooled approach or time-series approach in estimating discretionary loan loss provisions (e.g., Nichols et al., 2009 and Beatty and Liao, 2011, respectively). The bank-specific measure has the advantage of providing a more powerful measure when documenting cross-sectional differences but requires a certain number of observations and may increase measurement error. Conversely, the pooled model does not impose the data constraints of the bank-level measure but restricts the regression coefficients cross-sectionally. My hypotheses involve cross-sectional comparisons of banks based on regulatory scrutiny and audit status. To mitigate data concerns related to the bank-specific measure but allow for cross-sectional differences, I estimate a pooled model with interactions between the cross-sectional variables and changes in non-performing loans.

Beatty and Liao (2014) compare several loan loss provision determinants models and find that the residual term of a base model including changes in non-performing loans, the change in total loans, bank size, and macroeconomic factors has the highest predictive power with respect to future comment letters and restatements related to the loan loss provision. The model used to test my hypotheses is based on this analysis and begins with the following:

$$\begin{aligned} LLP_{i,t} = {} & \tau_t + \beta_1 \Delta NPL_{i,t+1} + \beta_2 \Delta NPL_{i,t} + \beta_3 \Delta NPL_{i,t-1} + \beta_4 \Delta NPL_{i,t-2} + \beta_5 EBLLP_{i,t} \\ & + \beta_6 Tier1_{i,t-1} + \beta_7 Size_{i,t-1} + \beta_8 \Delta Loans_{i,t} + \epsilon_{i,t} \end{aligned} \quad (1)$$

where subscript i indexes the bank, t indexes the year and variables are defined as follows:

LLP = loan loss provision scaled by lagged total loans
ΔNPL = change in non-performing loans scaled by lagged total loans
$EBLLP$ = earnings before the loan loss provision and taxes scaled by lagged total loans
$Tier1$ = Tier 1 risk-based capital ratio
$Size$ = log of total assets
$\Delta Loans$ = change in total loans scaled by lagged total loans

ΔNPL captures changes in the quality of the underlying loan portfolio and $\Delta Loans$ controls for changes in the size of a bank's loan portfolio. I include year fixed effects, τ_t, to control for time effects (e.g., macroeconomic related) common to all banks in a particular year. I augment the model by including earnings before the loan loss provision and taxes ($EBLLP$) to capture earnings smoothing and the Tier 1 risk-based capital ratio ($Tier1$) to capture capital management (e.g., Ahmed et al., 1999; Collins et al., 1995; Beatty et al., 1995). My hypotheses involve examining differences in β_1 and β_2 across banks based on regulatory

but I discuss instances where the results are inconsistent.

scrutiny and external audit status. However, as previously discussed, I also allow β_3 and β_4 to vary with these variables but do not explicitly examine them as part of my hypotheses.

I use audit status to capture the effect of auditors on loan loss provision timeliness. Specifically, $Audit$ is equal to one if the bank receives an external audit or if the bank is a member of a one-bank holding company that receives an external audit and zero otherwise. Capturing regulatory scrutiny is more challenging given that information regarding the specific dates and results of on-site examinations is not publicly available. I rely on a measure constructed in Agarwal et al. (2014) that utilizes a proprietary dataset from the Federal Reserve Board regarding examinations at state-chartered banks. Their empirical approach is based on the fact that the majority of state banks are examined on an alternating basis by their state regulator and their federal regulator (Fed or FDIC). They find that federal regulators of state banks assign higher (worse) CAMELS ratings compared to state regulators and interpret this as state bank federal regulators are "stricter" than state supervisors on average. In supplementary analyses, the authors develop a state-level version of this measure, which involves estimating the difference in CAMELS ratings assigned by state versus federal (Fed or FDIC) regulators in each state ("regulatory index") and is publicly available. To aid in the interpretation of this measure, they show that larger values of the regulatory index (state regulators more lenient compared to federal regulators at state banks) are associated with higher bank failure rates, higher problem bank rates, larger asset sale discounts, and lower Troubled Asset Relief Program (TARP) repayment rates.

I define $Strict$ as an indicator variable equal to one if the bank is located in a state where the regulatory index from Agarwal et al. (2014) is below the median. Given that federal regulators at state banks are stricter than state regulators on average, I interpret banks in states with smaller differences between federal and state regulators as subject to greater regulatory scrutiny relative to states with larger differences between the two. More specifically, state-chartered banks located in states where $Strict = 0$ ("lenient" states) are subject to lower regulatory scrutiny while banks in states where $Strict = 1$ ("strict" states) are subject to greater regulatory scrutiny. Agarwal et al. (2014) examine potential reasons for differences across states given that the regulatory requirements are held constant and conclude that the primary factor driving state regulator leniency is the ramifications of a bank failure to the local economy.

4.2. Research Design

An inherent challenge for my analyses involves disentangling discretionary accounting choices from the underlying economic activity, particularly given that the Agarwal et al. (2014) variation is based on bank location. The specific concern is that differences in local economic conditions, including local economic shocks or differences in loan portfolio composition stemming from the lending needs of the surrounding area, could confound my examination of state banks alone. Specifically, when varying regulatory scrutiny, the timeliness of state banks located in strict states may be different from state banks located in lenient states simply due to differences in local economic conditions rather than differences in regulatory scrutiny. For the analyses holding regulatory scrutiny constant, even though audited and unaudited banks within each subsample are located in the same state, it is plausible that auditors have different responses depending on the underlying economic conditions. Therefore, the concern with examining state banks only is that an audit in lenient states has a different effect on timeliness compared to an audit in strict states due to local economic conditions rather than any conflict between regulators and auditors.

In an attempt to mitigate these concerns, I use national banks as a control group. National banks are a reasonable control group because they are examined solely by the OCC.[14] Thus, $Strict$ captures regulatory scrutiny at state banks only, but state and national banks operating in the same state face similar local economic conditions.[15] There are two implicit assumptions in this design. The first is that OCC supervision is relatively similar across the country, which is consistent with the fact that the OCC conducts supervision on a national basis. The second is that except for the difference in regulators, national and state banks in the same state are similarly affected by local economic conditions and by an external audit.[16] Blair and Kushmeider (2006) discuss how differences between national and state banks have narrowed, particularly following the

[14] $Strict$ does not incorporate the relative strictness of national bank regulators (OCC) into the analysis. There is little research on the relative strictness between federal regulators of national banks (OCC) and federal regulators of state banks (Fed or FDIC). Exceptions include Alexander, Dahl and Spivey (2009) and Matasar and Pavelka (1998), who examine Community Reinvestment Act (CRA) examination results and document that the OCC appears to be more lenient relative to the other federal regulators.

[15] Untabulated analysis verifies that the bank state location listed on the Call report is the state where the bank does the majority of its business, as measured using deposit information from the FDIC's Summary of Deposits (SOD) files.

[16] Several factors mitigate concerns regarding differential effects of an audit at national versus state banks. First, communications with auditors indicate that the audit program does not differ with the bank's charter. Second, although the name of the audit firm is unavailable in the Call report, I can observe this information for banks that are part of a holding company for the last year in my sample period (2005). This analysis reveals that the proportion of banks that are audited by one of the 10 largest or one of the 25 largest audit firms is not significantly different between the national and state banks in my sample.

passage of FDICIA in 1991. I corroborate this by comparing the national and state banks in my sample and showing that they are similar on observable characteristics in Section 5.1. With these assumptions, I argue that loan loss provision timeliness observed for national banks conditional on the state location and audit status represents a reasonable benchmark for the state bank treatment group.

To use national banks as a control group, I define $StateCharter$ as an indicator variable equal to one if the bank is chartered by the state and zero otherwise. The main equation modifies equation (1) by interacting the ΔNPL variables with $StateCharter$.

$$\begin{aligned} LLP_{i,t} = {} & \alpha_0 StateCharter_{i,t} + \alpha_1 \Delta NPL_{i,t+1} + \alpha_2 \Delta NPL_{i,t} + \boldsymbol{\beta_1\ StateCharter_{i,t} * \Delta NPL_{i,t+1}} \\ & + \boldsymbol{\beta_2\ StateCharter_{i,t} * \Delta NPL_{i,t}} + \alpha_3 NPL_{i,t-1} + \alpha_4 StateCharter_{i,t} * \Delta NPL_{i,t-1} \\ & + \alpha_5 \Delta NPL_{i,t-2} + \alpha_6 StateCharter_{i,t} * \Delta NPL_{i,t-2} + \alpha_7 EBLLP_{i,t} + \alpha_8 Tier1_{i,t-1} \\ & + \alpha_9 Size_{i,t-1} + \alpha_{10} \Delta Loans_{i,t} + \tau_t + \phi_s + \epsilon_{i,t} \end{aligned} \tag{2}$$

The above equation is estimated separately for each of the four subsamples related to my hypotheses. Specifically, there are four different variations based on whether the bank is unaudited (UA) or audited (A) and whether the bank is located in a lenient state (L) or in a strict state (S). The hypothesis tests then involve comparing β_1 and β_2, the coefficients on the interaction terms $StateCharter * \Delta NPL_{t+1}$ and $StateCharter * \Delta NPL_t$, across the different subsamples. The comparisons involved in my hypotheses are summarized in Appendix B.

In each subsample regression, α_1 and α_2 capture average loan loss provision timeliness for the national bank control group and are interpreted as the extent of timeliness conditional on both the local economics in those states and bank audit status. The coefficients β_1 and β_2 (collectively, β) then capture the incremental difference for state-chartered banks, which is interpreted as the effect of regulatory scrutiny conditional on an external audit or as the effect of an audit conditional on regulatory scrutiny. In the predictions that follow, I focus on the β coefficients as they account for differences in local economic conditions. The equation also includes state fixed effects, ϕ_s, to capture time invariant state-specific characteristics. To further rule out additional factors that may be driving the results, I use a second specification that augments the regression model by including year fixed effects interacted with the ΔNPL variables. This specification accounts for trends in loan loss provision timeliness that similarly affect all banks in each subsample. Therefore, the use of the national bank control group combined with the interactive fixed effects mitigates concerns

regarding alternative factors driving the results.[17] Finally, continuous variables are winsorized at the 1st and 99th percentiles to mitigate the influence of outliers, and standard errors are clustered by bank due to the bank-specific, persistent nature of loan loss provisions and regulator/audit status (Petersen 2009).

The first set of hypotheses focuses on the individual effects of regulators or auditors when any conflict is expected to be minimal. For Hypothesis 1, which examines whether loan loss provision timeliness is different at unaudited banks subject to lower versus greater regulatory scrutiny, I compare the coefficients in the equation estimated on the subsample of unaudited banks located in lenient states ($\beta_{UA,L}$) to the coefficients estimated for the subsample of unaudited banks located in strict states ($\beta_{UA,S}$). If $\beta_{UA,S}$ is larger than $\beta_{UA,L}$, this suggests that the net effect of strict regulator objectives and incentives is associated with more timely loan loss provisions. If other incentives dominate, these coefficients will be insignificantly different. I use a similar approach to test Hypothesis 2, which examines the effect of an audit in the presence of lower regulatory scrutiny. If the net effect of auditor objectives and incentives increases loan loss provision timeliness, $\beta_{A,L}$ will be larger than $\beta_{NA,L}$. Alternatively, if concerns related to client retention or the incurred loss model interpretation dominate, this difference will be insignificant.

The second set of hypotheses examines the effects of regulators and auditors when any conflict is likely to be present. The documented effects are a function of whether a conflict exists as well as which group's effect dominates in the case of a conflict. Hypothesis 3a compares timeliness at audited banks with lenient versus strict regulators and is tested by comparing $\beta_{A,L}$ and $\beta_{A,S}$. To test Hypothesis 3b, I compare timeliness at unaudited banks located in strict states ($\beta_{UA,S}$) to that at audited banks located in strict states ($\beta_{A,S}$). As discussed in Section 3.2, an insignificant result for each of the hypotheses individually is ambiguous in drawing conclusions regarding a conflict. As such, I draw inferences based on the combined results of these two hypotheses. Specifically, if auditors present a constraint on strict regulators, $\beta_{A,S}$ will be smaller than $\beta_{UA,S}$ (Hypothesis 3b) but $\beta_{A,S}$ may not be significantly different from $\beta_{A,L}$ (Hypothesis 3a). If regulators present a constraint on auditors, I expect $\beta_{A,S}$ to be smaller than $\beta_{A,L}$ (Hypothesis 3a) but an insignificant difference between $\beta_{UA,S}$ and $\beta_{A,S}$ (Hypothesis 3b). Finally, if the groups do not conflict, the difference between both sets of coefficients will be insignificant or positive.

[17] For example, my sample period contains the Enron/Andersen collapse, which is an event that would differentially affect audited versus unaudited banks in certain states (e.g., Texas) for reasons other than the regulator/auditor conflict.

4.3. Sample Selection

The sample selection starts with all banks filing annual Call reports between 1997 and 2005 with positive total assets. The sample begins in 1997 after several changes to on-site examinations took effect. The sample period ends in 2005 due to the timing of the financial crisis and the validity of the Agarwal et al. (2014) regulatory index, which is constructed using data through 2010. Thus, the end year is chosen to ensure that the regulatory index is appropriately capturing the underlying construct and to eliminate the effects of the financial crisis of 2007 - 2009 given that the discretionary loan loss provision model requires leads and lags of changes in non-performing loans and that regulator and auditor behavior may change during the crisis.

The analyses are performed at the bank level rather than the bank holding company level for the following reasons. First, data on audit status at the bank holding company level is not available in the consolidated financial statements filed with the Federal Reserve (FR Y-9C) until 2005. Second, bank holding companies with assets less than $150 million ($500 million) are not required to file the FR Y-9C before (after) March 2006. While bank holding companies below this threshold are required to file parent stand-alone financial statements (FR Y-9), there are limited financial schedules and data on non-performing loans is not available. The remaining sample selection procedures aim to remove banks that are required to receive an external audit or those that likely receive special examination procedures.

The majority of banks are held by holding companies and are affected by audit procedures occurring at the holding company level. However, restricting the sample to stand-alone banks would substantially reduce the sample size (Lo, 2015). To reduce concerns related to audit procedures performed over subsidiary banks of multi-bank holding companies but maintain sample size, I remove banks belonging to a holding company with more than one depository institution. The remaining sample comprises stand-alone banks that are not members of a holding company and one-bank holding companies for which the subsidiary bank typically comprises the vast majority of the consolidated entity's assets.[18] To obtain variation in audit status, I remove publicly traded banks (including subsidiaries of public banks) as well as banks with assets greater than $500 million. This allows me to examine a group of smaller private banks that are more likely to have similar business models and be more comparable on observable dimensions. It also allows me to separate other

[18] Consistent with this, the mean (median) bank total assets as a percentage of bank holding company total assets is 99.5% (99.9%) for the subsample of banks that are part of a one-bank holding company.

factors that are associated with mandatory audits, such as the effects of publicly traded securities.

I next remove banks that are subject to different supervision from regulators or that do not engage in typical domestic deposit-taking and lending activities. This ensures that banks across audit or charter type partitions have relatively similar business models. Banks in this group include those that were recently chartered, credit card banks, industrial banks, and Edge corporations. Further, troubled banks and recently acquired banks typically receive special supervision from regulators and may be required to receive an external audit. Thus, I remove banks that are subject to a formal enforcement action, that do not meet the criteria to be considered well-capitalized, or that were acquired in the previous two years.

The final sample selection step creates a matched sample in order to mitigate observable differences between audited and unaudited banks given that the sample banks are likely receiving an audit on a voluntary basis. I match each audited bank to an unaudited bank with the same charter type (National or State), in the same year, in the same state, closest in asset size.[19] I match on these characteristics directly as they are the most critical characteristics for my design. Specifically, matching on charter type and state results in audited and unaudited banks that face similar regulatory scrutiny and local economic conditions. Further, prior literature documents that bank size is the most significant determinant of audit status (Lo, 2015; Barton et al., 2015) and untabulated analysis reveals that audited banks in my pre-matched sample are significantly larger than unaudited banks. Thus, finding a similarly-sized matched bank controls for both the direct effect of size and additional characteristics that are likely correlated with size. I perform the matching with replacement because the pre-matched sample has fewer unaudited observations relative to those that are audited and because I have several criteria to match on. Moreover, this technique is not sensitive to the order in which observations are matched and allows me to attain relatively good matches.[20]

As previously discussed, I argue that this sample provides a powerful setting to examine my research question given the need for variation in both audit status and regulatory scrutiny while holding the business

[19] The northeast region comprises many states with relatively few observations per state. To maintain sample size, I match northeast banks in states with a large proportion of observations (New York, Pennsylvania and Massachusetts) to a bank in the same state. For the remaining observations, I match each audited bank to an unaudited bank following the same guidelines but only require the control bank to be located in the same region and to have the same value of $Strict$. However, results are similar if I also match all audited observations in the northeast region to an unaudited observation in the same state.

[20] A disadvantage of this procedure is that certain observations may be included as controls multiple times. To assess whether results are sensitive to this choice, I reperform the analysis after removing banks located in the northeast region given that this is the subset of observations having the lowest percentage of unaudited banks relative to audited banks in the pre-matched sample. Results are qualitatively similar to those presented in the main analyses.

model and environment relatively constant. Nonetheless, my focus on small, privately-held banks does present possible generalizability issues. Although the results may generalize to larger, private banks since their environments are relatively similar, it is not necessarily clear whether or how these results would generalize to public banks. More specifically, although public and private banks face similar regulatory demands from their primary regulator, public banks are also subject to additional regulatory factors, such as oversight by the Securities and Exchange Commission (SEC). Thus, I acknowledge that the ability to draw inferences for a broader sample of banks from this setting may be limited.

5. Descriptive Statistics and Results

5.1. Descriptive Statistics

Table 1, Panel A provides descriptive statistics for the pooled sample. The table shows that 77.8% of the observations are state-chartered banks, which indicates that approximately 22% are national banks. By construction, half of the observations receive an external audit, which is similar to the percentage of observations receiving an audit in the pre-matched sample (53%). Panel B of Table 1 provides Pearson and Spearman correlations. The table reveals that the matching procedures reduce the Pearson correlation between $Audit$ and $Size$ (0.41 prior to matching and 0.05 after).

(Insert Table 1 here)

To assess whether audited and unaudited as well as national and state banks are similar on observable dimensions, I compute normalized differences for all covariates. The normalized difference is a means of assessing covariate overlap between groups and differences greater than 0.25 may be indicative of specification sensitivity in the linear regression model (Imbens and Wooldridge, 2009). Panel A of Table 2 provides descriptive statistics separately for national and state banks, partitioned by regulatory scrutiny. The table reveals that all differences within each $Strict$ subsample are below the 0.25 threshold, indicating that national banks are a reasonable control group. Panel B provides a similar comparison for audited and unaudited banks and also shows that all differences are below the 0.25 threshold, suggesting that the matching procedures mitigate observable differences between audited and unaudited banks.

(Insert Table 2 here)

5.2. Main Results

The results of estimating equation (2) for each of the four subsamples and performing hypothesis tests are provided in Table 3. Panel A presents the estimation for unaudited banks located in lenient states in column (1), unaudited banks located in strict states in column (2), audited banks located in lenient states in column (3), and audited banks located in strict states in column (4). Panel B summarizes differences in the coefficients of interest across the four subsamples and provides the formal hypothesis tests. The coefficients on ΔNPL_{t+1} and ΔNPL_t represent the extent of timeliness for the control group (national banks) in each of the subsamples. I interpret the magnitude of these coefficients as the extent of timeliness conditional on local economic conditions and audit status. These coefficients are jointly positive and significant in three of the four subsamples.[21] Assuming that national and state banks are similarly affected by local economic conditions and audit status, the incremental coefficients on $StateCharter * \Delta NPL$ in each subsample capture differences in timeliness due to regulatory scrutiny at state banks. In particular, the negative coefficients in column (1) are consistent with the Agarwal et al. (2014) interpretation that lenient state regulators are concerned with the potential ramifications of bank closure on the local economy and do not enforce certain actions.

(Insert Table 3 here)

The first set of hypotheses focuses on the individual effect of either regulators or auditors in the relative absence of any conflict. The test of Hypothesis 1 involves a comparison between columns (1) and (2) of Panel A and indicates that banks subject to greater regulatory scrutiny recognize more timely loan loss provisions as evidenced by the positive and significant joint difference in coefficients between $StateCharter * \Delta NPL_{t+1}$ (0.203) and $StateCharter * \Delta NPL_t$ (0.202). This result indicates that objectives and incentives related to safety and soundness appear to outweigh other incentives, such as regulatory forbearance. Hypothesis 2 involves differences in loan loss provision timeliness for audited versus unaudited banks subject to lower regulatory scrutiny with the relevant comparison being columns (1) and (3) of Panel A. Panel B indicates that the difference between the interaction coefficients is 0.078 and 0.131, which are are jointly different from

[21] This lack of significance in the fourth subsample may be driven by differences in loan type as banks in strict states have a lower proportion of commercial loans, and Bhat, Lee and Ryan (2016) document that commercial loans are more timely relative to either consumer or real estate loans. Importantly, this proportion is similar between state and national banks in each $Strict$ subsample. Further, I perform my tests controlling for different loan types and continue to find similar results.

zero.[22] These results suggest that auditor objectives and incentives for timely loan loss provisions dominate other incentives. Overall, the results for the first set of analyses indicate that greater regulatory scrutiny and an external audit are positively associated with loan loss provision timeliness in the relative absence of a conflict between regulators and auditors.

The second set of hypotheses examines the effect of regulators and auditors when any conflict is more likely to occur. Specifically, Hypothesis 3a examines the effect of greater regulatory scrutiny on loan loss provision timeliness at audited banks. This hypothesis involves a comparison between columns (3) and (4) of Panel A, and the formal test in Panel B indicates that the coefficients are not significantly different from each other. Hypothesis 3b examines the effect of an audit at banks subject to greater regulatory scrutiny and involves a comparison between columns (2) and (4) of Panel A. Panel B shows that the difference in coefficients is -0.123 and -0.086, respectively, which are jointly statistically significant. As previously discussed, I draw inferences based on these two hypotheses collectively given that the interpretation of each hypothesis individually is ambiguous. Taken together, these results are consistent with a conflict between regulators and auditors and more specifically, with the auditor influence dominating that of the regulator.

Table 4 presents the results of the second specification, which interacts the ΔNPL variables with the year fixed effects in order to more explicitly control for trends in loan loss provision timeliness that similarly affect all banks in each subsample. Similar to Table 3, Panel A presents the regression estimation and Panel B provides the statistical comparisons of coefficients across the four different subsamples. The results are similar across the four different hypotheses examined, with the exception of Hypothesis 2 for which the joint significance becomes marginally insignificant. It is important to note that because all banks are subject to some level of regulation, auditing may have less of an effect for banks compared to other industries, particularly given my sample of smaller private banks. Further, it could be that the incorporation of forward-looking information (i.e., ΔNPL_{t+1}) is inconsistent with the auditor interpretation of the incurred loss model, which is part of this paper's motivation (Dugan, 2009). Overall, this table indicates that the results are largely consistent with those reported in Table 3. Given that this specification more explicitly accounts for general trends in timeliness and for the sake of brevity, I continue to include the interactive fixed effects in

[22] The difference for $StateCharter * \Delta NPL_{t+1}$ is not individually significantly different from zero (p-value = 0.195). Thus, the results of this hypothesis are sensitive to examination of ΔNPL in both $t+1$ and t versus only $t+1$ as discussed in Section 4.1.

the remaining specifications.

(Insert Table 4 here)

5.3. Discussion and Interpretation

To depict the collective interpretation of the findings, I present the results from Table 4 in two additional ways. For ease of exposition, I focus on the ΔNPL_{t+1} coefficients in this section but note that the discussion also applies to the joint interpretation of ΔNPL_{t+1} and ΔNPL_t coefficients. Figure 1 presents the $StateCharter * \Delta NPL_{t+1}$ coefficient for each of the four subsamples, which captures timeliness at state banks relative to national banks. The figure illustrates two main conclusions that are supported by the empirical results of the previous section. First, greater regulatory scrutiny and audits are each positively associated with timeliness when any conflict between the two groups is expected to be relatively absent. Second, the results indicate that regulators and auditors conflict over loan loss provision timeliness. Specifically, they show that auditors constrain loan loss provision timeliness in the presence of a conflict, because audited banks attain a similar level of timeliness regardless of the extent of regulatory scrutiny.

(Insert Figure 1 here)

As a second presentation of the collective results, Table 5 provides a summary comparison of coefficients across national and state banks. Specifically, the first row of data in the table presents the coefficient on ΔNPL_{t+1} for the national bank control group in each subsample. The second row provides the sum of the main effect coefficient (ΔNPL_{t+1}) and interaction term ($StateCharter * \Delta NPL_{t+1}$) to assess the extent of timeliness for state banks. The bottom rows present the difference between national and state banks and statistical significance of the difference, which is equivalent to the interaction term. The table indicates that within the subsample of unaudited banks, state banks are less timely relative to national banks in lenient states but are more timely in strict states. However, at audited banks, there are not significant differences in timeliness across national and state banks. This further illustrates the conflict interpretation because if a bank is audited, national and state banks located in the same state attain a similar level of timeliness, suggesting that auditors inject consistency into the process.

(Insert Table 5 here)

It is important to point out that although these findings are consistent with a conflict between regulators and auditors, they do not imply that either regulators or auditors are implementing the incurred loss model improperly. Instead, I interpret these results as consistent with differences in interpretation given the discretion provided under GAAP. Thus, I am unable to draw further inferences regarding the optimality of the conflict or the consequences of this arrangement for the banking system.

6. Additional Analyses

In this section, I provide discussion and additional analyses related to the results interpretation and research design choices, including: (1) the conflict interpretation, (2) the national bank control group, (3) the loan loss provision timeliness outcome, and (4) alternative explanations.

6.1. Loosened Lending Standards

If the results related to Hypotheses 3a and 3b are due to a conflict, I would expect them to be strongest in time periods when a conflict between strict regulators and auditors is most likely to arise. I argue this occurs when banks take on more risk in their loan portfolio, but the higher loss rates are not yet reflected in historical information, indicating that subjective adjustments are necessary to recognize more timely loan loss provisions. I expect a conflict in this instance because the regulatory objective of safety and soundness indicates that regulators are particularly concerned when loan portfolio quality decreases (Spong, 2000). Further, auditors may be hesitant to permit subjective adjustments to the loan loss provision given that the AICPA (2015) identifies inadequate testing of "subjective, qualitative components" of the loan loss provision as one of the most commonly identified issues during peer reviews of bank audits. One instance reflecting this type of scenario is when banks loosen their credit standards. Specifically, ceteris paribus, banks lend to less credit-worthy borrowers and realized loss rates increase when lending standards are loosened (Dell'Ariccia and Marquez, 2006). However, the increased loss rates are generally not apparent until several quarters following the changes in lending standards, which affects the ability of historical information to reflect the loss rate on the current loan portfolio and leads to changes in lending practices being identified as a subjective adjustment factor (Grant Thornton 2012; OCC 2012).

To capture variation in lending standards, I follow prior literature and use the net percentage of banks reporting loosened standards in the Federal Reserve Board's Senior Loan Officer Opinion Survey (SLOOS)

(Lown, Morgan and Rohatgi, 2000; Lown and Morgan, 2006). Specifically, I allow changes in non-performing loans to differ with the extent of credit loosening by constructing the variable ΔNPL_Loosen, which is ΔNPL multiplied by the average net percentage of banks reporting loosened standards on small commercial and industrial loans in the current and previous year. Given the nature of the SLOOS data, each loan type and time period results in a net percentage of banks that are either tightening or loosening. Therefore, the coefficient on ΔNPL_Loosen serves as an offset to the main variable ΔNPL and effectively captures the relative difference in timeliness during times of loosening compared to tightening. I modify equation (2) by including additional interactions between $StateCharter$ and each ΔNPL_Loosen variable. If the results in Tables 3 and 4 capture a conflict, I would expect the negative effect documented for Hypothesis 3b to be concentrated in times of loosening credit standards.

Table 6 provides results of estimating a modified equation (2). Similar to the previous table, this equation is estimated separately for the different subsamples. Given that this table is related only to Hypotheses 3a and 3b, only the relevant subsamples are presented. Specifically, Panel A provides the estimation for unaudited banks located in strict states in column (1), for audited banks located in lenient states in column (2), and for audited banks in strict states in column (3). Panel B presents the tests of differences between the coefficients of interest. Similar to Tables 3 and 4, there is not a significant difference in loan loss provision timeliness in times when banks are loosening credit standards for audited banks in strict versus lenient states (Hypothesis 3a). However, the test of Hypothesis 3b reveals that the negative effect is stronger in times when banks are reporting loosened credit standards relative to tightened credit standards. Collectively, this table corroborates the main results and is consistent with auditors restricting the ability of regulators to require timely loan loss recognition (Dugan, 2009).[23]

(Insert Table 6 here)

[23] As an additional test to corroborate the conflict, I examine the probability of recognizing a non-positive (zero or negative) loan loss provision. I find that audited banks subject to greater regulatory scrutiny are *more likely* to recognize a non-positive loan loss provision compared to unaudited banks subject to greater regulatory scrutiny. This is consistent with the conflict interpretation as auditors may not allow the provision of additional losses or may even require banks to reduce provisions if they do not have appropriate documentation to support the current level of the loan loss reserve (Dugan, 2009).

6.2. Within State-Chartered Banks Analysis

My research design relies on the use of national banks as a control group. Although I show that the national and state banks in my sample are relatively similar on observable characteristics, my inferences could be confounded if national and state banks are differentially affected by other factors (e.g., local economic conditions). Thus, as an additional test, I remove national banks from the analysis altogether and utilize additional variation at state-chartered banks. Specifically, I take a similar approach to prior literature that uses variation in the distance between firm headquarters and regulator offices to capture variation in regulatory monitoring. For example, Kedia and Rajgopal (2011) exploit variation in the distance between SEC offices and firm headquarters while Berger et al. (2016) use variation in the distance between bank headquarters and the bankers associations' insurance provider. An advantage of this test is that it does not rely on national banks as a control group. However, the distance measure is potentially noisier in capturing regulatory scrutiny and in particular, the interpretation of distance from the regulator office in states with lenient regulators is potentially ambiguous.

To construct a distance variable, I calculate the distance between the bank headquarters zip code, taken from the Call report, and the location of the state regulator, taken from the Conference of State Bank Supervisors (CSBS). I then create an indicator variable, $ShortDist$, which is equal to one if the distance is less than 50 miles and zero otherwise.[24] This is consistent with the findings of Kedia and Rajgopal (2011) who document that much of the effect occurs at firms in a narrow band surrounding the regulator office. I replace the $StateCharter$ variable in equation (2) with $ShortDist$ and estimate this modified equation separately for each of the four subsamples. The analysis uses variation in whether the bank is located in a strict state as well as in how close the bank is to the state regulator headquarters. For example, the test of Hypothesis 1 estimates the difference in timeliness for banks located closer to bank regulator headquarters compared to those located further from headquarters and then compares this difference between banks located in strict versus lenient states.

Table 7 provides the estimation of this regression for each of the subsamples in Panel A and the tests of coefficient differences across the subsamples in Panel B. The table reveals evidence consistent with the

[24] These results are robust to removing banks located in small states for which all of the banks are classified as $ShortDist$ = 1.

previous finding for Hypothesis 1 in that unaudited banks subject to greater regulatory scrutiny are more timely compared to the control group. However, the difference in coefficients with respect to Hypothesis 2 is not significantly different from zero, which may be due to the previously discussed challenge of interpreting distance to the state regulator office in lenient states. Specifically, it is plausible that distance may still be significantly associated with timeliness despite the fact that regulators are more lenient. Finally, the results related to the conflict interpretation are consistent with those reported in the main analysis. Collectively, these results largely suggest that the use of the national bank control group is not driving the results.

(Insert Table 7 here)

6.3. Loan Loss Reserves over the Credit Cycle

I examine loan loss provision timeliness given my interest in whether auditors and regulators conflict over forward-looking information that is permitted under GAAP and given that the comparison between loan loss provisions and non-performing loans is discussed in bank regulatory handbooks (OCC 2012). However, the model depends on recognition of losses with respect to changes in non-performing loans in the current and following year. To examine whether the results are specific to loan loss provision timeliness, I use an alternative measure where I would still expect a conflict between regulators and auditors but that does not depend on changes in non-performing loans. Specifically, I focus on how the ratio of loan loss reserves to total loans (LLR) varies with the credit cycle. The intuition for this test is based in part on discussion by Dugan (2009) who outlines the regulatory view: "By allowing banks to recognize losses early, it should result in charges against earnings (and possibly capital) during the part of the economic cycle when times are good, as banks anticipate higher future losses when the cycle turns negative, and less such charges when times are bad, as banks anticipate lower future losses when the cycle turns positive." However, auditors may not permit as extensive provisioning in "good" parts of the credit cycle if information is not available to justify the losses.

To capture variation in the credit cycle, I use the average net percentage of banks reporting loosened (positive number) or tightened (negative number) credit standards on small commercial and industrial loans in the current and previous year ($Cycle$). I use this variable based on evidence in Lown et al. (2000) who show that tightened lending standards are associated with economic downturns and that loosened standards

tend to precede downturns. Thus, if loan losses are being reserved for on a timelier basis over the credit cycle, I would expect a positive association with $Cycle$. I regress LLR on $StateCharter$, $Cycle$ and $StateCharter * Cycle$ as well the other determinants of loan loss provisions in equation (2) except that I include the level of NPL rather than changes in non-performing loans given that the dependent variable is the loan loss reserve. The coefficient of interest is that on $StateCharter * Cycle$. I expect to document similarly signed effects to those reported in the main tables if the predictions hold for this outcome variable.

The results of this test are located in Table 8 with each column of Panel A providing the estimation for each of the four subsamples. Panel B provides tests of significance for each of the relevant hypotheses. The results reveal that greater regulatory scrutiny at unaudited banks is associated with larger loan loss reserves during "good" times (Hypothesis 1). The results for Hypothesis 2 are insignificant, which may be attributable to the fact that changes in loan loss reserves throughout the credit cycle may not be consistent with the auditor interpretation of GAAP as auditors may require some kind of objective information (e.g., changes in non-performing loans) to justify loan loss reserves. The tests of Hypotheses 3a and 3b are consistent with the conflict interpretation in that audited banks subject to greater regulatory scrutiny recognize smaller loan loss reserves during "good" times relative to unaudited banks subject to greater regulatory scrutiny. This analysis suggests that the results are generally not restricted to the timeliness measure. However, it is important to point out that these measures are selected based on the fact that a conflict between regulators and auditors is most likely to arise and therefore, my results may not generalize to other characteristics of the loan loss provision.

(Insert Table 8 here)

6.4. Alternative Explanations

An additional concern related to my inferences is that the charter type and audit status are bank choices. Thus, my study may suffer from an omitted correlated variables problem if there are unobservable characteristics associated with the charter type or audit decision and discretionary accounting choices (Roberts and Whited, 2013). I note that the different results documented in each subsample make it more challenging for alternative explanations to explain my results. Nonetheless, I argue that if banks that choose to receive an audit or to obtain a specific charter type have inherently different discretionary accounting choices, I would

expect these differences to arise in other decisions. I use two different decisions that are both considered to be "real" actions, and therefore shouldn't be affected by auditors or regulators, but would potentially capture other managerial incentives. Specifically, I examine the timing of asset sales to manage earnings (Beatty and Harris, 1998) as well as adjustments to investment portfolio maturity in response to changes in interest rates. The former is advantageous due to the fact that it is a decision that also affects earnings while the latter is more related to the speed with which managers respond to changes in underlying economics, which is the basis of the loan loss provision timeliness model. For each of these decisions, I do not find any significant associations with audit status or regulatory scrutiny, which mitigates concerns that unobservable bank characteristics associated with the audit or charter type decision are driving the results.

An additional concern is that regulatory resources are allocated based at least in part on a bank's external audit status. Under this scenario, the negative effect of an audit at greater regulatory scrutiny banks is attributable to the fact that regulators spend less time at audited banks rather than a conflict between auditors and regulators rather than a conflict. To address this concern, I examine whether the conflict result holds for banks more likely to have regulatory resources allocated to them. Hirtle, Kovner and Plosser (2016) use proprietary Federal Reserve data to show that the largest bank holding companies within each Federal Reserve district have a disproportionate amount of hours spent on them, even after controlling for bank size and complexity. Using this intuition, I rank state-chartered banks in each state based on total asset size. I then re-perform my test using the state banks above the median in their state and the national bank control group, with the assumption that these state banks are more likely to have regulatory resources allocated to them. The results indicate that the negative effect continues to hold, suggesting that it is unlikely to be driven by differences in regulator resource allocation.

7. Conclusion

This paper examines how the objectives and incentives of bank regulators and external auditors affect loan loss provisions. The results indicate that greater regulatory scrutiny and external audits are each positively associated with loan loss provision timeliness, relative to the group of unaudited banks subject to lower regulatory scrutiny. I next document that at banks subject to greater regulatory scrutiny, audited banks are less timely relative to unaudited banks, which is consistent with a conflict between regulators and auditors and

with auditors serving as the dominating influence. I corroborate the interpretation by showing that the conflict results are strongest when banks are loosening their credit standards and that the results generally hold using a within state bank design. Although I am unable to draw inferences regarding the optimality of this conflict or the implications for the banking system, my findings suggest that the different parties involved in bank monitoring influence how accounting guidance is applied, which should be of interest to bank managers, regulators, auditors, and those involved in audit oversight.

This paper contributes to our understanding of the role of regulators and auditors in the bank financial reporting process and how their different objectives and incentives affect accounting choices. Despite a large literature on loan loss provision discretion, relatively little is known about the influence of the supervision and auditing functions or the factors that drive loan loss provision timeliness (Beatty and Liao, 2014; Bushman, 2014). My focus on loan loss provision timeliness provides insight into the debate following the financial crisis regarding differences in implementation of the incurred loss model. Specifically, my documentation of differences in loan loss provision timeliness based on regulator and auditor variation has implications for the upcoming Current Expected Credit Loss (CECL) model and suggests that differences in regulator and auditor objectives and incentives may influence the application of the new standard.

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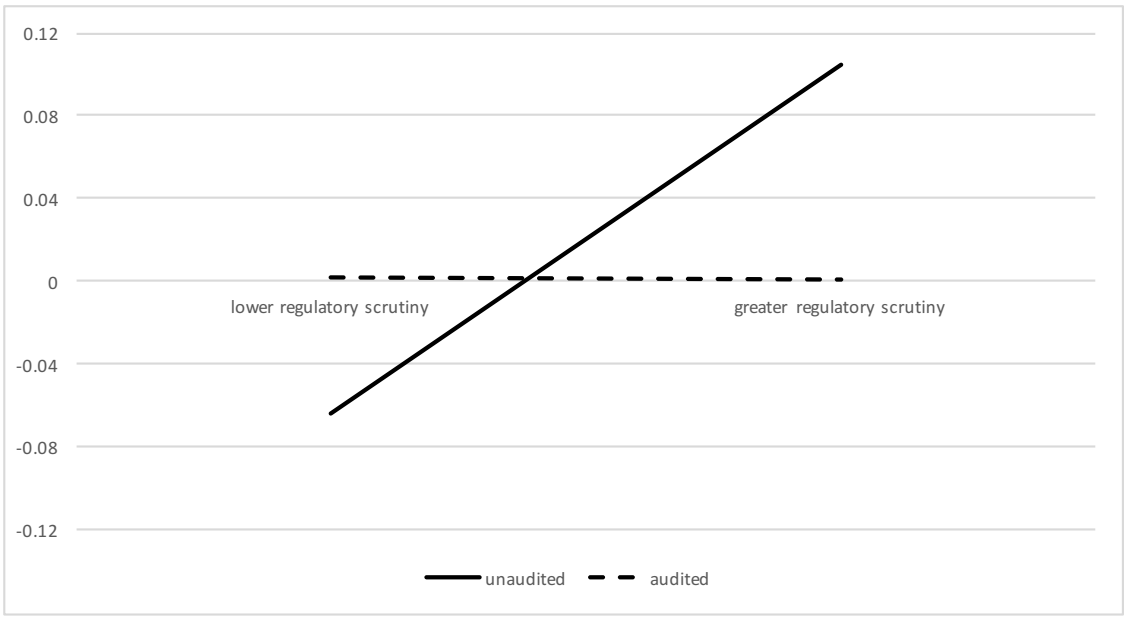
Figure 1: Loan loss provision timeliness (LLPT) coefficient for state banks relative to national banks



This figure presents the $StateCharter * \Delta NPL_{t+1}$ coefficient, which captures LLPT at state banks relative to national banks, in each of the four subsamples from Table 4. The figure indicates that both greater regulatory scrutiny and an audit increase LLPT when any conflict is unlikely to be present. The figure also indicates that regulators and auditors conflict over LLPT and that the auditor effect appears to dominate.

Table 1: Pooled sample descriptive statistics and correlations

Panel A provides descriptive statistics and Panel B provides correlations for the pooled sample of banks between 1997 and 2005. Pearson (Spearman) correlations are presented above (below) the diagonal. Correlations in bold are significant at the 5% level or better. All variables are defined in Appendix A. Continuous variables are winsorized at the 1st and 99th percentiles.

Panel A: Descriptive statistics

Variable	N	Mean	Std Dev	10th Pctl	25th Pctl	Median	75th Pctl	90th Pctl
$Strict$	37,908	0.527	0.499	0.000	0.000	1.000	1.000	1.000
$StateCharter$	37,908	0.778	0.416	0.000	1.000	1.000	1.000	1.000
$Audit$	37,908	0.500	0.500	0.000	0.000	0.500	1.000	1.000
LLP	37,908	0.004	0.005	0.000	0.001	0.002	0.005	0.008
ΔNPL	37,908	0.000	0.007	-0.006	-0.002	0.000	0.002	0.007
$Size$	37,908	11.480	0.775	10.451	10.950	11.535	12.033	12.467
$Tier1$	37,908	0.184	0.090	0.104	0.123	0.157	0.214	0.293
$EBLLP$	37,908	0.033	0.019	0.016	0.022	0.030	0.039	0.051
$\Delta Loans$	37,908	0.098	0.134	-0.038	0.016	0.077	0.150	0.252

Panel B: Correlation matrix

	Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
(1)	$Strict$		**-0.15**	0.00	-0.01	-0.01	**0.01**	**-0.02**	**-0.02**	**-0.07**	**0.05**	**0.01**	**-0.04**
(2)	$StateCharter$	**-0.15**		0.00	0.01	0.00	0.01	0.00	0.01	**0.02**	**-0.05**	**-0.06**	**0.04**
(3)	$Audit$	0.00	0.00		**0.02**	0.00	**0.01**	0.00	0.00	**0.05**	**-0.13**	**-0.12**	**0.07**
(4)	LLP	-0.01	**0.01**	**0.03**		0.00	**0.12**	**0.11**	**0.07**	**-0.02**	**-0.11**	0.01	**0.07**
(5)	ΔNPL_{t+1}	0.00	0.01	0.00	**-0.02**		**-0.22**	**-0.05**	**-0.05**	0.01	**-0.02**	0.00	**0.08**
(6)	ΔNPL_{t}	**0.01**	**0.01**	**0.01**	**0.08**	**-0.21**		**-0.28**	**-0.06**	0.00	**-0.04**	**-0.02**	**0.05**
(7)	ΔNPL_{t-1}	**-0.01**	0.01	0.00	**0.10**	**-0.06**	**-0.24**		**-0.26**	0.00	**-0.04**	**-0.02**	**-0.03**
(8)	ΔNPL_{t-2}	**-0.01**	**0.02**	0.00	**0.08**	**-0.03**	**-0.05**	**-0.23**		**0.01**	**-0.04**	**-0.02**	**-0.02**
(9)	$Size$	**-0.07**	**0.03**	**0.04**	**0.06**	**0.02**	0.00	0.01	**0.02**		**-0.22**	-0.01	**0.15**
(10)	$Tier1$	**0.06**	**-0.03**	**-0.13**	**-0.26**	**-0.03**	**-0.05**	**-0.04**	**-0.04**	**-0.25**		**0.50**	**-0.15**
(11)	$EBLLP$	**0.01**	**-0.07**	**-0.12**	**0.07**	0.00	**-0.01**	-0.01	**-0.01**	0.01	**0.36**		0.00
(12)	$\Delta Loans$	**-0.05**	**0.03**	**0.07**	**0.11**	**0.08**	**0.05**	**-0.04**	**-0.03**	**0.17**	**-0.21**	**0.05**	

Table 2: Subsample descriptive statistics

This table provides descriptive statistics separately for national vs. state-chartered banks in Panel A and unaudited vs. audited banks in Panel B. All variables are defined in the appendix. Continuous variables are winsorized at the 1st and 99th percentiles. The last column presents the normalized difference to assess the covariate balance between the subsamples and is calculated as follows: $\frac{\bar{X}_a - \bar{X}_b}{\sqrt{s^2{}_a + s^2{}_b}}$ where $\bar{X}$ and s^2 are the sample mean and sample variance. All values are below the recommended 0.25 threshold (Imbens and Wooldridge 2009).

Panel A: National vs. state-chartered banks

		National		State		
	Variable	**Mean**	**Std Dev**	**Mean**	**Std Dev**	**Normalized Diff**
Strict = 0	ΔNPL	0.000	0.008	0.000	0.007	0.01
	$Size$	11.584	0.710	11.525	0.805	-0.06
	$Tier1$	0.163	0.070	0.182	0.090	0.16
	$EBLLP$	0.031	0.012	0.033	0.021	0.10
	$\Delta Loans$	0.097	0.126	0.105	0.139	0.05
Strict = 1	ΔNPL	0.000	0.008	0.001	0.007	0.02
	$Size$	11.379	0.724	11.452	0.770	0.07
	$Tier1$	0.206	0.111	0.181	0.083	-0.18
	$EBLLP$	0.038	0.022	0.032	0.016	-0.22
	$\Delta Loans$	0.085	0.130	0.095	0.133	0.05

Panel B: Unaudited vs. audited banks

		Unaudited		Audited		
	Variable	**Mean**	**Std Dev**	**Mean**	**Std Dev**	**Normalized Diff**
Strict = 0	ΔNPL	0.000	0.007	0.000	0.008	0.05
	$Size$	11.498	0.774	11.571	0.804	0.07
	$Tier1$	0.188	0.096	0.169	0.078	-0.16
	$EBLLP$	0.036	0.024	0.030	0.015	-0.22
	$\Delta Loans$	0.094	0.130	0.113	0.142	0.10
Strict = 1	ΔNPL	0.000	0.007	0.000	0.007	0.00
	$Size$	11.396	0.736	11.467	0.778	0.07
	$Tier1$	0.201	0.100	0.175	0.083	-0.20
	$EBLLP$	0.035	0.020	0.032	0.017	-0.14
	$\Delta Loans$	0.082	0.124	0.103	0.139	0.11

Table 3: Difference in loan loss provision timeliness for each of the four subsamples

Panel A: Regression estimation

	$Audit = 0$		$Audit = 1$	
	$Strict = 0$	$Strict = 1$	$Strict = 0$	$Strict = 1$
	(1)	(2)	(3)	(4)
VARIABLES	LLP_t	LLP_t	LLP_t	LLP_t
$StateCharter$	-0.000	0.001	0.000**	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
ΔNPL_{t+1}	0.053	-0.031	0.028	0.037**
	(0.040)	(0.021)	(0.022)	(0.015)
$\boldsymbol{StateCharter * \Delta NPL_{t+1}}$	**-0.078**	**0.125****	**-0.000**	**0.002**
	(0.055)	**(0.054)**	**(0.025)**	**(0.019)**
ΔNPL_t	0.166***	0.047	0.106***	0.111***
	(0.042)	(0.033)	(0.029)	(0.018)
$\boldsymbol{StateCharter * \Delta NPL_t}$	**-0.104****	**0.098**	**0.027**	**0.012**
	(0.052)	**(0.060)**	**(0.032)**	**(0.023)**
ΔNPL_{t-1}	0.152***	0.048*	0.090***	0.115***
	(0.037)	(0.027)	(0.023)	(0.017)
$StateCharter * \Delta NPL_{t-1}$	0.000	0.062	0.036	0.027
	(0.050)	(0.048)	(0.025)	(0.022)
ΔNPL_{t-2}	0.095***	0.020	0.032	0.073***
	(0.025)	(0.029)	(0.021)	(0.013)
$StateCharter * \Delta NPL_{t-2}$	0.024	0.056	0.050**	0.009
	(0.039)	(0.036)	(0.023)	(0.017)
$EBLLP_t$	-0.015	0.027**	0.029***	0.012
	(0.010)	(0.013)	(0.008)	(0.008)
$Tier1_{t-1}$	-0.005**	-0.007***	-0.008***	-0.006***
	(0.003)	(0.002)	(0.001)	(0.001)
$Size_t$	-0.000**	-0.000	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
$\Delta Loans_t$	0.000	-0.000	0.002***	0.003***
	(0.002)	(0.001)	(0.001)	(0.001)
Constant	0.011***	0.006**	0.007***	0.008***
	(0.002)	(0.002)	(0.001)	(0.001)
Year Fixed Effects	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes
Observations	8,967	9,987	8,967	9,987
R-squared	0.229	0.127	0.149	0.135

Panel B: Tests of difference in coefficients across subsamples

Regression columns:	(2) - (1)	(3) - (1)	(4) - (3)	(4) - (2)
Hypothesis:	**1**	**2**	**3a**	**3b**
$StateCharter * \Delta NPL_{t+1}$ coeff diff	0.203	0.078	0.002	-0.123
$StateCharter * \Delta NPL_t$ coeff diff	0.202	0.131	-0.015	-0.086
p-value, joint	0.020	0.099	0.912	0.093
p-value, $t+1$ only	0.008	0.195	0.934	0.031

Panel A presents the results of estimating equation (2), which examines the difference in loan loss provision timeliness for state-chartered banks relative to a control group of national banks, separately for each of the four subsamples. Each column contains the following subsamples: unaudited banks located in lenient states in column (1), unaudited banks located in strict states in column (2), audited banks located in lenient states in column (3), and audited banks located in strict states in column (4). All variables are defined in Appendix A, and all continuous variables are winsorized at the 1st and 99th percentiles. Robust standard errors are clustered by bank and are provided in parentheses below the coefficient. Significance at the .10, .05 and .01 level for two-sided tests is denoted by *, ** and ***, respectively.

Panel B presents χ^2 tests of differences in coefficients across the different subsample regressions in Panel A. Specifically, the top of the table indicates which regression columns are compared and the relevant hypothesis. The middle two rows in the table provide the calculated difference between the $StateCharter * \Delta NPL_{t+1}$ and $StateCharter * \Delta NPL_t$ coefficients, respectively, for the indicated regression columns. The bottom two rows provide the associated p-value for the test of joint significance of both $StateCharter * \Delta NPL_{t+1}$ and $StateCharter * \Delta NPL_t$ coefficients as well as the p-value for the significance of $StateCharter * \Delta NPL_{t+1}$ alone.

Table 4: Difference in loan loss provision timeliness including interacted year fixed effects

Panel A: Regression estimation

	$Audit = 0$		$Audit = 1$	
	$Strict = 0$	$Strict = 1$	$Strict = 0$	$Strict = 1$
	(1)	(2)	(3)	(4)
VARIABLES	LLP_t	LLP_t	LLP_t	LLP_t
$StateCharter$	-0.000	0.000	0.000**	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
ΔNPL_{t+1}	0.086	-0.010	0.051	0.009
	(0.083)	(0.043)	(0.033)	(0.028)
$\boldsymbol{StateCharter * \Delta NPL_{t+1}}$	**-0.064***	**0.105*****	**0.002**	**0.001**
	(0.036)	**(0.032)**	**(0.024)**	**(0.019)**
ΔNPL_t	0.153	0.045	-0.004	0.075**
	(0.093)	(0.047)	(0.038)	(0.034)
$\boldsymbol{StateCharter * \Delta NPL_t}$	**-0.066**	**0.046**	**0.040**	**0.012**
	(0.045)	**(0.039)**	**(0.030)**	**(0.023)**
Controls & Interactions	Yes	Yes	Yes	Yes
Year $\times\Delta NPL$ Fixed Effects	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes
Observations	8,967	9,987	8,967	9,987
R-squared	0.281	0.182	0.173	0.140

Panel B: Tests of difference in coefficients across subsamples

Regression columns:	(2) - (1)	(3) - (1)	(4) - (3)	(4) - (2)
Hypothesis:	**1**	**2**	**3a**	**3b**
$StateCharter * \Delta NPL_{t+1}$ coeff diff	0.169	0.066	-0.001	-0.104
$StateCharter * \Delta NPL_t$ coeff diff	0.112	0.106	-0.028	-0.034
p-value, joint	0.002	0.116	0.737	0.016
p-value, $t+1$ only	0.000	0.120	0.970	0.005

Panel A presents the results of estimating equation (2), which examines the difference in loan loss provision timeliness for state-chartered banks relative to a control group of national banks, separately for each of the four subsamples. Each column contains the following subsamples: unaudited banks located in lenient states in column (1), unaudited banks located in strict states in column (2), audited banks located in lenient states in column (3), and audited banks located in strict states in column (4). All additional interactions and controls are included but are suppressed for brevity. All variables are defined in Appendix A, and all continuous variables are winsorized at the 1st and 99th percentiles. Robust standard errors are clustered by bank and are provided in parentheses below the coefficient. Significance at the .10, .05 and .01 level for two-sided tests is denoted by *, ** and ***, respectively.

Panel B presents χ^2 tests of differences in coefficients across the different subsample regressions in Panel A. Specifically, the top of the table indicates which regression columns are compared and the relevant hypothesis. The middle two rows in the table provide the calculated difference between the $StateCharter * \Delta NPL_{t+1}$ and $StateCharter * \Delta NPL_t$ coefficients, respectively, for the indicated regression columns. The bottom two rows provide the associated p-value for the test of joint significance of both $StateCharter * \Delta NPL_{t+1}$ and $StateCharter * \Delta NPL_t$ coefficients as well as the p-value for the significance of $StateCharter * \Delta NPL_{t+1}$ alone.

Table 5: Summary of ΔNPL_{t+1} coefficient at national and state banks in each of four subsamples

	$Audit = 0$		$Audit = 1$	
	$Strict = 0$	$Strict = 1$	$Strict = 0$	$Strict = 1$
Regression column:	**(1)**	**(2)**	**(3)**	**(4)**
$StateCharter = 0$	0.086	-0.010	0.051	0.009
$StateCharter = 1$	0.022	0.095	0.053	0.010
difference	-0.064	0.105	0.002	0.001
p-value	0.072	0.001	0.922	0.949

This table presents the coefficient on ΔNPL_{t+1} from Table 4 separately for national and state banks within each of the four subsamples. The $StateCharter = 0$ row corresponds to the coefficients for the national bank control group in each subsample, i.e., the main effect on ΔNPL_{t+1}. The $StateCharter = 1$ row represents ΔNPL_{t+1} for the group of state banks and is calculated as the sum of the main effect on ΔNPL_{t+1} and the interaction $StateCharter * \Delta NPL_{t+1}$. The difference row corresponds to the coefficient on the interaction term, $StateCharter * \Delta NPL_{t+1}$, while the p-value row provides a test of whether the difference is significantly different from zero.

Table 6: Difference in loan loss provision timeliness in times of loosened credit standards

Panel A: Regression estimation

	$Audit = 0$ $Strict = 1$	$Audit = 1$ $Strict = 0$	$Audit = 1$ $Strict = 1$
VARIABLES	(1) LLP_t	(2) LLP_t	(3) LLP_t
$StateCharter$	0.001	0.000***	-0.000
	(0.000)	(0.000)	(0.000)
ΔNPL_Loosen_{t+1}	0.019	0.002	-0.002
	(0.012)	(0.006)	(0.011)
$\boldsymbol{StateCharter * \Delta NPL\_Loosen_{t+1}}$	**0.025****	**0.004**	**-0.000**
	(0.012)	**(0.005)**	**(0.007)**
ΔNPL_Loosen_t	-0.027*	-0.005	-0.006
	(0.015)	(0.009)	(0.010)
$\boldsymbol{StateCharter * \Delta NPL\_Loosen_t}$	**0.034****	**0.005**	**-0.009**
	(0.014)	**(0.006)**	**(0.007)**
ΔNPL_{t+1}	-0.092*	0.038	0.014
	(0.055)	(0.039)	(0.037)
$StateCharter * \Delta NPL_{t+1}$	0.070**	-0.004	0.002
	(0.031)	(0.029)	(0.021)
ΔNPL_t	0.094	0.006	0.105**
	(0.061)	(0.045)	(0.041)
$StateCharter * \Delta NPL_t$	-0.006	0.033	0.023
	(0.044)	(0.034)	(0.026)
Controls & Interactions	Yes	Yes	Yes
Year $\times \Delta NPL$ Fixed Effects	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes
Observations	9,987	8,967	9,987
R-squared	0.198	0.173	0.142

Panel B: Tests of difference in coefficients across subsamples

Regression columns:	(3) - (2)	(3) - (1)
Hypothesis:	**H3a**	**H3b**
$StateCharter * \Delta NPL\_Loosen_{t+1}$ coeff diff	-0.004	-0.025
$StateCharter * \Delta NPL\_Loosen_{t}$ coeff diff	-0.014	-0.043
p-value, joint	0.330	0.022
p-value, $t+1$ only	0.607	0.078

Panel A presents the results of estimating a regression examining the difference in loan loss provision timeliness in times of loosening credit standards for state-chartered banks relative to a control group of national banks, separately for three subsamples. Each column contains the following subsamples: unaudited banks located in strict states in column (1), audited banks located in lenient states in column (2), and audited banks located in strict states in column (3). All additional interactions and controls are included but are suppressed for brevity. All variables are defined in Appendix A, and all continuous variables are winsorized at the 1st and 99th percentiles. Robust standard errors are clustered by bank and are provided in parentheses below the coefficient. Significance at the .10, .05 and .01 level for two-sided tests is denoted by *, ** and ***, respectively.

Panel B presents χ^2 tests of differences in coefficients across the different subsample regressions in Panel A. Specifically, the top of the table indicates which regression columns are compared and the relevant hypothesis. The middle two rows in the table provide the calculated difference between the $StateCharter * \Delta NPL\_Loosen_{t+1}$ and $StateCharter * \Delta NPL\_Loosen_{t}$ coefficients, respectively, for the indicated regression columns. The bottom two rows provide the associated p-value for the test of joint significance of both $StateCharter * \Delta NPL\_Loosen_{t+1}$ and $StateCharter * \Delta NPL\_Loosen_{t}$ coefficients as well as the p-value for the significance of $StateCharter * \Delta NPL\_Loosen_{t+1}$ alone.

Table 7: Difference in loan loss provision timeliness using within state-chartered variation

Panel A: Regression estimation

	$Audit = 0$		$Audit = 1$	
	$Strict = 0$	$Strict = 1$	$Strict = 0$	$Strict = 1$
	(1)	(2)	(3)	(4)
VARIABLES	LLP_t	LLP_t	LLP_t	LLP_t
$ShortDist$	-0.001***	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
ΔNPL_{t+1}	-0.006	0.051	0.070**	0.004
	(0.090)	(0.060)	(0.035)	(0.030)
$\boldsymbol{ShortDist * \Delta NPL_{t+1}}$	**-0.018**	**0.262*** **	**-0.009**	**-0.007**
	(0.049)	**(0.065)**	**(0.025)**	**(0.027)**
ΔNPL_t	0.008	0.058	0.038	0.046
	(0.055)	(0.048)	(0.036)	(0.037)
$\boldsymbol{ShortDist * \Delta NPL_t}$	**0.085***	**0.174*** **	**0.008**	**0.025**
	(0.046)	**(0.049)**	**(0.030)**	**(0.035)**
Controls & Interactions	Yes	Yes	Yes	Yes
Year $\times \Delta NPL$ Fixed Effects	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes
Observations	7,572	7,169	7,572	7,169
R-squared	0.290	0.242	0.179	0.171

Panel B: Tests of difference in coefficients across subsamples

Regression columns:	(2) - (1)	(3) - (1)	(4) - (3)	(4) - (2)
Hypothesis:	**1**	**2**	**3a**	**3b**
$ShortDist * \Delta NPL_{t+1}$ coeff diff	0.280	0.009	0.002	-0.269
$ShortDist * \Delta NPL_t$ coeff diff	0.089	-0.077	0.017	-0.149
p-value, joint	0.003	0.342	0.922	0.000
p-value, $t+1$ only	0.001	0.874	0.957	0.000

Panel A presents the results of estimating a modified equation (2), which examines the difference in loan loss provision timeliness for state-chartered banks located less than 50 miles from the state regulator office relative to banks further from the regulator's office, separately for each of the four subsamples. Each column contains the following subsamples: unaudited banks located in lenient states in column (1), unaudited banks located in strict states in column (2), audited banks located in lenient states in column (3), and audited banks located in strict states in column (4). All additional interactions and controls are included but are suppressed for brevity. All variables are defined in Appendix A, and all continuous variables are winsorized at the 1st and 99th percentiles. Robust standard errors are clustered by bank and are provided in parentheses below the coefficient. Significance at the .10, .05 and .01 level for two-sided tests is denoted by *, ** and ***, respectively.

Panel B presents χ^2 tests of differences in coefficients across the different subsample regressions in Panel A. Specifically, the top of the table indicates which regression columns are compared and the relevant hypothesis. The middle two rows in the table provide the calculated difference between the $ShortDist * \Delta NPL_{t+1}$ and $ShortDist * \Delta NPL_t$ coefficients, respectively, for the indicated regression columns. The bottom two rows provide the associated p-value for the test of joint significance of both $ShortDist * \Delta NPL_{t+1}$ and $ShortDist * \Delta NPL_t$ coefficients as well as the p-value for the significance of $ShortDist * \Delta NPL_{t+1}$ alone.

Table 8: Difference in loan loss reserves over credit cycle

Panel A: Regression estimation

	$Audit = 0$		$Audit = 1$	
	$Strict = 0$	$Strict = 1$	$Strict = 0$	$Strict = 1$
	(1)	(2)	(3)	(4)
VARIABLES	LLR_t	LLR_t	LLR_t	LLR_t
$StateCharter$	0.001**	0.001	0.000	0.001***
	(0.001)	(0.001)	(0.000)	(0.000)
$Cycle$	1.174	0.719	0.532	0.189
	(1.171)	(0.879)	(0.333)	(0.330)
$\boldsymbol{StateCharter * Cycle}$	**-0.242**	**0.480***	**-0.098**	**0.016**
	(0.226)	**(0.260)**	**(0.094)**	**(0.070)**
Controls & Interactions	Yes	Yes	Yes	Yes
Year $\times NPL$ Fixed Effects	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes
Observations	8,967	9,987	8,967	9,987
R-squared	0.343	0.247	0.252	0.260

Panel B: Tests of difference in coefficients across subsamples

Regression columns:	(2) - (1)	(3) - (1)	(4) - (3)	(4) - (2)
Hypothesis:	**1**	**2**	**3a**	**3b**
$StateCharter * Cycle$ coeff diff	0.722	0.144	0.114	-0.464
p-value	0.036	0.556	0.331	0.089

Panel A presents the results of estimating a regression examining how loan loss reserves vary with the credit cycle for state-chartered banks relative to a control group of national banks, separately for the four subsamples. Each column contains the following subsamples: unaudited banks subject located in lenient states in column (1), unaudited banks located in strict states in column (2), audited banks located in lenient states in column (3), and audited banks located in strict states in column (4). All additional interactions and controls are included but are suppressed for brevity. All variables are defined in Appendix A, and all continuous variables are winsorized at the 1st and 99th percentiles. Robust standard errors are clustered by bank and are provided in parentheses below the coefficient. Significance at the .10, .05 and .01 level for two-sided tests is denoted by *, ** and ***, respectively.

Panel B presents χ^2 tests of differences in coefficients across the different subsample regressions in Panel A. Specifically, the top of the table indicates which regression columns are compared and the relevant hypothesis. The middle row provides the calculated difference between the $StateCharter * Cycle$ coefficients, respectively, for the indicated regression columns. The bottom row provides the associated p-value for the test of significance.

Appendix A: Variable Definitions

Variable	Definition	Calculation
$Audit$	Indicator variable equal to 1 if the bank receives an audit or is a member of a holding company that receives an audit; 0 otherwise	Equal to 1 if $RCFD6724$ = 1 or 2; 0 otherwise
$Cycle$	Average percentage of banks reporting net loosened (net tightened) credit standards for small C&I loans in the current and previous year	from Senior Loan Officer Opinion Survey
$EBLLP$	Earnings before the loan loss provision, taxes and extraordinary items scaled by lagged total loans	$\frac{RIAD4301_t + RIAD4230_t}{RCFD2122_{t-1}}$
LLP	Loan loss provision scaled by lagged total loans	$\frac{RIAD4230_t}{RCFD2122_{t-1}}$
LLR	Loan loss reserve scaled by total loans	$\frac{RCFD3123_t}{RCFD2122_t}$
$\Delta Loans$	Change in total loans scaled by lagged total loans	$\frac{RCFD2122_t - RCFD2122_{t-1}}{RCFD2122_{t-1}}$
ΔNPL	Change in nonaccrual loans scaled by lagged total loans	$\frac{RCFD1403_t - RCFD1403_{t-1}}{RCFD2122_{t-1}}$
ΔNPL_Loosen	ΔNPL multiplied by the average net percentage of banks reporting loosened credit standards for small C&I loans in the current and previous year	from Senior Loan Officer Opinion Survey
$ShortDist$	Indicator variable equal to one if the distance between the bank's headquarters and state regulator office is less than 50 miles; 0 otherwise	$RSSD9200$ (bank location); Conference of State Bank Supervisors (state regulator location)
$Size$	Log of total assets	$log(RCFD2170_t)$
$StateCharter$	Indicator variable equal to 1 for state banks; 0 for national banks	Equal to 1 if RSSD9055 = 0; 0 otherwise
$Strict$	Indicator variable equal to 1 if the bank is located in a state where the regulatory index is below the median (stricter state regulators); 0 otherwise.	from Agarwal et al. (2014)†
$Tier1$	Tier1 risk-based capital ratio, defined as the ratio of Tier 1 capital to risk-weighted total assets	$\frac{RCFD8274_t - RCFDC228_t}{RCFDA223_t - RCFDB504_t}$

All variables are defined using annual Call report data items unless otherwise specified.

† Available at: http://faculty.chicagobooth.edu/amit.seru/research/data.html

Appendix B: Summary of Predictions

		Audit		
		No (UA)	Yes (A)	
Regulatory	Lower - Lenient states (L)	$\beta_{UA,L}$	$\beta_{A,L}$	**H2**
Scrutiny	Greater - Strict states (S)	$\beta_{UA,S}$	$\beta_{A,S}$	**H3b**
		H1	**H3a**	

This matrix provides a summary of the groups involved in each hypothesis. Specifically, each hypothesis involves a comparison across either the regulatory scrutiny partition (lower vs. greater regulatory scrutiny) or the audit partition (unaudited vs. audited). The research design then involves comparing the difference in loan loss provision timeliness (LLPT) at state banks across the relevant partition (i.e., regulatory scrutiny or audit) to the same difference for a control group of national banks. As such, β in the matrix above captures β_1 and β_2 from the following baseline regression:

$$\begin{aligned} LLP_{i,t} = {} & \alpha_0 StateCharter_{i,t} + \alpha_1 \Delta NPL_{i,t+1} + \alpha_2 \Delta NPL_{i,t} + \boldsymbol{\beta_1\ StateCharter_{i,t} * \Delta NPL_{i,t+1}} \\ & + \boldsymbol{\beta_2\ StateCharter_{i,t} * \Delta NPL_{i,t}} + \alpha_3 NPL_{i,t-1} + \alpha_4 StateCharter_{i,t} * \Delta NPL_{i,t-1} \\ & + \alpha_5 \Delta NPL_{i,t-2} + \alpha_6 StateCharter_{i,t} * \Delta NPL_{i,t-2} + \sum_i Controls + \tau_t + \phi_s + \epsilon_{i,t} \end{aligned}$$

Given this framework, all of the tests involve the relative extent of timeliness for state banks compared to the control group of national banks. For example, H1 involves comparing $\beta_{UA,S}$ (the difference in LLPT between unaudited state and national banks in strict states) to $\beta_{UA,L}$ (the difference in LLPT between unaudited state and national banks in lenient states).