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Gauri Bhat, Hemang Desai, Christoffer Koch, Erik J. Mayer and Nitzan Tzur-Ilan

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Audit Partners and Loan Loss Provisioning: Evidence from U.S. Bank Holding Companies*

Gauri Bhat†, Hemang Desai‡, Christoffer Koch±, Erik J. Mayer§ and Nitzan Tzur-Ilan°

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Abstract

Using confidential data on audit partner names from 2006 to 2019 for bank holding companies (BHCs), we examine partners' impact on loan loss provisioning. Using a fixed effects approach, we find some evidence that individual partners affect provisioning, especially at public BHCs and BHCs audited by large audit firms. However, these results do not obtain in the post-financial crisis period. We also examine the association between partner tenure and provisioning. We find that allowance is increasing in partner tenure, particularly for public BHCs and small BHCs. The result for small BHCs is obtained in the post-financial crisis period as well. Thus, partner heterogeneity is more likely to manifest during periods of high economic uncertainty, but otherwise, there seems to be consistency in approach to audits across partners within a firm. Our results also suggest that audit scrutiny increases as the partners stay longer with the engagement, especially for small BHCs.

Data Availability: Data (except the confidential audit partner names) are available from the public sources cited in the text.

JEL Classifications: G21, J01, J44, L84, M21, M42

Keywords: auditing, banking, audit partner names, audit engagement, PCAOB, audit partner tenure, auditor rotation

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†Corresponding Author: Gauri Bhat, Southern Methodist University, gbhat@smu.edu.

‡Hemang Desai, Southern Methodist University, hdesai@smu.edu.

±Christoffer Koch, International Monetary Fund, ckoch2@imf.org.

§Erik J. Mayer, University of Wisconsin-Madison, erik.mayer@wisc.edu.

°Nitzan Tzur-Ilan, Federal Reserve Bank of Dallas, nitzan.tzurilan@dal.frb.org.

I. INTRODUCTION

Using a long-time series of confidential data on the identity of audit partners for U.S. bank holding companies (BHC), we address two questions: whether individual audit engagement partners have an impact on loan loss provisioning and whether the length of partner tenure affects loan loss provisioning. Understanding whether partner heterogeneity (or style) and partner tenure affect financial reporting outcomes is of great interest to academics, practitioners, and, most importantly, regulators and standard-setters.[1] However, most of the evidence on these topics is from foreign jurisdictions since the names of the audit engagement partners became available for public companies in the U.S. only after 2017.[2]

Our first question addresses the heterogeneity between partners, which is particularly important considering the controversy surrounding Rule 3211, Auditor Reporting of Certain Audit Participants. In 2015, the Public Company Accounting Oversight Board (PCAOB) mandated Rule 3211, requiring the disclosure of the names of audit engagement partners in Form AP by registered audit firms for audit reports issued on or after January 31, 2017. This rule was adopted after multiple rounds of public comments, as well as comments from the PCAOB's Standing Advisory Group and Investor Advisory Group. The primary driver of this rule was the PCAOB's firmly held view that the quality of individual audit engagements varies *within* audit firms despite the existence of firmwide quality control systems. In fact, the PCAOB uses engagement partner history as one of the factors in its risk-based selection of audit engagements for inspections (PCAOB Release No. 2015-008. December 15, 2015).

However, the PCAOB's view is not universally accepted. The audit firms strongly opposed this rule. The firms contend that the entire quality control system within the audit firm stands behind the audit report. The quality control system includes the audit methodology, internal processes and procedures, engagement partner, secondary review partner, and national office consultation partner. These systems are designed to constrain and produce consistency in audits across partners for a given audit client. Thus, identifying an individual audit partner limits the importance of the audit firm (Bob Kueppers, Senior Partner, Global Regulatory and Public Policy, Deloitte LLP at PCAOB meeting, February 2005). According to this alternate view, identifying

[1] We refer to the audit engagement partner as audit partner, AP or partner throughout the study.

2 We are not aware evidence using U.S. data that examines the effect of partner heterogeneity on reporting outcomes, for either non-financial firms or banks. There is evidence using U.S. data on the effect of partner tenure on certain outcomes (example, Gipper et al., 2021). We discuss our contribution relative to other studies in detail in the latter part of Section I.

partners would not improve audit quality but would likely raise liability risks for the audit partners and the audit firms, thereby potentially increasing audit fees.

Since the recent availability of data on the identity of audit partners in the U.S., a few studies have examined the impact of Rule 3211. For example, Cunnigham et al. (2019) use a difference-in-differences approach to provide early evidence that Rule 3211 does not affect audit quality (measured as discretionary accruals, the propensity to misstate, and the likelihood of issuing an incorrect material weakness opinion). Using a pre vs. post design, Burke et al. (2019) found a significant increase in audit quality (measured as discretionary accruals) and a significant decrease in cost (audit fees) and delay (audit report lag) following the disclosure of audit partner names under Rule 3211. However, each study has only one year of data after Rule 3211 became mandatory, which limits the inference that can be drawn from the results.

Given the rule's recency and the lack of a long-time series to estimate individual partner's impact, research examining partner heterogeneity or style in the U.S. is still limited. The extant evidence from foreign jurisdictions suggests that audit partner heterogeneity affects earnings quality, audit quality, and audit fees. Using data on Chinese firms, Gul et al. (2013) document that audit partner style affects accounting quality. Cameran et al. (2020) find that partner fixed effects add to the model's explanatory power, explaining variation in earnings quality, going concern opinions, and audit pricing. Using Australian data, Taylor (2011) finds that audit fees are significantly affected by partners' innate characteristics. However, the study has only one year of data and, therefore, cannot control for audit firms or clients.

Our second question examines the impact of the length of a partner's tenure on the financial reporting outcomes. To put it differently, does a partner's approach to auditing change over the tenure of the audit partner? Our focus is not on the switch between partners or the year of the switch but on the length of the partner tenure. Do the financial reporting outcomes exhibit a trend over the course of the partner's tenure? On the one hand, the longer the relationship between the partner and the client, the more likely the partner would be reluctant to rock the boat and make changes. On the other hand, over time, partners acquire more knowledge about their clients and industry, which could influence their audit approach. This is particularly relevant in the banking industry, which demands a high degree of specialization from auditors to audit complex and subjective accounting estimates.

Studies relating to the impact of partner tenure and rotation on audit quality provide mixed evidence. Many of these studies rely on small proprietary samples (from specific audit firms) or settings outside the U.S. Carey and Simnett (2006), and Ye et al. (2011) use Australian data to document that longer tenure is associated with a lower audit quality (propensity to issue Going Concern reports). Using Taiwanese data, Chen et al. (2008) show that longer tenure is associated with smaller discretionary accruals, suggesting higher audit quality. Manry et al. (2008) use U.S. data from three large international audit firms and show that discretionary accruals are negatively associated with the lead audit partner's tenure. Litt et al. (2014) find that audit quality declines following partner rotation, while Lennox et al. (2014) and Laurion et al. (2017) find that audit quality improves following mandatory partner rotation. A recent study by Gipper et al. (2021) uses confidential data on partner names as well as other details such as audit tenure, audit fees, audit hours, etc., for public companies from the PCAOB for the period 2008-2014 and does not find evidence of any significant change in audit quality or earnings quality over the partner tenure or following mandatory partner rotation. However, their sample comprises non-financial firms and firms audited by the Big 6 auditors.[3]

Overall, the extant evidence on whether audit partner heterogeneity and partner tenure affect reporting outcomes is mostly from jurisdictions outside the U.S. The evidence from the studies that use U.S. data either has a limited time series or has examined non-financial public firms. However, since March 2005, the BHCs in the U.S. have been required to disclose the name of the audit engagement partner in their regulatory filings, permitting a relatively long period to test for partners' impact. We leverage this confidential data collected by the Federal Reserve but not released to the public to create an audit partner-BHC matched dataset and track individual partners across different BHCs. Thus, our study examines both public and private bank holding companies (BHCs) that are audited by larger as well as smaller audit firms, and we examine a longer period (2006 to 2019) to provide out-of-sample evidence on the impact of audit partner heterogeneity and tenure on financial reporting outcomes.

Using the banking setting offers four significant advantages. First, it allows us to examine a set of firms that have a similar business model (deposit-taking and loan-making) and where there is wide agreement on the key financial reporting outcome that is subject to managerial discretion

[3] Though Gipper et al. (2021) do not explicitly exclude banks, their research design ends up excluding banks from their sample. For example, their regressions use operating accruals to measure earnings quality and COGS and A/R as control variables. This would necessarily cause banks to drop out of their analyses. We have confirmed this with the authors.

and, consequently, auditor and regulatory scrutiny - loan loss provisioning (Beatty & Liao, 2014). Second, as mentioned above, while the names of the audit partners have been available for U.S. listed public companies since 2017, banks have had to disclose the names of their audit partners to the Federal Reserve since 2005, thus yielding a long time series and a large number of observations per audit partner. Third, our sample period spans from 2006 to 2019. An advantage of such a long time series is that it includes the financial crisis period 2008-2009 and a reasonably long period following the crisis (2010-2019). Given that the crisis period had heightened uncertainty for banks (with a significant increase in provisions and allowances), we can test whether partners' impact is different during normal economic periods vs. periods of heightened economic uncertainty, as auditor judgment is more likely to be critical during such uncertain periods. Finally, while partner names are available only for public companies on form AP, they are available for public and private banks as both must disclose this information to the Federal Reserve. Private banks are typically smaller but account for a vast majority of the banks in the U.S. These banks are also more likely to be audited by smaller audit firms. Small, private community banks tend to be less complex in their business models, and their models to estimate allowance are not as sophisticated as the larger banks. [4] Thus, using the banking industry as a setting allows us to examine a unique setting where the impact of a partner may be more pronounced. [5]

We focus on loan loss provisioning as our proxy for financial reporting outcomes. Loan loss provisioning is an ideal measure to test for the impact of audit partners as both the auditors and the regulators spend considerable effort evaluating and scrutinizing this inherently uncertain estimate. Moreover, a significant difference exists between the intent of the banks, their auditors, and the regulators regarding the magnitude of loan loss provisioning (Balla, Rose & Romero, 2012; Dahl, 2013; Nicoletti, 2018).[6] Bank managers value flexibility, and the regulators concerned with the safety and soundness of the bank view loan loss allowance as the 'war chest' that a bank puts in place based on their estimation of likely losses on their loan portfolio well before the loans are charged off. Thus, the regulators often give banks more latitude in exercising judgment in

[4] One of the unique features of the U.S. banking industry is the significant number of small community banks that are private. For example, in year 2013, which falls in the middle of our sample period, 92% of the total BHCs were small community BHCs with total assets below $10 billion, 71% of which are private community BHCs.

[5] Based on our conversations with bank examiners, smaller banks tend to be more conservative in estimating allowance and tend to rely on less sophisticated models to estimate allowance as they lack in-house risk modeling expertise. As a result, they often do not challenge recommendations from auditors. Moreover, they are very relationship based and have fewer options for auditor shopping.

[6] Our sample comprises BHCs, which come under the supervision of the Federal Reserve Board, thus removing the variation in the regulators and any role that may play in influencing the loan loss provisioning.

establishing provisions and recommend that provisioning be more 'forward-looking' (Dugan, 2009).

In contrast, the auditors' approach aligns with the Financial Accounting Standards Board's (FASB) position. Auditors evaluate whether loan loss provisioning is based on current information and events. Given the inherent uncertainty in the estimation process and the potential to manipulate these estimates, auditors often restrict the use of non-historical information to constrain banks' ability to use loan loss provisions to manage earnings (Black, 1990; Wall & Koch, 2000; Balla et al., 2012; Nicoletti, 2018).[7] While banks are permitted to use non-historical, forward-looking judgmental factors to justify provisions, many banks and their auditors focus on historical loan loss data to justify the loan loss provision and limit the use of forward-looking information due to a strict interpretation of FAS 5 (now ASC 450) and to allay earnings management concerns (Dugan, 2009). This view is further supported by anecdotal evidence collected by the Financial Stability Forum, which explicitly lists auditor practices as one of the factors that explain the diversity in implementing loan loss provisioning practices in its 2009 report.[8]

We rely on multiple measures to capture BHC's loan loss provisioning: loan loss provisions (LLP), allowance for loan losses (ALL), net charge-offs (NCO), and non-performing loans (NPL), as well as two measures of adequacy of allowance: allowance relative to current NPL (ADQ_NPL) and allowance relative to future NCO (ADQ_NCO). For our primary analysis, we focus on LLP and ALL, as these two measures involve the most discretion among all the measures. LLP is a flow measure that reflects annual changes, while ALL is a stock measure. ADQ_NPL and ADQ_NCO capture whether allowance will adequately cover future losses with ADQ_NPL based on current information in NPL and ADQ_NCO based on future realized losses.[9]

Our analysis focuses on community and regional banks that are the backbone of the economy and whose primary business is deposit-taking and loan-making. Specifically, we eliminate banks with assets above $100 billion. This restriction eliminates 46 banks, which include the systematically important banks as well as large banks. Although these large banks are deposit-

[7] Also, note the SEC's highly publicized investigation of SunTrust Bank and the subsequent downward restatement of its earnings. http://www.washingtonpost.com/archive/business/1998/12/29/suntrust-got-caught-in-reserves-tug-of-war/c08038f8-50c6-4cd6-bff5-2815c767f1a4/?tid=ss_mail

[8] During our sample period, banks estimated their credit losses using the incurred loss model (ASC 450), which allows accrual only for loan losses that are currently incurred, probable, and capable of reasonable estimation. Effective as of 2020 (2023), large public (small public banks and private) banks must estimate their allowance using the current expected credit loss (CECL) model (ASC 326), which requires accrual of all loan losses that are currently expected based on reasonable and supportable forecasts. We discuss how we expect this change in GAAP measurement requirements to affect our analysis and results briefly in Conclusion.

[9] It is used in literature as one of the measures of timeliness of loan loss provisions (Beatty & Liao, 2011).

taking institutions and make loans, they differ significantly from community and regional banks. They have diverse lines of business and, hence, diverse sources of income, including significant non-interest revenue and revenue from foreign operations. Most importantly, multiple audit partners from the audit firm are likely to be involved in the audit of these large banks, making it difficult to identify the impact of the signing audit partner. As mentioned earlier (footnote 4), while our sample banks account for approximately 20% of the aggregate banking assets, they account for the vast majority of banks operating in the U.S. These banks are critical for small and medium businesses and the communities they serve.

An audit involves frequent audit partner changes (every five years) due to the mandatory rotation required under the Sarbanes-Oxley Act of 2002 (SOX).[10] This mandatory rotation allows us to track the impact of audit partners across different BHCs over time. In addition, since an audit partner often audits multiple BHCs simultaneously, we have data on multiple engagements, cross-sectionally and over time, allowing us to estimate variation in loan loss provisioning that can be attributed to an audit partner after controlling for other time-varying BHC characteristics and fixed effects for BHC, year and audit firm.

To answer our first question on partner heterogeneity, we examine the magnitude of the unexplained variation in LLP and ALL that can be attributed to audit partner fixed effects after controlling for observable and unobservable differences across BHCs and over time by including time-varying BHC characteristics and BHC and year-fixed effects. In addition, we have audit firm fixed effects to control for the methodology and quality control that audit firms implement to guide their respective audits. Our approach mirrors the approach used by Bertrand and Schoar (2003), which was subsequently used in several other studies, including several papers in the accounting literature to test for audit partner heterogeneity (for example, Gul et al., 2013; Cameran et al. (2020); Aobdia et al., 2015). Our research design is illustrated in Figure 1.

It is important to note that bank managers can exercise discretion on LLP in either direction (to increase or decrease the provision) to achieve their financial reporting objectives. Thus, a conservative partner may allow bank managers less latitude to adjust the provisions (either up or down). In contrast, a more lenient partner would permit greater discretion. If so, partner heterogeneity will manifest in an increase in adjusted R^2 after adding the audit partner fixed effects

[10] The mandatory rotation of audit partner applies only to public firms. Since we have both public and private BHCs in our sample, we have several instances where an audit partner audits a BHC for more than five years.

to the model that controls for time-varying BHC characteristics, BHC fixed effects, year fixed effects, and audit firm fixed effects. However, inferring the significance of audit partner fixed effects using the F-statistic for the *joint* significance of individual audit partner fixed effects is problematic as the F-statistics are biased when the number of observations per individual is small, and the serial correlation in the dependent variable is high (Bertrand et al., 2004).[11] Therefore, we use an alternate non-parametric approach to test for the significance of individual audit partner fixed effects. This approach does not rely on the distributional properties of the error terms but still maintains the relation between the dependent variable and the regressors in the data. This approach generates an empirical distribution of the increase in adjusted R^2 under the null hypothesis, i.e., where there is no partner heterogeneity by construction. We randomly assign a partner to each audit partner-BHC spell in our dataset (details in section IV). Since assigning a partner to an actual audit partner-BHC spell is random, audit partner heterogeneity or style should not affect the outcome variable by construction. We repeat this process 1,000 times to generate an empirical distribution of an increase in adjusted R^2 under the null of no audit partner style. Once we have the empirical distribution of adjusted R^2 under the null, we can compare the increase in adjusted R^2 after adding audit partner fixed effects in the actual model to the empirical distribution under the null hypothesis to infer the p-value and, hence, the significance of audit partner fixed effects.[12]

Our results show that adding audit partner fixed effects to the loan loss provisioning models results in increases in the adjusted R^2 of approximately 1.5% to 3.8%, and the increase is statistically significant based on the F-statistic for joint significance of audit partner fixed effects. Our bootstrapped p-values based on the 1,000 iterations using random audit partner assignments for the increase in adjusted R^2 is 0.047 for the LLP model and 0.072 for the ALL model. Thus, our results suggest some evidence of partner heterogeneity in our sample of BHCs. Given that we have several private BHCs and a wide variation in size of the BHCs and the audit firms in our sample, we also examine several sub-samples based on whether the BHC is public or private, small or

[11] For example, Fee et al. (2013), using the same setting as Bertrand and Schoar (2003), find that when they randomly assign CEO-to-CEO moves to a firm that the CEO did not join, the F-statistic continues to be significant, indicating the presence of CEO style in a sample where there should be no style, by construction.

[12] This bootstrap approach has been used extensively to infer significance of long-run buy-and-hold abnormal returns (BHAR). The standard approach using event time cumulative abnormal returns (CAR) to calculate standard errors is not appropriate for long-run BHAR for a variety of reasons such as skewness and event time clustering of long-run BHAR among others (see Brock et al., 1992; Desai & Jain, 1996; Ikenberry et al., 1995 for example).

large, and whether a large or small audit firm audits a BHC.[13] The results of the sub-sample analysis show that the increase in adjusted R^2 is significant for the subsample of public BHCs and BHCs audited by large audit firms.[14]

Given that our sample period includes the financial crisis years (2008-2009), we separately examine the post-crisis period from 2010 to 2019. The crisis period was marked by heightened uncertainty about the general economy, particularly in financial institutions and banks, resulting in a significant increase in LLP and ALL. We do not find evidence of partner heterogeneity in the post-crisis period for the full sample nor in any of the six sub-samples for either LLP or ALL.

The null result during the normal period suggests that the evidence of audit partner heterogeneity is confined to the financial crisis period. This is not entirely surprising as estimating and justifying provisions based on historical loss information becomes increasingly challenging during a crisis, warranting the increased use of non-historical, forward-looking information (Dugan, 2009). Thus, auditing loan loss provisioning during uncertain times would require more than the usual degree of individual partner judgment. While the structured processes and standardized approaches followed by the audit firms may constrain idiosyncratic partner differences during normal times, the partner differences would manifest during the crisis years. The anecdotal evidence collected by the Financial Stability Forum in its 2009 report finds auditor practices as one of the factors that explain the variation in loan loss provisioning practices.

The fact that we do not find evidence of partner heterogeneity in the post-crisis period is comforting. Given the mandatory rotations, it would be discomforting for regulators and disruptive for the BHCs if the audit approach changed with partner rotation or partner tenure, even during normal times. Audit firms intend to instill standardization; most partners are trained in-house and promoted from within the firm. They are groomed for years in the firm's culture before being promoted to partners.[15] A related reason is that the assignment of an audit partner to a client is never random. It works in both directions. For example, an audit firm may assign an experienced

[13] We define small BHCs as BHCs with total assets less than $10 billion. Our large BHCs are BHCs with total assets equal to or more than $10 billion but less than $100 billion. We define large audit firms based on total BHC assets audited and the number of BHCs audited. There are seven audit firms in our sample that are part of the top ten firms based on BHC assets audited or the number of BHCs audited in each of the 14 years in our sample period. These seven firms are designated as large audit firms. The remaining audit firms are designated as small audit firms.

[14] There is significant overlap in these two sub-samples - 60% of public BHCs are audited by large audit firms and almost half of the BHCs audited by large audit firms are public BHCs.

[15] Our null results are not surprising based on our numerous discussions with three audit partners from three different firms. There is a strong connection between leadership and the team within the audit. Partners know every manager and staff working on that audit, and the many of those staff are groomed and rise to become partners.

partner to a more complicated audit. At the same time, clients can also influence the choice of partners. This should again constrain audit partner style and its impact on the outcome variable.

We also examine whether an audit partner's approach to auditing loan loss provisioning changes over the course of their tenure. In some sense, this approach tries to capture whether audit partners exhibit a consistent approach to audit over their tenure. To test the effect of audit partner tenure on reporting outcomes, we measure the partner's aggregate experience in that engagement (TENURE) and relate it to LLP and ALL.[16] We find that the magnitude of ALL (but not LLP) increases with the length of the audit partner TENURE, especially for public and small BHCs. Given that our sample period includes the years of financial crisis marked by high economic uncertainty and that the crisis period also experienced an unusually high number of partner rotations following the adoption of the mandatory audit partner rotation rules in 2003, we examine the post-crisis period (2010 to 2019) separately. We find that a positive trend in allowance is obtained for small BHCs even in the post-financial crisis period. However, the result is weaker after excluding audit partner-BHC spells longer than five years.[17] Overall, evidence suggests that audit scrutiny increases as the partners stay longer with the engagement, especially in small BHCs.[18]

Our findings should be of interest to rule-makers, investors, audit committees, audit firms, and academia. First, it is imperative to examine the U.S. setting, given that the large body of work documenting the impact of partner heterogeneity is primarily from jurisdictions outside the U.S. and mainly relates to non-financial firms. We contribute to this stream of literature by examining the partner heterogeneity in a unique industry. Second, evidence based on firms in the U.S. is emerging and mainly relates to partner tenure or rotation. In a recent paper, Gipper et al. (2021) use proprietary data from the PCAOB over the period 2008-2014 and do not find evidence of a decline in audit quality over the length of audit partner tenure nor a change in audit quality

[16] The design of this test is distinct from our fixed effects approach. For example, if most of the audit partners within an audit firm exhibit a certain pattern relating to loan loss provisioning across tenure, and this holds true in majority of the audit firms, we may observe a significant coefficient in our tenure tests but since there are no differences between partners (all exhibit similar patterns) our fixed effects test will not show significant results between partners in an audit firm. However, if there are differences in patterns exhibited by partners across tenure, but there is no consistent pattern, our fixed effects test may capture the partner differences within the audit firm, but the tenure tests will not show any significant results due to lack of consistent pattern.

[17] It should be noted that private BHCs are not required to have mandatory partner rotation. However, many private firms voluntarily rotate audit partners after five years. Moreover, SOX allows audit firms with fewer than five audit clients and fewer than ten partners to be exempt from the partner rotation, subject to a special review by the PCAOB, at least every three years. Less than 10% (5%) of the total audit partner-BHC spell relating to private (public) BHCs are longer than five years.

[18] Unlike non-financial institutions, where a larger magnitude of discretionary accruals is interpreted as lower audit quality (Manry et al., 2008, among others) a higher allowance is interpreted as a sign of higher auditor scrutiny (Stuber & Hogan, 2021; Ege et al., 2023).

following mandatory partner rotations for a cross-section of U.S. publicly listed (non-financial) firms audited by six large audit firms. Our findings complement the results of Gipper et al. (2021) by providing evidence for what is effectively a hold-out sample. Moreover, unlike Gipper et al. (2021), our sample comprises small and large audit firms, public and private BHCs, and primarily smaller firms (community banks). The U.S. banking industry is unique. Out of 1,614 BHCs during our sample period, more than 90% are community banks (assets below $10 billion and median assets of $1.1 billion). Most banks are private, operate in rural communities, make local loans, lack sophisticated in-house risk modeling, and are managed conservatively. Thus, the role of the auditors becomes crucial. Hence, a separate analysis of these banks is important and can generate useful insights.[19] Finally, as discussed in Kinney (2015), the PCAOB and some media outlets have relied on extant evidence to infer that auditors exhibit idiosyncratic styles that impact audit outcomes and suggest that individual partner characteristics influence those audit outcomes. Our evidence for a sample of community and regional banks is, thus, vital as it indicates that, except for periods characterized by uncertainty, overall, audits, even for small and private firms, are not as idiosyncratic as some would believe, especially during normal economic periods.

II. BACKGROUND AND HYPOTHESIS DEVELOPMENT

Prior Literature

There is extensive literature in economics and finance that examines whether an individual manager's characteristics or "style" influences firm policies such as investment spending, acquisitions, leverage, and profitability (Bertrand & Schoar, 2003), compensation levels (Graham et al., 2009) and performance variability (Adams et al., 2005). Bertrand and Schoar (2003) discuss two theoretical views on whether individual managers matter. Under the narrow neoclassical view of the firm, top managers are homogeneous and selfless inputs into the production process. They are regarded as perfect substitutes for one another and do not matter in corporate decisions. While individual executives might differ, two firms sharing similar technologies, factors, and product market conditions will make similar choices. Under this view, managers will matter for corporate decisions, not because they impose their idiosyncratic style on the firm but because the firms

[19] Our discussions with multiple experienced bank examiners revealed that not only do bank examiners communicate with external auditors but also have access to audit workpapers. The general anecdotal consensus opinion was that they rely on the reputation of the audit firm but seem unaware or indifferent to individual partner reputations.

intentionally choose managers with matching attributes. For example, a firm experiencing declining performance may hire a manager with experience in restructuring and turning around poorly performing firms.

On the other hand, agency theory acknowledges that managers may have discretion and can influence corporate decisions. However, these models attribute variations in managers' behavior to differences in governance mechanisms that constrain managers. Under this view, the manager matters only when internal and external governance is weak and that firms with better governance optimally choose managers that contribute positively to firm performance.

Using the individual fixed effects approach of Bertrand and Schoar (2003), several papers in the accounting literature have examined whether manager style affects financial reporting outcomes. Bamber et al. (2010) find that individual managers play a significant role in shaping their firms' voluntary financial disclosure choices and that these choices are associated with managers' observable demographic characteristics. Ge et al. (2011) examine the impact of the CFO style on various accounting decisions, such as off-balance sheet operating lease activity, the expected rate of return on pension assets, earnings smoothness, and the likelihood of misstatement. They report mixed evidence on whether "CFO-style" impacts these specific accounting decisions. DeJong and Ling (2013) report evidence of CEO/CFO styles on a 'firm's accruals. They find that the CEO affects accruals through firm policies, while the CFO affects accruals through accounting choices.

Similarly, Wells (2020) finds that individual managers incrementally explain the cross-sectional variation in accounting quality, measured as the inverse of the standard deviation of abnormal accruals, using the modified Dechow and Dichev model. Demerjian et al. (2013) examine the impact of managerial traits on accounting quality measures, such as restatements, the persistence of earnings, errors in bad debt expense, and changes in accrual quality. They study the association between managerial ability and accrual quality. They find statistically significant coefficients when firm fixed effects are excluded from the regression but insignificant results in the presence of firm fixed effects, highlighting the problematic nature of inferring managerial attributes from observable firm-level information. Dyreng et al. (2010) find that top executives have incremental effects on their firms' tax avoidance that the firm's characteristics cannot explain.

Overall, the evidence seems to suggest that manager style influences firm policies. This framework has been extended to the auditing literature in recent years to examine whether individual audit partners affect outcomes such as accruals, going concern opinions, audit quality, etc. (see Lennox & Wu, 2018 for a review of this literature). However, most of these studies examine companies in non-U.S. jurisdictions, as individual audit partner data have been available in many countries for some time. The evidence suggests that individual audit partners seem to influence their clients' earnings, accruals, going concern opinions, audit fees, comparability of earnings, etc. (Gul et al., 2013; Knechel et al., 2015; Chen et al., 2020; Zerni, 2012).

Three prior studies use the joint significance of individual fixed effects to infer the presence of auditor style. Using data on Chinese firms, Gul et al. (2013) document an increase in adjusted R^2 after adding audit partner fixed effects and conclude that audit partner style affects accounting quality. Cameran et al. (2020) use the same fixed effects approach to test for partner heterogeneity in the U.K. and find that partner fixed effects add to the explanatory power of the model, explaining variation in earnings quality, going concern opinions, and audit pricing. Using Australian data from 2005, Taylor (2011) finds that audit fees are significantly affected by partners' innate characteristics. However, the study has only one year of data and, therefore, cannot control for audit firms or clients.

Studies examining the consequences of partner disclosure requirements report mixed evidence. Carcello and Li (2013) find that audit quality and audit fees are higher for the U.K. companies after the mandatory signature requirement, which requires the engagement partner to sign the audit report for financial years ending in April 2009 or later. In contrast, Blay et al. (2014) detect no substantial change in audit quality following the partner signature mandate in the Netherlands. A large majority of studies examining the impact of audit partner characteristics on audit outcomes, as well as those investigating the impact of disclosure of audit partner names on audit outcomes, are based on jurisdictions outside the U.S.

Cunnigham et al. (2019) use the U.S. setting to provide early evidence for Rule 3211. They do not detect significant changes in audit quality (measured as discretionary accruals, the propensity to misstate, and the likelihood of issuing an incorrect material weakness opinion) attributable to Rule 3211. Burke et al. (2019) use a difference-in-differences design and find a significant increase in audit quality and audit fees and a significant decrease in audit delay following the disclosure of audit partner names under Rule 3211. However, when exploring

whether partner characteristics are associated with variations in audit outcomes, they find evidence relating to audit fees and audit delay but not to audit quality. It should be noted that both of these studies that provide early evidence relating to the adoption of Rule 3211 are based on a very short time series (one year in the pre-period and one year in the post-period).

Prior evidence on whether the length of partner tenure affects audit outcomes and whether there are benefits to mandatory partner rotation is also mixed.[20] Manry et al. (2008) find that discretionary accruals decrease over the length of partner tenure, suggesting that earnings quality improves over the course of partner tenure. In contrast, Fitzgerald et al.'s (2018) examination of internal control opinions for non-profit firms that received federal funds (hence, the partners' names were disclosed) suggests evidence consistent with a decline in audit quality over partner tenure. Laurion et al. (2017) find that the frequency of restatement discoveries and announcements increases following partner rotation, indicative of the benefits of partner rotation. However, Litt et al. (2014) report evidence of lower reporting quality (discretionary accruals and going-concern opinions) following partner rotation, while Kuang et al. (2020) do not find evidence that audit quality declines over partner tenure, or that mandatory partner rotation improves audit quality.

Gipper et al. (2021), using a large sample of U.S.-based public companies audited by Big 6 auditors, do not find evidence of any significant change in audit quality or earnings quality over the course of partner tenure or following mandatory partner rotation. They find that audit fees decline after partner rotation and that partner hours increase after the rotation, but the increase seems to reverse over time. A concurrent working paper by Gopalan et al. (2024) finds declining partner professional skepticism over the course of partner/client relationships from 2010 to 2019. They document that banks systematically report higher loan loss reserves at the beginning of audit partner/client relationships relative to the later years.

The above review suggests that the evidence on whether individual audit partners affect financial reporting outcomes or whether the length of partner tenure affects reporting outcomes is decidedly mixed. The mixed results are likely due to evidence coming from different jurisdictions and the fact that, in a lot of instances, the studies have had to rely on a short time series of data. An important benefit of our paper is that we have a long time series of data and that our sample comes from a single industry where there is wide agreement on the importance of an estimate (loan

[20] Tenure is the length of time a partner is with a given client whereas rotation studies is the change in partners which necessarily involves the comparison of one partner to a new partner.

loss provisions) that is scrutinized by regulators and auditors alike. Thus, our sample and setting is well suited to answering our two research questions: whether auditor heterogeneity and length of audit partner tenure affect financial reporting outcomes.

Hypothesis Development

Unlike managers, auditors are not insiders but can significantly influence firms' financial reporting outcomes by establishing guardrails and forcing the firms to make financial reporting choices within those constraints. For example, an auditor could constrain an estimate of LLP that is deemed too high (or too low) if she does not feel the estimate can be justified based on available information. In this sense, auditor heterogeneity could manifest – i.e., some auditors may grant greater latitude to bank managers when coming up with an estimate of LLP, while others may be stricter and grant less latitude. However, studies on auditor heterogeneity or style are only recent. Many prior empirical studies in the auditing literature implicitly assume that audit partners within a firm are homogeneous in that they would make similar audit judgments under similar circumstances. Hence, the unit of analysis is typically the audit firm (see Lennox & Wu, 2018, for a review of this literature). To some extent, the lack of empirical evidence on this issue is due to the lack of data on audit partner identity in the U.S. until the recent mandate by PCAOB. However, the assumption that audit partners within a firm are substitutes for one another is not that unreasonable. It stems from the belief that a single person cannot easily circumvent the audit firm's framework, policies, and extensive quality controls. While individual audit partners may differ in their preferences, risk aversion, or skill levels, it is not clear that these differences should manifest in observable differences in audit quality or audit outcomes for their clients. Audit firms establish standards and control systems to maintain audit quality and consistency and rein in individual idiosyncrasies. Also, the audit partners themselves may curb any individual style outside the framework provided by the audit firm, given that the PCAOB's current quality control standards expose audit partners to personal sanctions and penalties and the threat of private litigation (EY 2009; KPMG 2009). This suggests that two audit partners that share similar audit technologies such as standardized work procedures, centralized models of risk and materiality decisions, staff training, rigorous promotion process, concurrent partner reviews, access to in-house knowledge libraries, and subject matter experts, among other things, and will likely make similar decisions. However, as discussed earlier, the PCAOB feels strongly that the quality of audit engagements

differs within firms and that the PCAOB uses individual audit partner history for a risk-based selection of engagements for inspections, hence the Board's push for public disclosure of audit partner identity.

On a related note, while there may not be differences between individual partners within an audit firm, partners may evolve their approach to audit over their tenure. On the one hand, the longer the relationship between the partner and the client, the more likely the partner would be reluctant to rock the boat and be more accommodating. On the other hand, a partner may understand the client's business model better over time, become more well-versed with the credit risk underlying the client's business, and may exercise greater influence on the estimates. This is particularly relevant in the banking industry, which demands a high degree of specialization from auditors to audit complex and subjective accounting estimates.

In 2005, the Federal Reserve added a new memorandum item in Y9C filings to collect the name and address of the BHC's external auditing firm and the name and email address of the engagement partner to facilitate more efficient supervision of the banking industry on issues related to accounting and auditing.[21] This change came in the aftermath of the Enron scandal and the Sarbanes Oxley Act of 2002. Although banking regulators relied on the quality assurance process of audit firms and the peer review process of the American Institute of Certified Public Accountants (AICPA) to monitor the quality of auditors in the past and they rarely used the authority under FDICIA to debar an auditor from serving as an auditor of a bank, they revisited the policy during this period (Bies, 2003). The OCC, the Board, the FDIC, and the OTS jointly published final rules pursuant to section 36 of the Federal Deposit Insurance Act (FDIA), authorizing the Agencies to remove, suspend, or debar accountants from performing the audit services required by section 36 if there is good cause to do so.

The above discussion suggests that notwithstanding extant evidence on the impact of individual auditors or the length of partner tenure on financial reporting outcomes outside the U.S., it is important to examine the issues in the U.S. separately. Moreover, given the tension highlighted earlier due to the differing objectives of regulators (and managers) relative to those of the auditors with regard to the magnitude of ALL, a study examining the impact of audit partner heterogeneity

[21] The information would also enable the Federal Reserve to more readily identify firms that may be interested in participating in regional CPA and examiner roundtable discussions and similar programs designed to improve communication between the accounting profession and the regulatory community

and partner tenure on financial reporting outcomes on a sample of U.S. banks, particularly relevant and important. We state our null hypotheses as follows:

Hypothesis 1: Audit partner heterogeneity is not associated with financial reporting outcomes after controlling for BHC characteristics, time, BHC, and audit firm fixed effects.

Hypothesis 2: The length of tenure of the audit partner is not associated with financial reporting outcomes after controlling for BHC characteristics, time, BHC, and audit firm fixed effects.

III. DATA

Sample Construction

Our primary sample consists of U.S. BHCs with the necessary data from the Q4 Y9C filings provided to the Federal Reserve. Given that the field to identify the name of the audit engagement partner was included on the Y9C starting in 2005, and the asset-size threshold for filing the Y9C changed from total assets of $150 million to $500 million in 2006, we started our sample period in 2006 (we use the 2005Q4 data for lags.).[22] Our initial sample comprises 12,043 BHC-year observations (1,614 unique BHCs) between 2006Q4 and 2019Q4 with non-missing total assets. Requiring data for our dependent and control variables (other than audit partners) yields us 11,025 BHC-year observations (1,468 unique BHCs). As mentioned earlier, we exclude 46 BHCs (399 BHC-year observations) with total assets above $100 billion in any year, resulting in 10,626 BHC-year observations (1,422 unique BHCs). The audit partner names are included as data item TEXTC704 on form Y9C. The BHCs are to report the name of the independent external auditing firm's engagement partner (partner in charge of the audit). This information is for the confidential use of the Federal Reserve and is not released to the public. We invested considerable effort in cleaning up the audit partner names.[23] We excluded 708 BHC-year observations (113 unique BHCs) due to missing audit firm or partner names, leaving us with 9,918 BHC-year observations (1,309 unique BHCs) and 941 individual audit partners. Following prior research (Gul et al., 2013),

[22] The Federal Reserve explicitly states on the form that it regards this information as confidential.
[23] For example, partner name may appear as John Smith in one year, and J. Smith in the next year.

we require that each audit partner audit a BHC for at least three years to allow the audit partner to "imprint his/her mark" on the BHC's financial reporting, and the audit partner audits at least two unique BHCs. This results in our final sample, which comprises 6,186 BHC-year observations relating to 871 unique BHCs audited by 360 unique audit partners associated with 88 audit firms. Details on the waterfall of BHC data extraction can be found in Panel A of Table 1.

Panel B of Table 1 provides more details on our sample composition based on three dimensions – public vs. private, BHC size, and audit firm size.[24] The split between public and private BHCs is as follows. There are 2,492 (2,971) BHC-year observations associated with 42 (71) unique audit firms and 187 (188) unique audit partners for 339 (447) unique public (private) BHCs. Our sample includes public and private BHCs in each of the 14 years.[25] Approximately 90 percent (769) of the BHCs in our sample are community banks with assets less than $10 billion. Almost all (87 out of 88) audit firms and 86% of the audit partners (308) in our sample audit small community banks. Our sample has 42 regional BHCs with total assets between $10 billion and $100 billion that are audited by 12 unique audit firms and 48 unique partners. Given that our sample has large audit firms (for example, Big 4) and very small audit firms, we classify audit firms as LARGEAF and SMALLAF. Seven audit firms in our sample appear in the top 10, based on the number of BHCs audited *and* the volume of aggregate banking assets audited for each year in our sample. We classify these seven audit firms as the LARGEAF. The rest of the audit firms are classified as SMALLAF. These seven audit firms designated as LARGEAF have 181 unique partners that audit 446 BHCs. While there is considerable overlap between private BHCs and small BHCs, with more than 95% of the private BHCs having total assets below $10 billion, interestingly, the overlap with small audit firms is not that stark. LARGEAFs account for 49% of private and small BHC audits.

Sample Description

[24] The Federal Reserve requires that top-tier BHCs with total consolidated assets of $500 million or more as of the end of the BHCs' fiscal year must have an annual audit of their consolidated financial statements (balance sheets, statements of income, changes in equity capital, and cash flows, with accompanying footnote disclosure) by an independent public accountant. In addition, the Federal Reserve may request audited consolidated financial statements from any BHC with total consolidated assets of less than $500 million if deemed warranted for supervisory purposes.

[25] The BHCs are identified as public based on the CRSP-FRB link file available from the New York Federal Reserve website. We have 289 (496) BHCs that are public (private) for the entire sample period. The 86 BHCs that switch between public and private are classified as public in sub-sample analysis. Our main results are robust to an alternate treatment of classifying these BHCs as private.

Table 2 presents descriptive statistics for the BHCs in our sample. All variables are winsorized at 1% and 99%. The mean (median) total assets for our sample of BHCs is $3.4 billion ($1.3 billion). Our main loan loss provisioning measures are LLP and ALL. LLP is measured as the loan loss provision deflated by total loans at the beginning of the period, and ALL is measured as the loan loss allowance deflated by total loans at the beginning of the period. The mean loan loss provision (LLP), a flow measure, is about 0.70% of total loans, and the stock measure loan loss allowance (ALL) is 1.7% of the loan portfolio.[26] NPLs are about 2.2% of the loan portfolio. Net charge-offs, on average, are 0.6% of the loan portfolio. ADQ_NPL is a ratio commonly used to measure the adequacy of loan loss allowance. It is calculated as the ratio of loan loss allowance divided by current non-performing loans.[27] The mean ADQ_NPL is 2.1, indicating that the loan loss allowance is 2.1 times the amount of non-performing loans (NPL). In addition, we use ADQ_NCO, which is the ratio of loan loss allowance divided by future net charge-offs. The mean ADQ_NCO is 7.6, indicating that allowance is almost eight times the net charge-offs in the following year.

The loan portfolio comprises 74.7% real estate loans, 17.5% commercial and industrial loans, and 4.6% consumer loans, and the annual loan growth is about 6.7%. The mean Tier 1 capital ratio is 13.0%, well above the minimum required ratio of 4%. Earnings before provision are 1.4% of the total assets. New additions to the NPL (ΔNPL) are about 0.1% of the loan portfolio at the beginning of the year. We compare these summary statistics to the larger sample of 10,626 BHC-year observations with a unique 1,422 BHC before requiring the audit firm and audit partner name.[28] We find that our sample is representative of the overall sample of BHCs. Detailed variable definitions are provided in Appendix Table A1.

Table 3 summarizes the interrelationships between audit partners, audit firms, and BHCs for our sample. On average, an audit partner audits 3.8 BHCs. Of the 360 unique audit partners in our sample, 146 audit two BHCs, while the rest audit three or more BHCs. The maximum number of BHCs audited by an audit partner is 14. As expected, audit partners change audit firms

[26] The loan loss indicators during the financial crisis of 2008-2009 are as follows: LLP is 1.5%, ALL is 1.8%, NPLs are 3.1%, and NCOs are 1.1% of the total loan portfolio. The adequacy measures indicate that allowance was 1.3 times the current non-performing loans and 4.1 times the future charge-offs. Given the extreme stress in the financial markets and banking systems during this period, we compare our results for the full sample period (2006-2019) with post crisis period (2010-2019).

[27] This ratio is also used to as a stock measure of timeliness of loan loss provisioning in the literature (Beatty & Liao 2011).

[28] We include the correlations and p-value of the significance of the correlation coefficient for our study's dependent and control variables in Appendix Table A5. As expected, LLP is positively correlated to ALL, NCO, NPL, and ΔNPL. LG is negatively correlated with NPL and NCO, in line with newly originated loans having fewer problem loans. The likelihood of loans becoming non-performing tends to increase as loans season.

infrequently. The mean number of audit firms for an audit partner is 1.2, and the median and the 75th percentile are 1. Approximately 15% of the audit partners are associated with more than one audit firm, the most common reason being audit firm mergers. A typical audit partner appears in our sample for 8.6 years. Audit firms, on average, have 4.9 audit partners and audit 12 BHCs, with approximately 60% of our sample of audit firms having two or more audit partners. The maximum number of audit partners associated with an audit firm is 50, and the maximum number of BHCs audited by an audit firm is 130. An audit firm appears in our sample for an average of 8.7 years.

The LARGEAFs in our sample have eight or more audit partners, and each of the LARGEAFs audits 14 or more BHCs. On average, BHCs are audited by 1.6 audit partners and 1.2 audit firms during the sample period. An average BHC appears in our sample for 7.1 years, consistent with the change in the threshold for total assets for filing the Y9C from $500 million to $1 billion in 2015 and to $3 billion in 2018, and the consolidation that has been taking place in the banking sector (note that our sample period is from 2006-2019). In a given year, 442 BHCs are audited by 55 audit firms and 220 audit partners. Identification restrictions allow us to estimate fixed effects for 274 individual audit partners.

IV. DOES HETEROGENEITY BETWEEN AUDIT PARTNERS OR THE LENGTH OF AUDIT PARTNER TENURE AFFECT LOAN LOSS PROVISIONING OUTCOMES?

We use two approaches to examine whether audit partners impact loan loss provisioning. Our first approach examines whether adding audit partner fixed effects improves the explanatory power of the loan loss provisioning model. Our second approach examines whether the length of the audit partner's tenure affects the magnitude of loan loss provisioning.

Fixed Effects Approach

Intuitively, we want to quantify how much of the observed variation in the loan loss provisioning of the BHCs can be attributed to audit partner fixed effects. Thus, we estimate the incremental R^2 from a restricted model that adds audit partner fixed effects to an LLP (and ALL) model that includes time-varying firm characteristics and BHC, year, and audit firm fixed effects. In this setting, to estimate audit partner fixed effects separately from audit firm fixed effects, we need an audit partner to be associated with more than one audit firm or the audit firm to have more than one audit partner. Similarly, to estimate audit partner fixed effects, separately from BHC fixed

effects, we need an audit partner to be associated with more than one BHC or the BHC to have more than one audit partner. Our analysis allows us to estimate fixed effects for the audit partners associated with more than one audit firm (i.e., movers) and those associated with only one audit firm (i.e., stayers) as long as they audit more than one BHC or the BHC has more than audit partner. Estimating fixed effects requires some movement in the sample but does not require focusing only on the movers, as Graham et al. (2012) have explained. The movers help establish audit firm fixed effects, which are then used to back out audit partner fixed effects for both the movers and the stayers (e.g., Abowd et al., 1999; Correia, 2017; Bushman et al., 2021).[29]

Our identification strategy can be explained with an example. Consider our outcome variable LLP. From a benchmark specification, we derive residual LLP at the BHC-year level after controlling for average differences across BHCs and years. This will control for any BHC-year-specific shock, such as loan growth or an increase in non-performing loans that might affect the LLP of a BHC. The addition of audit firm fixed effects to the model accounts for the average difference across the audit firms that captures the impact of differences in procedures and, perhaps, in quality controls between audit firms on LLP. Finally, adding audit partner fixed effects to the model allows us to quantify how much of the variation in LLPs can be attributed to audit partner-specific effects as long as the BHC has more than one audit partner or the audit partner audits more than one bank.

Our independent variables include BHC-level control variables and various fixed effects. We employ several specifications, introducing one set of fixed effects at a time:

$$y_{i,t} = \beta X_{i,t} + \varepsilon_{i,t} \quad (1)$$

$$y_{i,t} = \beta X_{i,t} + YR_t + \varepsilon_{i,t} \quad (2)$$

$$y_{i,t} = \beta X_{i,t} + YR_t + BHC_i + \varepsilon_{i,t} \quad (3)$$

$$y_{i,t} = \beta X_{i,t} + YR_t + BHC_i + AF_{af} + \varepsilon_{i,t} \quad (4)$$

$$y_{i,t} = \beta X_{i,t} + YR_t + BHC_i + AF_{af} + AP_{ap} + \varepsilon_{i,t} \quad (5)$$

where $y_{i,t}$ is a BHC-level outcome, $X_{i,t}$ are the control variables, YR_t is a time-fixed effect, BHC_i is a BHC-level fixed effect, AF_{af} is the audit firm fixed effect, AP_{ap} is the audit partner

[29] STATA will estimate the first fixed effect and drop the subsequent two fixed effects in a scenario that cannot identify the audit partner, audit firm, and BHC separately from each other. Therefore, we follow the most stringent ordering to introduce fixed effects in our STATA code. Our order is year, BHC, audit firm, and finally, audit partner to allow the code to impose the most restrictions on estimating audit partner fixed effects if there is a lack of identification.

fixed effect, and $\varepsilon_{i,t}$ is an error term. We account for serial correlation in the dependent variable by clustering at the BHC level. Our interest lies in examining whether adding audit partner fixed effects improves the model's explanatory power.

In equation (1), we control BHC characteristics using time-varying controls. Our BHC-level control variables are guided by prior research (Wahlen, 1994; Liu & Ryan, 1996; Bhat et al., 2021, among others). We include SIZE measured as a log of total assets to control for size-related differences in BHCs' business models. Larger BHCs tend to have multiple subsidiaries and a higher proportion of non-lending-related activities than smaller BHCs. We include EBP, measured as earnings before provisions deflated by total assets at the beginning of the year to capture incentives for income smoothing. T1CAP is the Tier 1 capital ratio intended to capture regulatory capital constraints. LG (loan growth) is measured as an annual change in loans deflated by total loans at the beginning of the year and captures the effects of loan seasoning. ΔNPL is measured as the annual change in non-performing loans deflated by total loans at the beginning of the year and captures loan performance.[30] We include the proportion of real estate loans (RE_LOANS), commercial and industrial loans (COMM_LOANS), and consumer loans (CONS_LOANS) to control for loan composition. All loan portfolio variables are deflated by the total loans measured at the beginning of the period.

We include year-fixed effects to extract the business cycle-specific movements in equation (2). In addition, we have BHC fixed effects to control for time-invariant BHC-specific effects in the loan loss provisioning in equation (3). Audit firms have firm-specific policies, procedures, and quality control standards, which we control using audit firm fixed effects in equation (4). Finally, we add audit partner fixed effects to examine whether individual audit partner fixed effects improve the model's explanatory power in equation (5). The research design is presented in Figure 1, and the results of the models from equations (1) to (5) are presented in Tables 4 and 5 and are discussed in the relevant sections below.

Impact of Partner Tenure on Reporting Outcomes

[30] We do not expect banks to use loan loss provisions to manage regulatory capital as from 1992, with the introduction of the Capital Accord in the U.S, loan loss reserves do not count as part of Tier I or primary capital and only count as part of Total capital up to 1.25 percent of risk-weighted assets, thus making it less attractive for low capital banks that have exceeded the upper bound on loan loss reserves to increase loan loss provisions.

To answer our second question, whether loan loss provisioning outcomes differ on average over an audit partner's tenure at the BHC, we include dummy variables that capture the audit partner's tenure in an LLP (and ALL) model that includes time-varying firm characteristics and BHC, year, and audit firm fixed effects as shown below in equation 6. This approach is based on Gipper et al. (2021).

$$y_{i,t} = \beta X_{i,t} + \gamma AP\ TENURE_{i,t} + YR_t + BHC_i + AF_{af} + \varepsilon_{i,t} \qquad (6)$$

Instead of audit partner fixed effects, we include TENURE, a continuous variable that reflects the number of years a partner has spent on the engagement. The coefficient γ indicates the trend in the dependent variable over the partner's tenure.

We start with a sample of 9,918 BHC-year observations relating to 1,309 BHCs and 3,254 audit partner-BHC spells for which we have partner names (see Panel A of Table 1).[31] We exclude 676 (123) audit partner-BHC spells of private (public) BHCs as we cannot determine the audit partner's true tenure.[32] Thus, we have 7,294 BHC-year observations relating to 2,455 audit partner-BHC spells, of which 3.1% have partner tenure longer than five years.[33] The partner rotation rules came into effect in the U.S. after May 2003 for lead partners and a year later for review partners. Thus, if a lead partner's tenure equals or exceeds five years for the first fiscal year ending after May 2003, the partner must rotate off the *following year*. This causes the number of rotations to be high during the early part of our sample period in general and the crisis period in particular. For example, we have almost 20% of the partner rotations between 2008 and 2009. Finally, in line with Gipper et al. (2021), we designate the spell length to be five years if there is a change in audit partner in the first four years (2006-2009). This results in re-designating spell length for 306 spells.

Results

Baseline Loan Loss Models

Table 4 reports the results of the baseline regression model in equations (1) to (5) for LLP

[31] While the conditions imposed for the fixed effects test require a certain number of observations per audit partner to allow the audit partner to "imprint his/her mark" on the BHC's financial reporting, these restrictions are not necessary for the tenure test. This approach is also in line with prior research on partner tenure.

[32] BHCs enter and exit our sample based on Y9C reporting threshold requirements. Therefore, we cannot determine the actual length of the audit partner-BHC relationship with certainty in the following spells: (i) BHC has only one spell in our sample; (ii) the first spell for BHC when it enters our sample is longer than five years, and (iii) the first spell for BHC when it enters our sample is less than five years and relates to a private bank.

[33] The descriptive statistics for the sample used in the tenure test are included in Appendix Table A3.

in Panel A and ALL in Panel B. We first discuss the results for the LLP models, followed by the ALL models. The coefficient on SIZE is positive and significant in all five models for LLP, suggesting larger BHCs have higher provisions than smaller BHCs. The coefficient on EBP is significantly negative across all five models. This indicates that the BHCs do not appear to exercise discretion over LLP to smooth their income or that the auditors curtail this discretion.[34] The coefficient on T1CAP is not significant in any of the models. This is not entirely surprising as there is little variation of T1CAP across BHCs, as seen in Table 2. The coefficient on LG is negative and significant at the 1% level. This suggests that loan growth disproportionately occurs during good times when loan default is low, consistent with Laeven and Majnoni (2003). We find a positive and significant coefficient on ΔNPL consistent with recent research that views a positive and significant association between LLP and changes in NPL as indicating greater timeliness of loan-loss provision (Beatty & Liao, 2011). The coefficient on COMM_LOANS and CONS_LOANS is not significant in any of the five models. The coefficient on RE_LOANS is significant in two of the five models, indicating the importance of controlling for loan composition.

Panel B of Table 4 shows that the coefficient on SIZE is negative and significant at the 1% level in three of the five models, suggesting larger BHCs have smaller ALL. The coefficient on EBP is negative and significant at the 1% level, consistent with the negative relation between EBP and LLP documented earlier. The coefficient on T1CAP is positive in two of the five models. The coefficient on LG is negative and significant in four out of five models, in line with loan growth occurring during good times and having fewer NPLs until the loans season. The coefficient on ΔNPL is positive and significant at the 1% level, indicating higher allowance for high levels of NPLs (Beatty & Liao, 2011). Except for a coefficient on CONS_LOANS in one of the five models, the coefficients on loan composition are not significant in any of the models.

The coefficients on fixed effects in a panel dataset are estimated relative to the dropped coefficient. Therefore, we do not analyze the signs on the coefficients. Unlike accrual measures for non-financial institutions, where a larger magnitude of accruals is used as a proxy for earnings management and earnings quality, earnings management in banks involves using discretion (say, over LLP) to smooth earnings. Thus, a higher or lower LLP or ALL, per se, is not a measure of earnings quality. Hence, our approach is non-directional. Specifically, we want to quantify the

[34] For example, Ahmed et al. (1999) find a negative and significant coefficient on earnings before the provision in the latter part of their sample period. They interpret that earnings management is not an important determinant of loan loss provisions.

incremental variation in the outcome variables (LLP and ALL) due to audit partner fixed effects. We do this by examining the incremental adjusted R^2 of the unconstrained model (with audit partner fixed effects, model 5) relative to the adjusted R^2 of the constrained model without the audit partner fixed effects (model 4).

Evidence from an increase in adjusted R2

Actual audit partner assignments

The coefficients on fixed effects in a panel dataset are estimated relative to the dropped coefficient. Therefore, we do not analyze the signs on the coefficients. Unlike accrual measures for non-financial institutions, where a larger magnitude of accruals is used as a proxy for earnings management and earnings quality, earnings management in banks involves using discretion (say, over LLP) to smooth earnings. Thus, a higher or lower LLP or ALL, per se, is not a measure of earnings quality. Hence, our approach is non-directional. Specifically, we want to quantify the incremental variation in the outcome variables (LLP and ALL) due to audit partner fixed effects. We do this by examining the incremental adjusted R^2 of the unconstrained model (with audit partner fixed effects, model 5) relative to the adjusted R^2 of the constrained model without the audit partner fixed effects (model 4).

Table 5 reports adjusted R^2 and the incremental adjusted R^2 from the estimation of models (1) to (5). In the first row, we report baseline specifications for each loan loss measure, including BHC-level controls. The following three rows report the adjusted R^2 when we successively add year fixed effects, BHC fixed effects, and audit firm fixed effects. The last row reports incremental adjusted R^2 after adding audit partner fixed effects.

The baseline specification includes controls for size, earnings before provisions, Tier 1 capital, loan growth, change in non-performing loans, and the proportion of the real estate, commercial, and consumer loans in the loan portfolio. The adjusted R^2 for this specification is 22.9% for LLP and 10.2% for ALL. After including year fixed effects, the adjusted R^2 increases to 37.1% for LLP and 20.6% for ALL, suggesting (not surprisingly) that provisions vary over time due to business cycles. BHC fixed effects also add significantly to the explanatory power of the models, as the adjusted R^2 is now 57.7% for LLP and 69% for ALL. However, audit firm fixed effects add only modestly to the explanatory power. This is likely due to the considerable overlap

between BHCs and audit firms. BHCs rarely change audit firms, with most observed changes due to mergers among audit firms.

Up to this point, the model explains 58.1 % (69.4%) variation in LLP (ALL). Finally, we add audit partner fixed effects, our primary variable of interest, to the constrained model (4) to measure its incremental explanatory power. We find that audit partner fixed effects increase the model's explanatory power by 1.5% for LLP and 3.8% for ALL. To put it differently, the audit partner fixed effects increase the model's explanatory power by 2.6% (1.5/58.0) and 5.5% (3.8/69.4), respectively, for our two proxies for provisioning. The F-statistic for joint significance is large and significant at the 1% level for the LLP (ALL) model at 2,469 (2,849), respectively, suggesting that audit partner fixed effects add significantly to the explanatory power of the model, consistent with prior research, such as Gul et al. (2013) and Cameran et al. (2020), who studied Chinese and the U.K. data respectively. The above estimation yields coefficients on 274 individual audit partners. About 42% (49 %) of our 274 estimated audit partner fixed effects are significant at the 1% level for the LLP (ALL) model. Another 10 % (5 %) and 5 % (4 %) are significant at the 5% and 10% levels in the LLP (ALL) model, respectively.[35]

Randomized audit partner assignments

As discussed earlier, we do not infer the presence of audit partner heterogeneity from the significance of the F-statistic for the increase in adjusted $R^{2,}$ as the F-statistic is biased when the number of observations per individual is small, and there is a serial correlation in the dependent variable. Instead, we test for the significance of audit partner fixed effects using an alternate non-parametric approach. Specifically, we create an empirical distribution of an increase in adjusted R^2 for audit partner fixed effects under the null hypothesis of no audit partner heterogeneity. To create such a distribution, we scramble audit partners' identities and randomly assign them to an actual audit partner-BHC spell and estimate model (5). Thus, for each audit partner-BHC spell, the assignment of the audit partner is random. We then store the magnitude of incremental adjusted R^2. This process is repeated 1,000 times. This gives us the empirical distribution of adjusted R^2 from the unconstrained model (5) under the null hypothesis of no audit partner heterogeneity or

[35] In comparison, Gul et al. find about 18 % t (10 %) [5 %] of the 861 audit partner effects they estimate in the abnormal accruals model using Chinese data are significant at the 10 % (5 %) [1 %] level. However, the percentage of estimated audit partner effects is only slightly different from the 1% (5 % , 10 %) in their bootstrapped tests, leading them to conclude that the audit partner fixed effects are significant.

no audit partner style by construction. We can then infer statistical significance at a given level, alpha, by comparing the increase in adjusted R^2 from our model to this empirical distribution under the null. If the increase in adjusted R^2 from our model is greater than the (1-alpha) percentile of our empirical distribution, then we can reject the null of no audit partner fixed effects at the alpha percent level. The benefit of this approach is that it does not rely on any distributional assumptions or the asymptotic properties of standard F-tests

Table 5 also reports the bootstrapped p-value of the incremental adjusted R^2 for model 5, the unconstrained model. The bootstrapped p-value for the incremental adjusted R^2 is 0.047 and 0.072 for the LLP and the ALL models, respectively. This suggests some (albeit weak) evidence of audit partner heterogeneity after controlling for relevant BHC characteristics and year and BHC and audit firm fixed effects. This result is obtained for the full sample period from 2006 to 2019. However, the percentage of estimated audit partner effects, though lower than observed in the actual data, remains high even in randomization tests. About 15 % (15 %) of our estimated audit partner fixed effects are significant at the 1% level for LLP (ALL). Another 11 % (11 %) and 7 % (8 %) are significant at the 5% and the 10% levels in the LLP (ALL) model in the bootstrapping tests, respectively. In contrast, Gul et al. (2013) report that the percentage of significant estimated audit partner effects is only slightly different from the 1 percent (5 percent, 10 percent) in their bootstrapped tests.[36]

While we do find evidence of partner heterogeneity, it is weaker than prior studies based on foreign jurisdictions. Thus, we want to ensure that our null results are not due to a lack of power in our test. To alleviate this concern, we undertake simulations where we add audit partner fixed effects and examine whether our tests can pick up the impact of audit partner fixed effects. Specifically, we use the actual data structure (relationships between year, BHC, audit firm, and audit partner) and draw observations from a standard normal distribution for control variable X, BHC fixed effects, audit firm fixed effects, and audit partner fixed effects. The dependent variable Y is the combination of X, BHC fixed effects, audit firm fixed effects, audit partner fixed effects, and noise. We vary the standard deviation for the audit partner fixed effects for each of the three

[36] Bertrand and Schoar (2003), employ an additional test of manager style. Specifically, they take average regression residuals from models that include firm-fixed effects, but no manager-fixed effects to estimate a manager's style at a given employer. They regress this estimated style at the new employer against the estimated style at the old employer. A positive association would be more direct evidence of the presence and persistence of individual styles across employers. However, this test is not suitable for our purpose as the bank manager would be expected to exercise discretion to smooth earnings via LLP. In other words, we do not expect that managerial discretion will result in consistently high or consistently low LLP.

simulation exercises (audit partner fixed effects are generated using a mean of 1 and standard deviation of 3, (2) [1] in SIMUL 1 (2) [3]). Results are presented in Appendix Table A2. In all three variations of simulated data (results available upon request), the adjusted R^2 with actual audit partner assignments is higher than the adjusted R^2 in the 1,000 iterations using randomized audit partner assignments.[37] This analysis validates our approach and suggests that our weak results cannot be attributed to the low power of the tests to detect audit partner fixed effects when one is present in the data.

Sub-sample Analysis

Although the Federal Reserve requires all top-tier BHCs with total consolidated assets of $500 million or more as of the end of an institution's fiscal year to have an annual audit of their consolidated financial statements (balance sheet, statement of income, changes in equity capital, and statement of cash flows, with accompanying footnote disclosure) by an independent public accountant, private BHCs are smaller, face less scrutiny from external constituents such as analysts, and are more likely to be audited by smaller audit firms. Thus, we examine public and private BHCs separately. Also, small BHCs (community BHCs with assets less than $10 billion) tend to be less complex and face lower regulatory scrutiny than larger BHCs (assets above $10 billion). Therefore, we examine sub-samples of BHCs partitioned on size (with total assets of less than $10 billion at the beginning of the year or more).[38] Moreover, the quality of an audit and auditors may differ between large and small audit firms (DeAngelo, 1981). Thus, we examine whether BHCs audited by smaller audit firms (SMALLAF) vs. large audit firms (LARGEAF) show evidence of audit partner heterogeneity. Our definition of SMALLAF is an audit firm that is not in the top 10 based on the number of BHCs audited or the number of audit partners in each year of our sample period. Seven audit firms are in the top 10 in both categories for each year in our sample and are designated LARGEAF for our analysis. We expect the partner differences, if

[37] We continue to find that the F statistic for the joint significance of the audit partner fixed effects is large and significant in all 1,000 randomizations confirming Fee et al.' s finding that the popular F-test approach for identifying individual fixed effects is problematic.

[38] Focusing on a sub-sample of small banks also allows us to rule out any inter office differences within the audit firm. There is prior literature that shows that there are audit quality differences between the largest offices of Big 4 firms and the other offices. The inter office differences dominate industry expertise when it comes earnings quality measured using abnormal accruals, earnings management to meet earnings benchmarks, the propensity to issue going concern reports, and incidence of client restatements (Francis & Yu 2009; Francis et al., 2013).

any, to manifest in sub-samples with higher complexity and, therefore, higher scope for partner judgment.

Table 6 reports the adjusted R^2 and the incremental adjusted R^2 from the estimation of equation (5) for our two main measures of BHCs' loan loss provisioning for each sub-sample.[39] Evidence shows that the bootstrapped p-values are significant for two sub-samples, BHCs that are public and BHCs that are audited by large audit firms. We delve into the composition of these two sub-samples to better understand the results. There is a significant overlap between these two sub-samples. Almost half of the BHCs audited by LARGEAF are public, and about 2/3rds of public BHCs are audited by LARGEAF. If we use large BHC or public BHC as a coarse proxy for complexity in estimates that may potentially require a higher level of judgment, more than half of the BHCs are large or public in the LARGEAF sub-sample, and by construction, all BHCs are large or public in the PUBLIC sub-sample.

So far, our evidence suggests that while we find weak evidence of audit partner heterogeneity in the full sample, we do find some evidence of audit partner heterogeneity in some of the sub-samples. We next investigate whether the financial crisis period is driving this result. As discussed earlier, not only did the financial crisis increase economic uncertainty, leading to large increases in LLP (from an average of 0.3% in 2006-2007 to 1.5% in 2008-2009) and ALL (from 1.4% in 2006-2007 to 1.8% in 2008-2009), that period also saw a large increase in partner turnover as the rules requiring mandatory partner rotation were adopted in 2003 leading to a higher incidence of partner rotation during the financial crisis period.[40]

Does crisis drive the results?

For the fixed effects test, examining the crisis period is challenging as this initial period is only four years (2006 to 2009, as our sample period begins in 2006). Therefore, we rely on comparing the results for the full period (2006 to 2019) to the post-crisis period (2010-2019).

Table 7 presents the results of the fixed effects approach for the full sample and the sub-samples, namely public BHCs or BHCs audited by LARGEAF, where we found evidence of partner heterogeneity for the post-financial crisis period 2010-2019. We present the results for all sub-samples in Appendix Table A7 for completeness. The bootstrapped p-value for the increase

[39] The descriptive statistics for the sub-samples are included in the Appendix Table A6.
[40] The descriptive statistics for the post crisis sample are included in the Appendix Table A4.

in adjusted R^2 is not significant in the full sample or any of the six sub-samples in the post-financial crisis period. Thus, it is plausible that partner heterogeneity manifests during periods of high economic uncertainty where greater judgment is involved and, hence, there is lesser standardization in audits.

Evidence from audit partner tenure length

Next, we turn to our second research question, which examines the association between loan loss provisioning outcomes and the length of the audit partner's tenure.

To test for the effect of partner tenure, we regress LLP (ALL) on a variable labeled TENURE, which equals the length of the partner tenure for each year. In other words, TENURE will be 1 in the first year of an engagement, 2 in the second year, and so on. We examine the full sample and the six sub-samples for the full sample period (2006-2019) and the post-financial crisis period (2010-2019). We perform additional checks conditional on the length of tenure to check the robustness of the results.

The results for the full sample are presented in Panel A of Table 8. Columns (1) and (3) show that the coefficient on TENURE is not significant for LLP but significant for ALL for the full sample period in a model that includes BHC-specific controls and the year, BHC, and audit firm fixed effects. This suggests that the allowance increases over the partner's tenure, but provisions do not. These results were also obtained in the post-financial crisis period, as shown in columns (2) and (4). This result differs from the null results of Gipper et al. (2021), who do not find evidence of the impact of partner tenure and tenure cycle on audit quality for mandatory partner rotations in non-financial public firms audited by Big 6 auditors. Given that higher allowance is not necessarily indicative of higher or lower earnings quality, it is difficult to interpret this result as evidence of the length of the tenure affecting earnings quality for BHCs. However, prior evidence suggests that higher allowance for banks implies higher auditor scrutiny (Stuber & Hogan, 2021; Ege et al., 2023).[41] Auditors consider income-increasing accruals to be a higher risk

[41] To better analyze the partner tenure dynamics, we also estimate the following year-by-year version of Eq. (6), in which we replace PARTNER TENURE with a series of dummy variables TENURE YEAR that captures each year in the tenure individually from 1 to 5 and gt5, where gt5 is a dummy variable equal to 1 if the tenure is greater than five years.

$$y_{i,t} = \beta X_{i,t} + \gamma_n * TENURE\ YEAR_{n,i,t} + YR_t + BHC_i + AF_{af} + \varepsilon_{i,t} \qquad (7)$$

TENURE YEAR$_3$ serves as the base period and is dropped from the model. The coefficients γ_1 to γ_n measure the incremental effects of a particular year in the tenure cycle relative to year 3. We examine this alternate specification to test whether the influence of a partner on loan loss provisions varies over the course of their tenure. Following Gipper et al. (2021), we make Tenure 3 the base year; hence, the coefficients on these dummy variables are interpreted as a deviation from the coefficient in Tenure Year 3. Since

from the point of view of misstatement and related litigation risk. Thus, increased audit scrutiny is more likely to result in higher allowance. Our finding is also consistent with Stuber and Hogan (2021), who document that, generally, the banks in their sample period spanning 2006-2017 were over-reserved.

As mentioned earlier, 3.2% (78) of the 2,455 audit partner-BHC spells in our full sample have audit tenure exceeding five years. Therefore, we conduct an additional check conditioning on the length of tenure. Untabulated analysis shows that while the coefficient on TENURE is not significant for the LLP model, the coefficient on TENURE is positive and significant for the ALL model at the 10% level in the full period (2006-2019), and the 5% level in the post-financial crisis period (2010-2019) when we exclude the audit partner-BHC spells longer than five years. This result appears to be consistent with the observation in Stuber & Hogan (2021) that banks have been over reserving in recent years.

Next, we turn to the tenure test results for sub-samples presented in Panel B of Table 8. The association between LLP and TENURE is not significant in any of the specifications except for large BHCs in the post-financial crisis period (column (4)). The negative coefficient for the large BHCs suggests that the LLP is decreasing in the length of partner tenure.[42] This result seems at odd with our earlier result that banks have been over-reserving in recent years. Thus, we examine this result further. It turns out that this result is influenced by just six observations where the audit partner-BHC spell for large BHCs is longer than five years. Untabulated analysis shows that this result is not obtained in the full or post-financial crisis period when we exclude these six audit partner-BHC spells.

The coefficient on TENURE for public BHCs is positive and significant at the 5% level in the ALL model (column 7). However, this association is not significant in the post-financial crisis period (column 8). It should be noted that while the mandatory partner rotations rule applies to public companies, SOX allows audit firms with fewer than five audit clients and fewer than ten partners to be exempt from the partner rotation, subject to a special review by the PCAOB, at least every three years. We have 28 audit partner-BHC spells spanning more than five years in the sub-

some partner tenures exceed five years, we have a dummy that captures all tenures longer than five years. Results documented in Appendix Table A8 suggest that ALL is higher in Year 5, the last year of a partner's tenure for the full sample period and the post financial crisis period. The coefficient on TENURE YEAR>5 is driven by 3% of audit partner-BHC spells, majority of which relate to the private banks.

[42] This result for large BHCs seems consistent with the findings of a concurrent working paper by Gopalan et al. (2024) who find that large banks systematically report higher loan loss reserves at the beginning of audit partner/client relationships relative to the later years for large BHCs. Their sample includes all BHCs, including BHCs with total assets above $100 billion.

sample of public BHCs. Untabulated results show that the coefficient on TENURE is positive and significant at the 10% level for public BHCs for the full period after excluding 28 audit partner-BHC spells exceeding five years. Thus, the positive trend in allowance over partner tenure for public BHCs is observed during the full period but is weakened when we exclude audit partner-BHC spells that last longer than five years. The coefficient on TENURE is positive and significant at the 5% level for the ALL model for the small BHCs in the full period and the post-financial crisis period (columns (5) and (6)). Prior evidence has documented that audit quality is increasing in the length of partner tenure, but only for small clients with longer tenure and not for small clients with shorter tenure (Manry et al., 2008). Seventy two out of the 78 audit partner-BHC spells, which are longer than five years in our full sample, are observed in small BHCs. Untabulated results show that the coefficient on TENURE in the ALL model is positive and significant at the 10% level for the full period and the post-financial crisis period for small BHCs after excluding 72 audit partner-BHC spells exceeding five years. The positive trend in allowance observed for small BHCs is not sensitive to the estimation period or the exclusion of spells with longer tenures.

Small BHCs tend to be more conservative, as reflected in their higher allowances and capital ratios. They often lack in-house expertise and sophisticated credit risk modeling and are willing to accept recommendations from auditors without challenge. Since auditors are often more concerned with under-reserving allowance than over-reserving and regulators encourage conservative allowance practices, finding a pattern in small banks is consistent with higher audit scrutiny and regulatory oversight.

Overall, the most consistent and robust pattern we observe is the positive trend in allowance over the partner tenure, which seems to be driven by small BHCs. This suggests increased audit scrutiny and an approach that aligns with regulatory oversight.

V. ADDITIONAL ANALYSES

We perform additional analyses using other measures related to loan loss provisioning to validate our results.

Other dependent variables

For the sake of completeness, we examine four additional measures: net charge-offs

(NCO), a flow measure of realized loan losses, non-performing loans (NPL), a stock measure of severe loan delinquencies, ADQ_NPL, a measure of loan allowance deflated by current NPLs, and ADQ_NCO, a measure of loan allowance deflated by future net charge-offs to capture further attributes of the loan loss provisioning of the BHCs. The first two measures are deflated by total loans at the beginning of the period.

Results included in Panel A of Appendix Table A9 show that adding audit partner fixed effects to the model results in a modest increase in the adjusted R^2 of 1.2% (2.6%) [3.1%] {1.6%} in the NCO (NPL) [ADQ_NPL] {ADQ_NCO} models for the period 2006-2019. The F-tests for the joint significance of audit partner fixed effects are large and significant. However, when we infer significance from incremental adjusted R^2 based on the random assignment of audit partners to actual audit partner-BHC spells, the bootstrapped p-values are not significant for any of the four measures. We obtain similar null results for all four measures during the post-crisis period 2010-2019.

When we examine the association of these additional measures with partner tenure, the coefficient on TENURE is not significant for any of the four variables for the full sample or post-financial crisis period (although the coefficient for ADQ_NPL in the post-crisis period is negative and weakly significant at 10% level).[43] Results are presented in Appendix Table A9, Panel B. Overall, we do not find evidence of heterogeneity between partners or association between partner tenure and loan provisioning outcomes using additional measures for the full period (2006-2019) or the post-crisis period (2010 to 2019), potentially reflecting the less discretionary nature of NPLs and NCOs.

VI. CONCLUSION

The primary objective of this paper is to examine whether audit partners affect financial reporting outcomes in the U.S. banking industry. It is important to examine this sample separately, not just due to the differing objectives of regulators and auditors regarding the reserving practices of banks but also due to the unusually high number of small private banks. Prior work, whether in the U.S. or foreign jurisdictions, has not examined a similar set of private firms. We focus on a

[43] The association between ADQ_NPL and TENURE is not significant when we exclude audit partner-BHC spells longer greater than five years for the full period and the post-financial crisis period.

key financial reporting outcome subject to managerial discretion and auditor and regulatory scrutiny. We ask two questions: First, does audit partner heterogeneity affect loan loss provisioning? Second, does the length of audit partner tenure influence loan loss provisioning?

The first question is crucial given the almost diametrically opposite views of the PCAOB and the audit firms. PCAOB argues that audit quality varies considerably between audit partners within an audit firm. In contrast, audit firms argue that an audit is shaped by an entire system of standards and quality control that includes standardized work procedures, centralized models of risk and materiality decisions, staff training, rigorous promotion process, concurrent partner reviews, access to in-house knowledge libraries, and subject matter experts, such that the idiosyncratic effect of an audit partner on financial reporting outcomes is mitigated.

We contribute to this debate by examining whether the individual audit partner heterogeneity affects loan loss provisioning for a sample of U.S. BHCs from 2006 to 2019. We find evidence of partner heterogeneity for both the loan loss provision and the allowance in the full sample and in sub-samples of BHCs that are public and audited by large audit firms, but only when we use the entire sample period (2006-2019), including the financial crisis years. We do not find evidence of audit partner heterogeneity in the post-financial crisis period (2010-2019).

Our results suggest that audit partner heterogeneity or style does not seem to affect financial reporting outcomes during normal times but is likely to manifest during periods of high economic uncertainty when the room for judgment is more likely. Thus, it seems that the audit firms' internal procedures, processes, and quality control mechanisms produce consistency in audits between partners, constraining idiosyncratic partner styles for banks during normal times.

We also undertake additional tests to examine whether partner tenure influences bank financial reporting outcomes. We find that allowance increases in the length of partner tenure for the full sample over the entire sample period (2006-2019), but the results are driven by public BHCs and small BHCs. The positive trend in allowance over the course of the partner's tenure is also obtained for small BHCs in the post-financial crisis period (2010-2019), consistent with the increased regulatory oversight and auditor scrutiny in recent years.

While a significant body of work investigates individual audit partner effects using non-financial firms, mainly in jurisdictions outside the U.S., our results should interest regulators, rule makers, investors, audit committees, and audit firms as most of the earlier evidence relates to non-financial firms. Our study provides valuable insights by focusing on the single most significant

accrual that involves considerable judgment in a sector whose contribution to the economy's financial stability cannot be overemphasized. In particular, our results speak to the conditions (periods of uncertainty) under which audit partner style may manifest. This finding should be of interest as we stand at a crossroads with banks switching to the current expected credit loss (CECL) model. Implementing CECL introduces increased subjectivity in the estimation and may pose different challenges for bank managers, audit firms, and regulators.

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Figure 1: Research Design

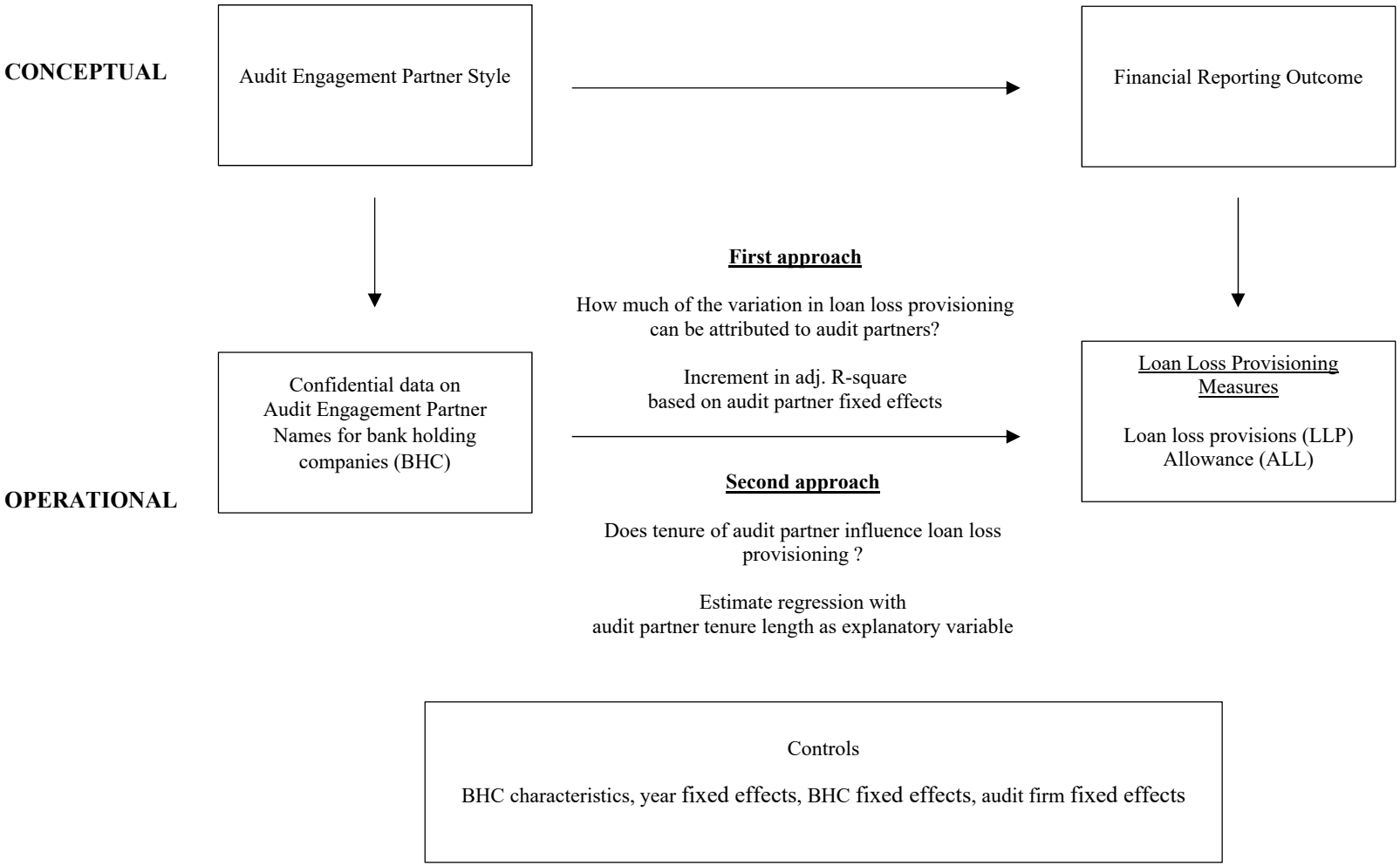

Figure 1 presents our main research design to identify the audit partner heterogeneity or style.

Table 1: Outline of the Sample Selection Process

Panel A of this table outlines the sample selection process. Panel B includes information about unique years (YEAR), bank holding companies (BHC), audit firms (AF), and audit partners (AP) for the full sample and various sub-samples based on public vs. private, size of BHC, and size of audit firms. The LARGEAF includes seven audit firms in the top ten for the number of BHC audits and aggregate BHC assets audited in each year between 2006 and 2019.

The BHC variables are obtained from the Y9C call reports available on the Chicago Federal Reserve website. The BHCs are identified as public based on the CRSP-FRB link file available from the New York Federal Reserve website. The confidential data on the names of the audit partners is obtained from the Federal Reserve Bank of Dallas. Variables are defined in Appendix Table A1.

Panel A: Sample selection

	N	UNIQUE BHCS
Observations between 2006 and 2019 with non-missing total assets	12,043	1,614
Excluding observations with missing BHC variables (LLP, ALL, SIZE, EBP, T1CAP, LG, RE_LOANS, COMM_LOANS, CONS_LOANS)	11,025	1,468
Excluding observations relating to BHCs with total assets above $100 billion	10,626	1,422
Excluding observations with missing AP names	9,918	1,309
Requiring at least two BHCs per AP and three years in which the AP audits the BHC	6,186	871

Panel B: Details of the sample used in the estimation

BHC SAMPLE	N	UNIQUE YEARs	UNIQUE BHCs	UNIQUE AFs	UNIQUE APs
FULL	6,186	14	871	88	360
PRIVATE	2,971	14	447	71	188
PUBLIC	2,492	14	339	42	187
SMALL (TA<$10 BILLION)	5,220	14	769	87	308
LARGE (TA>=$10 BILLION)	520	14	71	12	48
AUDITED BY **SMALLAF**	2,931	14	462	73	178
AUDITED BY **LARGEAF**	3,173	14	446	7	181

Table 2: Summary statistics for BHCs

This table reports the number of observations, mean, standard deviation (SD), 25th percentile, median, and 75th percentile for the two main dependent variables, loan loss provisions (LLP) and allowance (ALL), additional dependent variables, net charge-offs (NCO), non-performing loans (NPL), adequacy related to NPL (ADQ_NPL) and adequacy related to NCO (ADQ_NCO), and control variables used in this study for the full sample.

The BHC variables are obtained from the Y9C call reports available on the Chicago Federal Reserve website. Variables are defined in Appendix Table A1.

VARIABLE	N	MEAN	SD	P25	MEDIAN	P75
ASSETS ($millions)	6,186	3,400	6,111	764	1,255	2,818
Dependent Variables						
LLP (%)	6,186	0.700	1.035	0.141	0.329	0.812
ALL (%)	6,186	1.673	0.732	1.201	1.496	1.958
NCO (%)	6,186	0.617	0.908	0.090	0.265	0.746
NPL (%)	6,186	2.162	2.439	0.658	1.342	2.725
ADQ_NPL	6,156	2.147	3.821	0.624	1.078	2.037
ADQ_NCO	5,670	7.583	43.977	1.585	3.883	9.352
Controls						
SIZE (LOG)	6,186	14.256	1.030	13.491	13.984	14.774
EBP (%)	6,186	1.352	0.888	0.968	1.421	1.813
T1CAP (%)	6,186	12.975	4.102	10.540	12.310	14.520
LG (%)	6,186	6.709	14.057	-1.075	5.435	12.170
ΔNPL (%)	6,186	0.116	1.593	-0.420	0.003	0.513
RE_LOANS (%)	6,186	74.710	14.591	66.925	77.165	85.228
COMM_LOANS (%)	6,186	17.468	11.117	9.356	15.154	22.761
CONS_LOANS (%)	6,186	4.599	6.227	0.982	2.411	5.204

Table 3: Interrelationships between audit partners, audit firms, BHCs, and years

This table reports the mean, standard deviation (SD), 25th percentile, median, and 75th percentile of the interrelationships between the audit partners (AP), audit firms (AF), bank holding companies (BHC), and years (YEAR) for the full sample (N=6,186).

The confidential data on the names of the APs is obtained from the Federal Reserve Bank of Dallas. Variables are defined in Appendix Table A1.

	MEAN	SD	P25	MEDIAN	P75
<u>For the 360 unique APs</u>					
Number of YEARs per AP	8.6	3.0	6.0	8.5	11.0
Number of BHCs per AP	3.8	2.2	2.0	3.0	5.0
Number of AFs per AP	1.2	0.5	1.0	1.0	1.0
<u>For the 88 unique AFs</u>					
Number of YEARs per AF	8.7	4.5	5.0	9.0	13.0
Number of BHCs per AF	12.0	22.3	2.0	4.0	14.0
Number of APs per AF	4.9	8.0	1.0	2.0	4.5
<u>For the 871 unique BHCs</u>					
Number of YEARs per BHC	7.1	3.3	4.0	7.0	10.0
Number of AFs per BHC	1.2	0.5	1.0	1.0	1.0
Number of APs per BHC	1.6	0.7	1.0	1.0	2.0
<u>For the 14 unique YEARs</u>					
Number of BHCs per YEAR	441.9	166.3	344.0	508.0	577.0
Number of AFs per YEAR	54.5	15.0	43.0	61.5	65.0
Number of APs per YEAR	220.1	64.2	193.0	245.5	274.0

Table 4: Loan loss provisioning models - baseline model and model including fixed effects

Panel A and Panel B of this table report the results of the regression models in equations (1) to (5) for our two main dependent variables, loan loss provisions (LLP) and allowance (ALL), respectively, for the full sample. The fixed effects relating to years (YEAR), bank holding companies (BHC), audit firms (AF), and audit partners (AP) included in the regression are reported in the respective columns.

The BHC variables are obtained from the Y9C call reports available on the Chicago Federal Reserve website. The confidential data on the names of the audit partners is obtained from the Federal Reserve Bank of Dallas. Variables are defined in Appendix Table A1. Standard errors are in parenthesis. Levels of significance are denoted as follows: *if p<0.10; ** if p<0.05; *** if p<0.01.

Panel A: Regression models for LLP

Dependent Variable	**LLP** (1)	**LLP** (2)	**LLP** (3)	**LLP** (4)	**LLP** (5)
SIZE	0.000*	0.001***	0.005***	0.005***	0.006***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
EBP	-0.139***	-0.109***	-0.185***	-0.189***	-0.185***
	(0.038)	(0.037)	(0.035)	(0.036)	(0.036)
T1CAP	-0.012	-0.003	-0.002	-0.002	0.004
	(0.012)	(0.012)	(0.008)	(0.008)	(0.008)
LG	-0.022***	-0.011***	-0.007***	-0.006***	-0.005***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
ΔNPL	0.230***	0.155***	0.125***	0.126***	0.121***
	(0.014)	(0.014)	(0.014)	(0.015)	(0.015)
RE_LOANS	-0.008	-0.010	0.008*	0.009**	0.006
	(0.008)	(0.008)	(0.005)	(0.005)	(0.006)
COMM_LOANS	-0.011	-0.013	0.004	0.006	0.006
	(0.009)	(0.008)	(0.006)	(0.006)	(0.007)
CONS_LOANS	-0.003	-0.007	0.001	0.004	-0.006
	(0.006)	(0.005)	(0.009)	(0.009)	(0.010)
CONSTANT	0.0143*	0.003	-0.082***	-0.082***	-0.095***
	(0.008)	(0.008)	(0.018)	(0.019)	(0.019)
N	6,186	6,186	6,186	6,186	6,186
Adj. R^2	22.9%	37.1%	57.7%	58.1%	59.6%
YEAR FE	No	Yes	Yes	Yes	Yes
BHC FE	No	No	Yes	Yes	Yes
AF FE	No	No	No	Yes	Yes
AP FE	No	No	No	No	Yes
PERIOD	2006-2019	2006-2019	2006-2019	2006-2019	2006-2019

Panel B: Regression models for ALL

Dependent Variable	**ALL** (1)	**ALL** (2)	**ALL** (3)	**ALL** (4)	**ALL** (5)
SIZE	-0.001	0.001	-0.002***	-0.003***	-0.003***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
EBP	-0.073***	-0.060**	-0.101***	-0.101***	-0.098***
	(0.028)	(0.029)	(0.022)	(0.022)	(0.022)
T1CAP	0.023***	0.019**	0.009	0.009	0.010**
	(0.009)	(0.009)	(0.006)	(0.007)	(0.006)
LG	-0.014***	-0.008***	-0.002**	-0.002**	-0.001
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
ΔNPL	0.059***	0.066***	0.056***	0.058***	0.060***
	(0.010)	(0.010)	(0.009)	(0.009)	(0.009)
RE_LOANS	-0.004	-0.006	-0.004	-0.004	-0.007
	(0.005)	(0.006)	(0.005)	(0.005)	(0.005)
COMM_LOANS	-0.004	-0.007	-0.004	-0.004	-0.006
	(0.006)	(0.006)	(0.005)	(0.005)	(0.006)
CONS_LOANS	-0.004	-0.005	-0.006	-0.007	-0.014*
	(0.006)	(0.007)	(0.008)	(0.008)	(0.008)
CONSTANT	0.023***	0.016***	0.065***	0.061***	0.054***
	(0.006)	(0.007)	(0.012)	(0.013)	(0.013)
N	6,186	6,186	6,186	6,186	6,186
Adj.R^2	10.2%	20.6%	69.0%	69.5%	73.2%
YEAR FE	No	Yes	Yes	Yes	Yes
BHC FE	No	No	Yes	Yes	Yes
AF FE	No	No	No	Yes	Yes
AP FE	No	No	No	No	Yes
PERIOD	2006-2010	2006-2010	2006-2010	2006-2010	2006-2010

Table 5: Evidence from audit partner fixed effects

This table reports the results of the benchmark regression and the fixed effects panel regressions for our two main dependent variables, loan loss provisions (LLP) and allowance (ALL), respectively, for the full sample. For each dependent variable in column (1), the fixed effects included in addition to the control variables are reported in column (2). The adjusted R^2 and the increase in adjusted R^2 resulting from including fixed effects relating to years (YEAR), bank holding companies (BHC), audit firms (AF), and audit partners (AP) are reported in columns (3) and (4), respectively. Rows 1 to 5 include the estimation results of the regression models represented in equations (1) to (5), respectively. Row 6 includes the results based on 1,000 iterations using random audit partner assignments, where the adjusted R^2 and the increase in adjusted R^2 are average for the 1,000 iterations. The range of adjusted R^2 for the 1,000 randomization iterations is reported in column (5). Column (6) reports the p-value for the randomization, which is the percentage of 1,000 iterations with a random audit partner in which the adjusted R2 is higher than the adjusted R2 based on the actual audit partner.

The BHC variables are obtained from the Y9C call reports available on the Chicago Federal Reserve website. The confidential data on the names of the audit partners is obtained from the Federal Reserve Bank of Dallas. Variables are defined in Appendix Table A1. Standard errors are in parenthesis. Levels of significance are denoted as follows: *if p<0.10; ** if p<0.05; *** if p<0.01.

Dependent Variable	**Fixed Effects**	**Adj. R^2**	**Increase in Adj. R^2**	**Range of Adj. R^2**	**p-value**
(1)	(2)	(3)	(4)	(5)	(6)
LLP	PERIOD 2006-2019 (N=6,186)				
	Controls	22.9%			
	Controls, YEAR	37.1%	14.3%		
	Controls, YEAR, BHC	57.7%	20.6%		
	Controls, YEAR, BHC, AF	58.1%	0.3%		
	Controls, YEAR, BHC, AF, AP	59.6%	1.5%		
	Controls, YEAR, BHC, AF, Random AP	59.3%	1.2%	58.6% - 60.0%	0.047**
ALL	PERIOD 2006-2019 (N=6,186)				
	Controls	10.2%			
	Controls, YEAR	20.6%	10.4%		
	Controls, YEAR, BHC	69.0%	48.4%		
	Controls, YEAR, BHC, AF	69.4%	0.4%		
	Controls, YEAR, BHC, AF, AP	73.2%	3.8%		
	Controls, YEAR, BHC, AF, Random AP	72.8%	3.4%	72.0%-73.9%	0.072*

Table 6: Evidence on partner heterogeneity from sub-sample analyses

The table reports the results for sub-samples conditioned on public vs. private, BHC size, and audit firm size.

Row 1 and row 2 report the adjusted R^2 and increases in adjusted R^2 for the two main variables, LLP and ALL, from the regression model in equation (5) for various sub-samples, respectively. The regression model in equation (5) includes control variables and fixed effects for the year, BHC, audit firm, and audit partner. For each sub-sample, columns (1), (3), (5), and (7) include results based on actual audit partner assignments. Columns (2), (4), (6), and (8) include the average adjusted R^2, the average increase in adjusted R^2, and the p-value based on 1,000 iterations using random audit partner assignments, respectively. The p-value is the percentage of 1,000 iterations with a random audit partner in which the adjusted R2 is higher than the adjusted R2 based on the actual audit partner.

The BHC variables are obtained from the Y9C call reports available on the Chicago Federal Reserve website. The confidential data on the names of the audit partners is obtained from the Federal Reserve Bank of Dallas. Variables are defined in Appendix Table A1. Standard errors are in parenthesis. Levels of significance are denoted as follows: * if p<0.10; ** if p<0.05; *** if p<0.01.

Dependent Variable	**LLP**				**ALL**			
Audit Partner=	Actual	Random	Actual	Random	Actual	Random	Actual	Random
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	PRIVATE		**PUBLIC**		**PRIVATE**		**PUBLIC**	
ADJ. R^2	61.0%	60.8%	58.8%	58.1%	75.3%	76.1%	74.3%	73.2%
INC. IN ADJ. R2	0.3%	0.1%	2.6%	1.9%	1.8%	2.6%	5.3%	4.2%
P-VALUE		0.14		0.03**		0.98		0.01***
PERIOD 2006-2019	N=2,971		N=2,492		N=2,971		N=2,492	
	SMALL		**LARGE**		**SMALL**		**LARGE**	
ADJ. R^2	59.2%	59.2%	71.4%	71.4%	72.7%	72.8%	78.5%	78.8%
INC. IN ADJ. R2	0.8%	0.8%	1.8%	1.8%	2.5%	2.6%	3.6%	3.9%
P-VALUE		0.41		0.51		0.76		0.71
PERIOD 2006-2019	N=5,220		N=520		N=5,220		N=520	
	SMALLAF		**LARGEAF**		**SMALLAF**		**LARGEAF**	
ADJ. R^2	62.7%	62.4%	57.4%	56.8%	74.5%	74.5%	72.9%	72.0%
INC. IN ADJ. R2	1.0%	0.7%	1.8%	1.2%	2.4%	2.4%	4.9%	4.0%
P-VALUE		0.10		0.03**		0.57		0.02**
PERIOD 2006-2019	N=2,931		N=3,173		N=2,931		N=3,173	

Table 7: Heterogeneity between audit partners in the post-financial crisis period. Are results driven by the crisis period?

The table reports the results for the full sample and selected sub-samples.

Row 1 and row 2 report the adjusted R^2 and increases in adjusted R^2 for the two main variables, LLP and ALL, from the regression model in equation (5) for the full sample and two sub-samples, public BHCs and BHCs audited by large audit firms for the post-financial crisis period 2010-2019. The regression model in equation (5) includes control variables and fixed effects for the year, BHC, audit firm, and audit partner. For each panel, columns (1) and (3) include results based on actual audit partner assignments. Columns (2) and (4) have the average adjusted R^2, the average increase in adjusted R^2, and the p-value based on 1,000 iterations using random audit partner assignments, respectively. The p-value is the percentage of 1,000 iterations with a random audit partner in which the adjusted R2 is higher than the adjusted R2 based on the actual audit partner.

The BHC variables are obtained from the Y9C call reports available on the Chicago Federal Reserve website. The confidential data on the names of the audit partners is obtained from the Federal Reserve Bank of Dallas. Variables are defined in Appendix Table A1. Standard errors are in parenthesis. Levels of significance are denoted as follows: * if p<0.10; ** if p<0.05; *** if p<0.01.

Dependent Variable	**LLP**		**ALL**	
Audit Partner=	Actual	Random	Actual	Random
	(1)	(2)	(3)	(4)
	FULL		**FULL**	
ADJ. R^2	55.2%	55.2%	84.0%	84.5%
INC. IN ADJ. R2	1.4%	1.4%	3.6%	4.1%
P-VALUE		0.66		0.96
PERIOD 2010-2019	N=3,626		N=3,626	
	PUBLIC		**PUBLIC**	
ADJ. R^2	49.9%	49.7%	84.6%	84.8%
INC. IN ADJ. R2	3.3%	3.1%	5.5%	5.7%
P-VALUE		0.39		0.75
PERIOD 2010-2019	N=1,377		N=1,377	
	LARGEAF		**LARGEAF**	
ADJ. R^2	48.5%	48.5%	84.0%	84.2%
INC. IN ADJ. R2	1.5%	1.5%	4.1%	4.3%
P-VALUE		0.48		0.74
PERIOD 2010-2019	N=1,946		N=1,946	

Table 8: Evidence from audit partner tenure

The table reports analyses of the relation between audit partner tenure (engagement) and our two main dependent variables, loan loss provisions (LLP) and allowance (ALL), respectively, for the full sample and sub-samples for the full period (2006-2019) and the post-financial crisis period (2010-2019).

Panel A reports coefficient estimates for TENURE, reflecting the number of consecutive years the audit partner manages the engagement based on equation (6) for the full sample. Columns (1) and (3) report the results for the full period. Columns (2) and (4) report the results for the post-financial crisis period. The regression includes control variables and fixed effects relating to years (YEAR), bank holding companies (BHC), and audit firms (AF). The coefficients have been multiplied by 1000.

Panel B reports coefficient estimates for TENURE, reflecting the number of consecutive years the audit partner manages the engagement based on equation (6) for the various sub-samples. Columns (1), (3), (5), and (7) report the results for the full period. Columns (2), (4), (6), and (8) report the results for the post-financial crisis period. The coefficients have been multiplied by 1000.

The BHC variables are obtained from the Y9C call reports available on the Chicago Federal Reserve website. The confidential data on the names of the audit partners is obtained from the Federal Reserve Bank of Dallas. Variables are defined in Appendix Table A1. Standard errors are in parenthesis. Levels of significance are denoted as follows: *if p<0.10; ** if p<0.05; *** if p<0.01.

Panel A: Aggregate tenure effects for the full sample

Dependent Variable	**LLP**		**ALL**	
	(1)	(2)	(3)	(4)
TENURE	0.014	-0.042	0.118**	0.103**
	(0.074)	(0.066)	(0.051)	(0.047)
N	7,294	5,247	7,294	5,247
R^2	65.4%	68.5%	74.0%	82.1%
CONTROLS	Yes	Yes	Yes	Yes
YEAR FE	Yes	Yes	Yes	Yes
BHC FE	Yes	Yes	Yes	Yes
AF FE	Yes	Yes	Yes	Yes
PERIOD	2006-2019	2010-2019	2006-2019	2010-2019

Panel B: Aggregate tenure effects for sub-samples

Dependent Variable	LLP				ALL			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	PRIVATE		**PUBLIC**		**PRIVATE**		**PUBLIC**	
TENURE	0.056	-0.007	0.010	-0.105	0.064	0.066	0.147**	0.094
	(0.101)	(0.100)	(0.107)	(0.091)	(0.063)	(0.070)	(0.075)	(0.062)
N	3,360	2,566	3,934	2,681	3,360	2,566	3,934	2,681
R^2	71.8%	74.7%	62.2%	61.62%	78.4%	84.7%	71.8%	81.2%
	SMALL		**LARGE**		**SMALL**		**LARGE**	
TENURE	0.040	0.012	-0.055	-0.269**	0.122**	0.111**	0.072	0.013
	(0.082)	(0.075)	(0.150)	(0.124)	(0.057)	(0.053)	(0.103)	(0.106)
N	6,157	4,410	1,137	837	6,157	4,410	1,137	837
R^2	65.9%	67.8%	69.1%	66.6%	73.8	82.1%	78.9%	84.1%
	SMALLAF		**LARGEAF**		**SMALLAF**		**LARGEAF**	
TENURE	0.022	0.028	(0.023)	-0.118	0.109	0.017	0.092	0.043
	(0.102)	(0.111)	(0.107)	(0.084)	(0.071)	(0.076)	(0.072)	(0.063)
N	3,358	2,357	3,936	2,890	3,358	2,357	3,936	2,890
R^2	69.8%	72.1%	63.4%	63.8%	78.0%	85.4%	72.5%	80.6%
CONTROLS	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
YEAR FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
BHC FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
AF FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SAMPLE PERIOD	2006-2019	2010-2019	2006-2019	2010-2019	2006-2019	2010-2019	2006-2019	2010-2019

Appendix

Table A1: Variable Definitions

Variable	Explanation	Data from Y9C
ADQ_NCO	Allowance for loan and lease losses$_t$ / Net chargeoffs$_{t+1}$	For Q1: Bhck3123$_t$/(bhck4635$_{t+1}$ - bhck4605$_{t+1}$) For Q2-Q4: Bhck3123$_t$/ (Δbhck4635$_{t+1}$ - Δbhck4605$_{t+1}$)
ADQ_NPL	Allowance for loan and lease losses$_t$ / (Past due 90 days or more and still accruing$_t$ + nonaccrual$_t$)	Prior to 2018: bhck3123$_t$ / (bhck5525$_t$ + bhck5526$_t$) 2018 Onward: bhck3123$_t$ /(Δbhck1407$_t$ + Δbhck3506$_t$ + Δbhck1403$_t$ + Δbhck3507$_t$)
AF	Audit Firm	textc703
ALL	Loan loss allowances$_t$ / Loans$_{t-1}$	bhck3123$_t$ / bhck2122$_{t-1}$
AP	Audit Partner	textc704
ASSETS	Total assets in millions$_t$	bhck2170$_t$ / 1000
BHC	Bank holding company	rssd9001
COMM_LOANS	Commercial loans$_{t-1}$ / Loans$_{t-1}$	Before 2019 Q4: (bhck1763$_{t-1}$ + bhck1764$_{t-1}$ + bhck1590$_{t-1}$) / bhck2122$_{t-1}$ 2019 Q4 Onward: (bhck1763$_{t-1}$ + bhck1764$_{t-1}$ + bhckkx56$_{t-1}$ + bhck1590$_{t-1}$) / bhck2122$_{t-1}$
CONS_LOANS	Consumer loans$_{t-1}$ / Loans$_{t-1}$	Before 2011: (bhckb538$_{t-1}$ + bhckb539$_{t-1}$ + bhck2011$_{t-1}$) / bhck2122$_{t-1}$ 2011 Onward: (bhckb538$_{t-1}$ + bhckb539$_{t-1}$ + bhckk137$_{t-1}$ + bhckk207$_{t-1}$) / bhck2122$_{t-1}$
EBP	Earnings before provisions$_t$ / Total Assets$_{t-1}$	For Q1: (bhck4301$_t$ + bhck4230$_t$) / bhck2170$_{t-1}$ For Q2-Q4: (Δbhck4301$_t$ + Δbhck4230$_t$) / bhck2170$_{t-1}$
FE	Fixed effects	
LARGE	Sub-sample of BHCs with total assets equal to or above $10 billion for the year (but below $100 billion)	
LARGEAF	A sub-sample of BHCs audited by seven audit firms in the top 10 based on the highest number of BHC audits and aggregate BHC assets audited each year in the sample period.	
LG	One-year loan growth$_t$	(bhck2122$_t$ - bhck2122$_{t-1}$) / bhck2122$_{t-1}$
LLP	Loan loss provisions$_t$ / Loans$_{t-1}$	For Q1: bhck4230$_t$ / bhck2122$_{t-1}$ For Q2-Q4: Δbhck4230$_t$ / bhck2122$_{t-1}$
NCO	Net charge-offs$_t$ / Loans$_{t-1}$	For Q1: (bhck4635$_t$ - bhck4605$_t$) / bhck2122$_{t-1}$ For Q2-Q4: (Δbhck4635$_t$ - Δbhck4605$_t$) / bhck2122$_{t-1}$
SMALLAF	A sub-sample of BHCs audited by audit firms is not classified as LARGEAF (the seven audit firms in the top 10 are based on the highest number of BHC audits	

	and aggregate BHC assets audited each year in the sample period).	
NPL	Non-performing $loans_t$ / $Loans_{t-1}$	Before 2018: $(bhck5525_t + bhck5526_t) / bhck2122_{t-1}$ 2018 Onward: $(bhck1407_t + bhck3506_t + bhck1403_t + bhck3507_t) / bhck2122_{t-1}$
PRIVATE	Sub-sample of BHCs that are not listed as a public company during the year	
PUBLIC	Sub-sample of BHCs that are listed as a public company during the year	
RE_LOANS	Real estate $loans_t$ / $Loans_t$	$bhck1410_{t-1} / bhck2122_t$
SIZE	Log(Total assets in $thousands_{t-1}$)	$Log(bhck2170_{t-1})$
SMALL	Sub-sample of BHCs with total assets below $10 billion for the year.	
T1CAP	Tier 1 capital $ratio_{t-1}$	Before 2015: $bhck7206_{t-1}$ 2015 Onward: $bhca7206_{t-1}$
TENURE	The number of consecutive years that the audit partner manages the engagement	
TENURE YEAR n	Separate dummy variables marking the nth year of the tenure cycle of the audit partner	
YEAR	Year	rssd9999
ΔNPL	One-year change in NPL_t / $Loans_{t-1}$	Before 2018: $(\Delta bhck5525_t + \Delta bhck5526_t) / bhck2122_{t-1}$ 2018 Onward: $(\Delta bhck1407_t + \Delta bhck3506_t + \Delta bhck1403_t + \Delta bhck3507_t)/ bhck2122_{t-1}$

Table A2: Simulation Exercise

The following table reports the results of the benchmark regression in models (4) and (5) using simulated data on the actual data structure, where the relationships between year, BHC, audit firm (AF), and audit partner (AP)are preserved. Specifically, we draw observations from a standard normal distribution for control variable X, BHC fixed effects, AF fixed effects, and audit partner fixed effects. The dependent variable Y is the combination of X, BHC fixed effects, audit firm fixed effects, audit partner fixed effects, and noise. We vary the standard deviation for the audit partner fixed effects for each of the three simulation exercises as indicated in column (1). The adjusted R2 and the increase in adjusted R2 resulting from including fixed effects are reported in columns (3) and (4), respectively. Rows 1 and 2 include the estimation results of the regression models represented in equations (4) and (5), respectively. Row 3 includes the results based on 1,000 iterations using random audit partner assignments. The adjusted R2 and the increase in adjusted R2 are average for the 1,000 iterations. The range of adjusted R2 for the 1,000 randomization iterations is reported in column (5). Column (6) reports the p-value for the randomization, which is the percentage of 1,000 iterations with random audit partner in which the adjusted R2 is higher than the adjusted R2 based on actual audit partner.

Dependent Variable	Fixed Effects	Adj. R^2	Increase in Adj. R^2	Range of Adj. R^2	p-value for randomization
(1)	(2)	(3)	(4)	(5)	(6)
AP fixed effects	Controls, YEAR, BHC, AF	86.0%			
Mean=1	Controls, YEAR, BHC, AF, AP	96.8%	10.8%		
Std. Dev =3	Controls, YEAR, BHC, AF, Random AP	94.5%	8.5%	93.7%-95.2%	0.00
AP fixed effects	Controls, YEAR, BHC, AF	90.7%			
Mean=1	Controls, YEAR, BHC, AF, AP	96.3%	5.6%		
Std. Dev=2	Controls, YEAR, BHC, AF, Random AP	95.1%	4.4%	94.7%-95.6%	0.00
AP fixed effects	Controls, YEAR, BHC, AF	94.4%			
Mean=1	Controls, YEAR, BHC, AF, AP	95.9%	1.5%		
Std. Dev=1	Controls, YEAR, BHC, AF, Random AP	95.6%	1.2%	93.9%-95.7%	0.00

Table A3: Summary statistics for the sample used in aggregate tenure tests

This table reports the number of observations, mean, standard deviation (SD), 25th percentile, median, and 75th percentile for the two main dependent variables, loan loss provisions (LLP) and allowance (ALL), additional dependent variables, net charge-offs (NCO), non-performing loans (NPL), adequacy related to NPL (ADQ_NPL) and adequacy related to NCO (ADQ_NCO), and control variables used in this study for BHCs for the aggregate tenure tests.

The BHC variables are obtained from the Y9C call reports available on the Chicago Federal Reserve website. Variables are defined in Appendix Table A1.

VARIABLE	N	MEAN	SD	P25	MEDIAN	P75
ASSETS ($millions)	7,294	3,899	6,609	853	1,444	3,483
Dependent Variables						
LLP (%)	7,294	0.673	1.029	0.118	0.300	0.776
ALL (%)	7,294	1.645	0.737	1.169	1.470	1.930
NCO (%)	7,294	0.602	0.912	0.078	0.245	0.721
NPL (%)	7,294	21.192	24.484	6.237	12.853	26.581
ADQ_NPL	7,251	2.162	3.811	0.631	1.097	2.054
ADQ_NCO	6,389	7.736	45.869	1.622	3.881	9.713
Controls						
SIZE (LOG)	7,294	14.397	1.055	13.614	14.113	14.988
EBP (%)	7,294	1.358	0.888	0.982	1.419	1.813
T1CAP (%)	7,294	13.109	4.159	10.670	12.410	14.655
LG (%)	7,294	7.110	14.318	(0.666)	5.687	12.176
ΔNPL (%)	7,294	0.092	1.575	(0.408)	(0.004)	0.444
RE_LOANS (%)	7,294	74.198	15.444	66.482	76.957	85.210
COMM_LOANS (%)	7,294	17.769	11.336	9.584	15.271	23.329
CONS_LOANS (%)	7,294	4.610	6.528	0.892	2.244	5.067

Table A4: Summary statistics for the full sample for the post-financial crisis period 2010-2019

This table reports the number of observations, mean, standard deviation (SD), 25th percentile, median, and 75th percentile for the two main dependent variables, loan loss provisions (LLP) and allowance (ALL), additional dependent variables, net charge-offs (NCO), non-performing loans (NPL), adequacy related to NPL (ADQ_NPL) and adequacy related to NCO (ADQ_NCO), and control variables used in this study for our full sample BHCs in the post-financial crisis period 2010-2019.

The BHC variables are obtained from the Y9C call reports available on the Chicago Federal Reserve website. Variables are defined in Appendix Table A1.

VARIABLE	N	MEAN	SD	P25	MEDIAN	P75
ASSETS ($millions)	3,626	3,819	6,425	859	1,451	3,402
Dependent Variables						
LLP (%)	3,626	0.482	0.789	0.101	0.250	0.529
ALL (%)	3,626	1.650	0.725	1.174	1.483	1.951
NCO (%)	3,626	0.500	0.785	0.070	0.217	0.572
NPL (%)	3,626	1.953	2.244	0.647	1.226	2.339
ADQ_NPL	3,626	2.154	3.678	0.689	1.159	2.058
ADQ_NCO	3,219	9.239	51.074	2.501	5.431	12.809
Controls						
SIZE (LOG)	3,626	14.384	1.038	13.624	14.108	14.947
EBP (%)	3,626	1.399	0.773	1.024	1.423	1.785
T1CAP (%)	3,626	13.657	4.319	11.240	12.980	15.200
LG (%)	3,626	7.277	14.147	-0.356	5.690	12.049
ΔNPL (%)	3,626	-0.291	1.306	-0.624	-0.121	0.147
RE_LOANS (%)	3,626	74.381	15.152	66.126	77.293	85.355
COMM_LOANS (%)	3,626	17.850	11.562	9.395	15.046	23.607
CONS_LOANS (%)	3,626	4.167	6.128	0.840	2.096	4.295

Table A5: Correlations

The table reports the correlations for the two main dependent variables, loan loss provisions (LLP) and allowance (ALL), additional dependent variables, net charge-offs (NCO), non-performing loans (NPL), adequacy related to NPL (ADQ_NPL) and adequacy related to NCO (ADQ_NCO), and control variables used in this study for full sample (N=6,186). Levels of significance are denoted as follows: * if p<0.10; ** if p<0.05; *** if p<0.01.

	LLP	ALL	NCO	NPL	ADQ_NPL	ADQ_NCO	SIZE	EBP	T1CAP	LG	ΔNPL	RE_LOANS	COMM_LOANS
ALL	0.599***												
NCO	0.893***	0.574***											
NPL	0.615***	0.559***	0.631***										
ADQ_NPL	-0.155***	-0.041***	-0.187***	-0.317***									
ADQ_NCO	-0.065***	-0.026**	-0.065***	-0.059***	0.018								
SIZE	-0.007	-0.080***	-0.018	-0.080***	-0.032**	0.009							
EBP	-0.200***	-0.144***	-0.265***	-0.320***	0.149***	0.009	0.174***						
T1CAP	-0.121***	0.073***	-0.112***	-0.085***	0.108***	-0.005	-0.021	0.209***					
LG	-0.290***	-0.258***	-0.404***	-0.302***	0.141***	0.012	0.115***	0.329***	0.124***				
ΔNPL	0.313***	0.079***	0.085***	0.329***	-0.085***	-0.027**	0.006	0.037***	-0.081***	0.137***			
RE_LOANS	0.033**	0.033**	0.051***	0.209***	-0.133***	0.023*	-0.234***	-0.239***	-0.037***	-0.090***	0.011		
COMM_LOANS	-0.065***	-0.059***	-0.088***	-0.172***	0.072***	-0.018	0.158***	0.152***	-0.104***	0.093***	-0.005	-0.767***	
CONS_LOANS	0.031**	0.004	0.026**	-0.089***	0.050***	0.007	0.100***	0.147***	0.061***	-0.006	0.014	-0.442***	-0.045***

Table A6: Descriptive statistics for sub-samples for the full period 2006-2019

Panel A reports the means in columns (3) and (4), the p-value of the difference in means in column (5), the medians in columns (6) and (7), and the p-value of the difference in medians in column (8) for PRIVATE vs. PUBLIC BHCs, SMALL vs LARGE BHCs, BHCs audited by SMALLAF vs LARGEAF. Levels of significance are denoted as follows: ** if p<0.05; *** if p<0.01.

	N	N	Mean	Mean	p-value of diff	Median	Median	p-value of diff
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	PRIVATE	**PUBLIC**	**PRIVATE**	**PUBLIC**		**PRIVATE**	**PUBLIC**	
ASSETS ($millions)	2,971	2,492	1,500	5,300	***	936	2,200	***
LLP (%)	2,971	2,492	0.670	0.710	ns	0.320	0.340	ns
ALL (%)	2,971	2,492	1.710	1.610	***	1.530	1.440	***
NCO (%)	2,971	2,492	0.600	0.620	ns	0.250	0.280	**
NPL (%)	2,971	2,492	2.160	2.090	ns	1.330	1.330	ns
ADQ_NPL	2,952	2,485	2.381	1.882	***	1.152	1.043	***
ADQ_NCO	2,703	2,315	9.321	5.644	***	4.277	3.601	***
SIZE	2,971	2,492	13.838	14.715	***	13.703	14.532	***
EBP (%)	2,971	2,492	1.300	1.430	***	1.340	1.510	***
T1CAP (%)	2,971	2,492	12.880	12.960	ns	12.220	12.410	***
LG (%)	2,971	2,492	4.410	8.980	***	4.500	6.440	***
ΔNPL (%)	2,971	2,492	0.060	0.150	**	0.000	0.010	ns
RE_LOANS (%)	2,971	2,492	73.930	75.250	***	76.430	77.730	***
COMM_LOANS (%)	2,971	2,492	18.320	16.500	***	15.750	14.660	***
CONS_LOANS (%)	2,971	2,492	4.600	4.830	ns	2.630	2.310	ns
	SMALL	**LARGE**	**SMALL**	**LARGE**		**SMALL**	**LARGE**	
ASSETS ($millions)	5,220	520	1,600	16,000	***	1,100	13,000	***
LLP (%)	5,220	520	0.700	0.710	ns	0.330	0.320	ns
ALL (%)	5,220	520	1.680	1.580	***	1.500	1.440	***
NCO (%)	5,220	520	0.620	0.630	ns	0.260	0.280	*
NPL (%)	5,220	520	2.170	1.990	*	1.340	1.250	ns
ADQ_NPL	5,190	520	2.239	1.510	***	1.087	1.024	ns
ADQ_NCO	4,768	488	7.886	5.892	ns	3.925	3.725	*
SIZE	5,220	520	13.946	16.295	***	13.842	16.318	***
EBP (%)	5,220	520	1.300	1.620	***	1.370	1.640	***
T1CAP (%)	5,220	520	13.000	12.430	***	12.350	11.920	***
LG (%)	5,220	520	6.010	10.030	***	5.080	6.860	***
ΔNPL (%)	5,220	520	0.120	0.170	ns	0.000	(0.010)	ns
RE_LOANS (%)	5,220	520	75.560	67.130	***	78.080	68.700	***
COMM_LOANS (%)	5,220	520	16.970	22.580	***	14.440	19.720	***
CONS_LOANS (%)	5,220	520	4.430	5.730	***	2.340	3.080	***
	SMALLAF	**LARGEAF**	**SMALLAF**	**LARGEAF**		**SMALLAF**	**LARGEAF**	
ASSETS ($millions)	2,931	3,173	1,500	5,000	***	963	2,000	***
LLP (%)	2,931	3,173	0.730	0.680	*	0.340	0.320	ns
ALL (%)	2,910	3,164	1.690	1.660	ns	1.510	1.490	**
NCO (%)	2,931	3,173	0.640	0.600	*	0.270	0.260	ns
NPL (%)	2,931	3,173	2.270	2.060	***	1.390	1.300	**
ADQ_NPL	2,931	3,173	2.181	2.103	ns	1.054	1.108	**
ADQ_NCO	2,666	2,933	7.661	7.403	ns	3.879	3.896	ns
SIZE	2,931	3,173	13.843	14.637	***	13.729	14.440	***
EBP (%)	2,931	3,173	1.260	1.440	***	1.310	1.510	***
T1CAP (%)	2,931	3,173	13.110	12.870	**	12.460	12.210	**
LG (%)	2,931	3,173	6.150	7.240	***	4.940	5.890	***
ΔNPL (%)	2,931	3,173	0.150	0.080	*	0.000	0.000	ns
RE_LOANS (%)	2,931	3,173	78.320	71.270	***	81.150	73.590	***
COMM_LOANS (%)	2,931	3,173	15.530	19.330	***	12.830	17.360	***
CONS_LOANS (%)	2,931	3,173	3.710	5.450	***	2.270	2.580	***

Table A7: Crisis vs. Post-Crisis Analysis for the sub-samples

The table reports the results for all sub-samples. Table 7 includes selected sub-samples from these results.

Row 1 and row 2 report the adjusted R^2 and increases in adjusted R^2 for the two main variables, loan loss provisions (LLP) and allowance (ALL), from the regression model in equation (5) for the sub-samples of private vs. public BHCs, small vs. large BHCs, and BHCs audited by large audit firms for the post-financial crisis period 2010-2019. The regression model in equation (5) includes control variables and fixed effects for the year, BHC, audit firm, and audit partner. For each panel, columns (1), (3), (5), and (7) include results based on actual audit partner assignments. Columns (2), (4), (6), and (8) inlcude the average adjusted R^2, the average increase in adjusted R^2, and the p-value based on 1,000 iterations using random audit partner assignments, respectively. The p-value is the percentage of 1,000 iterations with a random audit partner in which the adjusted R^2 is higher than the adjusted R^2 based on the actual audit partner.

The BHC variables are obtained from the Y9C call reports available on the Chicago Federal Reserve website. The confidential data on the names of the audit partners is obtained from the Federal Reserve Bank of Dallas. Variables are defined in Appendix Table A1. Standard errors are in parenthesis. Levels of significance are denoted as follows:* if p<0.10; ** if p<0.05; *** if p<0.01.

Dependent Variable	**LLP**				**ALL**			
Audit Partner=	Actual	Random	Actual	Random	Actual	Random	Actual	Random
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	PRIVATE		**PUBLIC**		**PRIVATE**		**PUBLIC**	
ADJ. R^2	63.1%	62.9%	49.9%	49.7%	83.6%	84.2%	84.6%	84.8%
INC. IN ADJ. R2	0.3%	0.1%	3.3%	3.1%	0.9%	1.5%	5.5%	5.7%
P-VALUE		0.16		0.39		0.98		0.75
PERIOD 2010-2019	N=1,746		N=1,377		N=1,746		N=1,377	
	SMALL		**LARGE**		**SMALL**		**LARGE**	
ADJ. R^2	56.7%	56.8%	51.5%	51.2%	83.1%	84.0%	90.5%	90.6%
INC. IN ADJ. R2	1.0%	1.1%	2.9%	2.6%	2.2%	3.1%	3.2%	3.3%
P-VALUE		0.78		0.10		0.98		0.78
PERIOD 2010-2019	N=3,005		N=289		N=3,005		N=289	
	SMALLAF		**LARGEAF**		**SMALLAF**		**LARGEAF**	
ADJ. R^2	61.8%	62.1%	48.5%	48.5%	84.4%	84.9%	84.0%	84.2%
INC. IN ADJ. R2	1.2%	1.5%	1.5%	1.5%	2.8%	3.3%	4.1%	4.3%
P-VALUE		0.90		0.48		0.93		0.74
PERIOD 2010-2019	N=1,640		N=1,946		N=1,640		N=1,946	

Table A8: Evidence from audit partner tenure cycle

The table reports analyses of the relation between audit partner tenure (engagement) and our two main dependent variables, loan loss provisions (LLP) and allowance (ALL), respectively, over the tenure cycle for the full sample.

We split TENURE into separate indicators, marking the years of the tenure cycle and reporting coefficient estimates for the individual tenure years. TENURE YEAR 3 serves as a base period and lacks a coefficient estimate. The regressions include control variables and fixed effects (FE) relating to years (YEAR), bank holding companies (BHC), and audit firms (AF). Columns (2) and (4) include an indicator variable CRISIS that takes the value equal to 1 if the year is 2008 or 2009, and 0 otherwise, and an interaction of TENURE YEAR and CRISIS. The coefficients for the models have been multiplied by 1000.

The BHC variables are obtained from the Y9C call reports available on the Chicago Federal Reserve website. The confidential data on the names of the audit partners is obtained from the Federal Reserve Bank of Dallas. Variables are defined in Appendix Table A1. Standard errors are in parenthesis. Levels of significance are denoted as follows: *p<0.10; ** if p<0.05; *** if p<0.01.

Dependent Variable	**LLP**		**ALL**	
	(1)	(2)	(3)	(4)
TENURE YEAR 1	0.021	0.294	-0.137	-0.072
	(0.256)	(0.249)	(0.157)	(0.147)
TENURE YEAR 2	0.144	0.351	0.037	0.051
	(0.224)	(0.230)	(0.115)	(0.124)
TENURE YEAR 4	-0.112	0.069	0.034	0.159
	(0.263)	(0.259)	(0.134)	(0.146)
TENURE YEAR 5	0.144	0.187	0.426**	0.358**
	(0.362)	(0.301)	(0.188)	(0.174)
TENURE YEAR >5	0.711	0.488	1.047*	0.846
	(0.789)	(0.714)	(0.572)	(0.570)
N	7,294	5,247	7,294	5,247
R^2	65.4%	66.6%	74.0%	82.1%
CONTROLS	Yes	Yes	Yes	Yes
YEAR FE	Yes	Yes	Yes	Yes
BHC FE	Yes	Yes	Yes	Yes
AF FE	Yes	Yes	Yes	Yes
SAMPLE PERIOD	2006-2019	2010-2019	2006-2019	2010-2019

Table A9: Evidence from additional measures of loan loss provisioning

The table reports the results for the additional dependent variables, NCO, NPL, ADQ_NPL, and ADQ_NCO.

Panel A reports the results of the benchmark regression and the fixed effects panel regressions for the additional dependent variables, NCO, NPL, ADQ_NPL, and ADQ_NCO. For each dependent variable in column (1), the fixed effects relating to years (YEAR), bank holding companies (BHC), audit firms (AF), and audit partners (AP) included in addition to the control variables are reported in column (2). The adjusted R^2 and the increase in adjusted R^2 resulting from including fixed effects are reported in columns (3) and (4), respectively. Rows 1 to 5 include the estimation results of the regression models represented in equations (1) to (5), respectively. Row 6 includes the results based on 1,000 iterations using random audit partner assignments, where the adjusted R^2 and the increase in adjusted R^2 are average for the 1,000 iterations. The range of adjusted R^2 for the 1,000 randomization iterations is reported in column (5). Column (6) reports the p-value for the randomization, which is the percentage of 1,000 iterations with a random audit partner in which the adjusted R^2 is higher than the adjusted R^2 based on the actual audit partner. Row 7 and 8 include the results of the regression models represented in equation (5) and the results based on 1,000 iterations using random audit partner assignments in the post-financial crisis period, respectively.

In Panel B, we report coefficient estimates for TENURE, reflecting the number of consecutive years the audit partner manages the engagement. Columns (2), (4), (6), and (8) are based on the post-financial crisis period. The regression includes control variables and fixed effects (FE) relating to years, BHC, and AF. The coefficients for the model with the dependent variable NCO and NPL have been multiplied by 1000.

The BHC variables are obtained from the Y9C call reports available on the Chicago Federal Reserve website. The confidential data on the names of the audit partners is obtained from the Federal Reserve Bank of Dallas. Variables are defined in Appendix Table A1. Standard errors are in parenthesis. Levels of significance are denoted as follows: *p<0.10; ** if p<0.05; *** if p<0.01.

Panel A: Adjusted R2 and increase in adjusted R2 using actual audit partner data and random audit partner data for additional measures

Dependent Variable	Fixed Effects	Adj. R^2	Increase in Adj. R^2	Range of Adj. R^2	p-value
(1)	(2)	(3)	(4)	(5)	(6)
NCO	**PERIOD 2006-2019 (N=6,186)**				
	Controls	21.0%			
	Controls, YEAR	33.3%	12.2%		
	Controls, YEAR, BHC	57.4%	24.1%		
	Controls, YEAR, BHC, AF	57.6%	0.3%		
	Controls, YEAR, BHC, AF, AP	58.9%	1.2%		
	Controls, YEAR, BHC, AF, Random AP	58.8%	1.1%	58.6%-60.%	0.36
	PERIOD 2010-2019 (N=3,626)				
	Controls, YEAR, BHC, AF, AP	61.0%	1.6%		
	Controls, YEAR, BHC, AF, Random AP	61.1%	1.7%		0.74
NPL	**PERIOD 2006-2019 (N=6,186)**				
	Controls	30.1%			
	Controls, YEAR	39.9%	9.7%		
	Controls, YEAR, BHC	73.1%	33.2%		
	Controls, YEAR, BHC, AF	73.7%	0.5%		
	Controls, YEAR, BHC, AF, AP	76.3%	2.6%		
	Controls, YEAR, BHC, AF, Random AP	76.6%	2.9%	76.6%-77.4%	0.85
	PERIOD 2010-2019 (N=3,626)				
	Controls, YEAR, BHC, AF, AP	80.8%	2.7%		
	Controls, YEAR, BHC, AF, Random AP	81.9%	3.8%		0.99
ADQ_NPL	**PERIOD 2006-2019 (N=6,156)**				
	Controls	6.6%			
	Controls, YEAR	12.5%	5.9%		
	Controls, YEAR, BHC	40.0%	27.5%		
	Controls, YEAR, BHC, AF	40.8%	0.8%		
	Controls, YEAR, BHC, AF, AP	43.9%	3.1%		
	Controls, YEAR, BHC, AF, Random AP	45.6%	4.8%	43.5% - 47.3%	0.97
	PERIOD 2010-2019 (N=3,609)				
	Controls, YEAR, BHC, AF, AP	55.2%	7.7%		
	Controls, YEAR, BHC, AF, Random AP	55.1%	7.6%		0.50
ADQ_NCO	**PERIOD 2006-2019 (N=5,670)**				
	Controls	0.1%			
	Controls, YEAR	0.4%	0.3%		
	Controls, YEAR, BHC	3.2%	2.8%		
	Controls, YEAR, BHC, AF	3.8%	0.5%		
	Controls, YEAR, BHC, AF, AP	5.4%	1.6%		
	Controls, YEAR, BHC, AF, Random AP	6.0%	2.2%	4.1%-7.9%	0.83
	PERIOD 2010-2019 (N=3,219)				
	Controls, YEAR, BHC, AF, AP	7.5%	3.3%		
	Controls, YEAR, BHC, AF, Random AP	7.6%	3.4%		0.67

Panel B: Aggregate tenure effects for additional measures

Dependent Variable	NCO		NPL		ADQ_NPL		ADQ_NCO	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TENURE	-0.034	-0.029	-0.010	-0.009	-0.029	-0.063*	0.512	0.363
	(0.067)	(0.062)	(0.159)	(0.155)	(0.028)	(0.032)	(0.546)	(0.667)
N	7,294	5,247	7,294	5,247	7,251	5,216	6,389	4,401
R^2	64.9%	70.0%	77.9%	81.9%	48.4%	53.8%	20.2%	24.2%
CONTROLS	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
YEAR FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
BHC FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
AF FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
PERIOD	2006-2019	2010-2019	2006-2019	2010-2019	2006-2019	2010-2019	2006-2019	2010-2019