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The Effects of Audit Partners on Financial Reporting: Evidence from U.S. Bank Holding Companies*

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Abstract

This paper uses confidential data on audit engagement partner names from regulatory filings of bank holding companies (BHC) to investigate whether partners display individual style that affects the financial reporting of the BHCs. We focus on loan loss provisioning. We construct an audit partner-BHC matched panel data set that enables us to track different partners across different BHCs over time. We employ two empirical approaches to investigate partner style. The first approach tests whether partner fixed effects are statistically significant in loan loss provisioning models. The second approach tests whether a partner's history of loan loss provisioning predicts future practices for the same partner. Our empirical evidence does not support systematic differences in loan loss provisioning across audit engagement partners, suggesting that the audit firm's standards and quality control constrain personal partner style.

Keywords: Accounting, Banking, Audit Partner Names, Audit Engagement, PCAOB

JEL codes: G21, M42

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1. Introduction

The primary objective of this paper is to examine whether there are systematic differences in financial reporting outcomes across individual audit partners for a sample of bank holding companies (BHC) over the period 2006-2019. This question is crucial in the wake of the adoption of the controversial and much-debated Public Company Accounting Oversight Board's (PCAOB) Rule 3211 (*Auditor Reporting of Certain Audit Participants*), which requires disclosure of audit engagement partners in Form AP by registered public accounting firms for audit reports issued on or after January 31, 2017 (PCAOB 2015). This rule was adopted after multiple rounds of public comments, as well as comments from PCAOB's Standing Advisory Group and Investor Advisory Group. The PCAOB's view can be summarized by the passage below.

> "Through its oversight activities, the Board has observed that the quality of individual audit engagements varies within firms, notwithstanding firmwide or networkwide quality control systems. Although such variations may be due to a number of factors, the Board's staff uses engagement partner history as one factor in making risk-based selections of audit engagements for inspection. Some firms closely monitor engagement partner quality history themselves, utilizing this information to manage risk to the firm and to comply with quality control standards."
> - PCAOB Release No. 2015-008 December 15, 2015

This view is not universally accepted, and the accounting firms strongly opposed this rule. They contend that audit partners already have a strong sense of accountability due to the accounting firm's quality control systems as well as PCAOB oversight. Furthermore, given the adequate oversight, identification of partners would not result in improvements to audit quality but would raise liability risks for the audit partners and potentially audit fees. The following quote from a partner in a Big 4 audit firm sums up the concern expressed by the audit profession.

> "I don't support this particular one. Let me tell why you. I think that the board and audit committee have absolute knowledge and understanding and, actually, a sense or a feeling of the capability of the individual on the

> engagement and they do so on behalf of the investors. I think the — the truth is that the entire firm stands behind the report. There is differential legal responsibility, frankly, as part of — you know – the individual people on the account have different liability profiles in terms of personal assets at risk as opposed to all other partners; but the real point is that the entire system of quality control what is stands behind that opinion. And the entire system of quality control includes the partner, the second reviewer, but what about the national office consultation partner? What about the methodology that underpins and documents the audit? So I think it tends to limit the importance, frankly, of the firm name when you do that; so I'm not in favor."
>
> - Bob Kueppers, Senior Partner, Global Regulatory and Public Policy for Deloitte LLP.

We contribute to this debate by studying the impact of partner heterogeneity or partner "style" on financial reporting outcomes in the banking industry.[1] This setting offers several advantages. First, restricting our sample to banking allows us to examine a set of reasonably homogenous firms where there is wide agreement on what constitutes a key financial reporting outcome that is discretionary and affects bank performance - loan loss provisions (Beatty and Liao 2014). Loan loss provisions reflect estimates of expected future loan losses and are subject to manager discretion and, consequently, auditor and regulatory scrutiny. There is a significant difference between bankers, auditors, and regulators' intent and actions regarding loan loss provisioning (Dahl 2013).[2] While the bank managers value flexibility, the regulators who are concerned with the safety and soundness of the bank think of the 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 it is the loss is charged off. They often give more latitude to exercise judgments in establishing provisions and recommend that provisioning be more 'forward-looking.' The auditor's approach mirrors the Financial Accounting Standards Board (FASB) position. They examine whether loan loss provisioning is based on current information and events. Given the inherent uncertainty in the estimation process and the potential to misstate estimates, auditors are

[1] We refer to the audit engagement partner as audit partner, AP or partner throughout the study.
[2] Our sample comprises BHCs, thus removing the variation in the regulators and any role that may play in influencing the loan loss provisioning.

often restrictive in using non-historical information.[3] These seeming contradictions make loan loss provisioning well suited to study the impact of individual audit partner style on the magnitude of loan loss provisions.[4]

Second, since audit engagement partner names were made public only after 2017 for all public firms in the U.S. under the PCAOB rule, there is limited evidence on the impact of audit partner heterogeneity or "style" on financial reporting outcomes using U.S. data. Extant evidence is mostly from foreign jurisdictions, primarily China, Taiwan, Sweden, the U.K, and Australia (Lennox and Wu 2018). Thus, it is unclear how well some of these findings would generalize to the U.S. However, the BHCs in the U.S. have been required to disclose the name of the audit engagement partner in their regulatory filings since March 2005, permitting a relatively long time series to test for the impact of auditor style on loan loss provisions. However, this data item is confidential and unavailable to the public. We have obtained access to this quarterly information covering 2005 to 2019 for publicly traded and privately held BHCs, which yields a large number of observations per audit partner.

Third, an audit involves frequent audit partner changes (every five years) due to the mandatory rotation required under the Sarbanes-Oxley act of 2002 (SOX). 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, 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 individual audit partners after controlling for other fixed effects and time-varying controls described below.

We rely on two measures to capture BHC's loan loss provisioning: loan loss provisions and adequacy of allowance.[5] The provision is an indicator of the health of the loan portfolio and BHC performance. It requires considerable managerial discretion and draws much attention from

[3] For example, Comptroller of Currency Dugan highlighted this in his speech in March 2009 during the financial crisis. He remarked that while some may believe that degree to which banks are permitted to use non-historical, forward-looking judgmental factors to justify provisions is understood, many banks and their auditors thought otherwise. Auditors leaned on bank management to reduce provisions if they could not justify it using historical documentation. Due to a strict interpretation of FAS 5 (now ASC 450) and earnings management concerns, auditors often focus on historical loan loss data to justify the loan loss provision and restrict the use of judgmental information.

[4] 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 in conclusion.

[5] We use the term 'loan loss provisioning' to refer to both the level of loan loss provisions as percentage of loans and adequacy of the loan loss allowance/reserve to cover future losses arising from current non-performing loans.

regulators and auditors. Our second measure captures how well the loan loss provisions covered the future losses arising from current non-performing loans. It is used in literature as one of the measures of timeliness of the loan loss provision (Beatty and Liao 2011).

Our sample comprises 1,346 BHCs, where we track 941 engagement partners across 210 audit firms. We represent our main research design in Figure 1. Since our primary interest is quantifying how much variation in provisioning can be attributed to an individual auditor (via auditor fixed effects), we include time-varying BHC characteristics. We control for observable and unobservable differences across BHCs and time by including BHC and quarter-fixed effects. We also include a BHC-specific linear time trend. In addition, we have audit firm fixed effects to control the standards and quality control that audit firms implement to guide audits.

We employ two approaches to test for audit partner heterogeneity or styles. Our first approach tests the joint statistical significance of partner fixed effects in loan loss models. This approach mirrors the method used by Bertrand and Schoar (2003) to study corporate managers' styles on company policies. We isolate variation in loan loss provisioning attributable to audit partners by controlling for time-varying BHC characteristics, quarter, BHC, BHC-specific time trend, and audit firm. Our second approach tests whether a partner's prior idiosyncratic auditing "style" (conservative or permissive) persists across their audit clients. This approach is also based on Bertrand and Schoar (2003). Specifically, we measure the persistence in partner style by regressing the average residual from the regression of our loan loss provisioning measures on observable BHC characteristics after controlling for the quarter, BHC, BHC-specific time trend, and audit firm for a particular audit partner-BHC spell on the average of the residuals from all prior audit engagements for that particular audit partner. If audit partner style affected loan loss provisioning, we would expect to find a strong positive relationship in these tests. It is important to recognize that our measures do not allow us to comment on the impact of an audit partner on accounting quality per se, but we can examine whether an audit partner's style affects the magnitude of loan loss provisioning.

Our initial results show that adding the audit partner fixed effects to models of loan loss provisioning that already account for time-varying BHC characteristics, quarter, BHC, BHC-specific trend, and audit firm causes moderate increases in adjusted R^2 of approximately 2% - 3%,

and the increase is statistically significant.[6] While this approach is widely used in the literature (Bertrand and Schoar 2003; Bamber Jiang and Wang 2010; Gul, Wu and Yang 2013; Cameran, Campa and Francis 2020, among others) to infer the presence of managerial or auditor style, Fee, Hadlock, and Pierce (2013) show that this approach has significant methodological limitations.[7] Following Fee et al. (2013), we also conduct a robustness test. Specifically, for each actual BHC-AP spell, we randomly assign an audit partner to the actual BHC-AP spell. This scrambled data set should exhibit no evidence of style by construction, so the audit partner fixed effects should not be significant. However, similar to Fee et al. (2013), we find virtually an identical increase in adjusted R^2 using random assignment. This suggests that the tests of individual fixed effects suffer from methodological issues and cannot be viewed as reliable evidence of individual auditor effects.[8]

Our second approach is a direct test of auditor style. We refer to this approach as the 'prior auditing style' approach, borrowed from Bertrand and Schoar (2003). This test estimates residuals by regressing the loan loss provisioning variables on time-varying BHC characteristics, quarter fixed effects, BHC fixed effects, BHC-specific time trend, and audit firm fixed effect. We then collapse the data at the audit partner/BHC level (spell). If an audit partner displayed a particular style, we should find a positive relationship between the audit partner's residuals from the current spell and their residuals from prior spells. However, we do not find that the residuals across spells are positively associated. This evidence corroborates the finding from the falsification tests and is not consistent with the presence of the individual auditor style effect. Overall, our evidence suggests that the audit firm's standards and quality controls (written policies, presence of a review partner, etc.) seem to leave relatively little discretion for idiosyncratic audit partner effects.

Thus, in contrast to the significant body of work that has documented significant individual audit partner effects (though mainly in jurisdictions outside the US), we fail to find robust evidence of individual audit partner effects on loan loss provisions. This finding should be of interest to the rule makers, investors, audit committees, accounting firms, and academia.[9] As discussed in Kinney

[6] For comparison, Gul, Wu, and Yang (2013) find that the adjusted R^2 increases between 4-8 percent when they include individual partner effects in models of audit outcomes using data on Chinese firms.
[7] Fee et al. (2013) cite two issues. First, they highlight that the high serial correlation in the dependent variables leads to potentially serious inference issues (Bertrand, Duflo, and Mullainathan 2004). Second, they state that standard asymptotic theory does not apply as the number of dummies to capture the fixed effects is large, and the properties of standard F-tests for joint significance of the coefficients on the fixed effects are unknown (Wooldridge 2002).
[8] A test of serial correlation on our panel data rejects the null hypothesis of no serial correlation.
[9] While we do not examine the direct impact of the PCAOB rule, we intend for our evidence to enhance our understanding of the implications of the new rule and its impact on practice

(2015), PCAOB and media outlets have relied on extant evidence to infer that auditors exhibit idiosyncratic styles that impact audit outcomes and suggest that perhaps audits have a significant element of subjectivity. Our evidence is important in this regard for at least two reasons. One, it shows that auditor style does not appear to affect loan loss provisioning, at least in the sample of BHCs in the U.S.suggesting that audits are not as subjective as some would believe. Second, our results appear to validate the concern raised in Fee et al. (2013) that individual fixed effects regressions to infer the impact of individual styles of firm-specific outcomes appear to suffer from methodological limitations. Hence, the prior academic evidence that has relied on the significance of audit partner fixed effects to infer the presence of auditor style needs to be interpreted with caution.

We also acknowledge that our lack of evidence supporting significant individual audit partner effects also comes with caveats. We do not suggest that audit is a mechanized process and that individual's traits, such as skill or experience, do not matter. It is possible that audit partner style may be evident in aspects of audit other than financial reporting outcomes, or it may manifest during times of extreme financial performance, such as during the early stages of a firm's life or during financial distress, where the skills and experience of an auditor would likely come into play. Our discussions with audit partners, audit committee members, and senior management suggest that an audit partner's experience and industry knowledge are important considerations in the audit partner's assignment to clients. This fit is based not only on the audit partner's expertise but also on individual personality traits that affect and shape the client-audit firm relationship. Second, it is possible that audit partner style may be evident in financial reporting outcomes of industries other than banking. The banking industry is unique with the additional layer of regulatory supervision. Despite differing objectives, auditors and regulators spend considerable effort evaluating loan loss provisioning (Balla, Rose, and Romero 2012). This level of scrutiny may further constrain idiosyncratic auditor style from impacting financial reporting outcomes for banking organizations.

2. Background and Hypotheses Development

2.1 Prior Literature

There is an extensive literature in economics and finance that focuses on manager characteristics and documents associations between manager style and firm policies such as

investment spending, acquisitions, leverage and profitability (Bertrand and Schoar 2003), compensation levels (Graham, Li, and Qiu 2009) and performance variability (Adams, Almeida, and Ferreira 2005). Bertrand and Schoar (2003) discuss two theoretical views on whether 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 intentionally choose the 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. Agency theory, on the other hand, acknowledges that managers may have discretion and can influence corporate decisions. However, these models attribute variations in corporate behavior to heterogeneity in governance mechanisms that constrain managers. Under this view, the manager matters only when internal and external governance is weak, or firms with better governance optimally choose managers that contribute positively to firm performance.

Similarly, some papers look at the manager style effects on financial reporting outcomes. Bamber et al. (2010) find that individual managers play an economically significant role in their firms' voluntary financial disclosure choices and that these choices are associated with their observable demographic characteristics. Ge, Matsumoto, and Zhang (2011) examine the impact of the CFO style on varied 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 that "CFO-style" impacts these specific accounting decisions. While it appears that CFOs' idiosyncrasies may influence some of the reporting choices, they are harder to detect when the choices are aggregated and measured as the level of discretionary accruals. 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, Lev, Lewis and

McVay (2013) also examine the impact of managerial traits on accounting quality measures, such as restatements, the persistence of earnings, errors in bad debt expense, and the changes in accrual quality. They study the association between managerial ability (ability to generate sales based on costs) and accrual quality (measured several different ways) and 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, Hanlon, and Maydew 2010 find that top executives have incremental effects on their firms' tax avoidance that the firm's characteristics cannot explain. There is almost an 11 percent difference in GAAP effective tax rate between the top and bottom quartile of executives. Thus, the economic magnitude of the executive effects on tax avoidance is large.

Studies that examine the impact of individual auditors on their clients' financial reporting quality find that the characteristics of individual partners, such as partner expertise, tenure, and style, exhibit an association with the quality and pricing of audits (see Lennox and Wu 2018 for a review of this literature). The majority of these studies examine companies in non-U.S. jurisdictions as individual audit partner data have been available in many of these countries for a relatively long time. The evidence suggests that individual partner characteristics influence their clients' earnings, accruals, going concern opinions, audit fees, comparability of earnings, etc. (Gul et al. 2013; Knechel, Vanstraelen and Zerni 2015; Chen, Chen, Chin and Lobo 2020; Zerni 2012).

Three prior studies use the fixed effects approach 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 characteristics affect accounting quality. Cameran et al. (2020) use the same fixed effects methodology to test for partner heterogeneities in the U.K. and find that partner fixed effects have greater explanatory in explaining the variation in earnings quality, GC reporting, and audit pricing. Taylor (2011) uses Australian data for 2005 to test whether audit fees are affected by partner characteristics and 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.

A concurrent working paper by Gopalan, Imdieke, Schroeder, and Stuber examines how the audit partner's effect on accounting estimates varies across the tenure of a partner/client relationship in the banking industry. Using a sample of U.S. BHCs, they find that audit partners

enforce higher quality accounting estimates measured as the absolute value of 1 minus future charge-offs divided by current loan loss reserve in the early years of the relationship relative to later years. They control for the bank-partner-engagement effect.

Finally, studies examining the consequences of introducing the partner disclosure requirement are very recent as most countries have not required the names of audit partners to be publicly disclosed. Carcello and Li (2013) find that audit quality and fees are higher for 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, Notbohm, Schelleman, and Valencia (2014) detect no substantial change in audit quality following the partner signature mandate in the Netherlands. In relation to the U.S., Cunnigham, Li, Stein, and Wright (2019) provide early evidence on Rule 3211. They do not detect a significant change 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, Hoitash, and Hoitash (2019) find a significant increase in audit quality and audit fees and a significant decrease in audit delay following the audit partner name disclosure under Rule 3211. While they find evidence that partner characteristics are associated with variations in audit fees and audit delay, they do not find evidence of an association with audit quality.

2.2 Hypothesis Development

Unlike the managers, the auditors are not insiders, but they can significantly influence firms' financial reporting outcomes by establishing guard rails and forcing firms to make choices within those constraints. However, studies on auditor 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, and hence the unit of analysis is typically the audit firm. To some extent, the lack of empirical evidence on this issue is due to the lack of data on audit partner identity in the US until the recent mandate by PCAOB. However, this 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, none translates into observable differences in audit quality or audit outcomes for their clients. Audit firms establish standards and control systems

precisely to maintain audit quality consistency and reign in individual idiosyncracies. 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 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 make similar choices.

This issue is particularly nuanced in the banking industry. The Federal Reserve added the new memoranda 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 in 2005 to facilitate more efficient supervision of the banking industry on issues related to accounting and auditing.[10] 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 public accounting 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, Board, FDIC, and 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 individual auditor effects on financial reporting outcomes outside the U.S., the effect may not carry over to the U.S., especially to the banking industry. Audit firms are particularly rigorous in implementing checks and balances due to a highly litigious environment, the stringent oversight provided by the PCAOB, and the additional regulatory oversight provided by various regulatory agencies.

3. Data

3.1 Sample Construction

[10] 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

Our primary sample contains US BHCs with the necessary data from the quarterly Y9C filings provided to the Federal Reserve. Given that the field to identify the audit engagement partner name was included on the Y9C from 2005, and the asset-size threshold for filing the Y9C changed from total assets of $150 million to $500 million in 2006, we start our sample period in 2006.[11] (But we use the 2005Q4 data for lags.) We begin with 46,954 (1,601) BHC-quarter observations (unique BHCs) between 2006Q1 to 2019Q4 with non-missing values for our dependent variables and control variables (other than audit partner). We invested considerable effort to clean up and standardize audit partner names. The audit partner's name is populated most frequently in the Q4 filings. If left blank, we use the Q4 data to populate the name for the prior three quarters for each BHC.[12] We exclude 5,606 BHC-quarter observations relating to 220 BHCs due to missing audit firm or partner names. We exclude 35 BHCs with total assets above $100 billion in any quarter in our sample period resulting in 39,997 (1,346) BHC-quarter observations (unique BHCs) for our analysis. We exclude the very largest BHCs for two reasons. First, these institutions have complex business models often beyond traditional lending and deposit-taking. Second, these large BHCs are audited by multiple partners, although only one partner's name gets reported as the engagement partner. 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. Our sample comprises 941 unique partners associated with 210 audit firms across 1,346 unique BHCs over 56 quarters. The split between public and private BHCs is as follows. Our sample comprises 15,473 (24,524) BHC-quarter observations associated with 69 (201) unique audit firms and 534 (763) unique audit partners for 538 (934) unique public (private) BHCs. We have public and private BHCs in each of the 56 quarters.[13] Included in the 1,346 BHCs are 412 BHCs that were public throughout the sample period, 808 BHCs that were private for the entire sample period, and 126 BHCs that changed status during the period.[14] Out of the 210 audit firms, 126 audit firms have audited both public and private BHCs, while nine are associated with the audit of public BHCs

[11] The Federal Reserve explicitly states on the form that it regards this information as confidential.
[12] We perform a robustness check using only Q4 data. The results are presented in Table 8 and discussed in the relevant section.
[13] 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.
[14] The BHCs are identified as public based on the CRSP-FRB link file available from the New York Federal Reserve website.

only, and 141 are associated with the audit of private BHCs only in our sample. Regarding audit partners, 356 partners audit both public and private BHCs, 178 audit public BHCs only, and 407 audit private BHCs only.

3.2. Sample Description

Table 2 presents descriptive statistics for the BHCs in our sample. All variables are winsorized at top and bottom 1%. The average BHC in our sample has total assets of $3.3 billion, with the median BHC having total assets of $1.1 billion. Our main loan loss provisioning measures are LLP and ADQ (Wahlen 1994; Beatty & Liao 2011). LLP is measured as the loan loss provision deflated by total loans at the beginning of the period. ADQ 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 non-performing loans. This ratio is also used to measure the timeliness of loan loss provisioning in the literature (Beatty & Liao 2011). Loan loss provision (LLP), a flow measure, is about 0.17% of total loans, with the stock measure loan loss allowance (ALL) being 1.6% of the loan portfolio. The mean ADQ is 2.4, indicating that the loan loss allowance is 2.4 times the non-performing loans (NLP), which are 2.1% of the loan portfolio. Net charge-offs, on average, are 0.15% of the loan portfolio. The loan portfolio comprises 75% real estate loans, 17% commercial and industrial loans, and 5% consumer loans, and quarterly loan growth is about 1.5%. The average tier 1 capital ratio is 13.2%, well above the minimum required 4%. Earnings before provision are 0.3% of the total assets, and new additions to NPL are about 0.03% of the loan portfolio at the beginning of the quarter.[15]

Table 3 summarizes the interrelationships between audit partners, audit firms, and BHCs for the sample. On average, audit partners audit 3.4 BHCs. Out of the 941 unique audit partners, 357 audit partners audit only one BHC, and 584 audit partners audit two or more BHCs. The maximum number of BHCs audited by an audit partner is 19. As expected, audit partners change audit firms infrequently, as evidenced by the mean of 1.1, median, and 75th percentile of 1 audit firm per audit partner. Only 10% (unreported) of the audit partners are associated with more than one audit firm, the most common reason being accounting firm mergers. On average, audit partners appear in our sample for 23.6 of the 56 quarters. Audit firms, on average, have five audit partners

[15] The descriptive statistics for the sample with 742 individual audit partners associated with 113 audit firms and 1208 BHCs for which the AP can be estimated are qualitatively similar.

and audit 9 BHCs, with more than half of our sample audit firms having two or more audit partners and auditing two or more BHCs. The maximum number of audit partners associated with an audit firm is 105, and the maximum number of BHCs audited by a firm is 210. An audit firm appears in our sample for an average of 30.2 quarters. About 10% (unreported) of the audit firms in our sample have 10 or more audit partners and audit more than 40 BHCs. BHCs are associated with 2.4 audit partners and 1.4 audit firms on average during the sample period. On average, BHC appears in 29.7 quarters out of the 56 quarters. On average, a quarter has 714 BHCs audited by 113 audit firms and 397 audit partners. Identification restrictions allow us to estimate fixed effects for 742 individual audit partners associated with 113 audit firms and 1208 BHCs.

Our main interest is in estimating audit partner fixed effects. In order to be able to estimate audit partner fixed effects, separate from audit firm fixed effects, we need the 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, separate from BHC fixed effect, we need the audit partner to be associated with more than one BHC or the BHC to have more than one audit partner. Figure 2 represents an excerpt of our panel data for five hypothetical audit partners (1, 2, 3, 4, and 5), three audit firms (X, Y, and Z), and four BHCs (A, B, C, and D). The shaded cells represent the fixed effects that can be estimated.

Figure 3 identifies the conditions for each of the 5 audit partners to estimate the fixed effect. We can identify AP 1 and AP 5 separately from the BHC as they audit more than one BHC. Similarly, we can identify AP 2 and AP 3 separately from the BHC, even though they are associated with the audit of one BHC, as the BHC has multiple audit partners. We cannot identify AP 4 separately from the BHC as neither of the two conditions is met – the audit partner is associated with more than one BHC, or more than one audit partner audits BHC. Though all five partners are associated with one audit firm, we can identify AP 1, AP 2, and AP 3 separately from the audit firm as the audit firm has multiple audit partners. However, we cannot identify AP 4 and AP 5 separately from the audit firm as neither of the two conditions is met – the audit partner is associated with more than one audit firm, or the audit firm has more than one audit partner in our sample. Thus, we can identify AP 1, AP 2, and AP 3 separately from the BHC and the audit firm.[16]

[16] 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 (AP 4, AF 5, and BHC C). Therefore, we follow the most stringent ordering to introduce fixed effects in our STATA code. Our order is quarter, BHC, BHC-specific linear time trend, 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.

4. Is there heterogeneity in loan loss provisioning outcomes across audit partners?

4.1. Empirical Methodology

Intuitively, we want to quantify how much of the observed variation in the loan loss provisioning of the BHCs can be attributed to the audit partner fixed effects. Since audit partner fixed effects might be correlated with other BHC and audit firm characteristics, we estimate the role of audit partners using a panel data set where we track different audit partners across different BHCs.

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-quarter level after controlling for any average differences across BHCs and quarters, any BHC-quarter specific shock such as loan growth or increase in non-performing loans that might affect the LLP of a BHC, and most importantly, the average difference across the audit firms that capture the standards and quality control differences between audit firms. We then ask how much of the variance in these residual LLP can be attributed to audit partner-specific effects.

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:

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

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

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

$$y_{i,t} = \beta X_{i,t} + Q_t + B_i + BT_i + \varepsilon_{i,t} \quad (4)$$

$$y_{i,t} = \beta X_{i,t} + Q_t + B_i + BT_i + AF_{af} + \varepsilon_{i,t} \quad (5)$$

$$y_{i,t} = \beta X_{i,t} + Q_t + B_i + BT_i + AF_{af} + AP_{ap} + \varepsilon_{i,t} \quad (6)$$

where $y_{i,t}$ is a BHC-level outcome, $X_{i,t}$ are the control variables, Q_t is a time-fixed effect, B_i is a BHC-level fixed effect, BT_i is a BHC-specific time trend, AF_{af} is the audit firm fixed effect, AP_{ap} is the audit partner fixed effect and $\varepsilon_{i,t}$ is an error term. Finally, we account for serial correlation by allowing for clustering of the error term at the BHC level. We are interested in AP_{ap}.

In equation (1), we control BHC characteristics using time-varying controls. Our BHC-level control variables are guided by prior research (Wahlen 1994; Liu and Ryan 1996; Bhat, Lee and Ryan 2021, among others). We include SIZE measured as a log of total assets to control for size-related differences in BHCs' business models. In addition, we have EBP measured as earnings before provisions deflated by total assets at the beginning of the quarter to capture incentives for income-smoothing, T1CAP is the Tier 1 capital ratio and is intended to capture regulatory capital constraints, LG (loan growth) is measured as one-quarter change in loans deflated by total loans at the beginning of the quarter and captures the effects of loan seasoning, ΔNPL is measured as the quarterly change in non-performing loans deflated by total loans at the beginning of the quarter and captures loan performance.[17] 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 and measured at the beginning of the period.

We include quarter-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). There is much cyclical variation, particularly during the Great Recession, and since each BHC may react differently, we introduce a BHC-specific time trend in equation (4). Audit firms have rigorous standards and quality control, which we extract using audit firm fixed effects in equation (5). Finally, we include audit partner fixed effects to explain the variation in provisioning not explained by BHC characteristics, the business cycle, individual BHC strategy (time-invariant and linear time trend), and audit firm strategy in equation (6). Results of the models from equations (1) to (6) are presented in Table 4 and discussed in the relevant sections below.

4.2 Results

Baseline Loan Loss Models

Table 4 reports the results of the baseline regression model in equations (1) to (6) for LLP in Panel A and ADQ in Panel B. We first discuss the results for the LLP models, followed by the

[17] 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 US, 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% 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.

ADQ models. The coefficient on SIZE is positive and significant at the 1% level in five of the six models for LLP, suggesting larger BHCs have higher provisions. The coefficient on EBP is significantly negative at the 1% level in all six columns, inconsistent with the notion that BHCs smooth their income by exercising discretion over LLP.[18] The coefficient on T1CAP is negative and statistically significant in the baseline model, but it loses significance once additional fixed effects are introduced. The coefficient on LG is negative and significant at a 1% level, reflecting the fact that growth in loans disproportionately occurs during good times when loan default is low, consistent with Laeven and Majnoni (2003). We find a positive and significant coefficient at the 1% level on ΔNPL consistent with recent research that views a positive and significant association between LLP and changes in NPL as indicating greater timeliness of provision (Beatty & Liao 2011). The coefficients on the control variables capturing loan portfolio composition are not significant once the bank-specific time trend is included, underscoring the importance of controlling for loan composition in models without such a trend.

The dependent variable ADQ captures the timeliness of provisions by capturing the adequacy of the loan loss allowance given the current level of NPLs. The higher the ADQ, the higher is the adequacy of the allowance. Panel B of Table 4 shows that the coefficient on SIZE is negative and significant at the 1% level suggesting larger BHCs are less timely in provisioning. The coefficient on EBP is positive and significant at the 1% level suggesting that BHCs exhibit timely LLPs when earnings are high. The coefficient on T1CAP is positive but insignificant in five of the six columns. The coefficient on LG is positive and significant at the 1% level, in line with loan growth occurring during good times and having fewer NPLs until the loans season. The coefficient on ΔNPL is negative and significant at the 1% level, indicating lower adequacy of loan loss allowance given the high level of NPLs (Beatty & Liao 2011). The coefficient on CONS_LOANS is negative and significant at the 10% level (at least) in all six columns, suggesting the ADQ ratio is lower for BHCs with a high proportion of consumer loans which is in line with the faster charge-offs of the consumer loans and highlights the importance of controlling for loan composition.

[18] For example, Ahmed, Takeda, and Thomas 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.

The estimation yields coefficients on 742 individual audit partners.[19] 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. Our approach is non-directional, and hence we cannot infer whether auditor style affects the quality of financial reporting outcomes. Instead, we test whether idiosyncratic auditor style affects these outcomes by testing the joint significance of the AP fixed effects and the incremental adjusted R^2 due to AP fixed effects.

Evidence from the F-test using actual audit partner assignments

Our first approach focuses on the incremental explanatory power of audit partner fixed effects and their joint significance. Panel A of Table 5 reports adjusted R^2 and the incremental adjusted R^2 from the estimation of equations (1) to (6), and the F-tests for the joint significance of audit partner fixed effects in equation (6) for both measures of BHCs' loan loss provisioning. We report the baseline specifications for each loan loss measure that include BHC level controls in the first row. The following three rows report the adjusted R^2 when we successively add quarter fixed effects, BHC fixed effects and BHC-specific time trends. The last two rows report incremental adjusted R^2 due to audit firm fixed effects and 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 11.52% for LLP and 4.31% for ADQ. After including quarter-fixed effects, the adjusted R^2 increases to 26.99% for LLP and 11.46% for ADQ. BHC fixed effects add significantly to the explanatory power of the models, as the adjusted R^2 is now 44.66% for LLP and 40.29% for ADQ. Adding a BHC-specific time trend also adds another 5.16% (14.6%) to the LLP (ADQ) model's explanatory power. However, audit firm fixed effects add only modestly to the explanatory power. This is due to considerable overlap between BHCs and audit firms as BHCs seem to rarely change audit firms, with most observed changes being driven by consolidation activities in the accounting industry.[20]

[19] About 33% (11%) of our 742 AP fixed effects are significant at the 1% level, and another 9% (3%) are significant at the 5% level in the LLP (ADQ) model. In comparison, Gul et al. find about 18% (10%) [5%] of the 861 audit partner effects they estimate in the abnormal accruals model using Chinese data are significant at 10% (5%) [1%] level.

[20] If we introduce audit firm FE in the model prior to introducing BHC fixed effect and BHC-specific trend, the increase in adjusted R^2 is 4% (6%) in the LLP (ADQ) model.

Up to this point, the model explains 50.24% (55.32%) of variation in LLP (ADQ). Finally, we add Audit partner fixed effects, our primary variable of interest, to the model to measure its incremental explanatory power. We find that AP fixed effects increase the model's explanatory power by 1.55% for LLP and 3.37% for ADQ. To put it differently, the AP fixed effects increase the model's explanatory power by 3.09% (1.55/50.24) and 6.09% (3.37/55.32), respectively, for our two proxies for provisioning. The F-statistic for joint significance is large and significant, suggesting that idiosyncratic audit partner style affects BHCs' provisioning. This result is consistent with prior research, such as Gul et al. (2013) and Cameran et al. (2020), who study Chinese and the U.K. data, respectively.

Evidence from the F-test using randomized audit partner assignments

While the evidence in the above tests suggests that audit partner style is associated with BHCs' loan loss provisioning, we perform additional analyses to address some methodological concerns with using F-tests to detect individual styles (Fee et al. 2013). For example, Wooldridge (2002) cautions against using F-tests to test the significance of a large set of individual effects in the absence of very strong assumptions about the error term (as error terms are likely to be serially correlated due to lumpiness of LLP and ADQ). While we have a large number of observations per audit partner, there is still concern that a large sample does not help because the number of parameters to be estimated grows proportionally with the sample.

We perform a falsification test using random assignment of audit partners to BHC-audit partner spell to investigate this issue. We scramble the audit partners' identities and randomly assign them to the actual BHC-audit partner spells. Thus, for each BHC-audit partner spell, the assignment of the audit partner is random. This process is repeated 1,000 times. We run the regression model in equation (6) for each scramble and calculate the mean F-statistic and p-value for the AP fixed effects. Thus, our scrambled data should not find significant evidence of audit partner "style". However, as we see in panel B of Table 5, the incremental adjusted R^2s, including the scrambled audit partner fixed effects, are almost identical to the actual sample tests. The adjusted R^2 ranges from 51.45% to 52.08% (58.62% to 60.15%) for the LLP (ADQ) model in the 1,000 iterations. The F- statistics of the joint significance of the AP fixed effects is large and significant in each of the 1,000 iterations. Figure 4 includes the graphical presentation of the increase in adjusted R^2 on adding AP fixed effects to the regression model (equation (5) vs. (6))

using actual AP data (solid line) vs. data from 1,000 iterations (dotted lines) using random AP assignments.

The result of the falsification tests causes us to question the robustness and reliability of evidence documented in Panel A of Table 5. Our findings support the concern raised in Fee et al. (2013) that the popular F-test approach for identifying individual effects for firm-specific outcomes is problematic and does not lead to a reliable inference.

Evidence from audit partner spells

Our second approach tests whether audit partners display persistent style across different banking clients using a more robust and direct approach. If an individual auditor's style affects provisioning, it is reasonable to expect that the auditor's style should persist across clients. This analysis is perhaps a more direct test of auditor style.

We follow Bertrand and Schoar (2003), who use 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 to offer. A positive association would be more direct evidence of the presence and persistence of individual style across employers.

To test for such persistence in our study, we calculate the average residuals from regression equation (5) before adding the audit partner fixed effects for each BHC-audit partner spell. We use observations for which the coefficient on AP could be estimated. We then regress the average residuals for a particular spell on the average residual for that audit partner over all her prior spells. If an audit partner displays an innate style, we would expect the residuals from the current spell to be positively related to residuals from the prior spells.

Results are presented in Panel A of Table 6. The coefficient on the average residual from prior spells for LLP is negative and statistically significant at the 10% level, and ADQ is insignificant. In other words, we do not find evidence of auditor style that persists across clients. This evidence does not support the presence of individual auditor effect on loan loss provisioning.

We also test whether a new audit partner displays a style that is evident by the change in the outcome variables (LLP and ADQ) in the quarters following the change of audit partner. Here we again regress residuals from equation (5), including control variables, BHC fixed effects, BHC-specific time trend, and audit firm fixed effects (but before adding the audit partner fixed effects)

for each BHC quarter on an indicator variable equal to 1 in the first eight quarters of an audit partner change; 0 otherwise. The results are documented in Panel B of Table 6. None of the coefficients on the indicator variable for the new audit partner are statistically significant.

Overall, our analysis using a multitude of approaches has allowed us to build a more complete picture of the impact of audit-partner style effects. Our collective evidence does not find reliable evidence of auditor style affecting loan loss provisioning. The lack of evidence suggests that audit firm standards and quality control constrain individual partners' idiosyncrasies. The standardized work procedures, centralized models of risk and materiality decisions, staff training, rigorous promotion process, concurrent partner reviews, in-house knowledge libraries, and access to subject matter experts, among other things, dominate. In addition, the highly regulated nature of the banking industry and the constant supervision and monitoring by regulators may be the additional layer contributing to constraining partner style.

5. Additional Analyses

We perform two sets of additional analyses. First, we perform tests using other measures related to loan loss provisioning. Second, we look at sub-samples based on fourth-quarter observations, private vs. public, size, and the length of BHC-AP spell to check the robustness of our results.

5.1 Other dependent variables – allowance, net charge-offs, and non-performing loans

We perform analysis using three additional measures, loan loss allowance (ALL), a stock measure of accrued loan losses, net charge-offs (NCO), a flow measure of realized loan losses, and non-performing loans (NPL), a stock measure of severe delinquencies, to capture further attributes of the loan loss provisioning of the BHCs. All three measures are deflated by total loans at the beginning of the period. Panel A of Table 7 reports adjusted R^2, the incremental adjusted R^2 from the estimation of equations (1) to (6), and the F-tests for the joint significance of audit partner fixed effects in equation (6) for all three measures of BHCs' loan loss provisioning in rows 1 to 6. 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 results are similar to those for the two primary variables

reported previously. Audit partner fixed effects, our primary variable of interest, causes a modest increase in the adjusted R^2 of 3.02 (1.55) [3.09] percent in the ALL (NCO) [NPL] model. The F-tests for the joint significance of audit partner fixed effects are large and significant, seemingly allowing us to reject the null hypothesis that all the audit partner fixed effects are zero for all three measures, ALL, NCO, and NPL.

However, when we perform a falsification test based on a random assignment of audit partners, we obtain similar results suggesting that testing the presence of individual styles using fixed effects may be problematic. The incremental adjusted R^2s from the 1,000 iterations, including the scrambled audit partner fixed effects and F- statistics on the joint significance of the scrambled audit partner fixed effects reported in Panel B of Table 7. For all three models, ALL, NCO, and NPL, the incremental R^2 are almost identical to the tests on the actual sample. The result for NCO and NPL is not necessarily surprising given that the level of discretion is low, especially over NPL. Results relating to the persistence of style using changes in outcome variables following a new auditor are presented in Panel B and C. Similar to results for LLP and ADQ, we do not find any evidence that the persistence of audit partner style is reflected in the level of loan loss allowance, net charge-offs, and non-performing loans of BHCs.

5.2 Sub-Samples

We repeat our two approaches on various sub-samples. First, while many public BHCs in our sample may have their quarterly statements audited, quarterly audits are not required, and often a limited audit is performed on quarterly reports. Hence it is possible that the results may differ for the fourth quarter relative to the other quarters. Second, we have public and private BHCs in our sample. 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 sheets, statements of income, changes in equity capital, and cash flows, with accompanying footnote disclosure) by an independent public accountant, private BHCs are smaller and face less investor scrutiny; thus, it is possible that an auditor style may manifest in private BHCs. Hence, we check the robustness of our results by examining the sample of public and private BHCs separately. Third, we also examine a sample of small BHCs as community BHCs with assets less than $10 billion potentially face lower regulatory costs and scrutiny than BHCs with assets above $10 billion. Also, the size and the quality of the audit firms

may differ between small BHCs and large BHCs. Therefore, we perform our analysis on a sub-sample of small BHCs with total assets of less than $10 billion at the beginning of the quarter. Finally, we impose conditions on the length of the BHC-AP relation to allow a longer time for the audit partner to display 'style'. Similar to Gul et al. (2013), each AP in this sub-sample must audit at least 2 BHCs, and for each BHC, there are at least 12 QTRs in which the AP audits the BHC and 12 QTRs in which the AP does not audit the BHC. Thus, we end up with five sub-samples – fourth quarter observations of BHCs, public BHCs, private BHCs, community BHCs and observations with conditions on the BHC-AP spells.

Panel A of Table 8 reports adjusted R^2, the incremental adjusted R^2 from the estimation of equation (6), and the F-tests for the joint significance of audit partners in equation (6) for our two main measures of BHCs' loan loss provisioning in the first row for each of the five sub-samples. Audit partner fixed effects, our primary variable of interest, causes a modest increase of 1.30% to 2.65% [2.26% to 4.99%] in the adjusted R^2 for the LLP [ADQ] model depending upon the sub-sample. The F-tests for the joint significance of audit partner fixed effects are large and significant, seemingly allowing us to reject the null hypothesis that all the audit partner fixed effects are zero for each of the five sub-samples. However, once again, the inference is not robust to the falsification test based on a random assignment of audit partners. The incremental adjusted R^2s from and F- statistics on the joint significance of the audit partner fixed effects from the 1,000 iterations of scrambling data and running tests are reported in the second row of Panel A for each sub-sample. The results are almost identical to the tests on the actual sample.

Results relating to the persistence of style and the changes in outcome variables following a new auditor are presented in Panel B and C, respectively. In Panel B, we expect a positive and significant coefficient on the average residual from prior spells. The coefficient is insignificant for the fourth quarter, public and private BHC samples. Contrary to expectation, the residuals across audit partner spells are negatively related for the community BHCs with total assets less than $10 billion and the sub-sample with observations with conditions on BHC-AP spells. In Panel C of Table 8, the evidence does not suggest any changes in the outcome variable in the first eight quarters following a new auditor except for community BHCs with total assets of less than $10 billion. We find marginal evidence (t-stat=1.74) suggesting that the LLP increases following a new audit partner in the first eight quarters for the community BHCs.

Overall, we do not find evidence of significant individual audit partner style in sub-samples

that limit the data to only the fourth quarter, public BHCs, private BHCs, community BHCs with total assets less than $10 billion, and observations with conditions on the BHC-AP spells.

6. Conclusion

The primary objective of this paper is to examine whether there exist idiosyncratic differences in individual audit partner approach to an audit such that they can have a significant impact on financial reporting outcomes. This question is important given the almost diametrically opposite views of the PCAOB and the accounting firms. PCAOB argues that audit quality varies considerably across audit partners within a firm. In contrast, accounting 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 individual audit partner effect on loan loss provisioning for a sample of US BHCs from 2006 to 2019. We construct an audit partner-BHC matched panel data set, where we track audit partners across different BHCs over time. This allows us to estimate how much of the unexplained variation in loan loss provisioning can be attributed to audit partner fixed effects after controlling for audit firm fixed effects, BHC fixed effects, BHC-specific time trend, quarter fixed effects, and time-varying BHC characteristics.

Though the above approach finds that audit partner fixed effects are statistically significant, this evidence is not robust as a falsification test that uses random assignment of audit partners to BHCs produces essentially the same results. Thus, our fixed effects approach does not support the view that heterogeneities across audit partners affect BHC's loan loss provisioning.

We also undertake a more direct test of audit partner style by examining persistence in style across clients and time. We measure the persistence of audit partner style by correlating the residual from the regression of our outcome variables on observable BHC characteristics after controlling for the quarter, BHC, BHC-specific time trend, and audit firm to the average of the residuals from all prior audit engagements for that particular audit partner in our sample. If auditor style influences firm-level reporting outcomes, we should find a positive correlation between these residuals. We do not find that these residuals are positively related. Furthermore, focusing on the

immediate period following the changes in audit partners does not reveal an associated shift in loan loss provisioning. Collectively, these results do not suggest that the audit partner's idiosyncratic style influences provisioning at BHC.

Thus, in contrast to the significant body of work that has documented significant individual audit partner effects (though mainly in jurisdictions outside the US), we fail to find robust evidence of individual audit partner effects on loan loss provisioning. This finding should be of interest to the rule makers, investors, audit committees, and accounting firms. In particular, if individual partners do not significantly influence financial reporting outcomes and the client audit is an outcome of the collective effort by various constituents within a firm, then does the disclosure of partner names place undue focus on the individual by minimizing the importance of the firm? We hope that our evidence informs this debate.

Our results appear to validate the concern raised in Fee et al. (2013) that individual fixed effects regressions to infer the impact of individual styles of firm-specific outcomes appear to suffer from methodological limitations. Hence, the prior academic evidence that has relied on the significance of audit partner fixed effects to infer the presence of auditor style needs to be interpreted with caution.

Despite our evidence that individual audit partner style does not seem to affect provisioning, we acknowledge that our findings do not suggest that audit is a mechanized process and individual traits do not matter. It is possible that audit partner style may be evident in aspects of audit other than financial reporting outcomes, during times of extreme financial performance, or in financial reporting outcomes of industries other than banking. In addition, while the banks accrued for credit losses using the incurred loss model of ASC 450 during our sample period, beginning in 2020 (2023), large public (small public and private) banks accrue for credit losses using the current expected credit loss (CECL) model of ASC 326. The implementation of CECL introduces increased subjectivity in the estimation and will create significant changes and challenges in auditing post-CECL. We leave it to future research to address these nuances.

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Figure 1: Research Design

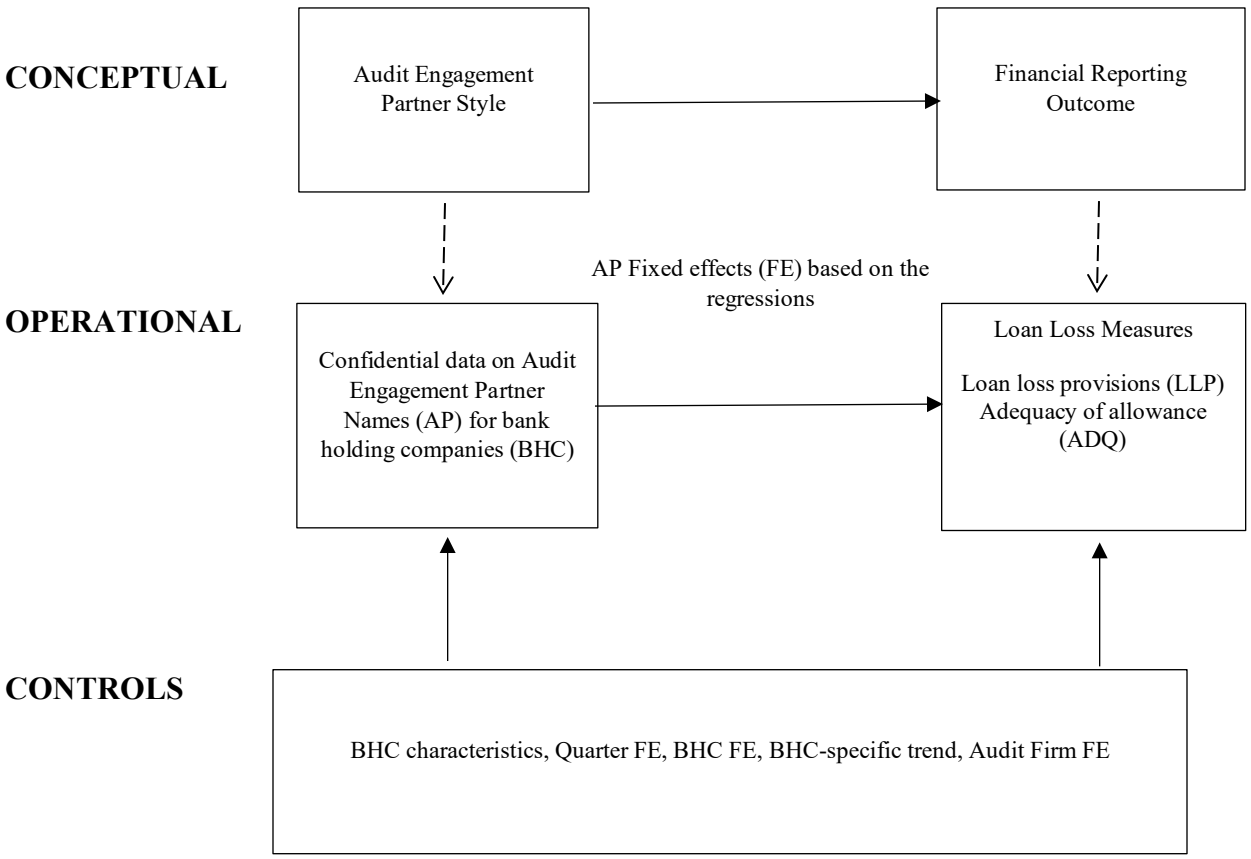

Figure 1 presents our main research design to identify audit partner fixed effects.

Figure 2: An excerpt of our panel data

QTR	BHC	AF	AP	BHC	AF	AP	BHC	AF	AP	BHC	AF	AP
QTR1	A	X	1	B	X	3	C	Y	4	D	Z	5
QTR2	A	X	1	B	X	3	C	Y	4	D	Z	5
QTR3	A	X	1	B	X	3	C	Y	4	D	Z	5
QTR4	A	X	1	B	X	3	C	Y	4	D	Z	5
QTR5	A	X	1	B	X	3	C	Y	4	D	Z	5
QTR6	A	X	1	B	X	3	C	Y	4	D	Z	5
QTR7	A	X	1	B	X	3	C	Y	4	D	Z	5
QTR8	A	X	1	B	X	3	C	Y	4	D	Z	5
QTR9	A	X	1	B	X	3	C	Y	4	D	Z	5
QTR10	A	X	1	B	X	3	C	Y	4	D	Z	5
QTR11	A	X	1	B	X	3	C	Y	4	D	Z	5
QTR12	A	X	1	B	X	3	C	Y	4	D	Z	5
QTR13	A	X	1	B	X	1	C	Y	4	D	Z	5
QTR14	A	X	1	B	X	1	C	Y	4	D	Z	5
QTR15	A	X	1	B	X	1	C	Y	4	D	Z	5
QTR16	A	X	1	B	X	1	C	Y	4	D	Z	5
QTR17	A	X	1	B	X	1	C	Y	4	D	Z	5
QTR18	A	X	1	B	X	1	C	Y	4	D	Z	5
QTR19	A	X	1	B	X	1	C	Y	4	D	Z	5
QTR20	A	X	1	B	X	1	C	Y	4	D	Z	5
QTR21	A	X	2	B	X	1						
QTR22	A	X	2	B	X	1						
QTR23	A	X	2	B	X	1						
QTR24	A	X	2	B	X	1						
QTR25	A	X	2	B	X	1						
QTR26	A	X	2	B	X	1						
QTR27	A	X	2	B	X	1						
QTR28	A	X	2	B	X	1						
QTR 29	A	X	2	B	X	1						
QTR 30	A	X	2	B	X	1						
QTR 31	A	X	2	B	X	1						
QTR 32	A	X	2	B	X	1						
QTR 33	A	X	2	B	Z	5						
QTR 34	A	X	2	B	Z	5						
QTR 35	A	X	2	B	Z	5						
QTR 36	A	X	2	B	Z	5						
QTR 37	A	X	2	B	Z	5						
QTR 38	A	X	2	B	Z	5						
QTR 39	A	X	2	B	Z	5						
QTR 40	A	X	2	B	Z	5						

Figure 2 presents how we identify audit partner fixed effects. We may have multiple BHCs audited by the same partner for any given year. An audit partner may reengage with the same BHCs after a gap. The shaded cells represent the fixed effects that can be estimated. We can estimate fixed effects for AP 1, 2, and 3, AF X, and BHCs A, B, and D.

Figure 3: Identification of the Audit Partner

Coefficient of interest	Can we estimate the FE on the audit partner?	Can we estimate the AP FE separate from AF?		Can we estimate the AP FE separate from BHC?	
		AP is associated with more than one AF	If AP is associated with only one AF, then does AF have more than one AP	AP is associated with more than one BHC	If AP audits only one BHC, then does BHC have more than one AP
AP 1	Yes	No	Yes	Yes	
AP 2	Yes	No	Yes	No	Yes
AP 3	Yes	No	Yes	No	Yes
AP 4	No	No	No	No	No
AP 5	No	No	No	Yes	

Figure 3 represents the identification of the AP relative to the AF and the BHC. The shaded cells represent the AP FE that can be estimated and the conditions that allow it. To estimate audit partner fixed effects, separate from audit firm fixed effects, we need the 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, separate from BHC fixed effect, we need the audit partner to be associated with more than one BHC or the BHC to have more than one audit partner.

Figure 4: Increase in adjusted R^2 on adding AP fixed effects- Actual vs. Random AP data

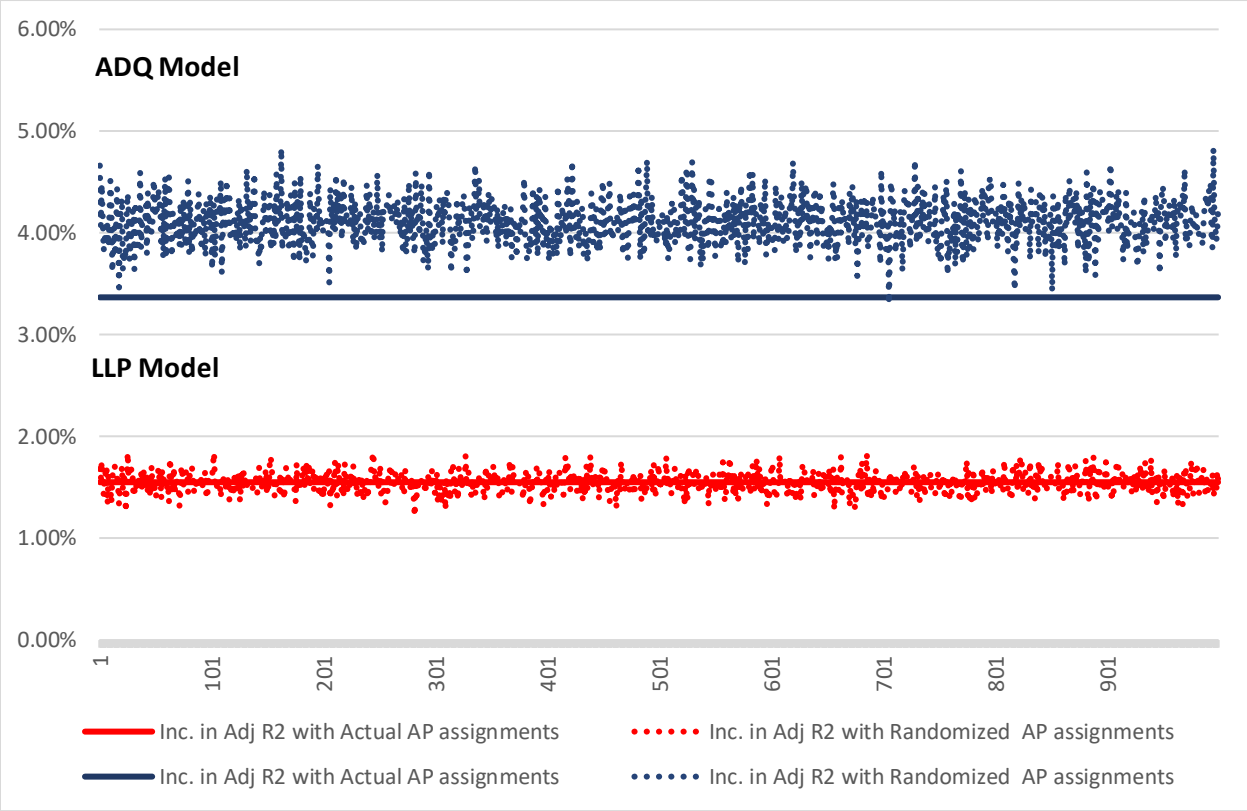

Figure 4 presents the increase in adjusted R^2 on adding AP fixed effects for the LLP and the ADQ model (equation (5) vs. (6)). The solid lines represent the model based on actual AP assignments. The dotted lines represent the results from the 1,000 iterations based on random AP assignments.

Appendix A
Table A1: Variable Definitions

Variable	Explanation	Data from Y9C
ADQ	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
BT	Bank-specific time trend	
COMM_LOANS	Commercial loans$_{t-1}$ / Loans$_{t-1}$	Prior to 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$_{t-1}$	Prior to 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$_{t-1}$
LG	One quarter 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$_{t-1}$
New AP	New Audit Partner	The indicator variable equals 1 in the first eight quarters of a new incoming audit partner; 0 otherwise.
NPL	Non-performing loans$_t$ / Loans$_{t-1}$	Prior to 2018: (bhck5525$_t$ + bhck5526$_t$) / bhck2122$_{t-1}$ 2018 Onward: (bhck1407$_t$ + bhck3506$_t$ + bhck1403$_t$ + bhck3507$_t$) / bhck2122$_{t-1}$
QTR	Quarter	rssd9999
RE_LOANS	Real estate loans$_t$ / Loans$_t$	bhck1410$_{t-1}$ / bhck2122$_t$
SIZE	Log(Total assets in thousands$_{t-1}$)	Log(bhck2170$_{t-1}$)
T1CAP	Tier 1 capital ratio$_{t-1}$	Prior to 2015: bhck7206$_{t-1}$ 2015 Onward: bhca7206$_{t-1}$
ΔNPL	One quarter change in NPL$_t$ / Loans$_{t-1}$	Prior to 2018: (Δbhck5525$_t$ + Δbhck5526$_t$) / bhck2122$_{t-1}$ 2018 Onward: (Δbhck1407$_t$ + Δbhck3506$_t$ + Δbhck1403$_t$ + Δbhck3507$_t$) / bhck2122$_{t-1}$

Table 1: Outline of the Sample Selection Process

Panel A of this table outlines the sample selection process. Panel B includes information about unique quarters (QTR), bank holding companies (BHC), audit firms (AF), and audit partners (AP) for all public and private BHCs. 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 APs is obtained from the Federal Reserve Bank of Dallas. Variables are defined in Appendix A.

Panel A: Sample selection

	All	
	BHC quarters	Unique BHCs
Observations between 2006Q1 to 2019Q4 with non-missing BHC variables	46,954	1,601
Less: Observations with missing/erroneous AP names	5,606	220
Less: Observations relating to banks with total assets above $100 billion	1,351	35
The sample used in regressions with LLP (ADQ) as the dependent variable	39,997 (39,706)	1,346 (1,343)

Panel B: Details of the sample used in the estimation

		BHC Quarters	Unique QTRs	Unique BHCs	Unique AFs	Unique APs
2006Q1 to 2019Q4	Public	15,473	56	538	69	534
	Private	24,524	56	934	201	763
	Total	39,997	56	1,346	210	941

Table 2: BHC Summary Statistics

This table reports the mean, standard deviation (SD), 25th percentile, median, and 75th percentile for the two main dependent variables, loan loss provisions (LLP) and adequacy of allowance (ADQ), additional dependent variables, allowance (ALL), net charge-offs (NCO), and non-performing loans (NPL), and control variables used in this study. The BHC variables are obtained from the Y9C call reports available on the Chicago Federal Reserve website. Variables are defined in Appendix A.

Variable	Mean	SD	P25	Median	P75	N
ASSETS (millions)	3,158	5,867	692	1,138	2,556	39,997
Dependent Variables						
LLP (%)	0.174	0.324	0.025	0.069	0.176	39,997
ADQ	2.417	4.550	0.620	1.108	2.206	39,706
ALL (%)	1.600	0.776	1.107	1.392	1.870	39,997
NCO (%)	0.153	0.290	0.009	0.047	0.159	39,997
NPL (%)	2.108	2.530	0.555	1.201	2.599	39,997
Controls						
SIZE	14.230	1.043	13.448	13.945	14.754	39,997
EBP (%)	0.333	0.236	0.237	0.349	0.451	39,997
T1CAP (%)	13.213	4.487	10.600	12.400	14.770	39,997
LG (%)	1.492	4.389	(0.839)	1.083	3.097	39,997
ΔNPL (%)	0.032	0.678	(0.163)	(0.006)	0.166	39,997
RE_LOANS (%)	74.688	15.230	67.133	77.354	85.536	39,997
COMM_LOANS (%)	17.387	11.153	9.262	14.933	22.904	39,997
CONS_LOANS (%)	4.663	6.525	0.933	2.382	5.274	39,997

Table 3: Audit Partner Data Interrelationships with Quarter, Bank, and Audit Firms

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 quarters (Q). The confidential data on the names of the APs is obtained from the Federal Reserve Bank of Dallas. Variables are defined in Appendix A.

Variable	Mean	SD	P25	Median	P75
For the 941 unique APs					
Number of QTRs per AP	23.6	14.1	12	20	32
Number of BHCs per AP	3.4	3.3	1	2	4
Number of AFs per AP	1.1	0.4	1	1	1
For the 210 unique AFs					
Number of QTRs per AF	30.2	16.9	14	32	48
Number of BHCs per AF	9.0	25.0	1	2	4
Number of APs per AF	5.0	11.9	1	2	3
For the 1,346 unique BHCs					
Number of QTRs per BHC	29.7	17.3	15	31	48
Number of AFs per BHC	1.4	0.6	1	1	2
Number of APs per BHC	2.4	1.2	1	2	3
For the 56 unique QTRs					
Number of BHCs per QTR	714.2	236.6	522	845	896
Number of AFs per QTR	113.2	41.7	75	141	145
Number of APs per QTR	396.9	107.4	316	462	476

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

Panel A and Panel B of this table report the results of the regression models in equations (1) to (6) for our two main dependent variables, loan loss provisions (LLP) and adequacy of allowance (ADQ), respectively.

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 APs is obtained from the Federal Reserve Bank of Dallas. Variables are defined in Appendix A. 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)	LLP (6)
SIZE	0.000	0.000***	0.001***	0.002***	0.002***	0.002***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
EBP	-0.217***	-0.154***	-0.232***	-0.210***	-0.207***	-0.198***
	(0.029)	(0.028)	(0.021)	(0.023)	(0.023)	(0.022)
T1CAP	-0.005**	-0.002	-0.003*	-0.001	-0.001	0.000
	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)
LG	-0.014***	-0.008***	-0.005***	-0.003***	-0.003***	-0.003***
	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)
ΔNPL	0.089***	0.062***	0.049***	0.037***	0.036***	0.034***
	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)
RE_LOANS	-0.003	-0.003*	0.002**	-0.000	-0.001	-0.000
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
COMM_LOANS	-0.004**	-0.004**	0.001	0.000	-0.000	0.000
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
CONS_LOANS	0.000	-0.001	0.003	-0.003	-0.003	-0.003
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
QTR FE	No	Yes	Yes	Yes	Yes	Yes
BHC FE	No	No	Yes	Yes	Yes	Yes
BHC Trend	No	No	No	Yes	Yes	Yes
AF FE	No	No	No	No	Yes	Yes
AP FE	No	No	No	No	No	Yes
Adj. R^2	0.115	0.270	0.447	0.498	0.502	0.518
N	39,997	39,997	39,997	39,997	39,997	39,997

Panel B: Regression models for ADQ

Dependent Variable	ADQ (1)	ADQ (2)	ADQ (3)	ADQ (4)	ADQ (5)	ADQ (6)
SIZE	-0.411***	-0.470***	-1.351***	-0.987***	-0.979**	-1.457***
	(0.058)	(0.061)	(0.270)	(0.378)	(0.381)	(0.361)
EBP	208.762***	120.987***	75.667***	37.114**	36.238**	28.239*
	(26.532)	(25.919)	(19.384)	(16.198)	(16.080)	(14.701)
T1CAP	2.135	5.819***	1.223	1.420	1.464	0.637
	(1.896)	(1.936)	(2.362)	(2.700)	(2.785)	(2.355)
LG	11.235***	5.327***	2.397***	1.927***	1.843***	1.704***
	(0.956)	(0.908)	(0.646)	(0.595)	(0.594)	(0.565)
ΔNPL	-42.924***	-44.883***	-38.714***	-39.465***	-39.362***	-39.584***
	(2.196)	(2.552)	(2.715)	(2.515)	(2.559)	(2.680)
RE_LOANS	-4.602**	-4.751***	-7.409***	-0.709	-0.741	-0.869
	(1.826)	(1.759)	(2.737)	(1.889)	(1.859)	(2.002)
COMM_LOANS	-3.343	-3.200	-6.292**	-2.443	-2.596	-3.233
	(2.075)	(2.008)	(3.054)	(2.709)	(2.690)	(2.667)
CONS_LOANS	-4.383*	-5.208**	-10.243***	-8.617**	-8.396**	-7.874*
	(2.654)	(2.612)	(3.736)	(3.981)	(4.014)	(4.286)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
QTR FE	No	Yes	Yes	Yes	Yes	Yes
BHC FE	No	No	Yes	Yes	Yes	Yes
BHC Trend	No	No	No	Yes	Yes	Yes
AF FE	No	No	No	No	Yes	Yes
AP FE	No	No	No	No	No	Yes
Adj. R^2	0.043	0.115	0.403	0.549	0.553	0.587
N	39,706	39,706	39,706	39,706	39,706	39,706

Table 5: Evidence from Audit Partner Fixed Effects

Panel A of this table reports the results of the benchmark regression and the fixed effects panel regressions. For each dependent variable in column (1), the fixed effects (FE) 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 FE are reported in columns (3) and (4), respectively. Rows 1 to 6 include the estimation results of the regression models represented in equations (1) to (6), respectively. The F-statistics and the joint significance of the audit partner FE for regression equation (6) are reported in columns (5)-(6).

Panel B of the table reports the results of the fixed effects panel regressions represented in equation (6) using random AP assignments based on 1,000 iterations. For each dependent variable, the fixed effects (FE) included in addition to the control variables are quarter FE, BHC FE, BHC-specific time trend, audit firm FE and audit partner FE. This specification is equivalent to the specification in row 6 for each dependent variable in Table 5, which uses the actual audit partner assignment for BHCs. The adj. R^2 and the increase in adj. R^2 is average for the 1,000 iterations.

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 APs is obtained from the Federal Reserve Bank of Dallas. Variables are defined in Appendix A. 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: Evidence from the F test – actual data

Dependent Variable	Fixed Effects	Adj. R^2	Increase in Adj. R^2	F-stat AP FE	p-value AP FE	N
(1)	(2)	(3)	(4)	(5)	(6)	(7)
LLP	Controls	11.52%				39,997
LLP	Controls, QTR	26.99%	15.47%			39,997
LLP	Controls, QTR, BHC	44.66%	17.67%			39,997
LLP	Controls, QTR, BHC, BHC Trend	49.82%	5.16%			39,997
LLP	Controls, QTR, BHC, BHC Trend, AF	50.24%	0.42%			39,997
LLP	Controls, QTR, BHC, BHC Trend, AF, AP	51.79%	1.55%	>100	<.0001	39,997
ADQ	Controls	4.31%				39,706
ADQ	Controls, QTR	11.46%	7.15%			39,706
ADQ	Controls, QTR, BHC	40.29%	28.83%			39,706
ADQ	Controls, QTR, BHC, BHC Trend	54.89%	14.60%			39,706
ADQ	Controls, QTR, BHC, BHC Trend, AF	55.32%	0.43%			39,706
ADQ	Controls, QTR, BHC, BHC Trend, AF, AP	58.69%	3.37%	>100	<.0001	39,706

Panel B: Evidence from the falsification test using random AP data based on 1,000 iterations

Dependent Variable	Fixed Effects	Average Adj. R^2	Average Increase in Adj. R^2	F-stat AP FE	p-value AP FE	N
(1)	(2)	(3)	(4)	(5)	(6)	(7)
LLP (Random AP)	Controls, QTR, BHC, BHC Trend, AF, AP	51.79%	1.55%	>100	<.0001	39,997
ADQ (Random AP)	Controls, QTR, BHC, BHC Trend, AF, AP	59.43%	4.11%	>100	<.0001	39,706

Table 6: Evidence from Prior Auditing Style

Panel A of the table reports the results of the average residual from regression (5) for a BHC-audit partner spell on the average residuals for such regressions on all the previous BHC-audit partner spells for that particular audit partner. Regression (5) includes control variables and fixed effects for QTR, BHC, BHC Trend, and AF.

Panel B of the table reports the results of the regression of the residual of the loan loss variable from the regression equation (5) on NEW AP, an indicator variable equal to 1 if the observation is within the first eight quarters of a new audit partner's spell; 0 otherwise. The regression equation (5) includes control variables and fixed effects for QTR, BHC, BHC Trend, and AF.

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 APs is obtained from the Federal Reserve Bank of Dallas. Variables are defined in Appendix A. 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: Do Audit Partners Exhibit Persistent Styles *Across* Spells?

Dependent variable	Average residual from AP-BHC spell	
	LLP	ADQ
	(1)	(2)
Average residual from prior AP-BHC spells	-0.053*	0.025
	(0.027)	(0.048)
R^2	0.002	0.000
Observations	1,862	1,861

Panel B: Do Audit Partner Changes Trigger Accounting Changes?

Dependent variable	Residual at BHC quarter level	
	LLP	ADQ
	(1)	(2)
New AP	0.000049	-0.043694
	(0.000033)	(0.046698)
R^2	0.000098	0.000044
Observations	34,693	34,441

Table 7: Evidence from Loan Loss Allowance, Net Charge-Offs, and Non-Performing Loans

Panel A of the table reports the baseline regression results and the fixed effects panel regressions for the three additional dependent variables, ALL, NCO, and NPL. The fixed effects (FE) included in addition to the control variables are reported in columns (2). The adjusted R-square and the increase in adjusted R^2 resulting from including FE are reported in columns (3) and (4), respectively. Rows 1 to 6 include the results of the estimation of the regression models represented in equations (1) to (6), respectively. The F-statistics and the joint significance of the audit partner FE for regression equation (6) are reported in columns (5)-(6).

Panel B of the table reports the results of the fixed effects panel regressions represented in equation (6) using random AP assignments based on 1,000 iterations. For each dependent variable, the fixed effects (FE) included in addition to the control variables are quarter FE, BHC FE, BHC-specific time trend, audit firm FE and audit partner FE. This specification is equivalent to the specification in row 6 for each dependent variable in Panel A, which uses the actual audit partner assignment for BHCs. The adj. R^2 and the increase in adj. R^2 is average for the 1,000 iterations.

Panel C of the table reports the results of the average residual from the regression equation (5) for a BHC-audit partner spell on the average residuals for such regressions on all the previous BHC-audit partner spells for that particular audit partner. The regression equation (5) includes control variables and fixed effects for QTR, BHC, BHC Trend, and AF.

Panel D of the table reports the results of the regression of the residual of the loan loss variable from regression equation (5) on NEW AP, an indicator variable equal to 1 if the observation is within the first eight quarters of a new audit partner's spell; 0 otherwise. The regression equation (5) includes control variables and fixed effects for QTR, BHC, BHC Trend, and AF.

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 APs is obtained from the Federal Reserve Bank of Dallas. Variables are defined in Appendix A. 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: Evidence from the F test – actual data

Dependent Variable	Fixed Effects	Adj. R^2	Increase in Adj. R^2	F-stat AP FE	p-value AP FE	N
(1)	(2)	(3)	(4)	(5)	(6)	(7)
ALL	Controls	10.61%				
ALL	Controls, QTR	24.41%	13.80%			39,997
ALL	Controls, QTR, BHC	69.33%	44.92%			39,997
ALL	Controls, QTR, BHC, BHC Trend	78.96%	9.62%			39,997
ALL	Controls, QTR, BHC, BHC Trend, AF	79.76%	0.80%			39,997
ALL	Controls, QTR, BHC, BHC Trend, AF, AP	82.78%	3.02%	>100	<.0001	39,997
NCO	Controls	12.51%				39,997
NCO	Controls, QTR	25.24%	12.73%			39,997
NCO	Controls, QTR, BHC	43.95%	18.70%			39,997
NCO	Controls, QTR, BHC, BHC Trend	48.78%	4.84%			39,997
NCO	Controls, QTR, BHC, BHC Trend, AF	49.16%	0.38%			39,997
NCO	Controls, QTR, BHC, BHC Trend, AF, AP	50.72%	1.55%	>100	<.0001	39,997
NPL	Controls	21.77%				
NPL	Controls, QTR	35.07%	13.31%			39,997
NPL	Controls, QTR, BHC	68.37%	33.30%			39,997
NPL	Controls, QTR, BHC, BHC Trend	78.67%	10.30%			39,997
NPL	Controls, QTR, BHC, BHC Trend, AF	79.34%	0.67%			39,997
NPL	Controls, QTR, BHC, BHC Trend, AF, AP	82.43%	3.09%	>100	<.0001	39,997

Panel B: Evidence from the falsification test using random AP data based on 1,000 iterations

Dependent Variable	Fixed Effects	Average Adj. R^2	Average Increase in Adj. R^2	F-stat AP FE	p-value AP FE	N
(1)	(2)	(3)	(4)	(5)	(6)	7)
ALL (Random AP)	Controls, QTR, BHC, BHC Trend, AF, AP	88.27%	8.51%	>100	<.0001	39,997
NCO (Random AP)	Controls, QTR, BHC, BHC Trend, AF, AP	49.61%	0.45%	>100	<.0001	39,997
NPL (Random AP)	Controls, QTR, BHC, BHC Trend, AF, AP	82.81%	3.47%	>100	<.0001	39,997

Panel C: Do Audit Partners Exhibit Persistent Styles *Across* Spells?

Dependent variable	Residual from AP-BHC spell		
	ALL	NCO	NPL
	(1)	(2)	(3)
Residual from prior AP-BHC spells	-0.073**	-0.072***	-0.042
	(0.028)	(0.026)	(0.033)
R^2	0.004	0.004	0.001
Observations	1,862	1,862	1,862

Panel D: Do Audit Partner Changes Trigger Accounting Changes?

Dependent variable	Residual		
	ALL	NCO	NPL
	(1)	(2)	(3)
New AP	0.000099	0.000038	0.000270
	(0.000073)	(0.000033)	(0.000243)
R^2	0.0001	0.0001	0.0001
Observations	34,693	34,693	34,693

Table 8: Evidence from conditional analyses

Panel A of the table reports the average adjusted R^2 and the average increase in adjusted R^2 for the two main variables LLP and ADQ from the regression model in equation (6) for various sub-samples (column 1) in columns (2) and (3). The regression model in equation (6) includes control variables and fixed effects for QTR, BHC, BHC Trend, AF, and AP. The F-statistics and the joint significance of the audit partner FE are reported in columns (5)-(6). For each sub-sample, row 1 includes the results based on actual AP assignments, and row 2 includes the results using random AP assignments based on 1,000 iterations. The adj. R^2 and the increase in adj. R^2 is average for the 1,000 iterations.

Panel B of the table reports the results of the average residual from the regression equation (5) for a BHC-audit partner spell on the average residuals for such regressions on all the previous BHC-audit partner spells for that particular audit partner. The regression equation (5) includes control variables and fixed effects for QTR, BHC, BHC Trend, and AF.

Panel C of the table reports the results of the regression of the residual of the loan loss variable from regression (5) on NEW AP, an indicator variable equal to 1 if the observation is within the first eight quarters of a new audit partner's spell; 0 otherwise. The regression equation (5) includes control variables and fixed effects for QTR, BHC, BHC Trend, and AF.

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 APs is obtained from the Federal Reserve Bank of Dallas. Variables are defined in Appendix A. 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: Evidence from the F test – actual data vs. falsification test using random AP data based on 1,000 iterations

Sub-sample	Dependent Variable	Adj. R^2	Increase in Adj. R^2	F-stat AP FE	p-value AP FE	N
(1)	(2)	(3)	(4)	(5)	(6)	(7)
Quarter 4 observations	LLP (Actual AP Data)	56.48%	1.30%	>100	<.0001	10,182
	LLP (Random AP Data)	56.15%	0.97%	>100	<.0001	10,182
	ADQ (Actual AP Data)	58.70%	2.56%	>100	<.0001	10,105
	ADQ (Random AP Data)	58.96%	2.82%	>100	<.0001	10,105
Public	LLP (Actual AP Data)	55.60%	2.65%	>100	<.0001	15,473
	LLP (Random AP Data)	55.58%	2.63%	>100	<.0001	15,473
	ADQ (Actual AP Data)	63.47%	4.99%	>100	<.0001	15,429
	ADQ (Random AP Data)	63.80%	5.32%	>100	<.0001	15,429
Private	LLP (Actual AP Data)	51.76%	1.32%	>100	<.0001	24.524
	LLP (Random AP Data)	51.95%	1.51%	>100	<.0001	24.524
	ADQ (Actual AP Data)	58.43%	3.53%	>100	<.0001	24,277
	ADQ (Random AP Data)	59.42%	4.52%	>100	<.0001	24,277
TA<10 billion	LLP (Actual AP Data)	51.01%	1.55%	>100	<.0001	35,057
	LLP (Random AP Data)	50.91%	1.45%	>100	<.0001	35,057
	ADQ (Actual AP Data)	58.22%	3.38%	>100	<.0001	34,771
	ADQ (Random AP Data)	59.06%	4.22%	>100	<.0001	34,771
BHC-AP conditions	LLP (Actual AP Data)	53.11%	2.03%	>100	<.0001	20,682
	LLP (Random AP Data)	53.01%	1.93%	>100	<.0001	20,682
	ADQ (Actual AP Data)	56.63%	2.26%	>100	<.0001	20,597
	ADQ (Random AP Data)	57.53%	3.16%	>100	<.0001	20,597

Panel B: Do Audit Partners Exhibit Persistent Styles *Across* Spells?

Sub-sample	Dependent variable	Residual from AP-BHC spell	
		LLP	ADQ
		(1)	(2)
Quarter 4 observations	Residual from prior AP-BHC spells	-0.045	-0.013
		(0.029)	(0.049)
	R^2	0.001	0.000
	Observations	1,830	1,825
Public	Residual from prior AP-BHC spells	-0.082	0.018
		(0.050)	(0.042)
	R^2	0.005	0.000
	Observations	711	711
Private	Residual from prior AP-BHC spells	-0.010	0.034
		(0.026)	(0.057)
	R^2	0.000	0.001
	Observations	985	984
TA<10 billion	Residual from prior AP-BHC spells	-0.062*	0.017
		(0.033)	(0.051)
	R^2	0.004	0.000
	Observations	1,603	1,602
BHC-AP conditions	Residual from prior AP-BHC spells	-0.114**	0.011
		(0.051)	(0.059)
	R^2	0.023	0.000
	Observations	627	625

Panel C: Do Audit Partner Changes Trigger Accounting Changes?

Sub-sample	Dependent variable	Residual	
		LLP	ADQ
		(1)	(2)
Quarter 4 observations	New AP	-0.000008	0.016164
		(0.000029)	(0.051494)
	R^2	0.000001	0.000008
	Observations	8,796	8,728
Public	New AP	0.000007	-0.020196
		(0.000051)	(0.059928)
	R^2	0.000003	0.000013
	Observations	14,308	14,269
Private	New AP	0.000050	-0.076766
		(0.000042)	(0.069744)
	R^2	0.0001	0.0001
	Observations	19,245	19,035
TA<10 billion	New AP	0.000061*	-0.059949
		(0.000035)	(0.055215)
	R^2	0.0001	0.0001
	Observations	29,764	29,514
BHC-AP conditions	New AP	0.000070	-0.059494
		(0.000047)	(0.079637)
	R^2	0.0002	0.0001
	Observations	17,529	17,463