Economic Growth and Financial Statement Verification

Economic Growth and Financial Statement Verification*

Petro Lisowsky
University of Illinois at Urbana-Champaign and
Norwegian Center for Taxation
lisowsky@illinois.edu

Michael Minnis
University of Chicago Booth School of Business
michael.minnis@chicagobooth.edu

Andrew Sutherland+
Massachusetts Institute of Technology Sloan School of Management
ags1@mit.edu

First draft: April 2014
This draft: August 2016

The online appendix is available at the end of this manuscript.

* Previously titled, "Credit Cycles and Financial Statement Verification." We thank Ray Ball, Itzhak Ben-David (discussant), Phil Berger, Hans Christensen, Christian Leuz, Canice Prendergast, Nayana Reiter (discussant), Stephen Ryan, Amit Seru, Douglas Skinner, Luigi Zingales, and participants at the PCAOB-JAR Conference on Auditing and Capital Markets, the FARS Midyear conference, and the Chicago Booth, Iowa, National University of Singapore, Singapore Management University, and Wharton workshops for helpful comments. We are extremely grateful to the Risk Management Association, Sageworks, Inc., and the Internal Revenue Service (IRS) for providing data for this project. Confidential tax return information was provided to Lisowsky by the IRS Large Business & International (LB&I) Planning, Analysis, Inventory, and Research Division (PAIR). Because tax data are confidential and protected by data non-disclosure agreements under the Internal Revenue Code, all statistics are presented in the aggregate so that no specific taxpayer can be identified from this manuscript. Any opinions are those of the authors and do not necessarily reflect the views of the IRS. Lisowsky acknowledges support from the MIT Sloan School of Management and the PricewaterhouseCoopers Faculty Fellowship at the University of Illinois at Urbana-Champaign. Minnis acknowledges support from the ARAMARK Faculty Research Fund and the Chicago Booth School of Business. Sutherland acknowledges support from the Ernie Wish Fellowship at the Chicago Booth School of Business and the MIT Sloan School of Management. Any errors or omissions are our own.
+ Corresponding author. Phone: 617.324.3894.

Economic Growth and Financial Statement Verification

Abstract

We use a proprietary dataset of financial statements collected by banks to examine whether economic growth is related to the use of financial statement verification in debt financing. Exploiting the distinct economic growth and contraction patterns of the construction industry over the years 2002 to 2011, our estimates reveal that banks reduced their collection of unqualified audited financial statements from construction firms at nearly twice the rate of firms in other industries during the housing boom period before 2008. This reduction was most severe in the regions that experienced the most significant construction growth. These trends reversed during the subsequent housing crisis in 2008 to 2011 when construction activity contracted. Moreover, using bank- and firm-level data we find a strong negative (positive) relation between audited financial statements during the growth period and subsequent loan losses (construction firm survival) during the contraction period. Collectively, our results reveal that macroeconomic fluctuations produce temporal shifts in the overall level of financial statement verification and that temporal shifts in verification are related to bank loan portfolio quality and borrower performance.

JEL Classification: D82, E32, E44, G21, M40.
Keywords: economic growth; commercial lending; banks; financial crisis; audit; verification; financial statements; lending standards.

Economic Growth and Financial Statement Verification

1. Introduction

A substantial literature in accounting highlights that audited financial statements are a key input into lenders' underwriting and monitoring practices. Banks can request financial statements audited by an independent accountant as part of a formal contract or as part of an implicit contracting process to learn more about the borrower (e.g., Armstrong, Guay, and Weber 2010; Christensen, Nikolaev, and Wittenberg-Moerman 2015). Studies examining the use of accounting in debt financing typically consider borrower, bank, or contract-specific characteristics. However, recent theories argue that broader economic conditions affect banks' lending standards (e.g., Ruckes 2004; Dell'Ariccia and Marquez 2006). In particular, favorable economic shocks within a sector create a surge in credit demand, which in turn compels competition among banks to reduce the actual or perceived benefit of firm-specific credit analysis. As a result, these theories broadly suggest that economic growth and underwriting standards—of which financial statement verification via auditing is an important input—are negatively related. Motivated by this theoretical framework, we investigate two research questions. Does financial statement verification in debt contracting change with the rate of economic growth? If so, does this variation have implications for loan portfolio performance?

We use the US construction industry from 2002 to 2011 to examine these questions. This setting has several useful features. First, the vast majority of the construction activity is conducted by privately held firms that do not face a regulatory audit mandate: construction firms with fewer than 500 employees represent over 84% (98%) of total employment (establishments) in the industry, versus 48% (84%) for all US industries (US Bureau of Economic Analysis). Thus, for the vast majority of firms in the construction industry, independent verification of financial performance is a market-driven equilibrium with the potential to vary with economic conditions.

Second, the industry experienced significant variation in economic growth over this period. Commercial construction and development loans outstanding increased from $200 billion in 2001 to nearly $700 billion by 2008 (see Figure 1 and OIG 2012). Employment (number of construction establishments) increased by 900,000 (74,716), or 13% (11%), during this same period (US Bureau of Economic Analysis, US Census Bureau). After the onset of the housing crisis, loan losses related to construction exceeded $100 billion (FDIC Call Reports); the construction industry accounted for over one-third of net private sector job losses from 2007 to 2011, or 2.1 million jobs (BEA); and the number of construction establishments decreased from a peak of 773,614 to 657,738 in 2011 (US Census Bureau). Moreover, the economic growth experienced by this industry over this time varied greatly across geographic region, allowing us to exploit not only temporal, but also spatial variation. Therefore, this setting provides the necessary economic significance and variation in auditing and economic performance to examine our research questions.

We use a proprietary dataset of borrower financial reports compiled by the Risk Management Association (RMA) in a difference-in-difference design to examine how intensively banks verified the financial condition of their construction borrowers relative to non-construction borrowers from 2002 to 2011. Each year, financial institutions submit to RMA financial reports collected by banks from about 200,000 commercial borrowers as a result of a loan application or ongoing loan monitoring process. The dataset contains firms from a broad spectrum of sizes cumulatively generating trillions of dollars of annual revenue from hundreds of industries. RMA reports the distribution of financial reports collected across five mutually exclusive categories, from highest to lowest verification level: unqualified audit, review, compilation, tax return, and other. RMA graciously tabulated these data for us by bank and year including each borrower's

industry, size category, and region. We then link this bank-level financial report data to banks' Report of Condition and Income reports (i.e., "Call Reports") from the Federal Deposit Insurance Corporation (FDIC).

Theory offers two broad predictions surrounding the relation between economic growth and lending standards (e.g., Ruckes 2004; Dell'Ariccia and Marquez 2006). First, as rising economic activity increases the number of firms seeking financing, banks' adverse selection concerns diminish as fewer commercial loan applicants are rejected applicants from other banks. Sectoral economic growth also provides information to banks. When macroeconomic reports indicate a sector is growing, firm-specific information is less likely to change a lender's priors about the applicant's type. Collectively, lower adverse selection concerns and awareness of the overall state of the economy reduce the benefit of firm-specific information collection. In the presence of bank competition, banks thus lower lending standards, and in particular reduce their use of costly firm-specific verified financial information.

Consistent with this prediction, we find that banks substantially decreased the proportion of unqualified audited financial reports collected from construction firms relative to firms in other industries in the years of the construction boom; instead, lenders disproportionally used tax returns or other financial reports with less independent verification.[1] This trend is particularly strong in medium-sized firms that represent a significant portion of the US construction industry. The economic magnitude of this change is large: whereas 19% of medium-sized construction firms provided unqualified audited financial statements to their lenders in 2002, only 10% did so

[1] In its annual Survey of Credit Underwriting Practices, the Office of the Comptroller of the Currency (OCC) notes that financial reporting is a key element of underwriting, defining underwriting standards as those "terms and conditions under which banks approve, extend, or renew credit such as *financial reporting*, collateral requirements, repayment terms, pricing, and covenants" (OCC 2014, pg. 2, emphasis added). See Becker and Ivashina [2016] for an analysis of a related phenomenon in which lenders significantly reduce their enforcement features, often referred to as "cov-lite" loans.

by 2007. This reduction in financial verification was nearly twice as large as the decrease for non-construction industries over this period. Reinforcing this main result, we find that the reduction in unqualified audit report collection is most pronounced in the US regions that experienced the most significant increase in construction lending: the West (e.g., California) and Southeast (e.g., Florida).[2] Moreover, consistent with the link between economic growth and lending standards, we find that the proportion of unqualified audited financial statements collected *increases* (i.e., reverses) for construction firms relative to other industries in conjunction with the onset of the crisis. This supports our inference that the pre-crisis reduction in financial statement verification was related specifically to changes in economic activity and not simply a broader secular trend.

The second theoretical prediction is that the reduction in lending standards triggered by the surge in economic activity leads to a weaker portfolio of borrowers in the event that the industry encounters a negative shock. We examine this prediction by testing whether banks with lower proportions of construction borrowers with unqualified audited financial statements at the height of the economic boom suffered higher levels of loan losses upon the onset of the crisis. As another advantage of the construction industry setting, starting in 2007 the FDIC required banks to report losses specifically attributable to construction and development lending, allowing us to match each bank's RMA financial collection data to the bank's level of construction lending and loan losses as reported in the Call Reports. We find that banks with lower rates of unqualified audit collection during the boom experienced significantly more construction portfolio losses in the years of the housing bust. We estimate that a one standard deviation increase in the percentage of construction firms with unqualified audits in a bank's portfolio during the housing boom

[2] In a complementary study on the home purchase segment of the market, Dell'Ariccia et al. (2012) provide empirical evidence that banks reduced the weight on borrowers' loan-to-income ratios in the subprime mortgage setting, and that this reduction occurred more intensively in regions with more credit demand growth.

is associated with a 12% lower construction chargeoff rate during the crisis. These results are robust to controlling for a variety of bank portfolio characteristics, including the geographic location of borrowers and losses in other portions of the banks' loan portfolios. Our main results are thus consistent with theory: bank screening and monitoring intensity varies with economic growth and this intensity is related to subsequent bank losses.

While our difference-in-difference design and regional cross-sectional tests attempt to isolate the relation between economic growth and lending standards, we conduct several additional tests to address alternative explanations. First, the RMA dataset does not provide borrower-level variables other than a size category, which limits the extent to which we can control for changes in borrower characteristics. Specifically, the economic shocks causing the economic growth and contraction could also create investment opportunities for new, small firms that are less likely to have audited financial statements and less likely to survive. Therefore, changes in the composition of firms over time could be mechanically driving our results. To address this concern, we restrict our analysis to the subsample of statements provided by firms with at least $10 million of revenue and find that our results are strongest in this group, which is least likely to include small startups.[3] We also use a separate firm-level panel dataset from Sageworks, Inc. We find that (1) our main results hold and are economically similar even after controlling for firm-level characteristics, including size, growth, leverage, and profitability; (2) construction firms had a 30% higher audit termination rate relative to non-construction firms during the construction boom years; and (3) changes in firm attributes over time (i.e., the mix of firm types) have little explanatory power for our main results. In all, these analyses highlight that our find-

[3] While startups and the arrival of new firms (i.e., a potential change in the mix of construction firms) are part of the theoretical framework as described in Section 2, these various robustness tests ensure that our main result is not mechanically driven by the addition of new, small construction firm to the sample, which would not typically have audited financial statements.

ings are neither idiosyncratic to the RMA sample nor driven by observable changes in the underlying mix of firm characteristics. Instead, construction firms change their relative propensity to receive audits *conditional* on firm attributes.

Next, to corroborate that independently verified financial statements are associated with future borrower performance, we use panel data from confidential US tax returns in estimating firm survival rates during the housing crisis. Starting in 2008, the IRS has required firms with assets of $10 million or more to indicate whether they receive a GAAP financial statement audit. Using this panel of all private firms during 2008-2010, we find that construction firms that were audited in 2008 had a 10% higher likelihood of surviving to 2010 relative to construction firms without an audit, controlling for factors related to survival and the decision to get an audit. While not providing a causal effect between audits and firm performance, this test reveals a crucial link showing that construction borrowers with audits performed better during the economic downturn.

Finally, we conduct a variety of cross-sectional and robustness tests. We first examine whether the relation between audit rates and economic growth is generalizable outside of the construction industry. In pooled cross sectional tests, we regress the audit rate for each industry on the annual industry-level GDP growth. The results reveal a significantly negative relation (though smaller in economic magnitude compared to the tests explicitly examining the construction industry) suggesting that the relation between economic growth and financial statement verification is a general phenomenon. We then return to our construction setting to examine whether speculative investments in land—which is tangible collateral that requires less financial statement verification—simultaneously drives the relation between credit demand during the boom, audit intensity, and subsequent loan losses. We re-estimate our tests on sub-industries within

construction that have cash flows tightly linked to construction activity, but are not significantly involved in land speculation. All inferences remain within this subset. Next, we analyze audit prices to assess whether construction firms potentially faced a higher inelasticity in the supply of audit services relative to non-construction firms. We find no evidence to support this explanation and, in fact, find some evidence that audit prices *decreased more* for construction firms relative to other firms. Finally, we include bank fixed effects and control for bank securitization activity. These tests account for time invariant bank characteristics, policies, and strategies. Our inferences do not change, suggesting that banks responded to the construction industry growth by establishing lending policies that vary over time and across industries and geography, and are not explained by other activities such as securitization.

Our results contribute to two lines of research. First, we enhance our understanding of how economic conditions are related to the role of accounting in debt contracting. Because publicly traded firms are required to receive a financial statement audit by regulatory mandate, settings to study the role and value of auditing are difficult to identify. In our setting, financial reporting verification is not mandated, but based on negotiation between the borrower and bank, similar to collateral or other lending terms. Recent studies examining privately held US firms show that in the absence of regulatory fiat, audited financial statements are not necessarily the equilibrium outcome, even in larger private firms with external capital (Blackwell et al. 1998; Allee and Yohn 2009; Minnis 2011; Lisowsky and Minnis 2015). We extend these studies by providing evidence of the equilibrium level of auditing not only in the cross section, but also over time, based on economic conditions.

Second, we provide novel evidence of factors associated with the housing crisis. Prior to the crisis, we find that banks shifted from highly verified to less (or un-) verified financial re-

ports for construction borrowers. During the crisis, we find an economically significant negative link between lending standards and subsequent bank losses. Although prior work shows that banks lowered their credit standards for individual home purchasers during the same period (e.g., Mian and Sufi 2009; Keys et al. 2010; Ben-David 2011; Demyanyk and Van Hemert 2011; Loutskina and Strahan 2011; Maddaloni and Peydró 2011; Dell'Ariccia et al. 2012), an important difference for our setting is that, unlike mortgages, banks typically retain small and medium commercial loans. Thus, securitization is unlikely to cause the variation in commercial lending standards we observe over the economic cycle.

Importantly, we identify *how* banks reduced their lending standards by analyzing the verification level of financial reports they collected. Because settings to study the information collected by banks from a large sample of firms are difficult to find, prior literature has been unable to separate the role of verification from other factors, such as borrower risk, in examining changes in lending standards. Our data allow us to link changes in verification to banks' loan losses and firm survival. Also, our research design, which controls for industry, time, and bank characteristics, mitigates concerns that other economic factors, sample composition issues, or audit market trends explain our results. In all, we demonstrate that lending standards in the commercial segment of the credit market vary significantly with economic growth, and provide direct evidence of a specific verification mechanism—audits—by which it happens.

2. Theoretical framework and setting

2.1. Theory

Ruckes (2004) and Dell'Ariccia and Marquez (2006) provide a theoretical framework for understanding how lending standards change with the economic growth of an industry. Ruckes (2004) shows that as the economic outlook improves for a segment of borrowers (e.g., an indus-

try), the default probability of the average borrower within the group declines. In turn, this improvement in the average borrower's prospects reduces the value of borrower-specific screening. This line of reasoning is related to models showing how disclosures by one firm can inform investors about related firms (Lambert, Leuz, and Verrecchia 2007), and empirical research relating a bank's experience in a given sector to its demand for verified information from borrowers in that sector (Berger, Minnis, and Sutherland 2016).

Dell'Ariccia and Marquez (2006) develop a complementary theory in which banks filter between two types of borrowers: "known" (bank knows the creditworthiness of the borrower) and "unknown" (the bank does not know the creditworthiness of the borrower). For the latter set of firms, the bank must decide if it is worth investing effort in filtering unknown borrowers into good types that are worthy of credit and bad types that may have been rejected by other lenders. Dell'Ariccia and Marquez (2006) show that when investment opportunities increase within an industry, the proportion of unknown borrowers in the economy increases. However, the likelihood that a particular unknown borrower is a rejected applicant from an alternative lender is lower because during good times, on average, firms have a higher probability of having positive NPV projects. In turn, conducting borrower-specific credit analysis is less beneficial for banks. This reduction in adverse selection during economic expansions can lead to deteriorated lending portfolios and higher subsequent loan losses in the event of an economic downturn.[4]

Collectively, these theories broadly suggest that when a sector receives a positive economic shock, the net benefit to banks of screening borrowers is lower. In expectation, firm-level information, such as that provided in audited financial reports, simply confirms what broader

[4] The essence of the Dell'Ariccia and Marquez (2006) model is captured by the following quote: "When the proportion of unknown projects in the economy increases, as may happen after a deregulation or during the expansionary phase of a business cycle, such adverse selection problems become less severe, reducing banks' lending standards. This in turn results in lower bank profitability, higher aggregate credit, and higher vulnerability to macroeconomic shocks" (pg. 2534, see also Section IV.A.).

economic data already suggest to banks—that the borrower is creditworthy. In the presence of bank competition, when costly information collection efforts do not alter a lender's priors (in expectation), then the lender will reduce these efforts. Thus, theory suggests that banks make an ex ante rational tradeoff between lower screening costs during economic expansions with higher expected levels of portfolio loan losses in the event of negative shock.

While theories typically model banks as screening borrowers, we caution that our setting does not permit us to track specific borrowers over time, or separate the statements collected by banks at the application stage from the post-origination stage. As a result, our analyses cannot distinguish between screening activities for new borrowers versus monitoring activities for existing ones. Although the theories we draw upon directly model screening, related research considers screening and monitoring as closely related activities that respond similarly to changes in economic conditions.[5]

2.2. Construction industry setting

To test the prediction that financial statement verification is related to economic growth, we examine differences between construction and non-construction firms between 2002 and 2011. This setting is powerful in that commercial borrowers in the construction industry experienced significant increases and subsequent decreases in economic growth during this period. The housing boom of the early-to-mid 2000s elevated the construction industry to an unprecedented role in the US economy, peaking at 6.7% (5.6%) of private sector employment (GDP) in 2006 and ranking slightly behind health care and professional services in terms of overall size (US Bu-

[5] For example, Hellwig (1991) states, "'Monitoring' ought to be understood in a broad sense as any form of collecting information about a firm, its investment prospects and its behavior. The information that is collected is useful because it serves to sort out 'bad' projects and/or punish 'bad' behavior" (pg. 46).

reau of Economic Analysis 2014).[6]

Construction activity contracted sharply starting in 2008, resulting in the industry shrinking by one-quarter to 5.1% (4.0%) of private sector employment (GDP) by 2011. This reduction—a net loss of 2.1 million jobs—represented 36% of the total decline in net US employment during the same period. Moreover, construction loan losses were massive, exceeding $100 billion from 2007 to 2011 (FDIC).[7] In short, during 2002-2011, the construction industry attracted, and then lost, significant amounts of financial and human capital, while leaving a bloated inventory of housing (Rajan 2010).

2.3. The role of auditing

Prior research demonstrates that audited financial statements play a meaningful role in a bank's lending process to mitigate informational risks.[8] Independent auditors ensure that the borrower's transactions and risk exposures are properly represented in the firm's financial statements according to Generally Accepted Accounting Principles (GAAP). In doing so, they provide the lender with informative signals of the borrower's credit risk and the expected proceeds in the event of default. The verification role of audits allows the parties to contract on financial information through covenants and track compliance on an ongoing basis. In the construction industry, audits help builders obtain surety bonds, third-party guarantees that the work will be performed as specified by contract. Audits can also discipline the activities of the firm by exposing and constraining agency problems and strengthening risk management and internal controls (Watts and Zimmerman 1983). In addition, audited financial statements provide information in

[6] We classify firms in NAICS industry 23 as belonging to the construction industry. Firms in this group engage in construction of buildings, heavy and civil engineering construction, and specialty trade contracting.

[7] Specifically, the sum of items RIADC891 (1-4 family residential construction loans, chargeoffs) and RIADC893 (Other construction loans and all land development and other land loans, chargeoffs) totaled $109 billion across all Call Reports filed with the FDIC from US-based, non-foreign owned, corporate banks from the first quarter 2007 until the fourth quarter of 2011.

[8] See Armstrong et al. (2010) for an overview and citations of this large literature. Also see DeFond and Zhang (2014) for a broader discussion of the archival audit research.

the form of footnotes, which—with the exception of reviews—other report types are not required to include. Moreover, given their relatively long operating cycle, construction firms have significant accrual activity that can be (intentionally or unintentionally) misrepresented in unaudited financial statements.

Pertinent to our study, construction activity is primarily driven by small and medium private firms that are not required to undergo financial statement audits. Employment in establishments with fewer than 100 (500) employees makes up 67.9% (84.3%) of the total construction industry (US Census Bureau Business Register 2010); in contrast, the level of employment contributed by public firms, which are required to obtain audits, peaked in 2005 at only 5.5% of total construction employment.[9] In private US firms, economic forces of supply and demand influence whether financial statements are produced, and if so, the level of verification assured by an independent auditor.

While banks prefer lending to firms with audited financial statements, all else equal, audits are costly and consume considerable management time.[10] Hence, audit and reporting requirements are a meaningful part of the bundle of contractual and price terms that banks offer in competing for potential borrowers (Minnis 2011; Cassar et al. 2015). Because theory suggests that banks reduce lending standards to firms in industries experiencing positive economic shocks, we predict that banks reduce their collection of unqualified reports from construction borrowers relative to non-construction borrowers during the 2002-2007 housing boom, and that this pattern reverses in the ensuing crisis.

[9] In 2005, there were 7.6 million people employed in the construction industry. The 74 construction firms in Compustat cumulatively employed 418,000 individuals, or 5.5% of the total.

[10] Audit fee data are not available for the private firms in our study; however, as a broad point of reference, the median publicly held construction firm (conditional on having less than $1 billion in revenue to be as consistent as possible with the firms in this study while maintaining adequate sample size) paid 0.26% of revenue for an audit during the time period under study, which is more than 20% of the median construction firm's profit margin. Of course, differences in firm size and public versus private ownership make these estimates difficult to generalize to the construction firms in our sample. See additional discussion about audit fees in Section 4.3.3.

3. Data

For our main analyses, we use data from the Risk Management Association's (RMA) Annual Statement Studies. RMA, a non-profit association of financial institutions, compiles the number and type of financial statements its member banks collect from commercial borrowers and loan applicants. The Annual Statement Studies provide summary financial statistics (e.g., income statement and balance sheet ratios) of these borrowers and applicants by year, industry, US region, and firm size for lenders to reference when evaluating borrowers. While participation is voluntary, hundreds of financial institutions participate, ranging from small community banks to nine of the ten largest banks by assets.[11] Thus, the dataset represents a broad cross section of banks and a sizable portion of the US commercial lending market. Industry is reported at the six-digit NAICS level; the regions roughly correspond to the six Federal Reserve regions as partitioned by the Office of the Inspector General (see Figure 2); and firms are partitioned into six borrower size categories based on revenues: <$1 million, $1-3 million, $3-5 million, $5-10 million, $10-25 million, and >$25 million. Because RMA does not provide borrower-level data, the primary unit of observation is the year-industry-region-size level.

Table 1 reports that RMA compiled nearly 1.8 million financial reports collected from financial institutions from 2002 to 2011.[12] After eliminating financial and real estate firms and units of observation with very few firms, we use data from over 1.5 million firm-year observations that are allocated across 4,881 year-industry-firm size observations and 24,551 year-

[11] For example, major commercial lenders including Bank of America, JPMorgan Chase, PNC Bank, SunTrust Banks, Wells Fargo, and Zions Corporation appear every year, and these six banks alone are responsible for 22.7% of US commercial and industrial lending by banks during the sample period. See the accompanying online appendix to this paper and Berger et al. (2016) for additional description of the RMA dataset and frequency of bank participation.

[12] We begin our sample in 2002 because this is the first year that RMA compiled the data electronically by bank, allowing us to merge the RMA data to bank level Call Report data, as described in Section 5.

industry-region-size category observations.[13] Within each unit of observation, RMA classifies the number of financial reports it collects into five mutually exclusive and collectively exhaustive categories: unqualified audit, review, compilation, tax return, and other. We discuss each of these reports in detail in Appendix A. Our main variable of interest is the proportion of the reports collected by banks that are unqualified audits—the only report type with the combination of the highest level of independent attestation, most complete disclosure of information, and lack of departures from GAAP.

Table 2 reports the number (Panel A) and proportion (Panel B) of financial reports across the three main categories—unqualified audits; reviews and compilations; and tax and other—by year for firms in the $3-to-$25 million revenue range. We focus on this size range because the unconditional likelihood of getting an unqualified audit is very low (high) for the smallest (largest) firms outside this range, though we conduct tests for all size ranges. Panel B reports that the proportion of unqualified audited financial statements decreases for non-construction firms, falling from 19.5% in 2002 to 16.2% in 2007. However, for construction firms, the decline over the same period is more than twice as sharp, from 18.6% to 10.1%. Meanwhile, the collection of tax and other statements—the reports with the least verification and information—increased at a significantly greater rate for construction relative to non-construction firms.[14] In contrast, after 2007 the incidence of audits (tax and other reports) rose slightly (fell) among construction firms rela-

[13] We eliminate financial firms because they can face various regulatory mandates for financial reporting, and real estate firms primarily because of concerns that real estate speculation would influence the results in favor of our prediction. If we include real estate industry firms with construction industry firms, our inferences are unchanged. To reduce the number of units of observation with fewer than five firms, we collapse six-digit NAICS industries into three-digit NAICS industries.

[14] While unqualified audit opinions are generally regarded as the highest level of verification, ordering the remaining four categories in terms of verification is imprecise. We group tax and other reports together because these report types explicitly do not include independent accountant verification. In addition, unlike unqualified audits, tax returns do not include a statement of cash flows or footnotes. However, discerning whether compilations provide "more verified" information than tax returns, for example, is beyond the scope of this paper. One caveat, though, is that the "other" category includes qualified audit opinions, which we are unable to disentangle from company generated reports. Our robustness test using Sageworks data (discussed below) mitigates concerns that our inferences are materially affected by this issue.

tive to non-construction firms, consistent with a reversal in the pre-crisis trend.[15]

The difference-in-difference column of Table 2 highlights the main result of this paper: banks' collection of unqualified audited financial statements from construction firms declines during the period of economic growth for the construction industry, hitting a trough in 2007, and then reversing upon the onset of the crash in construction. Figure 3 illustrates the main result. We plot the cumulative difference-in-difference in banks' financial statement collection for construction firms relative to non-construction firms alongside plots of the economic activity for construction firms. The figure reveals that as construction activity grew during the credit boom, the proportion of unqualified audits (tax returns and other) decreased (increased) for the construction industry relative to non-construction industries. The next section more thoroughly examines this result.[16]

4. Analysis of bank collected financial reports

4.1. Changes in the frequency of financial statement verification

To examine whether banks change their verification standards with changes in economic growth, we begin our analysis by estimating the following difference-in-difference model using weighted least squares (subscripts for industry *i*, borrower size category *j*, and year *t* are suppressed for brevity) in which each year-industry-size observation is weighted by the number of firms in the observation:

[15] Two additional observations are worth noting from Table 2. First, the decline in the proportion of unqualified audits collected from construction borrowers is not only driven by the denominator. The absolute number of unqualified audits also generally declines over the years 2002 to 2007, suggesting that the result is not simply driven by new, unaudited firms entering the sample, but rather firms dropping audits as well. Second, Table 2 suggests that a larger portion of the reduction in unqualified audits for construction firms (relative to other firms) occurs in years leading up to 2006. This result is consistent with the fact that the construction industry grew more rapidly (in terms of GDP and employment) relative to other industries in the years 2003 to 2005; thus, while the construction peak occurred in 2007, more significant *growth* occurred earlier.

[16] To more closely investigate changes in lending market conditions following the onset of the crisis, we examine origination volumes post-2007. Consistent with Figures 1 and 3, we find that Acquisition, Development, and Construction lending peaked in late 2007, then declined each quarter through the end of 2010. Importantly, lending activity did not tighten rapidly in 2008 with a gradual improvement through 2010, but instead continued to decline over the period 2008 to 2011.

$$PercentUnqualified = \beta_1 Year2007 + \beta_2 Year2007 \times Construction + \eta^{size} + \lambda^{industry} + \varepsilon . \quad (1)$$

The dependent variable *PercentUnqualified* is the percentage of financial statements with unqualified audits in the year-industry-size unit of observation and is our proxy for banks' verification standards. *Construction* is an indicator for construction industry borrowers (NAICS code 23), with the main effect included in the industry fixed effects ($\lambda^{industry}$) which account for the distinct economic characteristics in each industry (e.g., litigation risk or availability of collateral) that lead to different levels of verification. The specification in equation (1) only uses years 2002 (beginning of the sample period) and 2007 (end of the economic growth for construction); thus, *Year2007* is an indicator which equals 1 for observations in the year 2007 and 0 for observations in the year 2002. We also include borrower size fixed effects and cluster standard errors at the three-digit NAICS industry level.[17] The coefficient on the interaction term *Year2007*×*Construction* is the difference-in-difference estimator that measures the differential change in the collection rate of unqualified audited financial statements (i.e., bank verification) from 2002 to 2007 from construction firms relative to non-construction firms (i.e., differential economic growth).

Our empirical design addresses two potential limitations of analyzing trends in financial verification. First, regulators have asserted that there has been a secular downward trend in the use of auditing (FAF 2011), such that identifying a trend in construction may not be a result of our prediction, but rather a more general, long run shift in the supply and demand of auditing. Second, because the RMA data are repeated cross-sections in which member banks (and their borrowers) may change year-to-year because of bank participation decisions, we could observe

[17] We use indicators for firm size that correspond to two of the revenue categories reported by RMA: \$5-\$10 million and \$10-\$25 million. The holdout size group is the \$3-\$5 million size group. In an alternative specification, we calculate the average firm size within each unit of observation (RMA reports the total assets and total sales for each unit of observation) and use this continuous variable rather than indicator variables. Our results (untabulated) are nearly identical.

trends due to sample composition issues rather than population dynamics.[18] Our difference-in-difference design mitigates both concerns. Because we control for firm size and industry, we only require the assumption that sample composition and population secular trend issues affect construction and non-construction firms similarly. Thus, our identifying assumption is that non-construction firms serve as an appropriate counterfactual for construction firms in that these firms are similarly affected by all issues (e.g., supply side shocks, sample selection concerns, and secular trends in audits) *except for* changes in economic growth, which is our treatment of interest.

Table 3 presents our regression results examining unqualified audit rates for firms with revenues between $3 million and $25 million. First note that the coefficient on *Year2007* in Column 1 is significantly negative, indicating that banks had a lower propensity to collect unqualified audited financial statements from medium-sized borrowers in all industries in 2007 relative to 2002, consistent with anecdotal claims of a secular decrease in the use of auditing in privately held firms (FAF 2011). More importantly, the difference-in-difference coefficient is a significant -3.8%, which indicates that banks reduced their collection of unqualified audited reports more for construction firms than non-construction firms during this time period. Given a base rate of audit collection from construction firms of 18.6% in 2002, this reduction is economically significant and almost twice the rate of non-construction firms over this period.

In Column 2 of Table 3 we consider each of the credit boom years (2002 to 2007) by replacing *Year2007* with *Trend*, a count variable from 2002 to 2007, in the following regression:

$$PercentUnqualified = \beta_1 Trend + \beta_2 Trend \times Construction + \eta^{size} + \lambda^{industry} + \varepsilon \ . \qquad (2)$$

The coefficient on $Trend \times Construction$ estimates the average annual difference-in-difference.

[18] Bank participation is relatively persistent in the data set, however, so this is likely not a first order issue. See Berger et al. (2016).

We find that the difference-in-difference estimate in column 2 is -0.82%, which, as expected, is approximately one-fifth the magnitude of *Year2007×Construction* in Column 1, and similar to the annual difference-in-difference estimates from descriptive statistics in Table 2. To examine which financial statements displace unqualified audits during the credit boom, we replace the dependent variable in Column 3 of Table 3 to be the proportion of statements collected that are tax returns or other. Unlike for unqualified audits, there is a positive and significant trend in gathering tax returns and other statements, which is particularly pronounced for construction firms (the coefficient on *Trend×Construction* is significantly positive in Column 3).[19]

Next, we examine the crisis years in which construction activity substantially declined to confirm the link between economic growth and financial statement verification. If our results for 2002-2007 are not simply identifying a secular reduction in the usefulness of unqualified audits for construction firms, then we should observe that the difference-in-difference estimates reverse during the years 2008-2011. Indeed, Columns 4 and 5 of Table 3 report that bank collection of unqualified audited financial statements from construction firms relative to non-construction firms increased when the construction industry growth became negative—the coefficients on both *Year2011×Construction* and *Trend×Construction* are significantly positive.[20] Thus, the evidence from Table 3 confirms both the inferences from Figure 3 and our predictions: bank collection of verified financial information has a negative covariation with economic growth.

To examine the role of firm size and to present a complete set of results, we re-estimate Eq. (2) separately within all of the RMA revenue size categories: <$1 million; $1 to $3 million;

[19] Recall that the "hold out" financial report types are reviews and compilations, which have middle levels of outside accountant involvement, so the results in this section are not the mechanical mirror image of the audit results.

[20] In Table 3 we estimate the regression separately for the boom (2002 to 2007) and crisis (2008 to 2011) periods to avoid presenting triple interaction terms; however, our inferences are the same when we use a triple interaction specification and pool all years in one regression. We emphasize that our difference-in-difference tests are meant to detect deviations in audit rates for construction relative to non-construction firms, rather than make statements about the absolute level of audit rates (or the sign of the trend). Specifically, audit rates of construction firms during 2008 to 2011 stayed constant as audit rates for non-construction firms declined.

$3 to $5 million; $5 to $10 million; $10 to $25 million, and >$25 million.[21] For brevity we do not tabulate all the results, but Figure 4 plots the coefficient estimates of the interaction term (*Trend*×*Construction*) for each of these groups during the pre-2008 period. The results reveal that much of the variation occurs in the middle firm sizes. This is a reasonable result because the smallest firms (with revenues <$3 million) have very low audit rates unconditionally, while larger firms (with revenues >$25 million) have more exposure to issues such as dispersed ownership and larger debt levels (Lisowsky and Minnis 2015). Middle-sized firms have substantial variation in auditing, but closer ownership and lower loan amounts. A particularly useful finding from Figure 4 is that, while the results are significant in each of the three middle size categories ($3 to $5 million, $5 to $10 million, and $10 to $25 million), the most economically significant effect is in the $10 to $25 million category. This finding suggests that our main results are not simply a function of new startup construction firms—which typically would not have their financial statements audited regardless of the economic state—entering the sample and mechanically decreasing the *PercentUnqualified* variable.[22] We conduct additional tests below to further consider and refute this sample composition explanation.

While the construction industry has several features which make it attractive as the focal point of our analyses, we also investigate whether our inferences are generalizable beyond this industry. To do so, we collect industry-level GDP data from the Bureau of Economic Analysis and measure economic growth for each three-digit industry as $(GDP_t - GDP_{t-1})/GDP_{t-1}$.[23] As

[21] We also estimate our model in the pooled sample after adding an interaction term between the 2007 indicator and firm size. Our inferences are unchanged. We present our tests by size category here to allow the interaction coefficient (*Construction*×*Trend*) to vary across firm size without a linear restriction (i.e., we are not forcing the relation to be fixed for each size category).

[22] To be clear, as described in Section 2 the theoretical framework suggests that new firm entrants during periods of growth within a sector are part of the explanation for the reduction in verification standards. New firm entrants reduce information asymmetry concerns between banks related to all borrowers. Our tests here simply reflect that the new entrants are not mechanically decreasing the *PercentUnqualified* variable.

[23] For instances where GDP figures are not available at the three-digit level, we measure GDP at the two-digit level.

shown in Table 4, we then regress the change in *PercentUnqualified*$_t$ on *Changes in GDP*$_t$ at the industry level and include industry fixed effects (column 1) and industry and year fixed effects (column 2). Consistent with our main analyses examining the construction industry, we find that the change in *PercentUnqualified* for a given industry decreases as GDP growth increases.

4.2. Regional based tests

We next exploit cross-sectional variation in economic activity across US regions. Academic research (e.g., Mian et al. 2013) and governmental reports (e.g., OIG 2012) have documented that housing loan growth and subsequent losses were most prominent in the West and Southeast regions (i.e., states in the San Francisco and Atlanta Federal Reserve regions, see Figure 2). If economic shocks alter bank use of verified financial information, then we should find that our results are more pronounced in regions with the most significant upturn in construction activity.

In Table 5, we create an indicator variable *HotRegion* equal to one for observations in the West and Southeast regions of the US; zero otherwise. Our specification includes all main and two-way effects, but we only report the double and triple interaction terms with *Construction* for brevity. Columns 1 and 2 show that, relative to non-construction firms, the collection of unqualified audits decreased for construction firms in the non-Hot regions (the coefficients on *Year2007*×*Construction* and *Trend*×*Construction* are significantly negative, though the economic magnitude is smaller than the estimate in Table 3). However, construction firms in regions with more economic growth in construction experienced a much steeper reduction in unqualified audited financial statement collection (the coefficients on the triple interaction terms of *Year2007*×*HotRegion*×*Construction* and *Trend*×*HotRegion*×*Construction* are significantly negative). Columns 4 and 5 reveal that the crisis period reversal is modestly (and insignificantly)

positive for the *HotRegion* observations.

Columns 3 and 6 indicate that reporting practices for 'tax' and 'other' statements again follow an opposite path—one that reinforces the results in Table 3 for these lower verification reports. The coefficient on the triple interaction term in Column 3 (6) of Table 5 shows that construction borrowers in the West and Southeast were significantly more (less) likely to provide tax and other statements to their bank prior to (after) 2008, at the same time banks were reducing (holding steady) collection of unqualified audited reports. Overall, the region-level results bolster the case that financial statement verification is an important component of lending standards that evolve with economic conditions. Moreover, these within-construction industry results mitigate concerns that the results from Table 3 are generated by unobservable national-level supply side factors specific to the construction industry, or that assertions of a secular decline in auditing is somehow more acute in the construction industry.

We take two steps to further ensure that our findings are not driven by unobservable bank characteristics (such as expertise—see Berger et al. 2016) or bank sample composition changes. First, we include bank fixed effects in our main specifications of Tables 3 and 5. Second, we use only the subsample of banks with construction lending activity in both "hot" and "not hot" regions such that we are identifying only the variation within bank across regions. As reported in Tables A1 and A2 of the online appendix, these alternative specifications do not alter our inferences, suggesting that neither unobservable bank characteristics nor a change in the mix of sample banks is driving the results. Rather, banks altered their underwriting and monitoring standards as it pertains to financial verification across regions and industries.[24]

[24] Berger et al. (2016) find that "novice" banks (with no construction lending in 2002) in fact had higher rates of audit collection from construction firms than "experienced" banks (with construction lending throughout the period). This evidence suggests that banks did not lower their financial statement verification policies to aggressively enter

4.3. Ruling out alternative explanations

4.3.1. Collateral

One potential concern with our inferences presented thus far is that the reduction in unqualified audit rates was driven by construction industry participants purchasing land as a speculative long-term investment amid considerable real estate price increases. As a result of the land purchases, banks demanded fewer audited financial statements because they used the land as collateral and relied on alternative information such as appraisal reports. To mitigate concerns over this collateral channel driving our results, we conduct our analyses within the construction industry using only those firms least likely to engage in land purchases. Specifically, we eliminate firms in single-family (NAICS 236115) and multi-family (236116) housing construction, new housing for-sale building (236117), residential remodeling (236118), industrial building construction (236210), commercial and institutional building construction (236220), land subdivision (237310), and heavy and civil engineering construction (237990).[25] The remaining construction firms are exposed to the cash flow channel related to the housing boom, but are not exposed (or at least significantly less exposed) to the collateral channel. Using only firms exposed to the cash flow channel does not affect any of our inferences. We tabulate these results in Table A3 of the online appendix.[26]

4.3.2. Borrower composition changes

---

the construction market, but instead suggests that new banks recognized they were less well-informed than incumbent banks and, as a result, implemented higher audit standards. This result holds even stronger in "hot" regions.

[25] To provide support for our categorization of those sub-industries which we eliminate, we use our firm-level tax data (described in more detail later). We note two pieces of descriptive evidence. First, firms are required to report the book value of land on the Schedule L balance sheet. We find that land holdings as reported on Schedule L are concentrated in firms in the industries we eliminate, and firms outside those industries report virtually no land as assets. This observation confirms that land speculation was not a significant issue for these firms. Second, the names of firms in eliminated industries contain such words as "development", "developers", "investors", "community"—i.e., words that signify potential land investment. In contrast, the names of firms in the remaining industries include "carpentry", "contractor", "plumbing", etc.—i.e., those firms exposed primarily to cash flow shocks.

[26] Focusing on firms exposed only to the cash flow channel (i.e., carpenters, plumbers, and electricians) also rules out the possibility that builders pre-selling units at a higher rate during the boom allows less monitoring by banks due to lower lending risks.

One disadvantage of the RMA data is that they are aggregated at the industry and borrower size-level, preventing an analysis of borrower-specific behavior or sample composition changes. Thus, one concern could be that our findings are the result of a changing mix of firms within the construction industry (relative to non-construction firms) when economic growth changes and that this altered mix of firms has lower levels of financial statement verification. To investigate this alternative explanation, we supplement our main analysis using an independently collected firm-level panel dataset compiled by Sageworks, Inc. Sageworks collects income statement and balance sheet data for privately held US firms and indicates the level of independent verification of those financial statements (i.e., an audit, review, or compilation). While the identity of the firms remains confidential, Sageworks assigns a unique identifier to each firm, generating a panel dataset. This dataset is not ideal for our main analysis because the data are primarily collected from accounting firms, not banks, creating a selection bias concern: firms are included in the sample conditional on using an external accounting firm. Firms that stop using an accounting firm would drop out of the sample, rather than be counted as an observation that changed its reporting status. Bearing the potential limitations of the data in mind, the structure of the data enables us to conduct analyses that control for firm-level characteristics and examine within firm verification decisions over the boom period.

Table 6, Panel A provides descriptive statistics for the 30,706 firm-years (15,742 unique firms) over the years 2002 to 2007 with valid values of leverage, profitability, and year-over-year sales growth and with sales between $3 million and $25 million. The 16% audit rate in the Sageworks sample is slightly higher than the RMA sample, as expected given that the sample is collected from accounting firms. About 26% of the Sageworks sample consists of firms in the construction industry, which is also slightly higher than the RMA sample.

We begin our firm-level analysis by replicating the results from Tables 3 and 5 to ensure that our main results hold after controlling for firm characteristics and are not dataset specific. In analyses tabulated in Table A4 of the online appendix, we find very similar results using the Sageworks data after including controls for firm size (sales), profitability (ROA), leverage, and growth (year-over-year sales growth). Specifically, audit propensity for construction firms decreased relative to other industries. The economic magnitude of the incremental reduction in audits for construction firms (i.e., the coefficient on *Trend*×*Construction*) is approximately 1.5% per year, slightly larger than the estimates using the RMA data. This effect is also larger in the 'hot' regions of the US, though with reduced statistical significance. These results corroborate our main analysis using an out of sample, firm-level dataset that permits us to control for firm-specific characteristics. Moreover, because the Sageworks data groups unqualified and qualified audit opinions together, these results also provide a check to ensure our inferences are robust to changes between these report types. In sum, construction firms substantially reduced their production of audited financial statements relative to non-construction firms during the construction boom years, even after conditioning on firm characteristics.

Next, we take advantage of the firm-level panel structure of the dataset to more explicitly examine the mix of firm types over time. We take an approach similar to the Fama and French (2001) dividend analysis. We first establish an audit likelihood model for each firm in the year 2003 and then predict the audit propensity for each firm in subsequent years using the parameters of this model.[27] The change in audit rates from 2003 to 2007 can then be partitioned into two explanations: changes in firm characteristics and changes in a firm's propensity to get an audit conditional on its characteristics. We estimate the following parsimonious audit prediction model for each firm *j* in year 2003, which includes industry indicators to account for differences in both

[27] We start in 2003 because the Sageworks sample size was substantially smaller in 2002 related to its startup.

characteristics and audit propensities across industry:

$$Audit_j = \beta_1 LN\_sales_j + \beta_2 ROA_j + \beta_3 Leverage_j + \lambda^{industry} + \varepsilon_j \ . \quad (3)$$

Table 6, Panel B presents the results of estimating the probit audit prediction model for firms in 2003. The coefficient on *LN_sales* (*ROA*) is significantly positive (negative) and the sign on *Leverage* is insignificantly negative. We use the estimated parameters of this model to predict which firms would have audits in each subsequent year. In Panel C, we report the results partitioned by construction and non-construction firms. Interestingly, in each year after 2003 the model predicts that audit rates would be slightly *increasing* for both construction and non-construction firms given the mix of firm characteristics in each of the years during the construction boom period. In other words, the mix of firm characteristics suggests that, if anything, the rate of auditing should have been increasing in the economy, not decreasing.

We then compare the predicted versus actual audit rates between construction and non-construction firms. The model performs significantly worse for construction firms, consistent with the hypothesis that the propensity for construction firms to receive audits, not the observable set of characteristics, explains the decline in financial verification during the housing boom. In fact, the difference-in-difference estimate in 2007 of -3.4 percentage points (t-stat of -4.40 accounting for dependence in the errors at the industry level) is nearly identical to the difference-in-difference estimates observed in the main analysis using the RMA data.[28]

In an alternative specification, we again follow Fama and French (2001) and use portfolios of firms based on firm size, ROA, and leverage to predict audit propensity over time. Firm size is defined as the three RMA sales size categories ($3-$5 million; $5-$10 million; $10-$25

[28] In results tabulated in Table A5 of the online appendix, we also include firm growth as an audit predictor (reducing the sample size in 2003 by about two-thirds because it requires data from 2002). Again, we find similar results—the model predicts that audit rates would have been slightly increasing based on the mix of firms, and the model does substantially worse for construction firms in predicting actual audit rates.

million), while ROA and leverage are each based on annual quintiles, resulting in 75 total annual portfolios of firms. Table 7 reports the number and percentage of construction firms in each portfolio that receive an audit for 2003 and 2007. Consistent with the Table 6 results, the overall sample rate of auditing decreased substantially from 17.6% to 11.7%. More interestingly, this decrease occurred in two-thirds of the portfolios in 2007 (almost evenly distributed across each of the three size categories), as shown by the shaded cells. If the mix of construction firms changed, but the audit propensity within portfolio remained constant, the predicted audit rate should have slightly *increased* to 18.3%, rather than decreased to 11.7%. Again, this result highlights that changes in audit propensity, not the mix of firm characteristics, are driving the results.

Finally, we examine firm-level decisions to terminate and initiate financial statement audits year-over-year. In Table A6 of the online appendix, we find that during the years 2003 to 2007, construction firms elected to terminate their audits from one year to the next (again controlling for size, profitability, leverage, and growth) at a rate 0.5 percentage points higher—or 30% more frequently than the sample base rate (t-stat of 2.53). At the same time, we find no difference in the decision to initiate audits between construction and non-construction firms. On net, this evidence reveals that firm-level audit decisions were an important contributing factor in the audit dynamics during the period of construction growth—construction firms actively dropped their financial statement audits at a much higher rate than non-construction firms.

4.3.3. Audit supply side explanation

In this section we consider whether our results are driven by differential inelasticity in the supply of auditors for construction firms relative to non-construction firms. Under this explanation, the positive economic shock to the construction industry increased the demand for financing by construction firms. To attract financing, more construction firms sought the services of inde-

pendent auditors to verify their financial accounts. In the short run, auditors are in limited supply and some friction exists such that this limited supply affects construction firms relative to non-construction firms more severely and, thus, auditors cannot provide all of the services demanded by construction firms. This alternative explanation suggests three related predictions: (1) the number of construction firms getting audits does not decline, but the proportion does (i.e., not a numerator effect); (2) audit services are concentrated in specialized service providers unable to respond with additional supply and that this concentration is particularly acute for the construction industry; and (3) audit fees increase as a result of the demand shock for auditing and it differentially affects construction audit pricing. We examine each of these predictions.

First, the supply-side inelasticity explanation suggests that construction demand for audits increased and that audit firms simply could not supply all services that were needed. Thus, at a minimum, the overall *level* of audit services would not be declining.[29] Table 2, which reports the number of firms receiving audits, suggests otherwise; during the housing boom, the number of firms receiving audits actually *decreases*.

Second, to examine the possibility that accounting providers servicing the construction industry were particularly concentrated, we use tax return data (described in detail in Section 5.2) that report the name of the external paid preparer of the tax return. Although a paid preparer is not necessarily equivalent to an auditor, there is overlap in this service (Klassen et al. 2016). Therefore, examining the supply of paid preparers can provide some insight into the supply of accounting professionals available to construction and non-construction firms. First, we find that only 1.6% of construction firms self-prepared their tax return in 2008, compared to 3.4% of non-

[29] This auditor supply side explanation suggests an outward shift in the demand curve with no shift of the supply curve, suggesting that the overall quantity of construction firm audits should weakly increase.

construction firms that self-prepared.[30] Contrary to a supply constraint, this result suggests that construction firms access a greater supply of accounting professionals than do non-construction firms. Next, we focus on 284 paid preparers that file 10 or more tax returns for their clients in 2008. These 284 preparers file 40% of construction firms' tax returns, compared to filing 56% of non-construction firms' tax returns. This result suggests that the supply of accounting professionals is less concentrated in construction compared to non-construction industries. Taking both results together, we find ample supply of accounting professionals serving construction relative to non-construction, i.e., construction firms have a higher rate of using paid preparers and they hire a greater multitude of preparers compared to other industries.

Third, if supply is inelastic to demand shocks, then prices increase in the presence of a demand shock. To examine this prediction, we use audit fee data from Audit Analytics, which collects audit fee data of public US firms.[31] Because our main argument and empirical tests are difference-in-difference tests, we are specifically concerned that audit supply was particularly inelastic for construction firms *relative* to all other firms—that is, we would be concerned if audit prices significantly increased for construction firms *relative* to non-construction firms, but would not necessarily be concerned about a broad secular increase in prices.

In results tabulated in Table A7 of the online appendix, we regress audit fees (both in log form and scaled by total revenues of the firm) on a trend variable, the trend variable interacted with a construction indicator, various firm level control variables (log sales, profitability, leverage, and sales growth), and industry fixed effects. We find that the coefficient on the trend varia-

[30] We use a sample of 22,655 tax returns in 2008 that serve as the basis for our tests reported in Table 8 column (2). See Section 5.2 for details on the tax return data source and sample selection.

[31] Privately held firms do not generally report audit pricing data in the US, so unfortunately we cannot test this prediction directly with privately held firms. Moreover, recent research suggests that audit pricing differs in levels between public and private firms (e.g., Badertscher et al. 2014). However, since our hypothesis is related to a difference-in-difference (i.e., the difference in the change over time between construction and non-construction firms), our identifying assumption for these tests is a common trend in audit fees between public and private firms.

ble is significantly positive (i.e., audit fees were generally increasing over the years 2002 to 2007); however, the coefficient on the interaction term between the trend and construction indicator is insignificantly negative (and in some specifications significantly negative) indicating that, if anything, audit fees for public construction firms *decreased* during the housing boom relative to public firms in other industries. While simultaneity is always a concern when prices are a dependent variable—and only public construction firm audit pricing data are available—these results suggest that a supply shock did not have a significant effect on audit prices for construction firms relative to non-construction firms. In sum, we find little support for audit supply side-related explanations.

4.3.4. Securitization

Both academic (e.g., Keys et al. 2010; Demyanyk and Van Hemert 2011; Dell'Ariccia et al. 2012) and regulatory (e.g., OIG 2012) studies document that securitization affected bank underwriting practices. We consider whether two alternative explanations related to securitization activity are responsible for our results. First, banks could sell or securitize the commercial and industrial loans they originate, providing them incentives to reduce construction firm financial statement verification standards. However, C&I loans are not frequently securitized. For example, Loutskina (2005, pg. 6) finds that "through the years C&I loans remain [the] least securitizable loan category." Moreover, a Small Business Administration study attributes the rather undeveloped secondary market for commercial loans to the heterogeneity in the loans and lending practices across banks (SBA 2003). Therefore, securitization of the construction firm C&I loans themselves should not be driving the results. [32]

[32] The securitization of C&I loans should not be confused with the securitization of commercial mortgages, which is more common. However, commercial mortgages are typically related to real estate developers and real estate firms, which we explicitly eliminate from our sample to avoid concerns related to real estate related loans. In addition, note that while securitization of C&I loans may not be common, this does not mean that banks hold all of a commercial

A second way in which securitization could play a role is indirectly through securitization of home mortgages. Under this explanation, banks could reduce their exposure related to real estate by securitizing their home mortgage portfolio, allowing them to bear more risk related to C&I lending to construction firms. In other words, securitization of home mortgages reduces the risk exposure to firms in the construction industry, thus banks reduce their lending standards to these construction firms. To address this potential concern, we measure the extent to which each bank followed an "originate-to-sell" practice in its home mortgage lending using variables from bank call reports. Starting in mid-2006, banks were required to report the dollar value of the home mortgages they sold without recourse each quarter. For each bank, we scale this mortgage sale activity by the ending dollar value of home mortgages held by the bank in its portfolio at the end of the quarter. We then average this variable over the four quarters ending September 2006 to June 2007.[33] We use this variable to conduct two robustness tests. First, we re-estimate our main tests in Table 3 at the bank level and include this new variable (and the log of total bank assets) to control for securitization activity. Second, we repeat the Table 3 tests after eliminating banks with significant home mortgage sale activity (defined as selling more than 50% of the mortgages held in the portfolio, on average). In both tests (untabulated), our inferences do not change and, in fact, our estimated coefficients are very similar to those in Table 3.[34]

5. Relation between audit collection and ex post loan losses

loan on their balance sheets, as syndication (in which several banks participate in a loan) occurs in C&I lending, especially for larger loans.

[33] Specifically, we divide the sum of variables RCONF070 and RCONF071 by sum of variables RCON5367 and RCON5368 for each quarter ending September 2006 to June 2007 from the call report database. We then average this variable over the four quarters for each bank and merge this variable with the RMA data and eliminate banks without a valid value for this mortgage securitization variable. This effectively creates a static measure of securitization activity for each bank, whereas ideally we would have a time-varying measure for each bank during the growth in construction activity during the sample period. Unfortunately, this data is not available.

[34] We also surveyed regulatory documents investigating bank lending activity related to construction and development loans leading up to the crisis and searched for any mention of securitization (e.g., OIG 2012). In contrast to studies examining the home mortgage market, we find no mention of home mortgage securitization as a factor in the commercial lending market.

5.1. Bank-level loan analysis

Having established that the proportion of highly verified financial reports collected by banks significantly dropped before the housing crisis, we now examine the implications related to subsequent loan losses during the bust. Theory suggests that the reduction in firm-specific screening allows bad type borrowers into the loan portfolios. To estimate the link between financial verification and future loan losses, we merge the RMA bank-level financial statement collection data with FDIC Call Reports, which include bank-level fundamentals and loan losses across several broad industries (including construction). We then regress subsequent loan losses on the extent to which the banks collected unqualified audited financial statements from their construction borrowers using the following OLS model:

$$\begin{aligned} Loan\,Losses_t = {} & \beta_1 \%Unqualified\,Construction_{t-1} \\ & + \beta_2 Construction\,Loan\,Growth_{t-1} + \beta_3 Bank\,Size_{t-1} \\ & + \beta_4 Bank\,Capital_{t-1} + \beta_5 HotRegionConstructionExposure\%_{t-1} \\ & + \beta_6 Avg\,Borrower\,Size_{t-1} + \varepsilon_t \end{aligned} \quad . \qquad (4)$$

Loan Losses$_t$ are the construction loan chargeoffs reported by the bank in year *t*, scaled by total construction loans outstanding at *t-1* (i.e., construction exposure). Our variable of interest is lagged *%UnqualifiedConstruction*, the percent of unqualified audited statements collected by bank *i* from its construction borrowers, per RMA. We control for several bank fundamental and portfolio composition variables, including loan growth, bank size, and bank capital. Importantly, we control for the extent to which each bank's portfolio is exposed to construction firms in the hot regions with the variable *HotRegionConstructionExposure%* (defined as the percent of the statements the bank has collected from construction firms that are from borrowers in a hot region). We do this because we have already shown that the audit rate decreased the most in the hot regions and we know (ex post) that banks experienced higher losses in the hot regions. If we

do not control for exposure to these regions, then we have an omitted variable which is related to both *Loan Losses* and *%UnqualifiedConstruction*. Finally, we control for the (log) size of the bank's typical construction borrower. We cluster standard errors at the bank level.

The reporting of construction loan losses and construction exposure began in the FDIC database only in 2007, and these fields are still missing for many banks. Combined with the fact that some banks in our sample do not participate in RMA on a recurring basis (precluding the availability of lagged variables), our sample size is limited. Though data and research design limitations prevent us from drawing causal inferences, these tests are designed to provide evidence about the relation between banks' lending standards and loan outcomes.

Panel A of Table 8 provides descriptive statistics for the RMA contributing banks, reported at the bank-year level. The intersection of the RMA and FDIC data represents a wide distribution of bank sizes, ranging from approximately $350 million at the first quartile to $3.5 billion at the third quartile. In addition, the dataset includes smaller community banks as well as several of the largest commercial lenders in the US in each of the tails. The average construction exposure and losses of the banks are 11.3% and 0.8% of the gross loan balances, respectively, and the average bank collects 29% of its construction statements from one of the hot regions.

Panel B of Table 8 presents our results of estimating Eq. (4). In Column 1, we find a significantly negative relation between the proportion of unqualified audited financial statements that a bank collects from its construction borrowers and subsequent loan losses. To assess the economic magnitude, ceteris paribus, a one standard deviation increase in unqualified audited financial statement collection is associated with a reduction of subsequent construction loan losses by 12% of the mean.[35] One way to place this economic magnitude into context is to compare

[35] Across banks, the standard deviation of audited statement collection for construction is 13.91%, while the mean ratio of construction loan losses to construction loans is 2.61%; hence (2.3%*13.91%)/2.61%=12.3%.

the estimated increase in loan losses with the estimated out-of-pocket cost of an audit. A typical construction firm in our sample with $25 million in revenues would have $5 million in debt. We estimate the annual audit fees for this firm would range from $65,000 to $137,000, whereas the estimated bank chargeoff reduction would be approximately $115,000.[36]

Columns 2-4 of Panel B provide robustness tests of the main result. In Column 2 we control for a bank's overall (rather than construction) loan growth. In Column 3 we scale construction loan losses by a bank's total gross commercial loan exposure, and then control for a bank's overall exposure to construction loans. Column 4 is similar to Column 3, except we use a bank's overall (rather than construction) loan growth only. Under each specification, the extent to which a bank collects unqualified audited financial statements is negatively associated with subsequent loan losses. However, one may be concerned that *%Unqualified* is simply identifying systematic underwriting deficiencies within a bank—that is, banks with poor underwriting both decreased their collection of audits from construction firms and subsequently suffered higher loan losses. As an attempt to address this concern, we include a variable that measures the bank's non-construction losses contemporaneously to the construction losses variable to proxy for the bank's overall underwriting practices. Consistent with an omitted variables concern, in Column 5 we find that non-construction losses are significantly related to construction losses; however, the coefficient on *%Unqualified* attenuates only slightly from the results in Column 4 and remains significantly negative. These findings support the joint hypothesis that reduced standards led to

[36] These estimates require multiple assumptions and should simply be used as a general point of reference. First, the $5 million in debt is derived using the average asset turnover ratio (sales to total assets) and leverage (debt to total assets) for a construction firm of approximately 1.0 and 20%, respectively, where these averages are derived from the Sageworks dataset. The lower bound audit fee estimate is based on assuming the average audit fees scaled by revenues for public construction firms of 0.26%, or $65,000 for a $25 million firm. The upper bound uses parameter estimates from an audit fee regression model using publicly held construction firms, with estimated discounts to allow for the privately held status. These calculations are available from the authors upon request. These results should be interpreted with caution because they do not consider other significant costs (e.g., management time in working with the auditors) or benefits (e.g., better internal control and information) associated with an audit.

weakened loan portfolios and that auditing is an important element of credit provision.

Not only does the loan losses analysis estimate the economic relation between reduced financial verification and subsequent losses, but it also rules out two additional alternative explanations. First, if lower unqualified audit rates are related to future losses, then it rules out that firms entering the RMA sample simply applied for credit but did not receive it. Second, if banks effectively substituted unqualified audits with other highly verified information or protection, such as collateral that we cannot observe within the RMA dataset, then we should not detect an association with subsequent losses.

5.2. Firm survival

The bank-level loan loss analysis shows that banks that collected more verified financial reports from their borrowers suffered lower subsequent loan losses during the construction bust. Despite controlling for the bank's overall loan losses as a proxy for broader underwriting issues, one might still be concerned that bank-level audit rates could proxy for a variety of other weaknesses in a bank's lending process, and that the lack of auditing is, in fact, unrelated to losses. In our next set of tests, we attempt to mitigate this concern by more directly linking financial statement audits to firm performance. To provide supporting evidence of a link between a construction firm's audit and its subsequent survival, we examine the population of firm tax returns over the years 2008 to 2010. In 2008, the IRS required firms with at least $10 million in assets to report whether they produce audited financial statements in accordance with Generally Accepted Accounting Principles, regardless of opinion (i.e., unqualified or qualified). We use this dataset to examine the relation between auditing in 2008 and firm survival through the financial crisis to 2010.[37]

[37] See Lisowsky and Minnis (2015) for a detailed description of the IRS dataset. Data on Schedule C sole proprietors are not available to the author with IRS data access because such activities are reported on individual taxpayers'

Table 9, Panel A reports two-way contingency tables sorted by whether a firm that exists in 2008 receives a financial statement audit in 2008 and whether that firm continues to exist in 2010 (i.e., survives the crisis). We report the contingency tables for all firms, construction firms only, and construction firms with revenues below $25 million.[38] We find that 71% of all firms existing in 2008 continue filing a tax return in 2010; however, firms with audits survived at a much higher rate: 76% compared to 67% for firms without audits. More interesting, though, the survival rate differential conditional on auditing is even higher for construction firms: 73% for audited construction firms versus 52% for unaudited construction firms. This differential is somewhat smaller focusing on construction firms with less than $25 million in revenues, but is still larger than for the overall population of firms.

We now test the survival probability differential more formally and control for a variety of firm characteristics using a linear probability model of survival with the following form:

$$Survive_{2010} = \beta_1 Audit_{2008} + \beta_2 Construction + \beta_3 Audit_{2008} \times Construction + \beta_k Controls + \varepsilon_{2010} \quad . \tag{5}$$

The dependent variable, $Survive_{2010}$, is an indicator equal to one if a firm filing its tax return in 2008 also filed a tax return in 2010, and zero otherwise.[39] $Audit_{2008}$ is an indicator equal to one if the firm reported undergoing an audit on their 2008 tax return, and *Construction* is an indicator equal to one if the filer is in the construction industry. We also include a broad set of controls

personal tax returns; the author only has access to business tax returns of Form 1120 (C corporations), Form 1120S (S corporations), and Form 1065 (partnerships and LLCs). Importantly, the audit variable for GAAP financial statements is not available before 2008. Also, in contrast to the RMA data, the IRS data do not indicate which type of financial statement was produced in the event that the firm did not receive an audit (e.g., a review or compilation).

[38] We use a sub-sample conditioning on revenues less than $25 million to be consistent with the size groupings in the RMA dataset; however, the IRS dataset still requires at least $10 million in assets so the IRS firm sample omits smaller firms.

[39] Survival is defined as those firms that filed a Schedule M-3 in 2008 and 2010 (71.2% of the sample). Non-survivors include those who only filed in 2008 (16.6%) or 2008 and 2009 (12.2%). One possibility is that firms stop filing M-3 because they no longer meet the $10 million asset threshold required for filing, so they would drop out of our sample and be incorrectly classified as a non-survivor. To mitigate this concern, we eliminate firms with less than $15 million in assets. Our results remain, as reported in Table A8 of the online appendix. Inferences are also unchanged using a logit specification.

from Lisowsky and Minnis (2015) for firm characteristics (the natural log of the number of owners and indicators for pass-through entities and corporations) and fundamentals (ratio of taxable income to total revenue, leverage, indicators for firms with losses and intangible assets, and the natural log of assets, total revenue, and accounts payable) to control for both selection into an audit and determinants of survival.

Panel B of Table 9 reports our results. Column 1 examines the subsample of construction firms and shows a significantly positive relation between having an audit in 2008 and surviving to 2010. Construction firms with an audit in 2008 have a 10.5% greater likelihood of survival than construction firms without an audit in 2008. Other control variables load as expected, with assets and revenue (loss firms and leverage) positively (negatively) related to survival. In Column 2 we use the entire sample of filers, add an indicator for construction firms, and interact it with $Audit_{2008}$. Although construction firms have a significantly lower propensity for enduring the 2008-2010 turmoil than non-construction firms as expected (the coefficient of -0.158 on *Construction* is significantly negative), the estimates show that receiving an audit is associated with a significant reduction of the marginal increased hazard probability for construction firms.

In Column 3, we focus again on construction firms, but this time those with less than $25 million of revenue to create a sample more consistent with the RMA sample of medium-sized firms. As in Column 1, we continue to find a positive relation between undergoing an audit in 2008 and surviving to 2010. Column 4 returns to the full sample of construction firms and includes an interaction between the audit variable and firm size. We find that the interaction term is negative, consistent with the decreasing relevance of an audit on firm survival as firm size increases. Smaller firms are where audits would have the stronger link to firm survival because of weaker internal control structures and sophistication in those firms (Minnis 2011), and these re-

sults are consistent with that hypothesis. This result also corroborates the analysis using the RMA data across various size partitions. Finally, Column 5 includes *Growth* (measured as percent sales growth from 2007 to 2008). The coefficient on $Audit_{2008}$ attenuates from Column 1, but remains significant.

It is important to emphasize that our results in Table 9 do not discern between a "screening/signaling" role and a "real effect" role of auditing. Borrower audits—as part of a bank's lending process—could either identify bad type borrowers and prevent them from entering a bank's portfolio in the first place (screening role), or improve bad type borrowers such that they present less risk (real effect role). Thus, the coefficient estimate on the $Audit_{2008}$ variable should not be interpreted as the causal effect of an audit on firm survival. However, our theoretical framework is agnostic between which mechanism is producing the result as long as audits are related to subsequent firm performance (and other omitted lending mechanisms are not the exclusive causal force of loan losses). Our hypothesis simply requires that a reduction in a bank's level of verification (i.e., use of audits) leads to poorer loan portfolio performance.[40]

6. Conclusion

We examine whether the use of financial statement verification in debt financing varies with economic growth and, in turn, whether changes in verification intensity are linked to loan portfolio quality and future borrower performance. We use the construction industry during the years 2002 to 2011 as a setting to investigate these questions. We find that as substantial credit flowed into construction leading up to the housing crisis of 2008, banks significantly reduced their collection of unqualified audited financial statements from construction borrowers (both in

[40] We supplement our firm survival regressions in Table 9 with tests that condition on firm survival and examine subsequent firm performance. In untabulated results, we find that surviving firms that were audited in 2008 increased their sales, profit margins, and return on assets more by 2010 than surviving unaudited firms. Thus, conditional on survival, audited firms performed better than unaudited firms.

absolute terms and relative to other industries), and that this reduction occurred most severely in the US regions with the highest construction growth and credit demand. After the onset of the crisis when economic growth became negative, these trends reversed.

We then find that banks that collected lower proportions of unqualified audited financial reports suffered higher construction loan losses during the housing crisis. Moreover, construction firms that had their financial statements audited were more likely to survive the crisis. Our results provide support for theories positing that banks reduce lending standards for firms in industries experiencing positive economic shocks, and that this behavior leads to worse bank and firm performance in the event of a crisis.

Our paper makes two contributions. First, we offer new evidence on how the use of financial statements in debt contracting varies with economic conditions. Motivated by theories on how lending standards co-vary with economic growth, we show that the housing boom led to a reduction in the use of audited financial statements by banks. Second, we extend prior work on the causes and consequences of the housing crisis, which to date has predominantly focused on the role of securitization. Our study is unique in allowing us to examine a setting where loans are typically retained by banks that actively collect information in their monitoring role. We show that, even absent a securitization motive, banks reduced their verification standards for financial reports during the boom, and this reduction is associated with loan losses and weaker performance during the bust. Moreover, we demonstrate *how* lending standards were lowered by offering direct evidence on changes in borrower audit rates.

References

Allee, K. and T. Yohn. 2009. The demand for financial statements in an unregulated environment: An examination of the production and use of financial statements by privately held small businesses. The Accounting Review 84 (1): 1-25.

American Institute of Certified Public Accountants (AICPA). 2013a. Construction contractors. May 2013. New York, NY: AICPA.

American Institute of Certified Public Accountants (AICPA). 2013b. Real Estate and Construction Industry Developments. 2013-2014 ed. New York, NY: AICPA.

Armstrong, C., Guay, W., and Weber, J. 2010. The role of information and financial reporting in corporate governance and debt contracting. Journal of Accounting and Economics 50: 179-234.

Ashcraft, A. and C. Steindel. 2008. Measuring the impact of securitization on imputed bank output. Unpublished manuscript accessed at: http://www.vanderbilt.edu/AEA/ AE-AStat/papers/Ashcraft_and_Steindel_1-2008.pdf

Badertscher, B., Jorgensen, B., Katz, S., and Kinney, W. 2014. Public equity and audit pricing in the United States. Journal of Accounting Research 52 (2): 303-339.

Becker, B. and V. Ivashina. 2016. Covenant-light contracts and creditor coordination. Available at SSRN: http://ssrn.com/abstract=2756926.

Ben-David, I. 2011. Financial constraints and inflated home prices during the real estate boom. American Economic Journal: Applied Economics: 55-87.

Berger, P. G., Minnis, M., & Sutherland, A. (2016). Commercial Lending Concentration and Bank Expertise: Evidence from Borrower Financial Statements. *Chicago Booth Research Paper*, (15-42).

Blackwell, D. W., Noland, T. R., and Winters, D. B. 1998. The value of auditor assurance: Evidence from loan pricing. Journal of Accounting Research 36(1): 57-70.

Cassar, G., C. Ittner, and K. Cavaluzzo. 2015. Alternative information sources and information asymmetry reduction: Evidence from small business debt. Journal of Accounting and Economics 59 (2-3): 242-263.

Christensen, H., Nikolaev, V., and Wittenberg-Moerman, R. 2015. Accounting information in financial contracting: An incomplete contract theory perspective. Working paper.

DeFond, M. and Zhang, J. 2014. A review of archival auditing research. Journal of Accounting and Economics: forthcoming.

Dell'Ariccia, G., Igan, D., and Laeven, L. 2012. Credit booms and lending standards: Evidence from the subprime mortgage market. Journal of Money, Credit and Banking 44 (2-3): 367-384.

Dell'Ariccia, G., and Marquez, R. 2006. Lending booms and lending standards. Journal of Finance 61 (5): 2511-2546.

Demyanyk, Y., and Van Hemert, O. 2011. Understanding the subprime mortgage crisis. The Review of Financial Studies 24 (6): 1848-1880.

Desai, M. A., Dyck, A., & Zingales, L. 2007. Theft and taxes. Journal of Financial Economics 84 (3): 591-623.

Fama, E. and French, K. 2001. Disappearing dividends: Changing firm characteristics or lower propensity to pay? Journal of Financial Economics 60: 3-43.

Federal Deposit Insurance Corporation (FDIC) Call Reports. Accessed via WRDS.

Financial Accounting Foundation (FAF) 2011. Blue ribbon panel on standards setting for private companies: Report to the board of trustees of the Financial Accounting Foundation, January.

Hellwig, M. (1991). Banking, financial intermediation and corporate finance. *European financial integration*, *35*, 63.

Keys, B., Mukherjee, T., Seru, A., and Vig, V. 2010. Did securitization lead to lax screening? Evidence from subprime loans. Quarterly Journal of Economics 125 (1): 307-362.

Klassen, K., Lisowsky, P., and Mescall, D. 2016. The role of auditors, non-auditors, and tax departments in corporate tax aggressiveness. The Accounting Review 91(1), 179-205.

Lambert, R., Leuz, C., and Verrecchia, R. E. (2007). Accounting information, disclosure, and the cost of capital. Journal of Accounting Research, 45(2), 385-420.

Lisowsky, P., and Minnis, M. 2015. Financial reporting choices of US private firms: Large-sample analysis of GAAP and audit use. Chicago Booth Research Paper (14-01).

Loutskina, E., and Strahan, P. E. 2011. Informed and uninformed investment in housing: The downside of diversification. The Review of Financial Studies 24 (5): 1447-1480.

Maddaloni, A., and Peydró, J. L. 2011. Bank risk-taking, securitization, supervision, and low interest rates: Evidence from the Euro-area and the US lending standards. The Review of Financial Studies 24 (6): 2121-2165.

Mian, A., K. Rao, and A. Sufi. 2013. Household balance sheets, consumption, and the economic slump. Quarterly Journal of Economics 128 (4): 1687-1726.

Mian, A., and Sufi, A. 2009. The consequences of mortgage credit expansion: Evidence from the US mortgage default crisis. Quarterly Journal of Economics 124 (4): 1449-1496.

Minnis, M. 2011. The value of verification in debt financing: Evidence from private U.S. firms. Journal of Accounting Research 49 (2): 457-506.

Minnis, M., and Sutherland, A. G. 2016. Financial statements as monitoring mechanisms: Evidence from small commercial loans. Journal of Accounting Research, *forthcoming*.

Office of the Comptroller of the Currency (OCC). 2014. Survey of credit underwriting practices. Washington, D.C., December.

Office of Inspector General (OIG). 2012. Acquisition, development, and construction loan concentration study. Office of Audits and Evaluations. Report No. EVAL-13-001. October.

Povel, P., Singh, R., and Winton, A. 2007. Booms, busts, and fraud. The Review of Financial Studies 20 (4): 1219-1254.

Rajan, R. 2010. Fault lines: How hidden fractures still threaten the world economy. Princeton University Press.

Ruckes, M. 2004. Bank competition and credit standards. The Review of Financial Studies 17 (4): 1073-1102.

US Bureau of Economic Analysis. Industry Economic Accounts. http://www.bea.gov/industry/gdpbyind_data.htm (accessed July 26, 2015).

US Census Bureau. Statistics of US Businesses. https://www.census.gov/econ/susb/historical_data.html (accessed June 7, 2016).

Watts, R. L., and Zimmerman, J. L. 1983. Agency problems, auditing, and the theory of the firm: Some Evidence. Journal of Law and Economics: 613-633.

Appendix A: Risk Management Association Data Description[41]

Overview

The Risk Management Association (RMA) "is a not-for-profit, member-driven professional association serving the financial services industry. Its sole purpose is to advance the use of sound risk management principles in the financial services industry." It has approximately 2,500 institutional members and 18,000 individual members, including "relationship managers, credit officers, risk managers, and other financial services professionals."

The RMA has been publishing the Annual Statement Studies© for nearly a century (first published in 1919). The purpose of these studies is to provide financial institutions (hereafter, banks) with benchmarking data to better understand the financial performance of commercial borrowers and prospects. Data for these studies are collected annually. Each year, the RMA begins its campaign to encourage members to participate. Participating banks typically have a deadline of June or July of each year to provide annual financial statements that the bank has collected from a borrower or prospect over the time period of April 1 of the previous year to March 31 of the current year. Historically banks have submitted their collection of financial statements manually (e.g., via mail and fax); however, the overwhelmingly predominant form of submission more recently is via electronic submission (for example, in 2014, 95% of the financial statements submitted by banks were done so electronically). Several software packages that banks use to analyze commercial loans have a compatible export feature, allowing banks to simply push the "submit" button to create an RMA submission file.

RMA collects financial reports from all industries, all borrower sizes, all loan grades or risk ratings, and all types of financial reports. However, observations will be rejected if any one of the following is not true (from the Submission Campaign Handbook):

- The fiscal year must fall within the current period—only 12-month fiscal statements falling within 4/1 to 3/31 are acceptable.
- The balance sheet must balance.
- The legal form of the entity must be noted.
- The type of financial statement must be noted.
- A valid NAICS or SIC code must be present. RMA accepts either an SIC code (four-digit) or a NAICS code (six-digit). Please note: We strongly encourage submission via 2012 NAICS.
- The income statement must be complete.

Importantly, reports are rejected if a valid industry and report type are not included. This mitigates concerns that industries or report types classified as "other" are simply picking up "missing" observations. RMA indicates that their credo is, "Contribute every statement you have," so they make a concerted effort to have each bank submit their entire portfolio of reports. The RMA also has controls in place to identify duplicate reports from the same borrower so the system will not allow more than one report from the same borrower within a bank. For the publicly available

[41] This section quotes frequently from RMA's homepage (www.rmahq.org) and the RMA 2015-2016 Annual Statement Studies© Submission Campaign handbook as accessed on August 25, 2015.

Annual Statement Studies, the RMA truncates firms with assets above $250 million. For purposes of our study, however, the RMA did not eliminate these observations prior to sending the data to us.

The statements that the RMA receives are not attributable to a particular borrower—i.e., it is anonymized. The dataset that the RMA provided to us is further aggregated to the bank-year-industry-region-borrower size level. For each unit of observation, the RMA tabulated for us the number of financial reports into one of five mutually exclusive, collectively exhaustive categories—unqualified audit, review, compilation, tax return, and other (see below for additional detail about these report types)—and the total sales ($) for all borrowers within the unit of observation.

Several important points and caveats regarding this dataset are worth noting:

- The data are not collected from a random sample of banks. Banks volunteer to participate. To the extent that this creates omitted variable selection bias in the data, we cannot control for this aspect. It is comforting to note that the results reported in the paper are robust to including only those banks that participate in each year and that those banks that choose to participate in the RMA sample are typically more important commercial lenders than those banks that do not participate—i.e., these are the more important banks for our study.
- There is no guarantee that the data represent the entire bank portfolios. The RMA only "encourages" banks to submit all financial reports. Moreover, banks do not collect any financial reports for a minority of their smallest borrowers (Minnis and Sutherland 2016). However, given the simple electronic submission process and the very high correlation between magnitude of RMA participation and bank commercial lending portfolios as tabulated in Call Reports, we believe that the RMA dataset is a very reasonable proxy for the banks' commercial lending portfolios.

Report Types

As noted above, RMA tabulates the number of financial reports collected by members into one of five different financial report types: unqualified audit, review, compilation, tax returns, and other. In this section, we will describe each of these report types.

Unqualified audit

A financial statement audit provides positive assurance that the financial statements are reported in accordance with Generally Accepted Accounting Principles. An unqualified audit opinion indicates that the auditor believes that the financial statements are materially in accordance with GAAP. Unqualified audited financial statements are accompanied by complete footnote disclosure, providing the most complete set of information of all of the reports along with the highest level of assurance and no detected material deviations from GAAP.

Review

Financial statement reviews provide negative assurance. An independent accountant performs analytical procedures (e.g., ratio analyses) and interviews management to assess whether the financial statements are misstated; however, the accountant does not perform substantive procedures to obtain positive evidence of an account balance. Reviews are generally accompanied by complete footnote disclosure; therefore, reviewed financial statements provide a simi-

lar information set to unqualified audits, but the information has a significantly lower level of assurance, reporting quality, and cost.

Compilation

A compilation provides no assurance about the financial statement balances reported in the financial statements. An accountant puts the firm's financial information in the form of financial statements but performs no procedures and provides no assurance as to the reporting quality. Compilations include all three standard financial statements, but are not required to report (and generally omit) footnote disclosures. Therefore, compilations provide substantially less assurance and information than either audits or reviews.

Tax Return

All firms are required to file a return with the Internal Revenue Service (IRS) annually. The nature of these returns differs by entity type (e.g., C Corporation, S Corporation, or Limited Liability Company) and entity size (e.g., firms with less than $250,000 in assets are not required to complete Schedule L which is a balance sheet). While all firms follow "tax basis" accounting to complete the form, the tax basis may differ based on firm size and various options that firms are able to elect (e.g., accrual versus cash basis; differing depreciation options, etc.). Therefore, even within the tax basis of accounting, the differing forms and various options result in heterogeneity. The focus of tax returns is the income statement, but firms exceeding $250,000 in assets also must provide a balance sheet. Important omissions from tax returns include both the statement of cash flows and financial footnotes. Moreover, while independent accountants are frequently involved in the production of these reports, they generally do not provide assurance about them. However, the IRS serves an implicit monitoring role, though the vast majority of returns are not audited on an annual basis by the IRS. Collectively, tax returns provide useful but limited financial information and have some, but weaker (and implicit) verification.

Other

The "other" category captures all reports that are not one of the above and per RMA mostly consist of two report types: company prepared financial statements and qualified audit reports. Company prepared financial reports are those prepared internally by management and provided to the bank without the involvement of an external accountant. Qualified audit reports are audits similar to "unqualified" audit reports described above but a qualification was made regarding some aspect of the financial statements. For example, the company prefers not to follow a particular accounting rule, so the independent accounting firm provides an "except for" opinion which states that the financial statements follow GAAP except for this aspect. Historically, the RMA reported qualified reports as a separate category, but because this category was infrequently used, RMA consolidated it with "other." Unfortunately, we are unable to disentangle qualified audits from other statements in this category, which is one caveat of our study, but given the assurance and information provided by qualified opinions, this omission works against our findings. Moreover, our results are consistent using the Sageworks dataset which does not differentiate between unqualified and qualified audit opinions.

Appendix B: Variable Definitions

Variables sourced from Risk Management Association Annual Statement Studies

Variable	Description
Percent Unqualified	The ratio of financial statements with unqualified audits to total financial statements collected by banks, measured at the year-industry-size category level.
Percent Tax, Other	The ratio of tax and other financial statements to total financial statements collected by the banks, measured at the year-industry-size category level.
Hot Region	An indicator equal to one if the financial statements were collected from the Southeast or West, and zero otherwise.
Construction	An indicator equal to one if the financial statements were collected from firms with a two-digit NAICS industry code of '23', and zero otherwise.
Hot Region Construction Exposure	The share of statements that a bank collected from construction firms that are from borrowers in a hot region.
Log Average Construction Borrower Size	The natural logarithm of the bank's average construction borrower's sales for the year.
Log Average Borrower Size	The natural logarithm of the bank's average borrower's sales for the year.

Variables sourced from banks' Report of Condition and Income (i.e., "Call Report")

Variable	Description
Construction Chargeoffs	The construction chargeoffs recorded by the bank during the period, calculated as (RIADC891+RIADC893+RIAD5409).
Non-Construction Chargeoffs	The chargeoffs recorded by the bank for all loans except construction related loans, calculated as the difference between total chargeoffs (RIADC079) and construction chargeoffs (defined above), divided by total gross loans and leases (RCFD1400).
Construction Loans	Total construction loans, calculated as (RCONF158+RCONF159).
Gross Loans	Total gross loans and leases, calculated using RCFD1400.
Construction Exposure	The ratio of construction loans to total gross loans and leases, calculated as (RCONF158+RCONF159)/RCFD1400.
Construction Loan Growth	The growth rate in construction loans over the past quarter, calculated as $(RCONF158_t+RCONF159_t)/(RCONF158_{t-1}+RCONF159_{t-1}) - 1$.

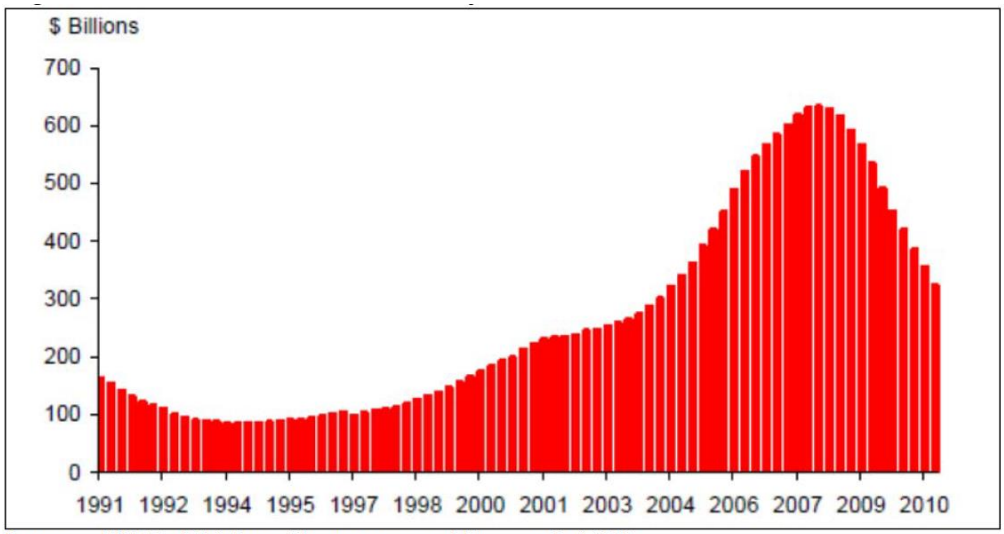
Loan Growth	The growth rate in total gross loans and leases over the past quarter, calculated as RCFD1400/RCFD1400(quarter-1)-1.
Size	The natural logarithm of the bank's total assets, calculated as ln(RCFD3210).
Capital	The ratio of tier 1 capital to risk weighted assets, calculated as RCON8274/RCFDA223.
Agricultural Exposure	The ratio of agricultural loans to total gross loans and leases, calculated as RCFD1590/RCFD1400.
Consumer Exposure	The ratio of loans to individuals to total gross loans and leases, calculated as RCFD1975/RCFD1400.

Variables sourced from IRS tax returns

Variable	Description
$Survive_{2010}$	An indicator equal to one if a firm that filed a tax return in 2008 also filed in 2010, and zero otherwise.
$Audit_{2008}$	An indicator equal to one if the firm underwent an audit in 2008, and zero otherwise.
Log Assets	The natural logarithm of one plus the firm's total assets. Total assets has been winsorized at the 1% level and is measured in millions.
Pass-Through Entity	An indicator equal to one if the firm filed Form 1120S or 1065, and zero otherwise.
Corporation	An indicator equal to one if the firm filed Form 1120 or 1120S, and zero otherwise.
Log Total Revenue	The natural logarithm of one plus the firm's total revenue. Total revenue has been winsorized at the 1% level and is measured in millions.
Taxable Income to Total Revenue	The ratio of taxable income (loss) before NOL to total revenue, bounded to [-1, 1].
Log Accounts Payable	The natural logarithm of one plus the firm's accounts payable. Accounts payable has been winsorized at the 1% level and is measured in millions.
Intangibles	An indicator equal to one if the firm's ending gross intangibles are greater than zero, and zero otherwise.
Loss	An indicator equal to one if the firm's Taxable Income to Total Revenue ratio is negative, and zero otherwise.
Log Number of Owners	The natural logarithm of one plus the number of shareholders. The number of owners has been winsorized at the 1% level.

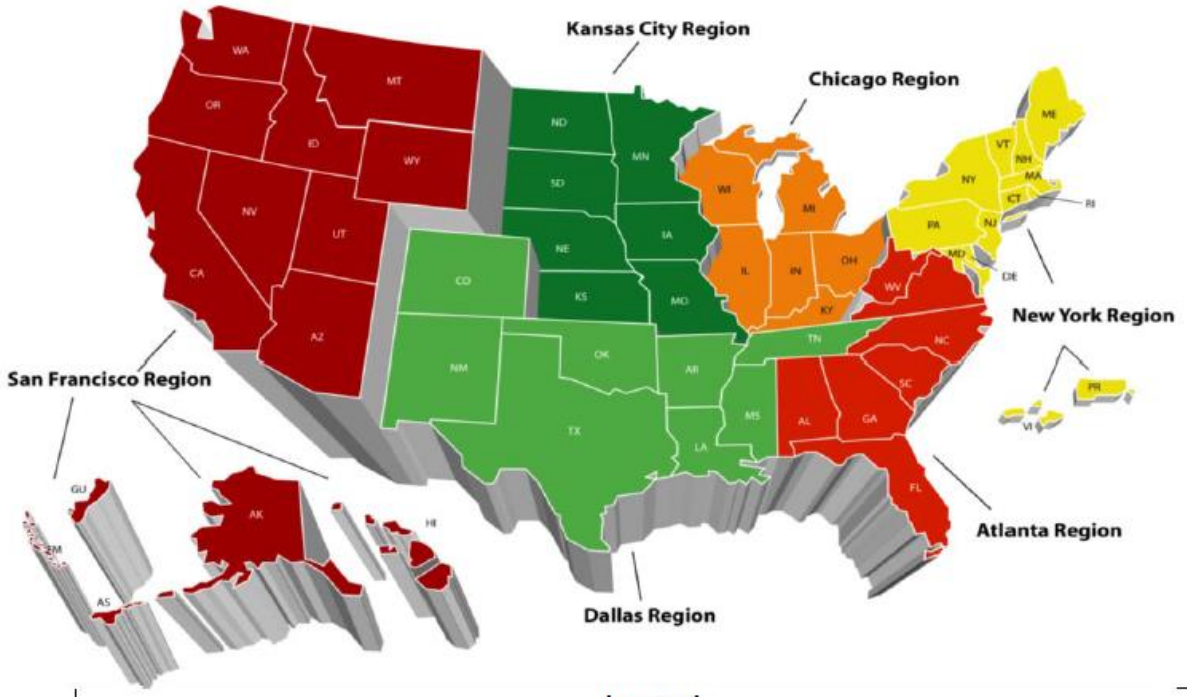
Leverage	The ratio of total debt to total assets. Leverage has been winsorized at the 1% level.

Figure 1: Acquisition, Development, and Construction (ADC) Loans at FDIC-Supervised Institutions, 1991 to 2010. This figure reports the amount ($ billions) of ADC loans outstanding each year. (Source: United States Office of Inspector General 2012, pg. 4)



Source: FDIC's Division of Insurance and Research (DIR).

Figure 2: Acquisition, Development, and Construction Loan Growth, Bank Failures, and Deposit Insurance Fund Losses by Region. (Source: United States Office of Inspector General 2012, pg. 6).



Legend	
San Francisco Region • 113.8% Growth • 84 Bank Failures • $30.8 Billion in DIF Losses	**Atlanta Region** • 95.3% Growth • 120 Bank Failures • $24.1 Billion in DIF Losses
Chicago Region • 66% Growth • 65 Bank Failures • $10.4 Billion in DIF Losses	**New York Region** • 73.8% Growth • 21 Bank Failures • $7.7 Billion in DIF Losses
Dallas Region • 95.3% Growth • 25 Bank Failures • $6.1 Billion in DIF Losses	**Kansas City Region** • 79.5% Growth • 36 Bank Failures • $2.5 Billion in DIF Losses

Source: Office of Inspector General (OIG) analysis of data from the FDIC's Loss History Reports and Quarterly Banking Profile.
Note: Bank failure and loss data are from January 2007 to March 2011.

Figure 3: Macroeconomic and Financial Statement Trends for the Construction Industry. This figure presents four data series for the construction industry over the years 2002 to 2011: Gross Domestic Product (GDP); Employment; % Unqualified (the proportion of financial reports that were audited for construction firms relative to non-construction firms); and, % Tax/Other (the proportion of financial reports that were tax returns or other financial statements for construction firms relative to non-construction firms). The financial report data was collected from the RMA's Annual Statement Studies. All data series are scaled relative to the base value in 2002.

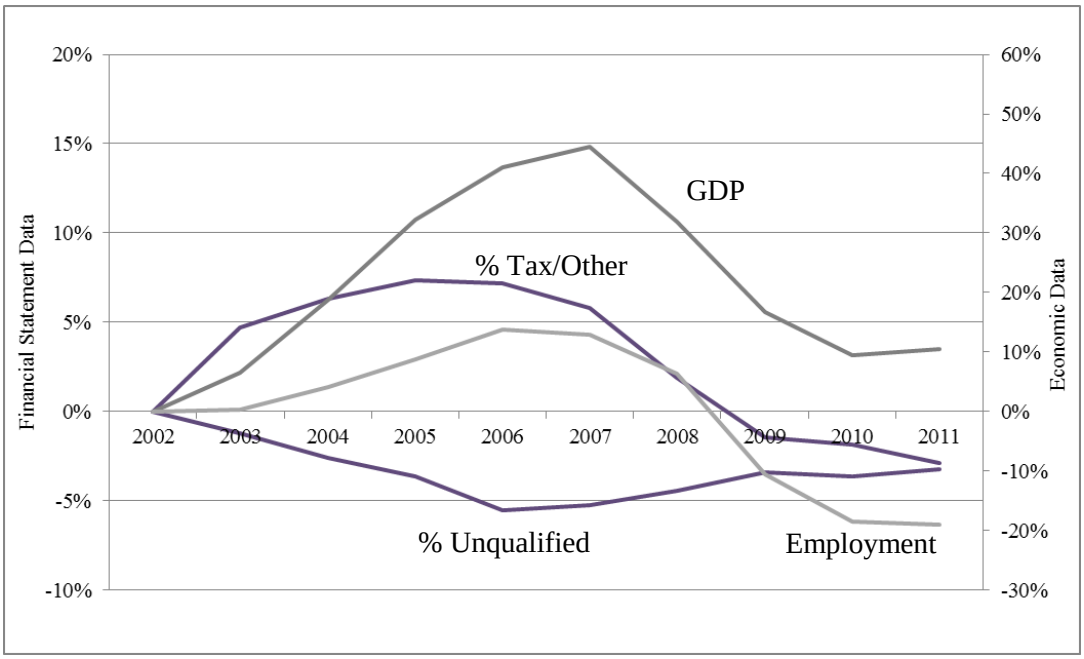


Figure 4: Coefficient Estimates by Firm Size Category. This figure reports the estimated coefficient on the *Trend* × *Construction* interaction term as a result of estimating Equation (2) separately for each firm size category. Reported below the estimated coefficients are t-statistics clustered at the three-digit NAICS industry level.

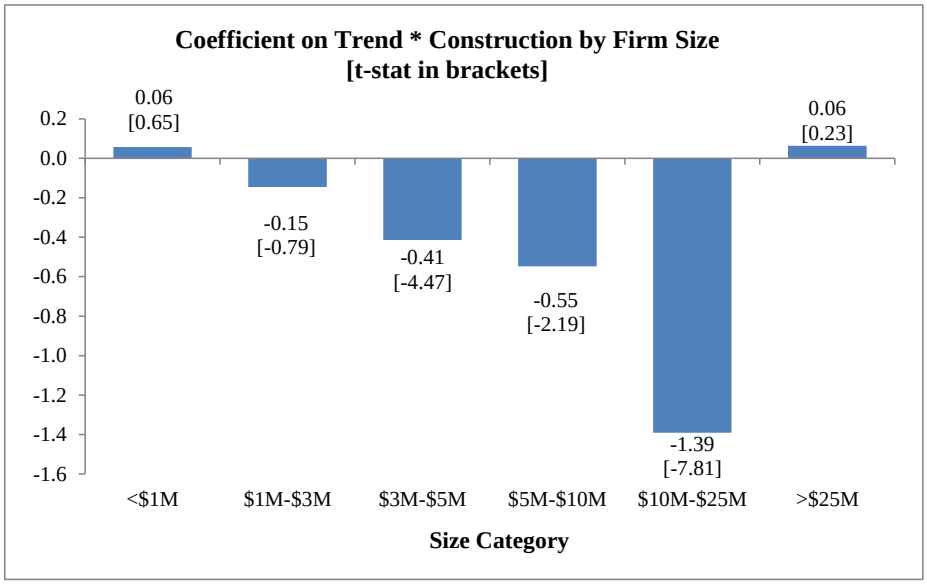


Table 1
Sample Selection of Financial Statement Data from RMA Dataset

This table presents the sample selection process for the financial statement observations used in the analyses from the RMA dataset. The top portion of the table reports the number of firm-years collected by RMA from its member banks over the entire sample period 2002-2011. The bottom portion of the table reports the number of observations based on the RMA aggregation level. Within each aggregated unit of observation, RMA reports the number of firms by financial report type. Industry-year-size category and industry-year-size category-region observations are measured at the three-digit NAICS industry level.

Financial documents submitted to RMA during the years 2002 - 20011	1,755,576
Eliminate Financial Services and Real Estate firms	(226,856)
Eliminate Industry-Year-Size categories with fewer than five statements collected in the region	(13,240)
Final sample	1,515,480

	<$3M Sales	$3M to $25M Sales	>$25M Sales	All Firms
Final sample by size	411,589	652,934	450,957	1,515,480
Construction firms	42,356	74,397	33,078	149,831
Non-Construction firms	369,233	578,537	417,879	1,365,649
Industry-Year-Size observations	1,606	2,420	855	4,881
Boom (2002-2007)	961	1,437	509	2,907
Crisis (2008-2011)	645	983	346	1,974
Industry-Year-Size-Region observations	7,677	12,157	4,717	24,551
Boom (2002-2007)	4,596	7,187	2,779	14,562
Crisis (2008-2011)	3,081	4,970	1,938	9,989

Table 2
Financial Statement Collection by Banks Each Year

This table presents the number (Panel A) and proportion (Panel B) of financial statement observations used in the analyses by year. Only statements from firms with between $3 million and $25 million in revenue are included. The statistics group financial statements into one of three mutually exclusive and collectively exhaustive report types: Audits, Reviews & Compilations, and Tax & Other. See Appendix A for a description of these different report types.

Panel A: Number of Statements Collected

	Non-Construction Firms				Construction Firms			
	Unqualified Audits	Reviews & Compilations	Tax & Other	Total Statements	Unqualified Audits	Reviews & Compilations	Tax & Other	Total Statements
2002	9,265	21,244	17,116	47,625	880	2,489	1,357	4,726
2003	9,225	25,389	19,258	53,872	1,074	3,679	2,364	7,117
2004	9,356	22,648	20,675	52,679	998	3,294	2,666	6,958
2005	8,809	20,660	24,286	53,755	944	3,384	3,587	7,915
2006	10,039	22,404	25,545	57,988	951	3,909	3,823	8,683
2007	9,011	20,285	26,416	55,712	783	3,407	3,569	7,759
2008	9,301	21,301	30,312	60,914	827	3,763	3,665	8,255
2009	10,034	21,470	31,322	62,826	915	3,667	3,208	7,790
2010	9,797	21,999	33,008	64,804	783	3,484	3,071	7,338
2011	9,818	22,170	36,374	68,362	809	3,660	3,387	7,856
Total	94,655	219,570	264,312	578,537	8,964	34,736	30,697	74,397

Panel B: Allocation of Financial Statement Type by Year

	Non-Construction Firms				Construction Firms				
	Unqualified Audits	Reviews & Compilations	Tax & Other	Δ Unqualified Audits	Unqualified Audits	Reviews & Compilations	Tax & Other	Δ Unqualified Audits	Difference-in-Difference
2002	19.5%	44.6%	35.9%		18.6%	52.7%	28.7%		
2003	17.1%	47.1%	35.7%	-2.3%	15.1%	51.7%	33.2%	-3.5%	-1.2%
2004	17.8%	43.0%	39.2%	0.6%	14.3%	47.3%	38.3%	-0.7%	-1.4%
2005	16.4%	38.4%	45.2%	-1.4%	11.9%	42.8%	45.3%	-2.4%	-1.0%
2006	17.3%	38.6%	44.1%	0.9%	11.0%	45.0%	44.0%	-1.0%	-1.9%
2007	16.2%	36.4%	47.4%	-1.1%	10.1%	43.9%	46.0%	-0.9%	0.3%
2008	15.3%	35.0%	49.8%	-0.9%	10.0%	45.6%	44.4%	-0.1%	0.8%
2009	16.0%	34.2%	49.9%	0.7%	11.7%	47.1%	41.2%	1.7%	1.0%
2010	15.1%	33.9%	50.9%	-0.9%	10.7%	47.5%	41.9%	-1.1%	-0.2%
2011	14.4%	32.4%	53.2%	-0.8%	10.3%	46.6%	43.1%	-0.4%	0.4%
Total	16.4%	38.0%	45.7%		12.0%	46.7%	41.3%		

Table 3
Unqualified Audit Frequency Difference-in-Difference Analysis—National Level

This table presents OLS regressions of the level of financial statement verification on time, industry, and firm size variables. The sample is limited to firms with between $3M and $25M in revenue. The unit of observation is industry-borrower size category-year. The regressions include three-digit NAICS industry fixed effects. The dependent variable in columns 1, 2, 4, and 5 (3 and 6) is the percent of statements collected by banks that are unqualified audits (tax returns and other statements). Column 1 (4) includes observations in the years 2002 and 2007 (2008 and 2011). The interaction term of *Year 2007(Year 2011)×Construction* is the diff-in-diff coefficient: the cumulative change in percent unqualified collection by banks from 2002 to 2007 (2008 to 2011) for construction firms relative to non-construction firms. Columns 2 and 3 (5 and 6) include observations in each of the years 2002 to 2007 (2008 to 2011). The interaction term *Trend×Construction* is the diff-in-diff coefficient: the average annual change in financial statement collection by banks from 2002 to 2007 (2008 to 2011) for construction firms relative to non-construction firms (for percent unqualified (percent tax, other) in columns 2 and 3 (5 and 6). See Appendix B for variable definitions. Reported below the coefficients are t-statistics with standard errors clustered at the three-digit NAICS industry level. *, **, *** indicate significance at the two-tailed 10%, 5% and 1% levels, respectively.

	Boom (2002-2007)			Crisis (2008-2011)		
	(1) Percent Unqualified	(2) Percent Unqualified	(3) Percent Tax, Other	(4) Percent Unqualified	(5) Percent Unqualified	(6) Percent Tax, Other
Year 2007	-4.675***					
	[-11.54]					
Year 2007 × Construction	-3.841***					
	[-4.05]					
Year 2011				-1.233***		
				[-4.54]		
Year 2011 × Construction				1.835**		
				[2.43]		
Trend		-0.817***	2.532***		-0.492***	1.088***
		[-9.01]	[16.35]		[-6.60]	[8.19]
Trend × Construction		-0.808***	1.008**		0.619**	-1.404*
		[-5.88]	[2.59]		[2.56]	[-1.87]
Borrower $5M-$10M in Sales	4.881***	4.685***	-6.640***	3.438***	3.729***	-8.652***
	[8.58]	[10.21]	[-13.97]	[5.56]	[6.48]	[-20.85]
Borrower $10M-$25M in Sales	13.750***	13.625***	-10.945***	10.378***	10.855***	-15.394***
	[13.76]	[15.11]	[-10.22]	[11.40]	[12.46]	[-17.44]
Fixed Effects	Industry	Industry	Industry	Industry	Industry	Industry
adj. R-sq	0.950	0.955	0.866	0.972	0.974	0.928
N	479	1,437	1,437	493	983	983

Table 4
Industry-Level Changes in GDP and Changes in Unqualified Audit Frequency

This table presents OLS regressions of changes in financial statement verification on changes in GDP at the industry level. The sample is limited to firms with between $3M and $25M in revenue. The unit of observation is industry-borrower size category-year. The dependent variable is the change in the percent of financial statements collected by banks from year t-1 to t, defined as Percent Unqualified$_t$ – Percent Unqualifed$_{t-1}$. The independent variable of interest is the percent change in GDP from year t-1 to t, defined as [(GDP$_t$ – GDP$_{t-1}$)/GDP$_{t-1}$]. The regression in column 1 includes three-digit NAICS industry fixed effects, and the regression in column 2 also includes year fixed effects. Indicators for borrower size are also included in both regressions. See Appendix B for variable definitions. Reported below the coefficients are t-statistics with standard errors clustered at the three-digit NAICS industry level. *, **, *** indicate significance at the two-tailed 10%, 5% and 1% levels, respectively.

	(1) Δ Percent Unqualified (t-1 to t)	(2) Δ Percent Unqualified (t-1 to t)
Δ GDP	-6.660***	-5.478**
(t-1 to t)	[-3.16]	[-2.19]
Fixed Effects	Borrower size, Industry	Borrower size, Industry, Year
adj. R-sq	0.024	0.059
N	2,064	2,064

Table 5
Unqualified Audit Frequency Difference-in-Difference Analysis—Regional Level

This table presents OLS regressions of the level of financial statement verification on time, industry, firm size, and firm region variables. The sample is limited to firms with between $3M and $25M in revenue. The unit of observation is industry-borrower size category-year-firm region. The regressions include three-digit NAICS industry and firm size category fixed effects. *HotRegion* is an indicator equal to 1 if the firm is in the west or southeast regions of the US and 0, otherwise. The dependent variable in columns 1, 2, 4, and 5 (3 and 6) is the percent of statements collected by banks that are unqualified audits (tax returns and other statements). Column 1 (4) includes observations in the years 2002 and 2007 (2008 and 2011). The interaction term of *Year2007(Year 2011)×HotRegion×Construction* is the triple difference coefficient: the cumulative change in percent unqualified collection by banks from 2002 to 2007 (2008 to 2011) for construction firms relative to non-construction firms in Hot Regions relative to non-Hot Regions. Columns 2 and 3 (5 and 6) include observations in each of the years 2002 to 2007 (2008 to 2011). The interaction term *Trend×HotRegion×Construction* is the triple difference coefficient: the average annual change in percent unqualified collection by banks from 2002 to 2007 (2008 to 2011) for construction firms relative to non-construction firms in Hot Regions relative to non-Hot Regions. See Appendix B for variable definitions. Reported below the coefficients are t-statistics with standard errors clustered at the three-digit NAICS industry level. *, **, *** indicate significance at the two-tailed 10%, 5% and 1% levels, respectively.

	Boom (2002-2007)			Crisis (2008-2011)		
	(1)	(2)	(3)	(4)	(5)	(6)
	Percent Unqualified	Percent Unqualified	Percent Tax, Other	Percent Unqualified	Percent Unqualified	Percent Tax, Other
Year 2007 × Construction	-2.384***					
	[-3.83]					
Year 2011 × Construction				1.445		
				[1.42]		
Trend × Construction		-0.546***	0.036		0.533	-0.777
		[-4.73]	[0.09]		[1.53]	[-1.23]
Year 2007 × Hot Region × Construction	-2.962**					
	[-2.52]					
Year 2011 × Hot Region × Construction				0.718		
				[0.81]		
Trend × Hot Region × Construction		-0.579**	1.865***		0.156	-1.157***
		[-2.00]	[9.96]		[0.54]	[-3.18]
Main and Two-Way Effects Included?	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Industry, Size	Industry, Size	Industry, Size	Industry, Size	Industry, Size	Industry, Size
adj. R-sq	0.845	0.842	0.585	0.911	0.906	0.691
N	2,386	7,187	7,187	2,484	4,970	4,970

Table 6
Firm Characteristics and the Propensity to Receive an Audit

This table presents firm-level audit propensity estimation results. The data come from the Sageworks, Inc. firm-level panel dataset for the years 2003 to 2007 for firms with sales between $3 million and $25 million. Observations with extreme values (defined as sales growth greater than 300% or less than -50%; ROA greater than 100% or less than -50%; Leverage greater than 200%) have been truncated. *LN(Sales)* is the natural log of sales revenue. *ROA* is calculated as Net income$_t$/Total assets$_t$. *Leverage* is calculated as Total liabilities$_t$/Total assets$_t$. Panel A presents the descriptive statistics for the 30,707 firm-years analyzed. Panel B presents the results of a probit model of a firm's choice to receive an audit for the 5,036 firms in 2003. The decision to get an audit is modeled as a function of the natural log of sales, return on assets, and leverage. The regressions include three-digit NAICS industry fixed effects and t-statistics are clustered at the three-digit NAICS industry level. Panel C presents the predicted and actual audit propensity for non-construction and construction firms based on the audit propensity model from Panel B. The difference-in-difference t-statistic presented in Panel C adjusts for clustering at the industry level.

Panel A: Sageworks Dataset Descriptive Statistics

	Mean	*Std Dev*	*25%*	*50%*	*75%*	*N*
Audit indicator	15.9%	36.6%				30,706
Construction indicator	25.8%	43.7%				30,706
$3-$5 million size indicator	27.8%	44.8%				30,706
$5-$10 million size indicator	38.1%	48.6%				30,706
$10-$25 million size indicator	34.2%	47.4%				30,706
Leverage	61.4%	30.9%	39.1%	61.4%	80.6%	30,706
ROA	10.3%	17.4%	1.0%	5.9%	15.9%	30,706
Sales growth	14.3%	31.9%	-1.9%	7.9%	22.3%	30,706

Panel B: Audit Choice Model

	Audit (1 or 0)
LN(Sales)	0.633***
	[13.68]
ROA	-0.775***
	[-4.09]
Leverage	-0.185
	[-1.57]
Years	2003
Fixed Effects	Industry
Area under ROC	0.741
N	5,036

Panel C: Predicted versus Actual Audit Propensities

	Predicted		**Actual**	
Year	Non-Cons	Construction	Non-Cons	Construction
2003	19.0%	17.5%	19.0%	17.6%
2004	19.1%	17.2%	18.5%	14.8%
2005	19.4%	17.1%	18.0%	13.9%
2006	19.6%	17.5%	17.7%	13.0%
2007	20.5%	17.9%	17.7%	11.7%
	2007 Actual minus predicted		-2.8%	-6.2%
	Difference-in-difference			-3.4%
	t-stat			-4.40

Table 7
Audit Propensity in 75 Portfolios Formed on Size, Profitability, and Leverage for Years 2003 and 2007

This table presents audit frequencies based on portfolio sorts of firm-level data from Sageworks, Inc. For the years 2003 and 2007, we sort privately held construction firms from the Sageworks dataset into 75 portfolios based on size, profitability, and leverage. Size portfolios are based on the RMA sales thresholds of $3-$5 million, $5-$10 million, and $10-$25 million. Profitability is defined as return on assets (net income divided by total assets at year-end). Leverage is defined as total liabilities divided by total assets. The portfolios for both profitability and leverage are sorted based on the quintile values for the full Sageworks sample. We report the number of firms and the percentage of firms receiving an audit in each portfolio each year. The 2007 portfolios with lower audit propensities than their respective counterparts in 2003 are shaded gray. The Actual total percentages are the grand means of audit propensities in each of the years across all portfolios. The Predicted total percentage in 2007 is the "expected" grand mean if the audit propensity for each of the portfolios remained constant (but the number of firms in each of the portfolios changed to the actual 2007 level).

		Number of firms					Audit propensity					Number of firms					Audit propensity				
		2003										2007									
		ROA					ROA					ROA					ROA				
		Low	2	3	4	High	Low	2	3	4	High	Low	2	3	4	High	Low	2	3	4	High
		Small firms					Small firms					Small firms					Small firms				
Leverage	Low	5	9	12	15	21	20%	0%	25%	20%	5%	18	31	38	33	59	17%	6%	8%	0%	2%
	2	14	14	20	15	20	7%	14%	5%	13%	5%	21	33	34	39	62	14%	9%	9%	8%	5%
	3	13	14	10	20	14	8%	0%	10%	10%	7%	21	40	30	36	16	5%	5%	0%	11%	6%
	4	22	25	18	15	6	14%	0%	6%	7%	0%	28	31	26	22	10	7%	0%	0%	5%	0%
	High	22	14	12	9	7	9%	7%	8%	0%	0%	45	22	15	10	16	2%	5%	0%	0%	0%
		Medium-sized firms					Medium-sized firms					Medium-sized firms					Medium-sized firms				
	Low	5	14	18	11	20	40%	21%	17%	9%	5%	18	20	27	48	72	17%	20%	26%	4%	4%
	2	14	15	28	22	16	36%	0%	18%	32%	31%	26	41	34	69	66	19%	5%	9%	10%	5%
	3	12	27	35	28	14	17%	19%	31%	14%	7%	38	61	64	47	42	5%	13%	11%	9%	2%
	4	18	26	28	23	6	17%	15%	21%	9%	0%	41	57	64	37	14	15%	9%	9%	3%	14%
	High	21	23	21	5	2	5%	4%	5%	0%	0%	61	36	26	15	10	10%	8%	4%	0%	0%
		Big Firms					Big Firms					Big Firms					Big Firms				
	Low	4	5	10	13	12	50%	60%	40%	31%	0%	10	10	10	30	53	30%	40%	70%	30%	9%
	2	5	13	19	22	4	40%	38%	26%	32%	50%	11	29	28	50	44	45%	24%	36%	14%	18%
	3	10	23	26	17	12	60%	26%	19%	6%	42%	23	49	56	48	28	39%	16%	20%	19%	21%
	4	19	37	26	22	8	21%	43%	15%	45%	13%	29	85	68	51	14	21%	22%	19%	12%	7%
	High	10	8	8	11	2	50%	0%	25%	27%	50%	49	56	25	21	12	31%	13%	12%	19%	8%
							Actual total %:			17.6%							Actual total %:			11.7%	
																	Predicted total %:			18.3%	

Table 8
Loan Losses and Unqualified Audits

This table examines the relation between borrower financial statement audits and subsequent loan losses. All data is from FDIC Call Reports except *% Unqualified within Construction* which is from the RMA data. Only banks with RMA data between 2007 and 2010 are included because the FDIC Call Report began reporting construction related losses in 2007. Panel A presents the descriptive statistics for bank fundamental variables used in the analyses. All variables are measured with a one year lag except chargeoff variables. Panel B presents OLS regressions of the scaled construction loan losses on construction exposure, percent unqualified, and bank characteristic variables. The unit of observation is bank-year. The dependent variable in the first column is the ratio of construction chargeoffs over the last four quarters to gross loans and leases at the beginning of the year. The dependent variable in the second and third columns is the ratio of total loan chargeoffs over the last four quarters to gross loans and leases at the beginning of the year. *Log Average Borrower Size* is measured for only construction firms in column 1, and for all firms in columns 2 and 3. See Appendix B for variable definitions. Reported below the coefficients are t-statistics with standard errors clustered at the bank level. *, **, *** indicate significance at the two-tailed 10%, 5% and 1% levels, respectively.

Panel A: Descriptive Statistics for RMA Member Banks with FDIC Call Report Data

	Mean	*Std Dev*	*25%*	*50%*	*75%*	*N*
Construction Chargeoffs/Construction Loans	2.61%	3.80%	0.00%	1.14%	3.70%	372
Construction Chargeoffs/Gross Loans	0.75%	1.40%	0.00%	0.20%	0.88%	372
% Unqualified within Construction	9.59%	13.91%	0.00%	4.55%	16.11%	372
Loan Growth (Construction)	2.33%	37.53%	-7.28%	-1.50%	4.97%	372
Loan Growth (All)	1.82%	11.51%	-0.83%	0.78%	3.00%	372
Log Borrower Size (Construction)	9.7	2.1	8.4	9.3	10.4	372
Log Borrower Size (All)	10.7	2.0	9.1	10.3	12.3	372
Log Total Assets	14.0	1.7	12.8	13.8	15.1	372
Capital	11.80%	3.02%	9.80%	11.14%	13.35%	372
Hot Region Construction Exposure %	29.29%	44.16%	0.00%	0.00%	98.35%	372
Construction Exposure	11.28%	7.94%	4.98%	9.54%	15.38%	372
Agricultural Exposure	1.86%	4.15%	0.00%	0.19%	1.85%	372
Consumer Exposure	7.01%	8.04%	1.38%	3.36%	10.45%	372

Panel B: Loan Losses and Unqualified Audits					
	(1) Construction Chargeoffs/ Construction Loans	(2) Construction Chargeoffs/ Construction Loans	(3) Construction Chargeoffs/ Gross Loans	(4) Construction Chargeoffs/ Gross Loans	(5) Construction Chargeoffs/ Gross Loans
% Unqualified within Construction (t-1)	-0.023*	-0.024**	-0.009***	-0.010***	-0.007**
	[-1.92]	[-2.04]	[-2.76]	[-2.92]	[-2.35]
Construction Loan Growth (t-1)	-0.006*		-0.009***		
	[-1.73]		[-2.76]		
Size (t-1)	0.006***	0.006***	0.002***	0.002***	0.001
	[3.19]	[3.24]	[2.93]	[2.96]	[1.16]
Capital (t-1)	0.028	0.018	0.037	0.032	0.013
	[0.32]	[0.21]	[0.93]	[0.83]	[0.46]
Hot Region Construction Exposure %	0.013**	0.013**	0.004*	0.003*	0.003*
	[2.45]	[2.50]	[1.76]	[1.75]	[1.88]
Log Avg Construction Borrower Size (t-1)	0.002*	0.002*			
	[1.77]	[1.75]			
Log Avg Borrower Size (t-1)			0.000	0.000	0.000
			[0.73]	[0.82]	[1.39]
Loan Growth (t-1)		-0.033		-0.014	-0.009
		[-1.30]		[-1.54]	[-1.47]
Construction Exposure (t-1)			0.067***	0.068***	0.061***
			[3.89]	[3.97]	[4.53]
Agricultural Exposure (t-1)			0.027**	0.024*	0.002
			[2.05]	[1.89]	[0.15]
Consumer Exposure (t-1)			-0.018**	-0.017**	-0.022***
			[-2.59]	[-2.50]	[-3.22]
Non-Construction Chargeoffs/Gross Loans (t)					0.235***
					[3.18]
Frequency of observations?	Annual	Annual	Annual	Annual	Annual
Clustering	Bank	Bank	Bank	Bank	Bank
adj. R-sq	0.110	0.116	0.259	0.270	0.364
N	372	372	372	372	372
Sample period	2007-2010	2007-2010	2007-2010	2007-2010	2007-2010

Table 9
Financial Statement Auditing and Firm Survival During the Crash

This table analyzes firm-level panel U.S. Tax Return dataset of Partnerships, S Corporations, and C Corporations provided by the IRS from the years 2008 to 2010. Panel A presents contingency table analyses examining whether firms that exist in the year 2008 survive to the year 2010, conditional on receiving a financial statement audit from an independent accountant in the year 2008. We classify the firm as "Survive" if the firm files a tax return in 2010 and classify it as "No survive" if it does not. We classify the firm as having an audit if the firm reports that it produces audited GAAP financial statements as reported on the IRS Schedule M-3. The sample includes only firms that e-file a Schedule M-3 on a consolidated tax Form 1120, 1120S, or 1065 in the year 2008. Only firms with at least $10 million in assets are required to file Schedule M-3. "All firms" includes all firms that are not in finance or real estate industries. "Construction firms only" includes all firms with a 2-digit NAICS code of 23. "Construction firms <$25M" include all construction firms that report less than $25 million in revenue, conditional on having at least $10 million in assets. The first section of Panel A reports the number of firms by cell; the second section reports the percentage distribution of firms; and the third section reports the survival rates conditional on whether the firm received an audit in 2008. Panel B presents linear probability model estimates of the probability of the firms surviving from 2008 to 2010. The variable $Survive_{2010}$ is an indicator variable equal to 1 if the firm files a tax return in 2008 and 2010; equal to 0 if the firm files a tax return in 2008 but not in 2010. The variable $Audit_{2008}$ equals 1 if the firm reports that it produces audited GAAP financial statements, as reported on the IRS Schedule M-3 in 2008; 0 otherwise. Samples include only e-filer firms that exist in 2008, i.e., firms in 2008 & 2009 & 2010 or in 2008 & 2009 or in 2008 & 2010 or in 2008 only. The variable Growth used in column (5) is the percent change in total revenues from 2007 to 2008, so requires the firm to exist in both 2007 and 2008. Continuous variables are winsorized at the 1 and 99 percentile levels. See Appendix B for variable definitions. Reported below the coefficients are t-statistics with standard errors clustered at the firm level, and standard errors are heteroskedasticity robust. *, **, *** indicate significance at the two-tailed 10%, 5% and 1% levels, respectively.

Panel A: Distribution of firms conditioning on whether the firm had a financial audit

Number of firms

	All firms			Construction only			Construction <$25M		
	Survive	No survive		Survive	No survive		Survive	No survive	
Audit	7,726	2,429	10,155	1,051	395	1,446	119	81	200
No audit	8,403	4,097	12,500	997	928	1,925	536	617	1,153
	16,129	6,526	22,655	2,048	1,323	3,371	655	698	1,353

Percent distribution

	All firms			Construction only			Construction <$25M		
	Survive	No survive		Survive	No survive		Survive	No survive	
Audit	34.1%	10.7%	44.8%	31.2%	11.7%	42.9%	8.8%	6.0%	14.8%
No audit	37.1%	18.1%	55.2%	29.6%	27.5%	57.1%	39.6%	45.6%	85.2%
	71.2%	28.8%	100.0%	60.8%	39.2%	100.0%	48.4%	51.6%	100.0%

Survival conditonal on audit

	All firms			Construction only			Construction <$25M		
	Survive	No survive		Survive	No survive		Survive	No survive	
Audit	76.1%	23.9%	100.0%	72.7%	27.3%	100.0%	59.5%	40.5%	100.0%
No audit	67.2%	32.8%	100.0%	51.8%	48.2%	100.0%	46.5%	53.5%	100.0%

Panel B: Firm Survival and Audits				
	(1)	(2)	(3)	(4)
	$Survive_{2010}$	$Survive_{2010}$	$Survive_{2010}$	$Survive_{2010}$
	(1=yes; 0=no)	(1=yes; 0=no)	(1=yes; 0=no)	(1=yes; 0=no)
$Audit_{2008}$	0.105***	0.042***	0.117***	0.297***
	[5.63]	[6.63]	[2.95]	[4.16]
Construction		-0.158***		
		[-13.01]		
$Audit_{2008}$ * Construction		0.105***		
		[6.29]		
Log Assets	0.146***	0.058***	0.184***	0.181***
	[11.92]	[14.81]	[7.87]	[9.86]
$Audit_{2008}$ * Log Assets				-0.060***
				[-2.84]
Growth				
Pass-Through Entity	0.087***	0.119***	0.047	0.089***
	[3.71]	[16.49]	[0.99]	[3.77]
Corporation	0.034	0.038***	-0.014	0.036
	[1.44]	[4.78]	[-0.43]	[1.51]
Log Total Revenue	0.027***	0.031***	0.02	0.027***
	[2.92]	[11.27]	[1.38]	[2.87]
Taxable Income to Total Revenue	-0.004	0.040***	0.03	-0.002
	[-0.09]	[2.99]	[0.64]	[-0.06]
Log Accounts Payable	-0.052***	-0.040***	-0.112***	-0.048***
	[-4.35]	[-10.40]	[-4.34]	[-3.99]
Intangibles	0.030*	0.017***	0.056*	0.030*
	[1.69]	[2.88]	[1.93]	[1.73]
Loss	-0.067***	-0.071***	-0.015	-0.067***
	[-2.77]	[-9.98]	[-0.41]	[-2.80]
Log Number of Owners	0.015	0.003	0.025	0.018*
	[1.56]	[1.24]	[1.38]	[1.84]
Leverage	-0.149***	-0.046***	-0.150***	-0.146***
	[-4.90]	[-4.76]	[-4.09]	[-4.80]
Constant	0.04	0.404***	-0.029	-0.074
N	3,371	22,655	1,353	3,371
Number of firms with Audit in 2008	1,446	10,155	200	1,446
Sample	Construction firms only	All firms	Construction firms only <$25M revenue	Construction firms only

Online Appendix to:

Economic Growth and Financial Statement Verification

August 2016

Table A1
Robustness Tests of Table 3 after including Bank Fixed Effects

This table presents OLS regressions of the level of financial statement verification on time, industry, firm size variables, and bank fixed effects. These regressions are the same as those of Table 3 in the paper except the unit of observation has expanded to include bank-level observations and the tests include bank fixed effects. The sample is limited to firms with between $3M and $25M in revenue. The unit of observation is bank-industry-borrower size category-year. The regressions include three-digit NAICS industry fixed effects. The dependent variable in columns 1, 2, 4, and 5 (3 and 6) is the percent of statements collected by banks that are unqualified audits (tax returns and other statements). Column 1 (4) includes observations in the years 2002 and 2007 (2008 and 2011). The interaction term of *Year 2007 (Year 2011)×Construction* is the diff-in-diff coefficient: the cumulative change in % unqualified audit collection by banks from 2002 to 2007 (2008 to 2011) for construction firms relative to non-construction firms. Columns 2 and 3 (5 and 6) include observations in each of the years 2002 to 2007 (2008 to 2011). The interaction term *Trend×Construction* is the diff-in-diff coefficient: the average annual change in % unqualified audit collection by banks from 2002 to 2007 (2008 to 2011) for construction firms relative to non-construction firms. See Appendix B for variable definitions. Reported below the coefficients are t-statistics with standard errors clustered at the three-digit NAICS industry level. *, **, *** indicate significance at the two-tailed 10%, 5% and 1% levels, respectively.

	Boom (2002-2007)			Crisis (2008-2011)		
	(1)	(2)	(3)	(4)	(5)	(6)
	Percent Unqualified	Percent Unqualified	Percent Tax, Other	Percent Unqualified	Percent Unqualified	Percent Tax, Other
Year 2007	-4.020***					
	[-7.19]					
Year 2007 × Construction	-4.076***					
	[-3.10]					
Year 2011				-0.914***		
				[-4.39]		
Year 2011 × Construction				1.091		
				[1.44]		
Trend		-0.745***	2.295***		-0.384***	0.795***
		[-7.18]	[12.64]		[-6.12]	[4.93]
Trend × Construction		-0.845***	0.911***		0.326	-0.897
		[-4.89]	[3.57]		[1.15]	[-1.19]
Borrower $5M-$10M in Sales	4.633***	4.393***	-6.266***	3.344***	3.684***	-8.899***
	[7.20]	[8.52]	[-11.97]	[5.02]	[5.50]	[-21.23]
Borrower $10M-$25M in Sales	13.191***	13.050***	-10.904***	9.987***	10.407***	-15.983***
	[12.14]	[12.63]	[-9.35]	[10.40]	[10.77]	[-18.57]
Fixed Effects	Bank, Industry	Bank, Industry	Bank, Industry	Bank, Industry	Bank, Industry	Bank, Industry
adj. R-sq	0.769	0.733	0.515	0.837	0.832	0.610
N	4,669	14,610	14,610	5,289	10,539	10,539

Table A2
Robustness Tests of Table 5 after including Bank Fixed Effects

This table presents OLS regressions of the level of financial statement verification on time, industry, firm size, firm region variables, and bank fixed effects. Panel A includes the same regressions as Table 5 in the paper except the unit of observation has expanded to include bank-level observations and the tests include bank fixed effects. Panel B includes the same regressions as Table 5 in the paper except for the inclusion of bank fixed effects and the requirement that banks have loans in both "hot" and "not hot" regions. The sample is limited to firms with between $3M and $25M in revenue. The unit of observation is bank-industry-borrower firm size category-year-firm region. The regressions include three-digit NAICS industry and firm size category fixed effects. *HotRegion* is an indicator equal to 1 if the firm is in the West or Southeast regions of the US, and 0 otherwise. The dependent variable in columns 1, 2, 4, and 5 (3 and 6) is the percent of statements collected by banks that are unqualified audits (tax returns and other statements). Column 1 (4) includes observations in the years 2002 and 2007 (2008 and 2011). The interaction term of *Year2007(Year 2011)×HotRegion×Construction* is the triple difference coefficient: the cumulative change in % unqualified audit collection by banks from 2002 to 2007 (2008 to 2011) for construction firms relative to non-construction firms in Hot Regions relative to non-Hot Regions. Columns 2 and 3 (5 and 6) include observations in each of the years 2002 to 2007 (2008 to 2011). The interaction term *Trend×HotRegion×Construction* is the triple difference coefficient: the average annual change in % unqualified audit collection by banks from 2002 to 2007 (2008 to 2011) for construction firms relative to non-construction firms in Hot Regions relative to non-Hot Regions. See Appendix B for variable definitions. Reported below the coefficients are t-statistics with standard errors clustered at the three-digit NAICS industry level. *, **, *** indicate significance at the two-tailed 10%, 5% and 1% levels, respectively.

Panel A: Robustness check of Table 5 including bank fixed effects

	Boom (2002-2007)			Crisis (2008-2011)		
	(1)	(2)	(3)	(4)	(5)	(6)
	Percent Audited	Percent Audited	Percent Tax, Other	Percent Audited	Percent Audited	Percent Tax, Other
Year 2007 * Construction	-1.773**					
	[-2.45]					
Year 2007 * Hot Region * Construction	-4.835***					
	[-3.26]					
Year 2011 * Construction				0.346		
				[0.30]		
Year 2011 * Hot Region * Construction				1.205		
				[1.27]		
Trend * Construction		-0.474***	-0.013		0.112	-0.076
		[-2.87]	[-0.04]		[0.27]	[-0.13]
Trend * Hot Region * Construction		-0.858***	1.811***		0.382	-1.543***
		[-2.86]	[8.74]		[1.28]	[-2.80]
Fixed Effects	Bank, Industry	Bank, Industry	Bank, Industry	Bank, Industry	Bank, Industry	Bank, Industry
adj. R-sq	0.746	0.71	0.486	0.816	0.809	0.563
N	5,559	17,236	17,236	6,577	13,072	13,072

Panel B: Robustness tests of Table 5 after including bank fixed effects and requiring banks to have exposure to both "hot" and "not hot" regions

	Boom (2002-2007)	
	(1)	(2)
	Percent Audited	Percent Audited
Trend	-0.639***	-0.948***
	[-5.60]	[-8.68]
Trend × Construction	-1.041***	-0.766***
	[-5.97]	[-4.27]
Hot Region		-3.485***
		[-3.77]
Trend * Hot Region		0.723***
		[3.83]
Construction * Hot Region		-1.545
		[-0.78]
Trend * Construction * Hot Region		-0.677***
		[-3.58]
Log Avg Borrower Size	9.542***	9.560***
	[13.43]	[13.29]
Fixed Effects	Bank, Industry	Bank, Industry
adj. R-sq	0.715	0.717
N	11,863	11,863
Size Categories	3-5	3-5
Sample: Has Hot=1 & Has Not=1	Yes	Yes

Table A3

Unqualified Audit Frequency Difference-in-Difference Analysis—National Level—Excluding Land Development Related Industries

This table presents OLS regressions of the level of financial statement verification on time, industry, and firm size variables. This is a robustness check table for Table 3 in the paper. In this table, we omit industries which were more likely exposed to land speculation (NAICS codes: 236115, 236116, 236117, 236118, 236210, 236220, 237310, and 237990). The sample is limited to firms with between $3M and $25M in revenue. The unit of observation is industry-size category-year. The regressions include three-digit NAICS industry fixed effects. The dependent variable in columns 1, 2, 4, and 5 (3 and 6) is the percent of statements collected by banks that are unqualified audits (tax returns and other statements). Column 1 (4) includes observations in the years 2002 and 2007 (2008 and 2011). The interaction term of *Year 2007 (Year 2011)×Construction* is the diff-in-diff coefficient: the cumulative change in % unqualified audit collection by banks from 2002 to 2007 (2008 to 2011) for construction firms relative to non-construction firms. Columns 2 and 3 (5 and 6) include observations in each of the years 2002 to 2007 (2008 to 2011). The interaction term *Trend×Construction* is the diff-in-diff coefficient: the average annual change in % unqualified audit collection by banks from 2002 to 2007 (2008 to 2011) for construction firms relative to non-construction firms. See Appendix B in the paper for variable definitions. Reported below the coefficients are t-statistics with standard errors clustered at the three-digit NAICS industry level. *, **, *** indicate significance at the two-tailed 10%, 5% and 1% levels, respectively.

	Boom (2002-2007)			Crisis (2008-2011)		
	(1)	(2)	(3)	(4)	(5)	(6)
	Percent Unqualified	Percent Unqualified	Percent Tax, Other	Percent Unqualified	Percent Unqualified	Percent Tax, Other
Year 2007	-4.673***					
	[-11.52]					
Year 2007 × Construction	-3.562***					
	[-5.80]					
Year 2011				-1.234***		
				[-4.55]		
Year 2011 × Construction				1.083***		
				[3.88]		
Trend		-0.816***	2.527***		-0.493***	1.089***
		[-8.99]	[16.29]		[-6.61]	[8.19]
Trend × Construction		-0.807***	0.936***		0.365***	-0.606***
		[-7.84]	[3.56]		[4.70]	[-3.13]
Borrower $5M-$10M in Sales	4.862***	4.670***	-6.430***	3.414***	3.673***	-8.500***
	[8.35]	[9.81]	[-13.72]	[5.29]	[6.14]	[-20.32]
Borrower $10M-$25M in Sales	13.705***	13.491***	-10.415***	10.195***	10.653***	-15.130***
	[13.57]	[14.71]	[-10.12]	[10.91]	[11.99]	[-17.00]
Fixed Effects	Industry	Industry	Industry	Industry	Industry	Industry
adj. R-sq	0.952	0.956	0.866	0.973	0.975	0.929
N	473	1,419	1,419	487	971	971

Table A4
Firm Level Evidence of Main Results using Data from Sageworks

This table presents firm level OLS regressions of a firm's choice to have their financial statements audited on a time trend and various control variables. This table is a robustness check of the results in Tables 3 and 5 of the paper. The dataset is an unbalanced firm level panel dataset from Sageworks, Inc. See the paper for details about the data. The dependent variable in all columns equals 1 if the firm received a financial statement audit and 0 otherwise. The sample is limited to firms with between $3M and $25M in revenue and years 2002 to 2007. The regressions include three-digit NAICS industry fixed effects. The interaction term *Trend×Construction* is the diff-in-diff coefficient: the average annual change in audited financial statement propensity from 2002 to 2007 for construction firms relative to non-construction firms. *Hot Region* is an indicator variable equal to 1 for firms located in states in the West and Southeast regions of the U.S. with the most house price appreciation from 2002 to 2006 (CA, NV, HI, AZ, FL, GA). *LN(Sales)* is the natural log of sales revenue. *Leverage* is calculated as Total liabilities$_t$/Total assets$_t$. *ROA* is calculated as Net income$_t$/Total assets$_t$. *Sales Growth* is (Sales$_t$ – Sales$_{t-1}$)/(Sales$_{t-1}$). Columns 1, 3, and 4 require a valid Sales Growth value; whereas, column 2 does not have this constraint. See additional data constraints in the description of Table 6 in the paper. Reported below the coefficients are t-statistics with standard errors clustered at the three-digit NAICS industry level. *, **, *** indicate significance at the two-tailed 10%, 5% and 1% levels, respectively.

	(1) Audit? Y=1; N=0	(2) Audit? Y=1; N=0	(3) Audit? Y=1; N=0	(4) Audit? Y=1; N=0
Trend	-0.001	-0.005***	-0.002	-0.003
	[-0.56]	[-3.16]	[-0.81]	[-1.37]
Trend × Construction	-0.015***	-0.010***	-0.013***	-0.012***
	[-5.46]	[-6.76]	[-5.70]	[-5.00]
Borrower $5M-$10M in Sales	0.053***	0.053***		
	[6.50]	[7.48]		
Borrower $10M-$25M in Sales	0.157***	0.160***		
	[13.43]	[13.97]		
Hot Region		-0.017		-0.014
		[-0.98]		[-0.51]
Trend × Hot Region		0.008*		0.008
		[1.81]		[1.18]
Hot Region × Construction		-0.039**		-0.050*
		[-2.23]		[-1.83]
Trend × Hot Region × Construction		-0.009*		-0.006
		[-1.85]		[-0.81]
LN(Sales)			0.121***	0.121***
			[13.58]	[12.92]
Leverage			-0.050**	-0.051**
			[-2.57]	[-2.56]
ROA			-0.128***	-0.125***
			[-5.16]	[-5.39]
Sales Growth			-0.001	-0.001
			[-0.10]	[-0.10]
Fixed Effects	Industry	Industry	Industry	Industry
adj. R-sq	0.102	0.105	0.109	0.110
N	30,706	51,096	30,706	30,706

Table A5
Firm Characteristics and the Propensity to Receive an Audit

This table presents a robustness check of the firm-level audit propensity estimation results presented in Table 6 of the paper. The results in this table include the variable *Sales Growth*, defined as $(Sales_t - Sales_{t-1})/(Sales_{t-1})$, which reduces the sample size because of requiring t-1 sales data. The data come from the Sageworks, Inc. firm-level panel dataset for the years 2002 to 2007 for firms with sales between $3 million and $25 million. Observations with extreme values (defined as sales growth greater than 300% or less than -50%; ROA greater than 100% or less than -50%; Leverage greater than 200%) have been truncated. *LN(Sales)* is the natural log of sales revenue. *ROA* is calculated as Net income$_t$/Total assets$_t$. *Leverage* is calculated as Total liabilities$_t$/Total assets$_t$. Panel A presents the results of a probit model estimating a firm's choice to receive an audit for the 1,718 firms in 2003 (which have sales data in 2002). The decision to get an audit is modeled as a function of the natural log of sales, return on assets, leverage, and sales growth. The regressions include three-digit NAICS industry fixed effects and t-statistics are clustered at the three-digit NAICS industry level. Panel B presents the predicted and actual audit propensity for non-construction and construction firms based on the audit propensity model from Panel A. The difference-in-difference t-statistic presented in Panel B adjusts for clustering at the three-digit NAICS industry level. *, **, *** indicate significance at the two-tailed 10%, 5% and 1% levels, respectively.

Panel A: Audit Choice Model

	Audit (1 or 0)
LN_sales	0.705***
	[8.18]
ROA	-0.550*
	[-1.81]
Leverage	-0.17
	[-1.03]
Sales growth	-0.113
	[-1.31]
Years	2003
Fixed Effects	Industry
Area under ROC	0.737
N	1,718

Panel B: Predicted versus Actual Audit Propensities

	Predicted		**Actual**	
Year	Non-Cons	Construction	Non-Cons	Construction
2003	17.4%	19.2%	17.4%	19.4%
2004	17.6%	18.4%	18.5%	15.8%
2005	18.0%	18.4%	18.0%	13.9%
2006	18.2%	19.0%	17.5%	13.1%
2007	19.2%	19.4%	17.5%	11.6%
	2007 Actual minus predicted		-1.7%	-7.8%
	Difference-in-difference			-6.1%
	t-stat			-4.47

Table A6

Propensity to Terminate or Initiate an Audit: Construction versus other Industries

This table presents OLS regressions of a firm's decision to terminate (initiate) a financial statement audit in year *t+1* as a function of firm characteristics in year *t* and conditional on (not) having an audit year *t*. . The data come from the Sageworks, Inc. firm-level panel dataset for the years 2002 to 2007 for firms with sales between $3 million and $25 million. Observations with extreme values (defined as sales growth greater than 300% or less than -50%; ROA greater than 100% or less than -50%; Leverage greater than 200%) have been truncated. *Construction* is an indicator variable equal to one for firms in the construction industry. *LN(Sales)* is the natural log of sales revenue. *ROA* is calculated as Net income$_t$/Total assets$_t$. *Leverage* is calculated as Total liabilities$_t$/Total assets$_t$. To maximize observations and to measure growth during the year of the decision to terminate or initiate the audit, *Sales Growth$_{t+1}$*, is defined as (Sales$_{t+1}$ – Sales$_t$)/(Sales$_t$). The audit initiation analysis has more observations because significantly more firms do not have financial statement audits in year *t*. Presented below the coefficient estimates are t-statistics with standard errors clustered at the three-digit NAICS industry level. *, **, *** indicate significance at the two-tailed 10%, 5% and 1% levels, respectively.

	(1) Terminate Audit t+1? Y=1; N=0	(2) Terminate Audit t+1? Y=1; N=0	(3) Initiate Audit t+1? Y=1; N=0	(4) Initiate Audit t+1? Y=1; N=0
Construction	0.006***	0.005**	0.001	0.001
	[3.07]	[2.53]	[1.14]	[1.06]
Borrower $5M-$10M in Sales	0.010**		0.002**	
	[2.17]		[2.13]	
Borrower $10M-$25M in Sales	0.004		0.005***	
	[0.95]		[4.81]	
LN(Sales)		0.001		0.004***
		[0.31]		[5.27]
Leverage		-0.010*		0.001
		[-1.71]		[0.63]
ROA		-0.007		0.001
		[-0.73]		[0.31]
Sales Growth$_{t+1}$		0.010**		0.002**
		[2.07]		[2.29]
Fixed effects	Year	Year	Year	Year
adj. R-sq	0.001	0.001	0.002	0.002
N	5,566	5,566	27,677	27,676

Table A7
Public Firm Audit Fee Analysis

This table presents OLS regressions of audit fees on a trend variable, firm characteristics, and an interaction between construction and the trend variable. Because audit fee data for privately held firms in the U.S. is not generally available, the data in this analysis is for publicly held firms. The audit fee data is sourced from AuditAnalytics and the firm level data is sourced from Compustat. The years included in the regression are the housing boom years 2002 to 2007. *LN(Sales)* is the natural log of sales revenue (*sale*). *ROA* is calculated as Net income$_t$/Total assets$_t$ (ib_t/at_t). *Leverage* is calculated as Total liabilities$_t$/Total assets$_t$ ($(dltt_t+dlc_t)/at_t$). The dependent variable in columns (1) and (2) is audit fees scaled by total sales revenue. The dependent variable in column (3) is the natural log of audit fees. Three-digit NAICS industry fixed are included in all regressions. Reported below the coefficients are t-statistics with standard errors clustered at the three-digit NAICS industry level. *, **, *** indicate significance at the two-tailed 10%, 5% and 1% levels, respectively.

	(1) Audit fees scaled by sales	(2) Audit fees scaled by sales	(3) LN(Audit fees)
Trend	0.069***	0.076***	0.185***
	[8.39]	[8.28]	[53.45]
Trend × Construction	-0.036***	-0.020***	-0.004
	[-4.33]	[-2.77]	[-0.98]
LN(Sales)		-0.189***	0.583***
		[-10.27]	[39.20]
ROA		-0.431***	-0.464***
		[-4.14]	[-4.70]
Leverage		-0.077***	-0.037
		[-2.91]	[-0.74]
Sales Growth		0.114***	0.081**
		[4.37]	[2.32]
Fixed effects	Industry	Industry	Industry
adj. R-sq	0.141	0.324	0.592
N	17,002	17,002	17,002

Table A8
Robustness Tests of Table 9, Panel B after Removing Firms with Assets <$15 million

This table analyzes a firm-level panel U.S. Tax Return dataset of Partnerships, S Corporations, and C Corporations provided by the IRS from the years 2008 to 2010. The regressions in this table are the same as those in Table 9, Panel B of the paper except firms with less than $15 million in assets are omitted. The regressions are linear probability model estimates of the probability of the firms surviving from 2008 to 2010. The variable $Survive_{2010}$ is an indicator variable equal to 1 if the firm files a tax return in 2008 and 2010, and equal to 0 if the firm files a tax return in 2008 but not in 2010. The variable $Audit_{2008}$ equals 1 if the firm reports that it produces audited GAAP financial statements, as reported on the IRS Schedule M-3 in 2008, and 0 otherwise. Samples include only e-filer firms that exist in 2008, i.e., firms in 2008 & 2009 & 2010 or in 2008 & 2009 or in 2008 & 2010 or in 2008 only. Continuous variables are winsorized at the 1 and 99 percentile levels. See Appendix B in the paper for variable definitions. Reported below the coefficients are t-statistics with standard errors clustered at the firm level. *, **, *** indicate significance at the two-tailed 10%, 5% and 1% levels, respectively. All models reported herein are significant at the 1% level using an F-test.

	(1)	(2)	(3)	(4)
	$Survive_{2010}$	$Survive_{2010}$	$Survive_{2010}$	$Survive_{2010}$
	(1=yes; 0=no)	(1=yes; 0=no)	(1=yes; 0=no)	(1=yes; 0=no)
$Audit_{2008}$	0.086***	0.049***	0.089	0.192**
	[3.90]	[6.72]	[1.57]	[2.05]
Construction		-0.125***		
		[-8.08]		
$Audit_{2008}$ * Construction		0.079***		
		[3.94]		
Log Assets	0.024	0.004	0.04	0.043*
	[1.58]	[0.80]	[1.13]	[1.86]
$Audit_{2008}$ * Log Assets				-0.030
				[-1.16]
Pass-Through Entity	0.160***	0.165***	0.176***	0.161***
	[5.57]	[19.60]	[2.62]	[5.60]
Corporation	0.076**	0.067***	0.003	0.077***
	[2.56]	[7.07]	[0.07]	[2.59]
Log Total Revenue	0.033***	0.029***	0.048**	0.033***
	[2.93]	[8.85]	[2.44]	[2.91]
Taxable Income to Total Revenue	-0.048	0.039**	-0.046	-0.046
	[-0.91]	[2.42]	[-0.71]	[-0.88]
Log Accounts Payable	-0.036***	-0.031***	-0.130***	-0.035**
	[-2.63]	[-7.15]	[-3.82]	[-2.51]
Intangibles	-0.003	0.006	0.056	-0.002
	[-0.13]	[0.87]	[1.44]	[-0.10]
Loss	-0.116***	-0.057***	-0.057	-0.116***
	[-3.90]	[-6.75]	[-1.07]	[-3.89]
Log Number of Owners	0.017	0.007**	0.029	0.018*
	[1.54]	[2.38]	[1.30]	[1.67]
Leverage	-0.147***	-0.042***	-0.139***	-0.146***
	[-3.84]	[-3.57]	[-2.80]	[-3.80]
Constant	0.372***	0.530***	0.287*	0.303***
	[5.90]	[24.76]	[1.94]	[3.37]
adj. R-sq	0.089	0.062	0.036	0.089
N	2,215	15,955	710	2,215
Number of firms with Audit in 2008	1,107	8,289	91	1,107
Sample	Construction firms only	All firms	Construction firms only <$25M revenue	Construction firms only