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Regulatory Spillovers in Local Mortgage Markets

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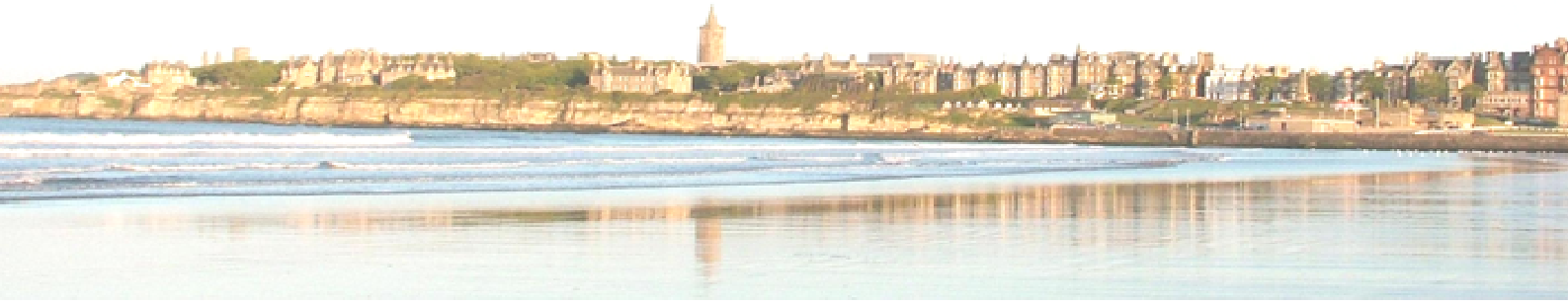

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Regulatory Spillovers in Local Mortgage Markets

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ABSTRACT

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[. . .] Regulatory spillovers across financial players may be more a source of concern. Regulatory spillovers have become both more likely and more difficult to identify.

—Benoît Cœuré, Member of the Executive Board of the European Central Bank

How do regulations spill over to unintended economic agents? While the direct effects of various regulations are studied extensively, much less work has been done on their spillover effects. Further, this issue is especially important in the context of financial firms because they are regulated by increasingly complex and intertwined regulations, making them particularly susceptible to regulatory spillovers.

Despite the importance of this question, detecting regulatory spillovers is inherently difficult. As firms often operate across multiple product lines and geographical areas, their behavior could be influenced by various industry, regional, or market factors. Thus, to attribute a specific change in firm behavior to regulatory spillovers, one must be able to rule out other confounding explanations. Our paper overcomes this challenge by relying on changes within a local market to detect regulatory spillovers.

Using the passage of Section 302 of the Sarbanes-Oxley Act (SOX 302), we show how shifts within local mortgage markets brought about by SOX 302 produce differential spillover effects to different groups of banks *untargeted* by the legislation.[1] SOX 302 requires managers to evaluate the effectiveness of their internal control systems. Firms whose internal control systems have material weaknesses are required to rectify their weaknesses while firms with no material weaknesses do not need to take any further action.

[1] The SOX Act, one of the most important securities laws in American business history, was enacted in July 2002 in response to a series of high-profile corporate accounting scandals in the early 2000s.

We find that public banks targeted by SOX 302 to rectify their material weaknesses reduce their mortgage lending, particularly risky mortgages. This, however, alters the competitive landscape within local mortgage markets, causing banks untargeted by SOX 302 to increase lending to capture clients who were turned down by targeted banks. Furthermore, we observe a shift in risky mortgages from public banks toward private banks as the latter lower their mortgage standards to compete for the market shares of the targeted banks. Consequently, private banks with a greater exposure to SOX 302 exhibit higher mortgage default rates. Overall, our results highlight how a regulation designed to improve the internal control quality of a set of firms affects the conduct of others via local market interactions.

We focus on mortgage lending markets for several reasons. First, we take advantage of the granular mortgage data where we can observe each loan a bank makes in a specific location in a given year. This enables us to observe the mortgage lending behavior of different banks that lend *within* the same county and thereby hold various location and demand-side factors constant. Second, mortgage lending is the most significant activity of a commercial bank, which on average accounts for more than 60% of total lending on the bank's balance sheet. Given the evidence that lax mortgage standards contribute to bank failures and the 2007–2009 subprime crisis (e.g., Keys et al. 2010), it is also important to understand how a bank's internal controls influence its mortgage lending behavior. Finally, changes in the mortgage lending market can have significant effects on the real economy, affecting homeowners, election outcomes, and social welfare (e.g., Antoniades and Calomiris 2020).

Our findings that the Sarbanes-Oxley Act, a regulation targeting both banks and nonbanks, creates a spillover effect on local mortgage markets have several important implications. First, it shows that regulatory changes can have far-reaching spillover effects beyond their originally

intended audience. While designed to protect corporate shareholders from fraudulent practices by managers in public firms (both financial and nonfinancial firms), SOX alters the competitive landscape within a local mortgage market, causing a reallocation of risky mortgages from public banks to private banks. Second, it underscores the importance of studying regulatory spillovers in banks, which have multiple regulators and are regulated by increasingly intertwined regulations. Finally, it cautions against evaluating regulatory effectiveness based solely on the behavior of targeted firms.

Our findings are built on three blocks. In the first block, we study the direct effects of complying with SOX 302 on the mortgage lending behavior of banks with material internal control weaknesses (MW banks). Appendix A shows an example of SunTrust Banks Inc. reporting a material weakness in its internal controls related to its Allowance for Loan and Lease Losses (ALLL) account. To address the weakness, SunTrust "terminated three members of its credit administration division, including its Chief Credit Officer" and "established additional remediation plans to address internal control deficiencies associated with the ALLL framework, including additional documentation, training and supervision, periodic testing and periodic updates to the Audit Committee."[2]

We expect MW banks to cut lending after SOX 302 for three nonmutually exclusive reasons. First, addressing internal control weaknesses imposes additional compliance costs on the bank, causing a depletion of its capital buffer and forcing it to cut lending.[3] Second, tightened internal controls after SOX will restrict credit officers' discretion in making lending decisions.

[2] Firms on average experience −1.8% abnormal stock returns upon disclosure of material weaknesses under SOX 302 (Beneish, Billings, and Hodder 2008).

[3] As illustrated in the SunTrust example, the bank incurred at least three different types of expenses in complying with SOX. First are labor costs when the bank fires its workers and key executives. Second, it is subject to technology and training expenses in improving its internal controls. Third and finally, it pays for external auditing services. These costs are substantial relative to a firm's total operating expenses (Krishnan, Rama, and Zhang 2008).

This means that they can no longer approve loans, for instance, before obtaining all the relevant paperwork from the borrowers (Hertzberg, Liberti, and Paravisini 2010). This sets a higher bar for any given loan to be approved, leading to a reduction in lending. Finally, banks with internal control weaknesses are likely to reduce lending as a response to greater supervisor scrutiny (Kupiec, Lee, and Rosenfeld 2017). That is, lending is likely to be reduced until the material weaknesses have been addressed and appropriate internal control mechanisms have been put in place.

Our data come from the Home Mortgage Disclosure Act (HMDA) and span the period 2000–2006. HMDA provides loan-level information on the mortgage application (e.g., approval status, loan amount, and location) and the mortgage applicant (e.g., sex, race, and income). To examine the effect of complying with SOX 302 on mortgage lending behavior of MW banks, we exploit *within*-MW bank variation and compare their lending behavior before and after the enactment of SOX 302. The main advantage of this approach is that it does not compare MW to non-MW banks and thus avoids potential issues related to MW banks having different characteristics and trends compared to other banks. Our main specification includes bank and county fixed effects, allowing us to compare the mortgage origination volume of the *same* bank before and after SOX 302, while controlling for time-invariant county characteristics.

We find that after the enactment of SOX 302, MW banks significantly reduce their mortgage origination volume. Given that MW banks on average originate 3.3% of the total number of mortgages in a given county, our most conservative estimate indicates that the average county in which MW banks lend would experience an annual reduction of $10 million in originated mortgage loans after SOX 302. Furthermore, consistent with the expectation that MW banks need to adopt a tighter loan origination procedure following SOX, we find that the lending reduction by MW banks is more pronounced among riskier borrowers, and non-white borrowers who on

average have less detailed credit histories than white borrowers (Cohen-Cole 2011). Our results thus indicate that complying with SOX 302 subdues aggressive lending.[4]

Having established that complying with SOX 302 causes MW banks to cut lending, we next investigate whether SOX 302 also leads to an indirect spillover effect on the lending behavior of non-MW banks, that is, (1) public banks that do not have material weaknesses and (2) private banks, since they do not need to comply with SOX. We argue that the lending reduction of MW banks after SOX 302 could incentivize their local competitors (i.e., non-MW banks that lend in the same county) to increase lending to capture the market share of MW banks. Furthermore, the responses of non-MW banks should be stronger in counties where MW banks have a greater market share because these counties have potentially greater gains to offer. Our hypothesis is consistent with that of Berger and Bouwman (2013), who show that banks with healthier balance sheets can capture borrowers from weaker banks to increase their market share (see also Calomiris and Wilson 2004; Kapan and Minoiu 2013).

Because we exploit the geographical distribution of the market share of MW banks as a source of variation to test for the spillover effects of SOX 302, we confirm an important identifying assumption that the geographical distribution of MW banks is plausibly random. Specifically, none of the county characteristics or their trends (including demographic, economic, mortgage, or housing characteristics) in the pre-SOX period predicts post-SOX MW bank presence in a given county.[5]

[4] In Table B3 in the appendix, we decompose *Post* into a set of indicators for years around SOX 302 in 2003 and find no significant effect in the years before 2003. The table also shows that our results are not driven by other regulatory changes, such as the majority board independence requirement. Moreover, while we focus our main analysis on using mortgage data because of its granularity, we also show that our findings are generalizable to the overall business strategy of MW banks. After the enactment of SOX 302, MW banks reduced their total lending and residential real estate lending.

[5] Moreover, to allow for variations in lending behavior across different types of banks and mortgages after 2003, all spillover tests include the interactions between the post-SOX indicator and all control variables. This accounts for the

We find that non-MW banks respond to MW banks' lending cuts by significantly increasing their mortgage lending volume in counties where MW banks have a large market presence (measured using the fraction of loan volume originated by MW banks). The effect is detected among non-MW public banks and private banks operating in less competitive counties.

Furthermore, we find that while non-MW public banks mainly expand lending to safer borrowers (i.e., they capture creditworthy clients turned down by MW banks following SOX), private banks mostly increase lending toward risky applicants (i.e., they take over the riskier applicants rejected by both MW and non-MW public banks after SOX). These results are consistent with the idea that, compared to public banks, private banks are less regulated and thus have more leeway to increase risky lending to compete for market share. Consequently, while SOX 302 curbs risky lending by banks with material weaknesses, the legislation causes a reallocation of risky mortgages to the less regulated segment of the local market.

In the final part of the paper, we evaluate the effects of SOX 302 on loan performance and aggregate market outcomes. We find loans originated by private banks in counties with a greater exposure to SOX 302 spillovers have marginally higher default rates. This arises as a likely consequence of the lowering of mortgage standards after SOX 302. In contrast, non-MW banks with greater exposure to SOX 302 spillovers do not experience higher mortgage defaults. Finally, we find that compliance to SOX 302 does not lead to an aggregate decline in mortgage credits as the lending reduction by MW banks is largely nullified by the lending expansion from non-MW banks. Overall, our results highlight how a regulation designed to improve the internal control quality of a set of firms affects the conduct of others via local market interactions.

possibility that after 2003, large banks, for instance, expand mortgage lending in all markets (Loutskina and Strahan 2011) or shift riskier lending to mortgage company subsidiaries.

Our paper contributes to several active strands of the literature. First, we add to the literature that examines the effects of SOX on the behavior of targeted firms (e.g., Ashbaugh-Skaife et al. 2009; Bargeron, Lehn, and Zutter 2010; Duarte et al. 2014; Guo and Masulis 2015; Iliev 2010). We are the first to show that SOX-induced improvements in internal control curb risky mortgage origination practices by banks targeted by the Act. Given the evidence that lax mortgage standards contribute to bank failures and the 2007–2009 subprime crisis (e.g., Keys et al. 2010), our findings indicate that regulations focusing on improving a bank's internal control system are effective in restraining risky mortgage lending.

Second, and more importantly, we show evidence that the direct effects of SOX 302 on targeted banks are accompanied by spillover effects on other untargeted local competitors. The SOX 302-induced effect causes private banks—the more lightly regulated firms in local mortgage markets—to lower their mortgage standards and become riskier. Therefore, while compliance with SOX 302 is useful in restraining the risky origination behavior of targeted banks, it does not result in an overall reduction in risky lending in the economy because the risk is reallocated, not eliminated. These findings are broadly related to the literature studying the spillover effects caused by accounting regulations (e.g., Chen, Young, and Zhuang 2013; Chen, Dou, and Zou 2021; Duguay, Minnis, and Sutherland 2019). Within the banking literature, our findings are related to the work of Garmaise and Natividad (2016), who document a positive financial spillover within a local banking market. While their focus is on the financial spillover across different firms sharing the same bank, we focus on how regulations affect the reallocation of risk *across* different types of banks operating within a competitive local market.

Finally, we see our work as complementary to research on the credit reallocation effect between different banks that compete in a local market. Cortés et al. (2020), for instance, show

that the lending reduction by stress-tested banks causes neighboring banks to increase lending. Berger and Bouwman (2013) show that banks with healthier balance sheets can capture borrowers from weaker banks to increase their market share. Our focus, however, is on how a regulatory intervention causes a shift of risky mortgages to a less regulated segment of the local market.

1. Institutional Setting, Hypothesis Development, and Data

1.1 Institutional setting

In July 2002, U.S. Congress passed the Sarbanes-Oxley Act (SOX) in response to corporate accounting scandals involving Enron, WorldCom, and Tyco International, among other firms. A major aim of SOX is to improve the quality of the internal controls and financial reporting of publicly listed U.S. firms. This aim is achieved through two provisions: Sections 302 and 404.

SOX 302 became effective on August 29, 2002. It requires the CEOs and CFOs of all publicly listed U.S. firms to evaluate the effectiveness of their firm's internal controls and report their evaluations to the firm's external auditor and audit committee. Most firms also report these evaluations in their annual or quarterly reports (Doyle, Ge, and McVay 2007a, 2007b).[6] If no control weakness is identified, no further action is required from the firm. In contrast, if a control weakness is discovered during the course of the evaluation, the firm then needs to take remedial action to rectify the weakness. The three levels of internal control weaknesses are (in ascending order of severity): control deficiencies, significant deficiencies, and material weaknesses.[7]

[6] Despite some ambiguity about whether or not it is mandatory for firms to disclose these evaluations in public annual reports under Section 302, most firms treat it as mandatory and opt to disclose (Doyle, Ge, and McVay 2007a). As an example of this ambiguity, the SEC stated that it would "welcome disclosure of all material changes to control" (SEC 2004). On another occasion, it stated without reservation that "a registrant is obligated to identify and publicly disclose all material weaknesses" (SEC 2004).

[7] A control deficiency "exists when the design or operation of a control does not allow management or employees, in the normal course of performing their assigned functions, to prevent or detect misstatements on a timely basis" (PCAOB 2004, appendix 8). A significant deficiency is "a control deficiency, or combination of control deficiencies,

Section 404 of SOX became effective for the fiscal year ending on or after November 15, 2004, for firms with a total market capitalization of more than $75 million. Section 404 mandates that internal control evaluation be attested by an external auditor and disclosed in annual reports (Doyle, Ge, and McVay 2007a). Thus, Section 404 eliminates any ambiguity as to whether firms could choose whether to disclose their material weaknesses.

We focus on material weakness disclosures made by banks between September 2002 and December 2004 under Section 302 (and not under 404) for three reasons. First, our reading of the SEC guidance suggests that most firms would have strong incentives to evaluate their internal control quality and disclose material weaknesses (if any) at the earliest instance, that is, under Section 302 (see also Doyle, Ge, and McVay 2007a, 2007b; Hermanson and Ye 2009). Early disclosures allow management to proactively deal with the issue and send a strong signal to investors that the firm does not have any more severe control deficiencies. Addressing the problems early also helps management to hedge against adverse career consequences when internal control issues develop into more serious corporate misconduct (Karpoff, Lee, and Martin 2008).

Second, managers face significant legal penalties when they intentionally conceal any material weaknesses that the firm might have. Specifically, both the CEO and CFO are required to personally certify in the SEC filings that (1) the financial report reflects the fair and true financial conditions of the firm, and (2) the quality of the firm's internal controls has been thoroughly evaluated and disclosed in the filing. Importantly, anyone who willfully certifies a noncompliant

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that adversely affects the company's ability to initiate, authorize, record, process, or report external financial data reliably in accordance with generally accepted accounting principles such that there is more than a remote likelihood that a misstatement of the company's annual or interim financial statements that is more than inconsequential will not be prevented or detected" (PCAOB 2004, appendix 9). A material weakness is "a significant deficiency or combination of significant deficiencies that results in more than a remote likelihood that a material misstatement of the annual or interim financial statements will not be prevented or detected" (PCAOB 2004, appendix 10).

financial statement will face a fine of up to $5,000,000, a maximum of 20 years in prison, or both (Sarbanes-Oxley Act 2002).[8] Fearing these potential legal consequences, firms with material weaknesses would have the incentive to "come clean" early under the Section 302 reporting regime (Ashbaugh-Skaife, Collins, and Kinney 2007).

Finally, Section 404 requires external auditors to attest to management's evaluation of the firm's internal controls. To protect themselves, auditors tend to apply a lower threshold for what constitutes weaknesses, causing many firms to be misclassified as having material weaknesses when they in fact do not (Doyle, Ge, and McVay 2007b). As a result, banks would take remedial action to rectify material weaknesses before SOX 404 becomes effective to avoid being wrongly attested in the future. Evidence suggests that Section 302 produces a greater effect on targeted firms' behavior when compared to Section 404 (Beneish, Billings, and Hodder 2008; Doyle, Ge, and McVay 2007b).[9] Therefore, in the main analyses, our treatment group includes banks that reported having material weaknesses under Section 302 between September 2002 (when SOX 302 became effective) and mid-November 2004 (when SOX 302's effectiveness was reduced by SOX 404) and are thus required to take remedial action to address their weaknesses.

1.2 Hypothesis development

[8] On January 13, 2003, the SEC levied their first charges of violation on Calixto Chaves (CEO) and Gina Sequeira (CFO) of Rica Foods for signing off on financial statements knowing that they were not accurate. Chaves eventually received a fine of $25,000 (SEC News Digest 2003). More importantly, both executives disappeared from the corporate world after the incident.

[9] Beneish, Billings, and Hodder (2008) show that while firms face significant negative abnormal returns and a higher cost of capital following SOX 302 disclosures, they do not experience any negative abnormal returns or a change in the cost of capital following SOX 404 disclosures. Doyle, Ge, and McVay (2007b) find that material weakness disclosures under Section 302 are strongly related to negative accrual quality, while disclosures under SOX 404, on average, are unrelated to accrual quality. More recently, Gupta, Sami, and Zhou (2018) find that most firms experience an improvement in their information environment after SOX 302 disclosures, but not after SOX 404 disclosures.

Based on the institutional setting, we proceed to develop our hypotheses. We first focus on the direct effects of complying with SOX 302 on the lending behavior of banks with material internal control weaknesses (MW banks).

Appendix A shows an example of SunTrust Banks Inc. disclosing its material weakness under Section 302 in its 2004 annual report. Specifically, the bank reports that "in the fourth quarter of 2004, the Company identified a material weakness in internal controls related to establishing the Allowance for Loan and Lease Losses (ALLL)." SunTrust also mentions the remedial actions it took to rectify the weakness. Among other measures, the bank terminated three members of its credit administration division, including its Chief Credit Officer [. . .] established additional remediation plans to address internal control deficiencies associated with the ALLL framework, including additional documentation, training and supervision, periodic testing and periodic updates to the Audit Committee [and] strengthened internal controls surrounding the validation and testing of systems and models relating to the ALLL process.

We argue that MW banks' remedial actions to address their control weaknesses could result in a lending reduction via three nonmutually exclusive channels: (1) higher compliance costs; (2) tightened loan origination processes; and (3) lending cuts as a response to supervisory scrutiny.

First, complying with SOX 302 imposes *additional* operating expenses on the bank (see Solomon 2005). This could cause a depletion of the bank's capital buffer and force it to cut lending (Gropp et al. 2018). For example, SunTrust incurred at least three different types of additional costs in complying with SOX. First, the bank is subject to internal labor costs when firing its key executives and workers. Second, it incurs technology and training expenses in rectifying its internal control weaknesses. Finally, SunTrust also has to pay audit expenses. These costs can be

substantial relative to a firm's total operating expenses (Iliev 2010; Krishnan, Rama, and Zhang 2008) and impose lending constraints on the bank through capital depletion.

Second, remediating internal control weaknesses will result in a more scrutinized and tightened loan origination process. Prior to SOX 302, weak internal control systems may have allowed credit officers to approve loans without obtaining sufficient paperwork from the borrower (e.g., Hertzberg, Liberti, and Paravisini 2010; Udell 1989).[10] Upon improving internal controls, credit officers are now required to follow stricter approval protocols, such as requiring borrowers to submit all documentation, before a loan can be approved. This raises the bar for any given loan to be approved and leads to a reduction in lending.

Finally, banks with internal control weaknesses may reduce lending as a response to greater supervisor scrutiny. Banks with material weaknesses could have their CAMELS ratings downgraded by supervisors.[11] This may put restriction on lending activities and result in lending cuts (Kupiec, Lee, and Rosenfeld 2017). Even if their ratings are not downgraded, banks may reduce lending to avoid further scrutiny from supervisors until material weaknesses are addressed and adequate internal control mechanisms are in place.

Hypothesis 1: *MW banks reduce lending after SOX 302.*

Our second hypothesis focuses on the spillover effects of SOX 302 on the lending behavior of non-MW banks: (1) public banks that do not have material weaknesses (and thus do not need to take any action to remedy their internal controls); and (2) private banks, as they are not required to comply with SOX. We argue that the lending reduction of MW banks after SOX could incentivize their local competitors to increase lending to capture MW banks' market share.

[10] Career and compensation incentives motivate credit officers to approve more loans (e.g., Cole, Kanz, and Klapper 2015; Tzioumis and Gee 2013).
[11] CAMELS ratings contain supervisory assessments of a bank's capital adequacy, asset quality, earnings, liquidity, sensitivity to market and interest rate risk, and bank management control systems and competency.

Furthermore, this effect should be stronger in areas where MW banks have a greater market share as this would offer greater potential gains for non-MW banks.

Our hypothesis is in line with Berger and Bouwman (2013), who show that banks with higher capital ratios (i.e., a stronger balance sheet) are able to capture the market share from banks with weaker balance sheets during times of crisis. In a similar vein, Cortés et al. (2020) find that the lending reduction by banks undergoing stress tests is largely nullified by the expansion in lending of untargeted banks, pointing to a lending substitution effect between these banks.

Hypothesis 2: *Following SOX 302, non-MW banks increase their lending in response to MW banks' lending cuts.*

1.3 Data

To construct our sample, we combine several data sources. We describe these data sets below.

HMDA: Our main source of mortgage data come from the HMDA database collected by the Federal Financial Institutions Examination Council. The HMDA database covers all mortgage applications that have been reviewed by qualified financial institutions. Specifically, an institution is required to complete an HMDA register if it has at least one branch office in any metropolitan statistical area and meets the minimum size threshold. In 2002, the year when SOX 302 was enacted, this reporting threshold was $32 million in book assets. Because of this low reporting threshold, almost all banks are included in the data set. For each loan application, the data set provides borrower demographic characteristics (e.g., income, gender, and race), loan characteristics (e.g., loan amount and purpose), property characteristics (e.g., type and location), the decision on the loan application (e.g., approved, denied, or withdrawn), and a lender identifier.

Our sample includes all loan applications reviewed by commercial banks and bank holding companies between 2000 and 2006 (3 years around the enactment of SOX 302). This timeline covers only the precrisis period and therefore avoids picking up confounding effects from the 2007 financial crisis. We then apply the following screening procedure. First, we drop applications that were closed for incompleteness or withdrawn by the applicant before a decision was made. Second, given that our main analyses focus on how banks without MW respond to local market opportunities, we restrict our sample to loans originated in locations in which banks have at least one physical branch. This is motivated by prior studies which argue that having a physical branch footprint gives banks an informational advantage in accessing new borrowers and responding to opportunities in local markets.[12] Consistent with this, as shown in Internet Appendix IA1, we detect no spillover effects in counties where banks do not have physical branches.[13]

Fannie Mae and Freddie Mac loan origination and performance data: Government-sponsored enterprises (GSEs), such as Fannie Mae and Freddie Mac, provide loan-level data on all 30-year single-family conforming fixed rate mortgages that they purchase or guarantee starting from 1999. The primary advantage of the GSE data set is that it provides direct measures of borrower risk. This includes two ex ante measures (FICO score and loan-to-value ratios) and an ex post measure based on mortgage defaults. The GSE data set also reports other variables, such as loan amount, size, type, purpose, and location. In part of our analysis, we merge the HMDA data set with the GSE data set. Loans are matched on location (state, MSA, and ZIP code), origination year, the exact loan amount, loan purpose, and owner-occupancy status. Similar to

[12] Gilje, Loutskina, and Strahan (2016) show that banks exposed to shale booms increase mortgage lending in nonboom counties, but only where they have branches. The literature also shows that local lenders enjoy advantages in screening and monitoring riskier borrowers (Cortés 2012; Ergungor 2010) and tend to focus on soft information intensive segments of the mortgage market (Loutskina and Strahan 2011).

[13] This finding also provides suggestive evidence that physical branches give banks access to local markets, allowing them to respond more quickly to market opportunities.

Buchak and Jorring (2021), to ensure the highest-quality match, we exclude all loans with duplicate observations and match without replacement.

Other data sets: We obtain from the AuditAnalytics 'SOX302 – Disclosure Control' database a sample of banks that disclose material internal control weaknesses between September 2002 (the first month after the enactment of SOX 302) and mid-November 2004 (when SOX 404 was enacted and reduced the effectiveness of SOX 302). We then merge AuditAnalytics with the HMDA database by following several steps. Specifically, we link Audit Analytics to Compustat identifiers using the bank's CIK code; Compustat identifiers to FR Y-9C Call Reports (which allows us to obtain bank financial data) using the PERMCO-RSSD link table from the Federal Reserve Bank of New York; and finally Call Reports to HMDA using the bank's RSSD ID.

In our final sample with nonmissing financial and mortgage data, 25 of 485 public banks (5.2%) disclose material weaknesses between September 2002 and mid-November 2004. Over our sample period of 2000–2006, MW banks have branches and lend in a total of 373 counties.[14] MW banks on average account for 3.3% of the total loans originated in a county. Given that an average U.S. county receives a yearly volume of 8,900 applications for a loan amount of $118,300, a rough estimate indicates that MW banks originate an annual $28 million of mortgage loans in a given county.[15]

2. Direct Effects of SOX 302 on the Lending Behavior of MW Banks

2.1 Baseline specification and results

[14] The median MW bank has branches in nine counties, whereas the median non-MW public bank has branches in six counties.

[15] 8,900 applications × $118,300 × 0.80 approval rate × 3.3% market share of MW banks = $28 million

We start by establishing the direct effects of complying with SOX Section 302 on the mortgage lending behavior of banks that have material internal control weaknesses (MW banks). We estimate the following linear fixed effects model:

$$ln(Mortgage\ originations)_{ikt} = \alpha + \beta_1 Post_t + Bank\ controls_{it} + Borrower\ controls_{ikt} + Fixed\ effects + \varepsilon_{ikt}, \quad (1)$$

where the subscripts *i*, *k*, and *t* denote the bank, county, and year, respectively. The dependent variable is $ln(Mortgage\ originations)_{ikt}$, the natural logarithm of the dollar amount of mortgages originated by a bank in a given county in a given year. The main independent variable of interest is $Post_t$, a dummy variable that equals one for all years from 2003 to 2006. Importantly, the sample includes only loans originated by MW banks. This allows us to exploit *within*-MW bank variations and compare their lending behavior before and after the enactment of SOX 302. The key advantage of this approach is that it does not compare MW to non-MW banks and thus avoids potential problems related to MW banks having different characteristics and trends compared to other banks (for a similar application of this approach, see Di Maggio et al. 2019).

Our main specification includes bank fixed effects and county fixed effects. This allows us to compare changes in lending volume of the *same* MW bank before and after SOX 302 while controlling for persistent differences across counties. Our model also controls for various bank, loan, and borrower characteristics. Bank characteristics include *ln(Assets)*, $ln(Assets)^2$, *Return on assets (ROA)*, *Deposits/Assets*, *Tier-1 capital/Assets*, *Loans/Assets*, and *Residential real estate loans/Loans*. Loan and borrower characteristics include the fraction of non-white applicants (*%non-white applicants*), the fraction of female applicants (*%female applicants*), the fraction of same-sex applicants (*%same-sex applicants*), the applicant's loan-to-income ratio (*Loan-*

applicant-income), and the applicant's income (*ln(Applicant income)*). Table B1 in the appendix defines these variables.

[Table 1 around here]

Table 1 provides summary statistics on loan applications, as well as other variables used in this study. The average borrower earns about $88,000 per year, applies for a $118,300 mortgage loan, and has a 1.35 loan-to-income ratio. The average bank in an average county receives 375 applications a year and approves 80% of the applications they receive. In the last two columns of Table 1, we divide the sample into two subsamples based on whether the proportion of loans originated by MW banks in the county is above the sample median.

In Internet Appendix IA2, we also report separate statistics for MW banks, non-MW public banks, and private banks. Compared to non-MW public banks, MW banks are marginally larger, less profitable, and hold less deposits in their balance sheets. Some differences also exist between MW banks and private banks: the former are larger, hold fewer deposits and residential real estate loans, and have a lower capital ratio. These observable differences underscore the advantage of our strategy in focusing on variations *within*-MW banks. Essentially, we can avoid comparing MW banks to other banks.

[Table 2 around here]

Table 2 presents the results. Model specifications vary across columns in terms of the set of fixed effects we include. Across all specifications, the point estimates for β_1 are negative and statistically significant at the 1% level, implying a reduction in mortgage origination volume at MW banks following the enactment of SOX 302. The effect is economically substantial. Given that MW banks on average originate 3.3% of the total number of mortgages in a given county, the estimate in column 3 indicates that after SOX 302 the average county in which MW banks lend

would experience a yearly reduction of $10 million in originated mortgage loans.[16] Overall, the results are consistent with our hypothesis that the mortgage origination volume at MW banks decreases as a result of MW banks taking remedial action to comply with SOX.

2.2 SOX 302 and risky lending

To provide a more direct test on how complying with SOX 302 results in lending cuts, we exploit cross-sectional variations in MW banks' lending cuts based on the riskiness of the mortgage loan. Loan risk should matter for several reasons. First, as MW banks experience a tightened loan origination process following SOX, they might refrain from originating risky loans to avoid regulatory scrutiny and internal control violations. Second, since MW banks face capital depletion through increased compliance costs after SOX, these banks can avoid violating capital requirements by reducing the risk of their loans (Cortés et al. 2020). Consequently, we predict that the lending cuts by MW banks would be more pronounced among riskier loans.

One approach to measuring loan risk using HMDA data relies on the application's loan-applicant-income ratio, where a higher ratio indicates that the loan is riskier because borrowers are less able to use their regular income to service the loan (Dagher and Sun 2016). However, because many borrowers overstate their income on their mortgage applications in the run-up to the financial crisis (Mian and Sufi 2017), the loan-applicant-income ratio might not be an accurate proxy for borrower risk.[17]

To overcome this issue, we perform the analysis using the GSE-HMDA matched data set from which we have access to additional underwriting variables. We use two measures of ex ante

[16] 8,900 applications × $118,300 × 80% approval rate × 3.3% x ($e^{0.314} - 1$) = $10 million

[17] We discuss this issue further in Section 5.1. By way of preview, we restrict our sample to counties that do *not* experience buyer income overstatement and arrive at a similar finding that MW banks reduce risky lending after SOX when using loan-applicant-income as a measure of mortgage risk.

loan risk, specifically the average applicants' (1) FICO score and (2) the combined loan-value ratio in a given bank-county-year. These two variables proxy for a borrower's creditworthiness as well as their ability to service the loan. We also use a measure of ex post loan risk based on historical mortgage defaults, that is, the average share of mortgages in a bank-county-year that become 90 days delinquent during the first 2 years of their life during the pre-SOX period from 2000 to 2002.[18] This variable captures the historical performance of loans originated by the bank in a given county in the pre-SOX years. As such, it informs the bank about the ex post riskiness of loans originated in a particular area for future lending decisions.

[Table 3 around here]

To test our hypothesis, we interact *Post* with the three measures of borrower risk, and Table 3, panel A, reports the results. We find a negative and statistically significant coefficient on the interaction term between *Post* and *Mortgage default*$_{2000\text{-}2002}$ (column 3). By contrast, the coefficients on the interactions between *Post* with *FICO score* and *Combined loan-value* ratio are statistically insignificant (columns 1 and 2).[19] This indicates that after SOX, MW banks reduce their lending to borrowers in counties with historically poorer performance records, that is, those more likely to default on their mortgages in the period immediately before SOX. Overall, we find support for the hypothesis that improvements in internal control systems mitigate aggressive loan origination practices.

2.3 SOX 302 and lending to marginal borrowers

[18] The advantage of focusing on the early years of a loan's life is that the borrower characteristics will more closely resemble those at the time the application was submitted for review (Rajan, Seru, and Vig 2015).

[19] It is worth pointing out that the GSE-HMDA data set contains only conforming loans, which tend to be safer loans and do not cover the full spectrum of borrower risk. This may explain why we do not find differential lending reductions based on FICO score and loan-value ratio using the GSE-HMDA sample.

We next examine the effects of MW banks' lending cuts on marginal borrowers, those who have historically been excluded from credit markets: low-income borrowers, female borrowers, borrowers belonging to racial minorities, and same-sex borrowers. Certain subsets of marginal borrowers have less detailed credit histories, and therefore their mortgage applications are more difficult to verify and require more screening effort from the bank (e.g., Cohen-Cole 2011; Ergungor 2010; Frame et al. 2022). MW banks may reduce lending to these groups of borrowers to avoid further internal control violations. Consequently, this has implications for marginal borrowers' access to credit and economic inequality (Buchak and Jorring 2021).

To test whether MW banks refrain from lending to marginal borrowers, we interact *Post* with (1) *ln(Applicant Income)*, the natural logarithm of the average applicant's income in a bank-county-year, (2) *%female applicants*, the proportion of female applicants in a bank-county-year, (3) *%non-white applicants*, the proportion of applicants whose reported race is other than white in a bank-county-year, and (4) %*same-sex applicants*, the proportion of mortgage applications in which the main applicant and the coapplicant report the same sex in a bank-county-year. Panel B of Table 3 reports the results.

We find that the interaction coefficient between *Post* and *%non-white applicants* is negatively significant, suggesting that after SOX, MW banks reduce credit supply to non-white applicants. This is consistent with prior studies showing that borrowers from racial minorities are more likely to have their mortgages denied. Such disparities could be because borrowers from minority races have less detailed credit histories than their white counterparts (Cohen-Cole 2011) and are therefore more likely to be turned down by MW banks to avoid further internal control violations. We do not find the interactions between *Post* and the other three measures of marginal applicants to be statistically significant. Combined, the results in Table 3 indicate that the lending

reduction after SOX does not take place equally across all groups of borrowers. Rather, MW banks selectively make a greater lending cut from borrowers with historically poorer performance and those from minority races.

2.4 Bank-level analysis

So far, we observe a notable reduction in mortgage lending by MW banks after SOX 302. This suggests that SOX 302 has a significant effect on the mortgage origination behavior of banks with material weaknesses. In this subsection, we show that the reduction in lending induced by SOX 302 can be generalized and observed at the aggregate bank-level.

To do so, we obtain quarterly bank data from Call Reports (FR Y-9C) to construct a bank-level sample for a period from 2000Q1 to 2006Q4 (matching the loan-level period). We examine the effects of complying with SOX 302 on the following bank-level outcomes: (1) *ln(Loans)*, total nominal lending, (2) *Loans/Assets*, loans as a proportion of their total assets; (3) *ln(Residential real estate loans)*, total nominal residential lending and; (4) *Residential real estate loans/Loans*, residential loans as a proportion of total loans. As before, we only include MW banks in our analysis. We include bank fixed effects in all regression specifications, allowing us to compare changes in lending volume of the *same* MW bank before and after SOX 302.

[Table 4 around here]

Table 4 presents the results. Across all columns, the coefficients on *Post* are negative and statistically significant, indicating that MW banks exhibit a reduction in total lending and residential real estate lending after SOX. This is consistent with our loan-level findings that MW banks reduce their mortgage lending following the enactment of SOX 302.

3. Spillover Effects of SOX on the Lending Behavior of Non-MW Banks

3.1 Baseline specification and results

Having shown that MW banks reduce their mortgage lending after SOX, we next investigate whether this creates any spillover effect on non-MW banks. We argue that this reduction could inadvertently alter the lending landscape *within* local mortgage markets. Specifically, it could incentivize non-MW banks that also lend in the same county as MW banks to increase their lending to capture borrowers turned down by MW banks following SOX.

The responses of non-MW banks should be stronger in counties where MW banks have a greater market share. As an illustration, consider that MW banks account for 10% of the total mortgage lending in Marion County, Kentucky, but they only account for 1% of the total mortgage lending in the nearby Boyle County, Kentucky. After SOX, while Marion County would experience a substantial $30 million reduction in mortgage credit, Boyle County would only experience a $3 million reduction. Therefore, non-MW banks will be more incentivized to increase their lending in Marion County (instead of Boyle) as there are greater potential gains for them.[20]

To this end, we investigate the lending behavior of non-MW banks in counties with different levels of *MW presence* following SOX 302. In our analyses, we distinguish between two types of non-MW banks: (1) public banks that do not have material weaknesses and are therefore not required to change their behavior to respond to SOX 302 (non-MW public banks); and (2) all private banks, since they do not have to comply with SOX. We focus on commercial banks instead of other nonbank lenders, such as credit unions, because of comparability in their business models. We use the following specification to test for the spillover effects of SOX 302:

[20] We show in Section 2.1 that the average in which MW banks lend would experience a yearly reduction of $10 million in originated mortgage loans after SOX 302. This represents a substantial amount of extra market share for non-MW banks. For robustness, we restrict the sample to counties where MW banks account for a significantly higher proportion of total lending in the county and continue to find consistent results.

$$ln(Mortgage\ originations)_{ikt} = \alpha + \beta_1 MW\ presence_{kt} * Post_t + \beta_3 MW\ presence_{kt}$$
$$+ Bank\ controls_{it} + Borrower\ controls_{ikt} + Fixed\ effects + \varepsilon_{ikt}, \quad (2)$$

where the subscripts *i, k,* and *t* denote the bank, county, and year, respectively. The dependent variable $ln(Mortgage\ originations)_{itk}$ is the natural logarithm of the dollar amount of mortgages originated by a bank in a given county in a given year. $Post_t$ is a dummy variable that is equal to one for all years from 2003 to 2006. $MW\ presence_{kt}$ is the market share of MW banks in a given county, defined as the loans originated by MW banks divided by the loans originated by all commercial banks (MW banks, non-MW public banks, and private banks) in the county.

Importantly, the regressions on non-MW public banks only include loans originated by non-MW public banks. Similarly, the regressions on private banks only include loans originated by private banks. In other words, we do not compare the lending behavior of MW banks with that of non-MW banks, thereby alleviating the concern that these banks have different characteristics and trends. Our main coefficient of interest $MW\ presence_{kt} * Post_t$ captures changes in the mortgage origination volume of non-MW public banks (or private banks), conditional on the market share of MW banks following the enactment of SOX 302.

Because we exploit the geographical distribution of MW banks' market share as a source of variation to test for the spillover effect of SOX 302, we first verify an important identifying assumption that the geographical distribution of MW banks is plausibly random. To do this, we examine whether the market share of MW banks in a given county can be predicted by historical county characteristics or changes in the county characteristics. If we were to find a correlation, for instance, that MW banks are more likely to lend in counties with deteriorating economic prospects, then the geographical distribution of MW banks' market share is not random.

The results shown in Table B2 in the appendix suggest this is not the case. Specifically, we do not find any county-level characteristics or their changes in 2000 (including population, unemployment, income per capita, HHI of originated mortgages, house prices, home foreclosures, and mortgage-related characteristics) to predict the market share of MW banks in 2003.[21] This implies that counties with a greater MW bank presence are similar to other counties and that our findings are unlikely to be driven by county-level differences. Therefore, the geographical distribution of MW banks' market share is likely to give us exogenous variation to test for the spillover effects of SOX 302. Because we exploit geographical variations, our results are unlikely to be driven by market-wide movements or bank-level changes in mortgage origination behavior after 2003.

The main specifications include bank, year, and county fixed effects.[22] We also include similar control variables as those in Equation (1). We further include a full set of the interactions between the post-SOX indicator and all control variables in all spillover tests. This means that we allow the coefficient for each control variable to be different before and after 2003. For instance, the interaction between the post-SOX indicator and bank size accounts for the possibility that, after 2003, large banks expand their mortgage lending across all markets (Loutskina and Strahan 2011).

Table 5 presents the results. For non-MW public banks, the coefficient estimates β_1 on *MW presence*Post* are positive and statistically significant (columns 1 and 2). Our most conservative estimate in column 2 indicates that following SOX, non-MW public banks increase their mortgage origination volume by 4.5% in counties where MW presence is one standard deviation (5.5%)

[21] 2003 was the first full year in which Section 302 became effective. Our results are quantitatively similar when we use county-level characteristics measured in 2001 and 2002. We show 2000 in the paper because we want to test if given sufficient time, local characteristics would be able to influence the *MW presence*.

[22] In unreported tests, we verify that our findings continue to hold even under stricter sets of fixed effects. For instance, our results are robust to a specification that includes bank-county fixed effects. This allows us to compare the mortgage origination volume of the same bank in the same county before and after SOX 302.

higher.[23] Importantly, the presence of MW banks does *not* explain the mortgage origination volume of non-MW banks before SOX, as indicated by the insignificant coefficient on *MW presence* in the full specification in column 2.

[Table 5 around here]

For private banks, the coefficient estimates β_1 on *MW presence*Post* are statistically insignificant across columns 3 and 4. One possible explanation for the differential response between public and private banks is that private banks have, on average, a more limited lending capacity and are thus less able to compete for new opportunities in competitive local markets. The next section provides evidence confirming this conjecture.

3.2 What explains the responses of private banks?

Next, we offer an explanation for the insignificant responses of private banks in Table 5. Our expectation is that, compared to public banks, private banks have a more limited lending capacity and thus less able to promptly compete and increase lending in response to opportunities in local markets. If this interpretation is true, we should find stronger lending increases in counties where they face less competition. To test this, we include a triple interaction term between *MW presence*Post* and the Herfindahl-Hirschman index (*HHI*) of the concentration of county-level originated mortgages. A higher HHI indicates a less competitive local banking market.

[Table 6 around here]

As shown in Table 6, the coefficient estimate on *MW presence*Post*HHI* is positive and statistically significant in the sample of private banks (column 2) and is insignificant in the sample of non-MW public banks (column 1). Thus, consistent with our expectations, private banks do

[23] A one-standard-deviation increase in *MW presence* leads to an increase in *ln(Originated mortgages)* of 0.044 (= 0.800 × 0.055). This, in turn, implies an increase of 4.5% (=$e^{0.044}$ – 1).

increase their lending to capture the market share of MW banks when they are able to effectively compete in local markets, such as in counties in which they face less competition.

3.3 Mortgage standards

To better understand the dynamics of the lending substitution effects between MW and non-MW banks induced by SOX 302, we next examine whether non-MW banks lower their mortgage standards and lend to riskier borrowers. Using the GSE-HMDA matched data set, we interact *MW presence*Post* with the three measures of borrower risk described in Section 2.2: (1) the average applicants' FICO score, (2) the combined loan-value ratio in a given bank-county-year, and (3) the average share of mortgage delinquencies in a bank-county-year in the pre-SOX period from 2000 to 2002.

[Table 7 around here]

Table 7 reports the results. We find that non-MW public banks expand their lending to safer borrowers with historically lower default rates (column 3). This is consistent with the fact that while non-MW public banks are not required to address any material weakness, they still attract increased regulatory scrutiny after SOX and therefore refrain from originating risky loans. For private banks, the increased lending effect concentrates on riskier borrowers with a lower FICO score (column 4) and higher combined loan-value ratio (column 5). We interpret these results as private banks taking over the riskier applicants that have been turned down by both MW and non-MW public banks following SOX.

In sum, our results show that the enactment of SOX 302 induces nuanced responses from different banks that compete *within* the same local mortgage market. While non-MW public banks capture safer clients that have been turned down by MW banks, private banks (which are exempted

from the most stringent regulations) take over the riskier clients to expand their market share. We later show that these increases in risky lending by private banks as a result of SOX 302 spillovers result in higher levels of loan defaults.

4. Consequences of SOX 302 Spillovers

4.1 Mortgage defaults

So far, we have documented a series of changes in local credit markets following SOX 302, starting with a reduction in mortgage lending at MW banks. This then incentivizes both private and non-MW public banks to increase their lending to capture MW banks' market share, with private banks shifting their lending toward the riskier applicants. Are there any consequences to this risky lending behavior? To answer this, we examine the ex post default rate of loans originated in spillover counties following SOX 302. Using the GSE-HMDA matched data set (which provides information on mortgage defaults), we estimate the following model:

$$\begin{aligned} \textit{Mortgage default}_{ikt} = \alpha + \beta_1 \textit{MW presence}_{kt} * \textit{Post}_t + \beta_3 \textit{MW presence}_{kt} \\ + \textit{Bank controls}_{it} + \textit{Borrower controls}_{ikt} + \textit{Fixed effects} + \varepsilon_{ikt}, \end{aligned} \quad (3)$$

where the subscripts *i*, *k*, and *t* denote the bank, county, and year, respectively. $\textit{Mortgage default}_{ikt}$ is the fraction of mortgages that become 90 days delinquent during the first 2 years of their life in a bank-county-year. As before, $\textit{Post}_t$ is a dummy variable that is equal to one for all years from 2003 to 2006 and $\textit{MW presence}_{kt}$ is the market share of MW banks in a given county. All regressions include a set of fixed effects and control variables similar to those in Equation (2). We also include two additional controls for borrower risk made available in the GSE-HMDA data set: the average applicants' FICO score and combined loan-value ratio in a bank-county-year.

[Table 8 around here]

As shown in Table 8, the coefficient estimate on *MW presence*Post* is positive and marginally statistically significant in the sample of private banks (column 2), but it is statistically insignificant in the sample of non-MW public banks (column 1). Thus, loans originated by private banks as a result of spillovers from MW banks' lending reduction have marginally higher default rates. This is a likely consequence of the lowered mortgage standards adopted by private banks after SOX 302 that we document in Table 7. In contrast, non-MW banks with greater exposure to SOX 302 spillover do not experience higher mortgage defaults.

Taken together, our evidence demonstrates that the implementation of SOX 302 has far-reaching consequences beyond its originally intended audience. While SOX 302 curbs risky lending among MW banks, it also alters the dynamics within local mortgage markets. In response to changes in the local mortgage market induced by SOX 302, private banks (exempted from increased control regulations) lower their mortgage standards which results in higher levels of loan defaults. This points to the effect of SOX on unintended firms.

4.2 Aggregate credit supply

Finally, we ask whether complying with SOX 302 affects the aggregate credit production in counties where MW banks have a greater presence. One may worry that due to liquidity constraints and market frictions (Bord, Ivashina, and Taliaferro 2021), the lending expansion by non-MW banks may not be sufficient to counterbalance the contraction of MW banks, causing an aggregate reduction in mortgage credit. At the other extreme, if non-MW banks (especially the private ones) overact and their lending exceeds that of the cuts made by MW banks, it will lead to an aggregate

credit expansion. To examine whether these scenarios occur, we aggregate our data at the county-year level and estimate the following model:

$$ln(County\ origination\ loans)_{kt} = \alpha + \beta_1 MW\ presence_{kt} * Post_t + \beta_2 MW\ presence_{kt} + County\ controls_{kt} + Fixed\ effects + \varepsilon_{kt}, \quad (3)$$

where the subscripts *k* and *t* denote county and year, respectively. Our dependent variable is $ln(County\ origination\ loans)_{kt}$, which is the natural logarithm of the dollar amount of mortgages originated in a county-year. β_1 is our main coefficient of interest and measures changes in mortgage origination volume in counties with a higher presence of MW banks after SOX 302.[24] We follow the literature (e.g., Cortés et al. 2020; Di Maggio and Kermani 2017) and control for the overall economic conditions at the county level by including county and year fixed effects in all regression specifications. We also control for time-varying county-level characteristics, including population, unemployment rate, income per capita, the HHI of originated mortgage concentration, as well as the changes in these county-level characteristics.

[Table 9 around here]

Table 9 displays the results. The coefficient estimates on *MW presence*Post* are statistically insignificant across all specifications. Thus, complying with SOX 302 does not reduce aggregate mortgage credit as the lending reduction by MW banks is offset by the lending expansion of non-MW banks. Therefore, the competition between different banks within a local credit market actually alleviates the credit contraction effects of MW banks after SOX 302. This, again, is

[24] It is important to reemphasize that the spatial distribution of counties where MW banks have a higher market share is random. That is, none of the pre-SOX county characteristics predicts post-SOX MW bank presence in a given county. Thus, β_1 is unlikely to be driven by differential pre-trends or characteristics in these counties.

consistent with our findings of (risky) credit reallocations in local markets in which untargeted banks act as substitutes for banks that are required to comply with SOX.

5. Robustness Tests

5.1 Robustness tests on MW banks' lending cuts following SOX 302

Table B3 in the appendix presents additional tests to buttress our findings that complying with SOX 302 causes a lending reduction among MW banks. In panel A, we replace *Post* with five year dummies: *Before2001*, *2003*, *2004*, *2005*, and *2006*. As indicated in panel A, we observe insignificant loading for *Before2001*, confirming that our results are not driven by events preceding SOX 302.

In panels B and C, we address the concern that the income data from HMDA can be unreliable. Mian and Sufi (2017) compare the difference in the growth in income reported in HMDA over 2002–2005 with that in the Internal Revenue Service (IRS) and find that income data from HMDA have been overstated during the run-up to the financial crisis. Further, they find that the extent of income overstatement varies significantly by geography and that buyer income overstatement is more prevalent in ZIP codes that also witness other forms of mortgage fraud, such as mortgages with falsifying information on second liens or owner-occupancy status (cf. Ben-David 2011; Jiang, Nelson, and Vytlacil 2014; Griffin and Maturana 2016; Piskorski, Seru, and Witkin 2015). To the extent that MW banks' decisions on where to lend are correlated with buyer income overstatement or mortgage fraud, this could bias our estimates.

To address this concern, panel B reestimates the regressions in column 3 of Table 2 after excluding counties that exhibit buyer income overstatement. Following Mian and Sufi (2017), we calculate *Buyer income overstatement*$_{2002\text{-}2005}$ as the difference in annualized growth in income

reported on mortgage applications of home buyers from 2002 to 2005 and the annualized IRS income growth of households living in a county from 2002 to 2005.[25] In our sample, 53% counties exhibit a positive *Buyer income overstatement*$_{2002\text{-}2005}$. In a simple correlation analysis, we observe a low and statistically insignificant correlation between *Buyer income overstatement*$_{2002\text{-}2005}$ and *MW presence* (−0.096), suggesting that MW banks do not make more lending in counties with buyer income overstatement. We also proceed to show in panel B of Table B3 in the appendix that our main findings that MW banks reduce their lending remain robust after we exclude counties with a positive *Buyer income overstatement*$_{2002\text{-}2005}$.

In panel C, we repeat the analyses in panel A of Table 3 by interacting *Post* with *Loan-applicant-income* instead of the ex ante and ex post risk variables from the GSE-HMDA data set. Although the GSE-HMDA data allow us to access additional underwriting variables, the data set contains only conforming loans which do not capture the full spectrum of borrower risk. Our analyses in panel C exclude counties in which *Buyer income overstatement*$_{2002\text{-}2005}$ is positive to minimize the effect of income overstatement. As shown in panel C, the interaction coefficient on *Post*Loan-applicant-income* is significantly negative. The results allow us to draw a more general conclusion about reduction in MW banks' risky lending after SOX 302.

In panel D, we show that our results are not driven by changes in loan demand at MW banks after SOX. We reestimate Equation (1) using an alternative dependent variable *ln(Applications)*, which is the natural logarithm of the number of mortgage applications submitted in a bank-county-year. We find that the coefficient estimate on *Post* is statistically insignificant, suggesting no change in the quantity of the mortgage applicant pool received by MW banks after SOX. Thus, our findings do not capture demand-side effects.

[25] Unlike Mian and Sufi (2017), who use ZIP-code-level income growth, we use county-level income growth to be consistent with our bank-county-year data set.

Panel E presents other robustness tests. We first confirm that our results are not driven by other SOX-related provisions, in particular, the requirement of majority board independence for firms listed on the NYSE and the NASDAQ (e.g., Duchin, Matsusaka, and Ozbas 2010). If an independent board also contributes to lower mortgage lending, then we overestimate the effects of SOX 302. To address this, we reestimate Equation (1) on a subsample of banks that have material internal control weaknesses but already meet the requirement of majority board independence.[26] If our main results in Table 2 are driven by the board independence requirement, we should not observe any lending reduction in this subsample. Column 1 of panel E indicates that *Post* remains statistically significant at the 1% level, thereby ruling out this possibility.

Next, we address the concern that our results could be driven by MW banks receiving enforcement actions from U.S. regulators. Banks could receive a regulatory enforcement action when they engage in unsafe and illegal banking practices that violate laws, such as violating consumer protection laws or facilitating money laundering activities. For robustness, we exclude banks that receive enforcement actions over the sample period 2000–2006 and display the results in column 2 of panel E. Finally, in column 3, we show that our results remain robust to including additional controls for time-varying county-level factors, such as population, income per capita, and the unemployment rate.

5.2 Robustness tests on the spillover effects of SOX 302 on non-MW banks

Table B4 in the appendix presents additional robustness tests to support our interpretation of the spillover effects of SOX 302 on non-MW banks' lending behavior. In panel A, we assess the time trend of the spillover results by replacing *Post* with five year dummies: *Before2001, 2003, 2004,*

[26] These are banks whose board of directors consist of more than 50% of outside directors in 2001. Therefore, they do not need to make any further adjustment to comply with this listing rule.

2005, and *2006*. We find that the interaction term between *Before2001* and *MW presence* is not significant across either sample for non-MW public or private banks, confirming that our results are not driven by events preceding SOX 302.

In panels B and C, we show that buyer income manipulation does not affect our spillover results. Specifically, in panel B, we find that the results on the lending increase by non-MW banks remain robust after we exclude counties with a positive *Buyer income overstatement*$_{2002\text{-}2005}$. In panel C, we continue to find that spillovers lead to (a) reductions in risky lending by non-MW public banks and (b) increases in risky lending by private banks when using *Loan-applicant-income* as a proxy for borrower risk in subsamples of counties that do not exhibit borrower income overstatement.

In panel D, we address the concern that the increase in mortgage lending at non-MW public banks is driven by the lending expansion at large banks after 2003. As shown in columns 1 and 2, we do not detect any significant spillover effects across the two subsamples of large and small non-MW public banks (split by the sample's median book assets). Moreover, the interaction coefficients on *MW presence***Post* are very similar in magnitude across the two subsamples. In column 3, we show that the results continue to hold when we exclude from the sample the four largest banks, that is, Wells Fargo, JP Morgan, Citigroup, and Bank of America. In column 4, we follow Ellul and Yerramilli (2013) and include bank size-decile fixed effects, as well as their interactions with the post SOX dummy, to further control for unobserved heterogeneity across banks in different size categories. The results remain robust.

Panel E presents other robustness tests. We begin by showing in row 1 of panel E that our results are robust to using *ln(MW presence)*, the natural logarithm of *MW presence*. To further evaluate the economic significance of our spillover effects, we restrict the sample to counties

where MW banks have a significantly higher presence. In row 2, we restrict the sample to counties where MW banks originate at least one mortgage application. In row 3, we restrict the sample to counties in which MW bank presence is above the full sample's median. Our results remain robust. Finally, in row 4, we find that the results remain virtually unchanged after the inclusion of two additional controls: the HHI of county-level originated mortgages concentration and its interaction with *Post*. This implies that our spillover effects capture distinct elements of competition distinct from the HHI.

6. Conclusions

How regulations yield inadvertent effects is a question of first-order importance. However, assessing such an impact is empirically challenging due to various confounding factors. We employ a key piece of legislation that aims to improve the financial reporting of public companies, namely, Section 302 of the SOX Act, and show how this exogenous event affects the mortgage lending behavior of two sets of banks: banks required and banks not required to comply with this regulation.

We show that the passage of Section 302 of the SOX Act influences local mortgage lending markets through a direct channel and a spillover channel. We find that for public banks directly targeted by SOX 302 to rectify their material weaknesses, they reduce their mortgage lending, in particular risky mortgages. This reduction in lending spills over and alters the competitive landscape within local mortgage markets, causing banks untargeted by SOX 302 to increase their lending in order to capture the clients turned down by targeted banks. Furthermore, we observe a shift of risky mortgages from public banks toward private banks, as the latter lower their mortgage

standards to compete for the market share of targeted banks. As a result of lowering mortgage standards due to spillovers, loans originated by private banks have marginally higher default rates.

Overall, our findings show that regulatory changes can have far-reaching spillover effects beyond their original intended audience. Furthermore, it underscores the importance of studying regulatory spillovers in banks that are subject to multiple regulators and are regulated by increasingly intertwined regulations. Finally, it cautions against evaluating regulatory effectiveness based solely on the behavior of targeted firms.

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Table 1. Summary statistics

This table reports summary statistics for bank, loan, mortgage applicants, and county characteristics in the sample. Table B1 in the appendix defines all variables.

	Full sample						*MW presence* >median	*MW presence* <median
	N	Mean	Std.	p25	p50	p75	Mean	Mean
Bank-county-year characteristics								
ln(Originated loans)	64,735	8.580	1.985	7.527	8.696	9.816	8.771	8.390
# of mortgage applications	64,735	375.400	1277.000	39.000	112.000	300.000	445.1	305.7
Approval rate	64,553	0.796	0.158	0.711	0.821	0.912	0.802	0.791
%female applicants	64,735	0.210	0.106	0.150	0.207	0.265	0.218	0.202
%non-white applicants	64,735	0.274	0.238	0.090	0.213	0.398	0.288	0.261
%same-sex applicants	64,735	0.018	0.033	0.000	0.012	0.023	0.019	0.017
Loan-applicant-income	64,735	1.353	1.957	0.904	1.235	1.587	1.522	1.184
Loan amount ($thousand)	64,735	118.300	205.700	56.050	84.740	130.400	151.100	85.610
Applicant income ($thousand)	64,735	88.480	117.200	55.430	68.420	90.000	103.400	73.570
FICO score	4,137	726.800	40.460	705.000	729.900	753.000	728.000	724.900
Combined loan-value	4,148	72.520	14.480	66.410	75.000	80.400	71.660	73.800
Mortgage default (%)	4,148	5.964	17.080	0.000	0.000	0.000	7.828	3.187
County-year characteristics								
ln(Population)	13,453	10.940	1.331	10.010	10.720	11.760	11.330	10.550
ln(Income per capita)	13,453	10.220	0.225	10.070	10.200	10.340	10.290	10.140
Unemployment rate	13,453	5.262	1.693	4.100	5.000	6.100	5.217	5.307
HHI	13,453	660.500	494.800	341.100	502.600	804.500	473.600	847.400
ln(House prices)	7,547	11.810	0.503	11.460	11.740	12.080	11.930	11.600
%Home foreclosed	2,708	27.710	35.850	6.139	15.030	35.530	29.660	22.360
Bank-year characteristics								
ln(Assets)	8,424	12.690	1.650	11.490	12.360	13.460	–	–
ROA (%)	8,424	0.867	0.738	0.557	0.869	1.193	–	–
Tier-1 capital/Assets	8,424	9.875	2.816	7.891	9.142	11.180	–	–
Deposit/Assets	8,424	0.806	0.097	0.761	0.828	0.874	–	–
Loans/Assets	8,424	0.654	0.146	0.575	0.674	0.757	–	–
Residential real estate loans/Loans	8,424	0.370	0.251	0.173	0.309	0.528	–	–

Table 2. SOX 302 & MW banks' lending behavior

This table reports bank-county-year regressions which estimate the effect of SOX 302 on the lending behavior of banks that report Material Weakness between September 2002 and December 2004 to comply with the SOX 302 provision (MW banks). The dependent variable is *ln(Originated loans)*, the natural logarithm of the dollar amount of mortgages originated in a bank-county-year. The sample contains loans originated by MW banks. *Post* is a dummy variable that equals one for all years from 2003 and later. Table B1 in the appendix defines all variables. Robust standard errors are clustered at the county-level and are reported in brackets. *$p < .1$; **$p < .05$; ***$p < .01$.

Dependent variable: *ln(Originated loans)*			
Sample: MW banks			
	(1)	(2)	(3)
Post	-0.783***	-0.578***	-0.314***
	[0.113]	[0.103]	[0.099]
ln(Assets)	6.814***	7.166***	4.352**
	[1.183]	[1.426]	[2.154]
$ln(Assets)^2$	-0.209***	-0.214***	-0.134*
	[0.036]	[0.044]	[0.068]
ROA	0.296	0.133	0.288
	[0.211]	[0.169]	[0.182]
Tier-1 capital/Assets	-0.224***	-0.094	-0.054
	[0.060]	[0.068]	[0.063]
Deposit/Assets	0.517	-0.591	3.635***
	[1.183]	[1.722]	[1.224]
Loans/Assets	0.852	-0.435	0.183
	[1.007]	[0.995]	[1.406]
Residential real estate loans/Loans	0.444	-0.699	-4.848***
	[0.700]	[0.941]	[1.597]
ln(Applicant income)	1.091***	0.073	0.373*
	[0.181]	[0.235]	[0.203]
Loan-applicant-income	0.061***	0.041***	0.032***
	[0.014]	[0.011]	[0.005]
%female applicants	0.324	0.212	0.592
	[0.750]	[0.792]	[0.669]
%non-white applicants	-1.805***	-2.701***	-2.688***
	[0.322]	[0.453]	[0.386]
%same-sex applicants	-2.794	-3.751*	-4.646***
	[2.537]	[2.031]	[1.755]
County fixed effects	No	Yes	Yes
Bank fixed effects	No	No	Yes
Sample	HMDA	HMDA	HMDA
Adjusted R^2	.198	.633	.696
Observations	2,170	2,170	2,170

Table 3. Heterogeneity in MW banks' lending cuts

This table explores the heterogeneity in the lending reduction at MW banks after SOX 302. Panel A interacts the *Post* dummy with two ex ante measures of borrower risk (*FICO score*, *Combined loan-value*) and an ex post measure of borrower risk (*Mortgage default*$_{2000\text{-}2002}$). Panel B interacts the *Post* dummy with *ln(Applicant income)*, *%female applicants*, *%non-white applicants*, and *%same-sex applicants*. These variables are centered at the sample average and are included in the regressions. Control variables are collapsed for brevity and are identical to those in Table 2. Table B1 in the appendix defines all variables. Robust standard errors are clustered at the county-level and are reported in brackets. *$p < .1$; **$p < .05$; ***$p < .01$.

A. SOX 302 & MW banks' lending to risky borrowers

Dependent variable: *ln(Originated loans)*			
Sample: MW banks			
	(1)	(2)	(3)
*Post*FICO score*	-0.001		
	[0.002]		
*Post*Combined loan-value*		0.011	
		[0.007]	
*Post*Mortgage default*$_{2000\text{-}2002}$			-0.012***
			[0.001]
Post	-2.887***	-2.898***	-1.792***
	[0.425]	[0.422]	[0.111]
Control variables	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes
Bank fixed effects	Yes	Yes	Yes
Sample	GSE-HMDA	GSE-HMDA	GSE-HMDA
Adjusted R^2	.850	.851	.871
Observations	449	449	471

B. SOX 302 & MW banks' lending to marginal borrowers

Dependent variable: *ln(Originated loans)*				
Sample: MW banks				
	(1)	(2)	(3)	(4)
*Post*ln(Applicant income)*	0.195			
	[0.174]			
Post%female applicants*		-0.129		
		[0.749]		
Post%non-white applicants*			-1.009***	
			[0.283]	
Post%same-sex applicants*				2.820
				[3.404]
Post	-0.305***	-0.318***	-0.133	-0.306***
	[0.098]	[0.103]	[0.107]	[0.100]
Control variables	Yes	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes	Yes
Bank fixed effects	Yes	Yes	Yes	Yes
Sample	HMDA	HMDA	HMDA	HMDA
Adjusted R^2	.696	.696	.700	.696
Observations	2,170	2,170	2,170	2,170

Table 4. SOX 302 & MW bank's lending: Bank-level analysis

This table reports bank-quarter regressions which estimate the effect of SOX 302 on the lending behavior of banks that report Material Weakness between September 2002 and December 2004 to comply with the SOX 302 provision (MW banks). The dependent variables are *ln(Loans)*, the natural logarithm of total loans (column 1); *Loans/Assets*, loans as a proportion of their total assets (column 2); *ln(Residential real estate loans)*, the natural logarithm of residential real estate loans (column 3); and *Residential real estate loans/Loans*, residential loans as a proportion of total loans (column 4). *Post* is a dummy variable that equals one for all years from 2003 and later. The data are from Call Reports (FR Y-9C) and contain observations by MW banks. Standard errors are in brackets. Control variables include *ln(Assets)*, *ln(Assets)*^{2}, *ROA*, *Tier-1 capital/Assets*, and *Deposit/Assets* and are collapsed for brevity. Table B1 in the appendix defines all variables. Robust standard errors are clustered at the bank-level. *$p < .1$; **$p < .05$; ***$p < .01$.

Dependent variables:	*ln(Loans)*	*Loans/Assets*	*ln(Residential real estate loans)*	*Residential real estate loans/Loans*
Sample: MW banks				
	(1)	(2)	(3)	(4)
Post	-0.057**	-0.025*	-0.200***	-0.031*
	[0.024]	[0.015]	[0.068]	[0.017]
Control variables	Yes	Yes	Yes	Yes
Bank fixed effects	Yes	Yes	Yes	Yes
Adjusted R^2	.992	.773	.943	.909
Observations	676	676	676	676

Table 5. SOX 302 and non-MW banks' lending behavior

This table reports bank-county-year regressions which estimate the effect of SOX 302 on the lending behavior of non-MW banks. The dependent variable is *ln(Originated loans),* the natural logarithm of the dollar amount of mortgages originated in a bank-county-year. Columns 1 and 2 include loans originated by public banks that do not need to disclose and improve their internal controls (non-MW public banks). Columns 3 and 4 include loans originated by all private banks. *MW presence* is the fraction of loans originated by MW banks in a given county. We center this variable at the sample average. *Post* is a dummy variable that equals one for all years from 2003 and later. The regressions also include the interactions between *Post* and all control variables. Table B1 in the appendix defines all variables. Robust standard errors are clustered at the county-level and are reported in brackets. *$p < .1$; **$p < .05$; ***$p < .01$.

Dependent variable: *ln(Originated loans)*				
Sample:	Non-MW public banks		Private banks	
	(1)	(2)	(3)	(4)
*MW presence*Post*	1.022***	0.743**	0.638	0.474
	[0.322]	[0.321]	[0.788]	[0.602]
MW presence	-0.607***	-0.233	0.362	0.164
	[0.164]	[0.165]	[0.553]	[0.454]
ln(Assets)	-0.536***	-0.223	1.430***	0.953
	[0.169]	[0.269]	[0.347]	[0.681]
ln(Assets)^2	0.025***	0.004	-0.034***	-0.014
	[0.005]	[0.008]	[0.013]	[0.027]
ROA	-0.140***	0.129***	0.089*	0.032
	[0.031]	[0.027]	[0.047]	[0.046]
Tier-1 capital/Assets	-0.002	0.056***	0.023*	0.006
	[0.016]	[0.013]	[0.013]	[0.012]
Deposit/Assets	0.31	1.128***	1.089**	0.558*
	[0.310]	[0.260]	[0.545]	[0.337]
Loans/Assets	0.711***	0.604***	1.705***	0.835***
	[0.197]	[0.183]	[0.250]	[0.275]
Residential real estate loans/Loans	0.955***	-0.810***	2.305***	0.477
	[0.294]	[0.164]	[0.193]	[0.319]
ln(Applicant income)	0.073	0.188***	0.623***	0.655***
	[0.060]	[0.058]	[0.086]	[0.065]
Loan-applicant-income	0.130**	0.126***	0.109***	0.091***
	[0.063]	[0.047]	[0.039]	[0.030]
%female applicants	-0.359	0.204	-0.221	-0.545**
	[0.278]	[0.237]	[0.270]	[0.245]
%non-white applicants	-1.600***	-1.516***	-0.709***	-0.288*
	[0.109]	[0.098]	[0.170]	[0.154]
%same-sex applicants	-1.534**	-0.942	-0.997*	-1.068**
	[0.687]	[0.694]	[0.518]	[0.483]
Post x Control variables	Yes	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes	Yes
Bank fixed effects	No	Yes	No	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Sample	HMDA	HMDA	HMDA	HMDA
Adjusted R^2	.427	.592	.597	.781
Observations	50,681	50,681	11,882	11,882

Table 6. Moderating effects of local competition

This table examines how local competition moderates the effect of SOX 302 on the lending behavior of non-MW banks. The dependent variable is *ln(Originated loans)*, the natural logarithm of the dollar amount of mortgages originated in a bank-county-year. *MW presence* is the fraction of loans originated by MW banks in a given county. *Post* is a dummy variable that equals one for all years from 2003 and later. HHI is the Herfindahl-Hirschman index which measures the concentration of originated mortgages at the county-level (measured on a scale between 0 and 10,000). Control variables are collapsed for brevity and are identical to those in Table 5. The regressions also include the interactions between *Post* and all control variables as well as *Post*HHI*, *MW presence*HHI*, and *HHI*. They are not reported for brevity. Table B1 in the appendix defines all variables. Robust standard errors are clustered at the county-level and are reported in brackets. **p* < .1; ***p* < .05; ****p* < .01.

Dependent variable: *ln(Originated loans)*		
Sample:	Non-MW public banks	Private banks
	(1)	(2)
*MW presence*Post*HHI*	-0.001	0.006**
	[0.001]	[0.003]
*MW presence*Post*	0.588*	1.104**
	[0.308]	[0.555]
MW presence	-0.207	-0.339
	[0.169]	[0.605]
Control variables	Yes	Yes
Post x Control variables	Yes	Yes
County fixed effects	Yes	Yes
Bank fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Sample	HMDA	HMDA
Adjusted R^2	.592	.781
Observations	50,681	11,882

Table 7. SOX 302 and non-MW banks' risky lending

This table examines the heterogeneity in the effect of SOX 302 on the lending behavior of non-MW banks. We interact *MW presence*Post* with two ex ante measures of borrower risk (*FICO* score and *Combined loan-value*) and an ex post measure of borrower risk (*Mortgage default*$_{2000\text{-}2002}$). The dependent variable is *ln(Originated loans)*, the natural logarithm of the dollar amount of mortgages originated in a bank-county-year. *Post* is a dummy variable that equals one for all years from 2003 and later. Control variables are collapsed for brevity and are identical to those in Table 5. The regressions also include the interactions between *Post* and all control variables. They are not reported for brevity. Table B1 in the appendix defines all variables. Robust standard errors are clustered at the county-level and are reported in brackets. *$p < .1$; **$p < .05$; ***$p < .01$.

Dependent variable: *ln(Originated loans)*						
Samples:	Non-MW public banks			Private banks		
	(1)	(2)	(3)	(4)	(5)	(6)
*MW presence*Post*FICO score*	-0.015			-0.020**		
	[0.013]			[0.009]		
*MW presence*Post*Combined loan-value*		0.010			0.052**	
		[0.038]			[0.020]	
*MW presence*Post*Mortgage default*$_{2000\text{-}2002}$			-0.367***			0.879
			[0.132]			[1.661]
*MW presence*Post*	-0.761	-0.248	0.760	-0.422	0.586*	0.547
	[0.617]	[0.533]	[0.555]	[0.414]	[0.299]	[0.953]
MW presence	-0.152	-0.29	-0.419	0.374	-0.615**	-0.542
	[0.492]	[0.433]	[0.374]	[0.418]	[0.302]	[0.949]
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Post x Control variables	Yes	Yes	Yes	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Bank fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Sample		GSE-HMDA			GSE-HMDA	
Adjusted R^2	.862	.861	.847	.955	.954	.947
Observations	3,315	3,325	3,096	373	374	439

Table 8. Exposure to SOX 302 spillover and loan performance

This table reports bank-county-year regressions which estimate the effect of SOX 302 on loan performance of non-MW banks. The dependent variable is *Mortgage default*, the bank-county-year average share of mortgages that become 90 days delinquent during the first 2 years of their life. *MW presence* is the fraction of loans originated by MW banks in a given county. *Post* is a dummy variable that equals one for all years from 2003 and later. The regressions also include the interactions between *Post* and all control variables. Table B1 in the appendix defines all variables. Robust standard errors are clustered at the county-level and are reported in brackets. *$p < .1$; **$p < .05$; ***$p < .01$.

Dependent variable: *Mortgage default*		
	Non-MW public banks	Private banks
	(1)	(2)
*MW presence*Post*	0.021	9.153*
	[0.126]	[5.519]
MW presence	-0.048	-8.468
	[0.080]	[5.433]
ln(Assets)	-0.262**	-0.902
	[0.108]	[1.075]
$ln(Assets)^2$	0.009***	0.025
	[0.003]	[0.037]
ROA	0.019	0.001
	[0.016]	[0.020]
Tier-1 capital/Assets	0.003	-0.015
	[0.009]	[0.011]
Deposit/Assets	-0.046	-0.175
	[0.169]	[0.172]
Loans/Assets	0.193	0.108
	[0.168]	[0.144]
Residential real estate loans/Loans	-0.063	-0.023
	[0.073]	[0.142]
ln(Applicant income)	-0.045**	0.029
	[0.020]	[0.027]
Loan-applicant-income	0.002	0.043*
	[0.017]	[0.023]
%female applicants	-0.109	-0.056
	[0.100]	[0.173]
%non-white applicants	-0.029	-0.042
	[0.040]	[0.078]
%same-sex applicants	0.126	-0.215
	[0.273]	[0.284]
FICO score	0.000	-0.000*
	[0.000]	[0.000]
Combined loan-value	0.000	-0.001*
	[0.000]	[0.000]
Post x Control variables	Yes	Yes
County fixed effects	Yes	Yes
Bank fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Sample	GSE-HMDA	GSE-HMDA
Adjusted R^2	.212	.695
Observations	3,315	373

Table 9. Aggregate county mortgage origination

This table reports county-year regressions which estimate the effect of a county's exposure to MW presence on its aggregate mortgage lending. The dependent variable is *ln(County originated loans)*, the natural logarithm of the dollar amount of mortgages originated in a county-year. *MW presence* is the fraction of loans originated by MW banks in a given county. *Post* is a dummy variable that equals one for all years from 2003 and later. Table B1 in the appendix defines all variables. Robust standard errors are clustered at the county-level and are reported in brackets. *$p < .1$; **$p < .05$; ***$p < .01$.

Dependent variables: *ln(County originated loans)*			
	(1)	(2)	(3)
*MW presence*Post*	-0.495	-0.551	-0.564
	[0.439]	[0.436]	[0.436]
MW presence	0.984***	0.982***	0.975***
	[0.256]	[0.255]	[0.258]
ln(Population)		0.003	0.005
		[0.012]	[0.012]
ΔPopulation			-1.134
			[0.791]
Unemployment		-0.016*	-0.022**
		[0.009]	[0.010]
ΔUnemployment			0.083
			[0.054]
ln(Income per capita)		-0.014	-0.034
		[0.081]	[0.085]
ΔIncome per capita			-0.082
			[0.234]
HHI		0.901*	0.909*
		[0.466]	[0.538]
ΔHHI			-0.001
			[0.036]
County fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Adjusted R^2	.725	.725	.725
Observations	13,453	13,453	13,453

Appendix A. Suntrust Bancorp Inc.'s disclosure of material weaknesses

Extract A: *Suntrust's* disclosure of material weaknesses

The Company's significant accounting policies are described in detail in Note 1 to the Consolidated Financial Statements and are integral to understanding Management's Discussion of results of operations and financial condition. Management has identified certain accounting policies as being critical because they require management's judgment to ascertain the valuations of assets, liabilities, commitments and contingencies. A variety of factors could affect the ultimate value that is obtained either when earning income, recognizing an expense, recovering an asset, or reducing a liability. The Company has established detailed policies and control procedures that are intended to ensure these critical accounting estimates are well controlled and applied consistently from period to period. In addition, the policies and procedures are intended to ensure that the process for changing methodologies occurs in an appropriate manner. However, in the fourth quarter of 2004 the Company identified a material weakness in internal controls related to establishing the Allowance for Loan and Lease Losses (ALLL). The Controls and Procedures section on pages 64 through 65 provides further discussion surrounding this internal control weakness. The following is a description of the Company's current accounting policies that are considered to involve significant management valuation judgments.

Extract B: *Suntrust's* plans to address the weaknesses

CHANGES IN INTERNAL CONTROL OVER FINANCIAL REPORTING

Management of the Company has evaluated, with the participation of the Company's Chief Executive Officer and Chief Financial Officer, changes in the Company's internal controls over financial reporting (as defined in Rule 13a-15(f) and 15d-15(f) of the Exchange Act) during the fourth quarter of 2004. In connection with such evaluation, the Company has determined that there have been changes in internal control over financial reporting during the fourth quarter that have materially affected or are reasonably likely to materially affect, the Company's internal control over financial reporting. As discussed in Management's Report on Internal Control Over Financial Reporting, in the fourth quarter of 2004, the Company identified a material weakness in internal controls over financial reporting relating to the Company's process of establishing the ALLL that existed during 2004.

As of the end of the period covered by this report, the Company has not fully remediated the material weakness in the Company's internal control over financial reporting relating to the ALLL. However, the Company has taken the following remedial actions:

- The Company terminated three members of its credit administration division, including its Chief Credit Officer.
- The Controller was reassigned to a position in the Company with responsibilities that involve areas other than accounting or financial reporting.
- The Company's ALLL Committee was reconstituted with certain members of senior management.
- The ALLL policies and procedures have been, and are continuing to be, documented and significantly augmented.
- The Company has established additional remediation plans to address internal control deficiencies associated with the ALLL framework, including additional documentation, training and supervision, periodic testing and periodic updates to the Audit Committee. Internal controls surrounding the validation and testing of systems and models relating to the ALLL process have been strengthened.
- Management has taken steps, and intends to take additional steps, to ensure that the Company's conservative credit culture does not interfere with the application of GAAP in the ALLL calculation process.

Other than the changes identified above, there have been no changes to the Company's internal control over financial reporting that occurred since the beginning of the Company's fourth quarter of 2004 that have materially affected, or are reasonably likely to materially affect, the Company's internal control over financial reporting.

Appendix B

Table B1. Definitions of variables

Variable	Definition	Source
Definitions of banks		
MW banks	Public banks that disclose material weaknesses between September 2002 and December 2004	AuditAnalytics
Non-MW public banks	Public banks that do *not* disclose material weaknesses between September 2002 and December 2004	AuditAnalytics
Private banks	Nonlisted commercial banks	FR Y-9C
Key explanatory variables		
Post	Dummy equals one for all years from 2003 onward after SOX 302 provision becomes effective	–
MW presence	The fraction of loans originated by MW banks in a given county	HMDA
Bank characteristics		
ln(Assets)	Natural logarithm of total assets	FR Y-9C
ROA (%)	Earnings before interest and taxes divided by book value of total assets	FR Y-9C
Tier-1 capital/Assets	Tier-1 capital divided by total assets	FR Y-9C
Deposit/Assets	Total deposits divided by total assets	FR Y-9C
Loans/Assets	Total loans divided by total assets	FR Y-9C
ln(Loans)	Natural logarithm of total loans	FR Y-9C
ln(Residential real estate loans)	Natural logarithm of residential real estate loans	FR Y-9C
Residential real estate loans/Loans	Total residential real estate loans divided by total loans	FR Y-9C
Borrower and loan characteristics		
ln(Originated mortgages)	The natural logarithm of the dollar amount of mortgages originated in a bank-county-year	HMDA
ln(Applications)	The natural logarithm of the number of mortgage applications submitted in a bank-county-year	HMDA
ln(Applicant income)	The natural logarithm of the average applicant's income in a bank-county-year	HMDA
Loan-applicant-income	The average ratio of the requested loan amount in a mortgage application to the applicant's income for applications reviewed in a bank-county-year	HMDA
%female applicants	The fraction of mortgage applications from female applicants in each bank-county-year	HMDA
%non-white applicants	The fraction of mortgage applications from non-white applicants (i.e., applicants whose reported race is other than white) in a bank-county-year	HMDA
% same-sex applicants	The fraction of mortgage applications in which the main applicant and the coapplicant reporting the same sex in a bank-county-year	HMDA
FICO score	The average applicant's FICO score in a bank-county-year	GSE-HMDA
Combined loan-value	The average combined loan-value ratio in a bank-county-year	GSE-HMDA
Mortgage delinquencies	The fraction of mortgages that become 90 days delinquent during the first 2 years of their life in a bank-county-year	GSE-HMDA
Mortgage default$_{2000\text{-}2002}$	The 2000–2002 average of mortgage delinquencies in a bank-county	GSE-HMDA
County-level characteristics		
ln(County originated mortgages)	The natural logarithm of the dollar amount of mortgages originated in a county-year	HMDA
ln(Population)	The natural logarithm of the county population	U.S. Census Bureau

ln(Income per capita)	The natural logarithm of the individual's income from wages, investment enterprises and other ventures in a county	U.S. Census Bureau
Unemployment rate	Unemployment rate of the county	Bureau of Labor Statistics
HHI	Herfindahl-Hirschman index measuring the concentration of originated mortgages at the county-level (on a scale between 0 and 10,000)	FR Y-9C
ln(House prices)	The natural logarithm of the average house price in the county	Zillow.com
%Home foreclosed	The number of houses closed out of 10,000 homes in the county	Zillow.com
ln(Mortgage applications)	The natural logarithm of the total number of mortgage applications in the county	HMDA
%female applicants	The fraction of mortgage applications from female applicants in the county	HMDA
%non-white applicants	The fraction of mortgage applications from non-white applicants (i.e., applicants whose reported race is other than white) in the county	HMDA
%same-sex applicants	The fraction of mortgage applications in which the main applicant and the coapplicant reporting the same sex in the county	HMDA
%sold mortgage	The fraction of originated mortgages sold off the lender's balance sheet in the county	HMDA

Table B2. Is MW bank presence correlated with county characteristics?

This table examines whether the presence of MW banks in a given county can be predicted by historical county characteristics. The dependent variable is *MW presence*$_{2003}$, the fraction of loans originated by MW banks in a given county in 2003, the first complete year after SOX 302 became effective. Panel A examines the correlation between *MW presence*$_{2003}$ and the *levels* of various county characteristics, measured in 2000: (1) *ln(Population)*, (2) Unemployment rate, (3) *ln(Income per capita)*, (4) HHI of originated mortgage concentration, (5) *ln(House prices)*, (6) *%home foreclosed*, (7) *%sold mortgages*, (8) *ln(Mortgage applicants)*, (9) *%female applicants*, (10) *%non-white applicants*, and (11) *%same-sex applicants*. Panel B examines the correlation between *MW presence*$_{2003}$ and the annual *change* of various county characteristics, measured in 2000: (12) *Δln(Population)*, (13) *ΔUnemployment rate*, (14) *Δln(Income per capita)*, (15) *ΔHHI of originated mortgage concentration*, (16) *Δln(House prices)*, (17) *ΔHome foreclosed*, (18) *ΔSold mortgages*, (19) *ΔMortgage applicants*, (20) *Δfemale applicants*, (21) *Δnon-white applicants*, and (22) *Δsame-sex applicants*. Table B1 in the appendix defines all variables. Robust standard errors are clustered at the state-level and are reported in brackets. *p < .1; **p < .05; ***p < .01.

A. The correlation between the levels of county characteristics and MW presence

Dependent variable: *MW presence*$_{2003}$

County characteristics:	*ln(Population)*	*Unemployment*	*ln(Income per capita)*	*HHI*	*ln(House price)*	*%home foreclosed*	*%sold mortgages*	*ln(Mortgage applicants)*	*%female applicants*	*%non-white applicants*	*%same-sex applicants*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
County characteristic	0.000	0.001	-0.001	0.000	0.004	0.000	-0.036	0.000	0.011	-0.002	0.029
	[0.001]	[0.002]	[0.011]	[0.000]	[0.007]	[0.000]	[0.021]	[0.001]	[0.010]	[0.007]	[0.042]
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	.195	.196	.195	.195	.161	.120	.202	.195	.195	.195	.195
Observations	1,923	1,923	1,923	1,923	1,058	312	1,923	1,923	1,923	1,923	1,884

B. The correlation between the changes in county characteristics and MW presence

Dependent variable: *MW presence*$_{2003}$

County characteristics:	*ΔPopulation*	*ΔUnemployment*	*ΔIncome per capita*	*ΔHHI*	*Δln(House price)*	*ΔHome foreclosed*	*ΔSold mortgages*	*ΔMortgage applicants*	*Δfemale applicants*	*Δnon-white applicants*	*Δsame-sex applicants*
	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(020)	(21)	(22)
County characteristic	0.019	-0.006	0.008	0.001	0.316	0.001	0.000	0.012	0.001	-0.001	0.000
	[0.041]	[0.006]	[0.030]	[0.002]	[0.376]	[0.001]	[0.000]	[0.007]	[0.001]	[0.001]	[0.001]
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	.195	.196	.195	.195	.160	.098	.195	.202	.196	.190	.179
Observations	1,923	1,923	1,923	1,923	1,053	278	1,917	1,847	1,828	1,784	1,524

Table B3. Lending behavior of MW banks: Robustness tests

This table presents various robustness checks on the effect of SOX 302 on the lending behavior of MW banks. Panel A examines the dynamic timing effects by replacing the *Post* dummy with a set of dummies: *Before 2001*, *2003*, *2004*, *2005*, and *2006*. Panels B and C exclude counties that exhibit buyer income overstatement. Panel D rules out demand-side explanations. The dependent variable in panel D is *ln(Applications)*, the natural logarithm of the number of mortgage applications submitted in a given bank-county-year. Panel E presents other robustness tests. Across all panels, control variables are collapsed for brevity and are identical to those in Table 2. Table B1 in the appendix defines all variables. Robust standard errors are clustered at the county-level and are reported in brackets. *$p < .1$; **$p < .05$; ***$p < .01$.

A. Dynamic timing effects of lending behavior of MW banks

Dependent variable: *ln(Originated mortgages)*	
	(1)
Before 2001	-0.064
	[0.110]
2003	-0.290***
	[0.106]
2004	-0.376**
	[0.188]
2005	-0.750**
	[0.297]
2006	-0.298
	[0.313]
Control variables	Yes
County fixed effects	Yes
Bank fixed effects	Yes
Sample	HMDA
Adjusted R^2	.701
Observations	2,170

B. Excluding counties with buyer income overstatement

Dependent variable: *ln(Originated mortgages)*	
	(1)
Post	-0.451***
	[0.127]
Control variables	Yes
County fixed effects	Yes
Bank fixed effects	Yes
Sample	HMDA
Adjusted R^2	.711
Observations	1,303

C. MW banks risky lending, excluding counties with buyer income overstatement

Dependent variable: *ln(Originated mortgages)*	
	(1)
Post Loan-applicant-income*	-0.184**
	[0.076]
Post	-0.504***
	[0.129]
Control variables	Yes
Post x Control variables	Yes
County fixed effects	Yes
Bank fixed effects	Yes
Year fixed effects	Yes

Sample	HMDA
Adjusted R^2	.713
Observations	1,303

D. Ruling out demand side explanations

Dependent variable: *ln(Applications)*	
	(1)
Post	0.031
	[0.050]
Control variables	Yes
County fixed effects	Yes
Bank fixed effects	Yes
Sample	HMDA
Adjusted R^2	.795
Observations	2,170

E. Other robustness tests

Dependent variable: *ln(Originated mortgages)*			
	Exclude banks targeted by majority board independence	Exclude banks receive regulatory enforcement actions	Control for additional county-level characteristics
	(1)	(2)	(3)
Post	-0.430***	-0.344***	-0.361***
	[0.103]	[0.101]	[0.115]
Control variables	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes
Bank fixed effects	Yes	Yes	Yes
Sample	HMDA	HMDA	HMDA
Adjusted R^2	.698	.689	.680
Observations	2,107	2,128	1,751

Table B4. Lending behavior of non-MW banks: Robustness tests

This table presents various robustness checks on the spillover effect of SOX 302 on the lending behavior of non-MW banks. Panel A tests the dynamic timing effects by replacing the *Post* dummy with a set of dummies: *Before 2001*, *2003*, *2004*, *2005*, and *2006*. Panels B and C exclude counties that exhibit buyer income overstatement. Panel D rules out the size-based explanation. Panel E presents other robustness tests. Across all panels, control variables are collapsed for brevity and are identical to those in Table 5. Table B1 in the appendix defines all variables. Robust standard errors are clustered at the county level and are reported in brackets. *$p < .1$; **$p < .05$; ***$p < .01$.

A. Timeline

Dependent variable: *ln(Originated mortgages)*		
	Non-MW public banks	Private banks
	(1)	(2)
*Before 2001*MW presence*	-0.285	1.098
	[0.242]	[0.797]
*2003*MW presence*	-0.329	1.000
	[0.378]	[0.701]
*2004*MW presence*	0.924	2.652**
	[0.608]	[1.099]
*2005*MW presence*	0.819*	1.276
	[0.474]	[1.156]
*2006*MW presence*	1.609***	0.523
	[0.525]	[1.139]
Control variables	Yes	Yes
Post x Control variables	Yes	Yes
County fixed effects	Yes	Yes
Bank fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Sample	HMDA	HMDA
Adjusted R^2	.592	.781
Observations	50,681	11,882

B. Excluding counties with buyer income overstatement

Dependent variable: *ln(Originated mortgages)*		
	Non-MW public banks	Private banks
	(1)	(2)
*MW presence*Post*	1.213**	0.148
	[0.581]	[0.692]
MW presence	-0.186	-0.095
	[0.229]	[0.581]
Control variables	Yes	Yes
Post x Control variables	Yes	Yes
County fixed effects	Yes	Yes
Bank fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Sample	HMDA	HMDA
Adjusted R^2	.590	.786
Observations	25,575	6,522

C. Non-MW banks' risky lending, excluding counties with buyer income overstatement

Dependent variable: *ln(Originated mortgages)*		
	Non-MW public banks	Private banks
	(1)	(2)
*MW presence*Post* Loan-applicant-income*	-1.815**	1.039***
	[0.740]	[0.394]
*MW presence*Post*	1.071*	0.243
	[0.623]	[0.972]
MW presence	-0.034	0.709
	[0.272]	[0.728]
Control variables	Yes	Yes
Post x Control variables	Yes	Yes
County fixed effects	Yes	Yes
Bank fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Sample	HMDA	HMDA
Adjusted R^2	.590	.577
Observations	25,575	6,522

D. Ruling out size-based explanations

Dependent variable: *ln(Originated mortgages)*				
Sample:	Non-MW public banks			
	Size above sample's median	Size below sample's median	Exclude the 4 biggest banks	Include bank size-decide fixed effects
	(1)	(2)	(3)	(4)
*MW presence*Post*	1.023**	1.099**	0.685**	0.765**
	[0.407]	[0.464]	[0.318]	[0.308]
MW presence	-0.182	-1.464***	-0.733***	-0.303*
	[0.178]	[0.401]	[0.176]	[0.162]
Control variables	Yes	Yes	Yes	Yes
Post x Control variables	Yes	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes	Yes
Bank fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Sample	HMDA	HMDA	HMDA	HMDA
Adjusted R^2	.682	.585	.577	.597
Observations	31,071	19,610	41,757	50,681

E. Other robustness tests

		Non-MW public banks		Private banks	
		Coefficient	Standard errors	Coefficient	Standard errors
(1)	*ln(MW presence)*	0.876**	0.358	0.489	0.674
(2)	Only counties where *MW presence* > 0	0.864**	0.363	0.466	0.661
(3)	Only counties where *MW presence* above sample median	0.859**	0.372	0.404	0.695
(4)	Control for HHI of originated mortgage concentration	0.634**	0.322	0.490	0.600

Internet Appendix

Regulatory Spillovers in Local Mortgage Markets

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Internet Appendix IA1: Local versus nonlocal loans

This table estimates the effect of SOX 302 on the lending behavior of non-MW banks. Columns 1 and 3 include loans originated in counties in which banks have physical branches (local). Columns 2 and 4 include loans originated in counties in which banks do not have physical branches (nonlocal). The dependent variable is *Ln(Originated loans)*, the natural logarithm of the dollar amount of mortgages originated in a bank-county-year. *MW presence* is the fraction of loans originated by MW banks in a given county. *Post* is a dummy variable that equals one for all years from 2003 and later. Control variables are collapsed for brevity and are identical to those in Table 5. The regressions also include the interactions between *Post* and all control variables. They are not reported for brevity. Table B1 in the appendix defines all variables. Robust standard errors are clustered at the county-level and are reported in brackets. *$p < .1$; **$p < .05$; ***$p < .01$.

Dependent variable: Ln(Originated loans)				
Sample:	**Non-MW public banks**		**Private banks**	
	Local	Nonlocal	Local	Nonlocal
	(1)	(2)	(3)	(4)
MW Presence*Post	0.743**	-0.427	0.474	0.562
	[0.321]	[0.275]	[0.602]	[0.397]
MW Presence	-0.233	-0.234	0.164	-0.214
	[0.165]	[0.162]	[0.454]	[0.282]
Control variables	Yes	Yes	Yes	Yes
Post x Control variables	Yes	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes	Yes
Bank fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Sample	HMDA	HMDA	HMDA	HMDA
Adjusted R^2	0.592	0.372	0.781	0.428
Observations	50,681	290,589	11,882	94,898

Internet Appendix IA2: Univariate comparisons across bank types

This table compares the characteristics of MW banks and those of non-MW public and private banks. For each variable, the p-value of the difference between the two samples is calculated. *$p < .1$; **$p < .05$; ***$p < .01$.

	MW banks	Non-MW public	p-value of difference	MW banks	Private	p-value of difference
	Mean	**Mean**		**Mean**	**Mean**	
Ln (Assets)	14.680	14.430	0.082*	14.680	11.990	0.000***
ROA (%)	0.912	1.037	0.010**	0.912	0.802	0.114
Tier-1 capital/Assets	8.445	8.621	0.290	8.445	10.370	0.000***
Deposit/Assets	0.723	0.751	0.004***	0.723	0.828	0.000***
Loans/Assets	0.663	0.667	0.749	0.663	0.649	0.269
Residential real estate Loans/Loans	0.320	0.298	0.150	0.320	0.399	0.001***

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