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IVAN LIM
JENS HAGENDORFF
SETH ARMITAGE

The Effects of Regulatory Office Closures on Bank Behavior

We investigate if the decentralized structure of regulatory office networks influences supervisory outcomes and bank behavior. Following the closure of an office, banks previously supervised by that office increase their lending and risk-taking. As a result, affected banks have larger loan losses and higher failure rates during the 2008–09 financial crisis. Analysis of the channels suggests that proximate supervisors enforce timelier provisioning practices, restrict large cash payouts, and provide advice that increases a bank's risk-adjusted returns. Overall, our findings imply that geographical proximity

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Ivan Lim *is an assistant professor in Finance at the Department of Finance, Durham University Business School, Durham University* (*E-mail*: ivan.lim@durham.ac.uk). Jens Hagendorff *is a professor of Finance at the Department of Accounting and Financial Management, King's Business School, King's College London* (*E-mail*: jens.hagendorff@kcl.ac.uk). Seth Armitage *is a professor of Finance at the Department of Accounting and Finance, University of Edinburgh Business School, University of Edinburgh* (*E-mail*: seth.armitage@ed.ac.uk).

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reduces informational frictions in supervisory monitoring and leads to more stable banks.

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Bank supervision and regulation are complementary tools in maintaining the safety and soundness of individual banks and the overall stability of the financial system. While banks in the United States (and many countries around the world) operate under a unified framework of regulations, the task of ensuring compliance with these regulations is delegated to different supervisory agencies and their respective networks of local offices. An important reason often cited for this decentralized structure is to enable supervisors easier access to local information and have better communication with geographically dispersed banks with the aim of improving supervisory outcomes (FDIC 2012b, Fed 2017). In this paper, we utilize the closures of regulatory offices to investigate if this institutional design leads to more stable banks.

Having a decentralized structure with regulatory offices that are proximate to banks could lead to safer bank policies. Proximity facilitates supervisory monitoring by reducing the cost of collecting information, in particular, soft information (Coval and Moskowitz 2001, Malloy 2005, Chhaochharia, Kumar, and Niessen-Ruenzi 2012, Giroud 2013, Bernstein, Giroud, and Townsend 2016). For instance, the Federal Reserve Bank of St. Louis states:

> … Gathering in-depth information … would be a challenging task to accomplish from a single location. Therefore, one of the key ways branches assist the St. Louis Fed is through the gathering of economic information from around their zones … producing a depth and breadth of information not possible from hundreds of miles away. (Federal Reserve Bank of St. Louis 2017)

When regulatory offices are close to banks, supervisors have easier access to information such as local economic conditions, the bank's risk culture, the qualities of bank executives, and other relevant information that would facilitate monitoring (Lerner 1995, Liberti and Petersen 2019). In addition, proximity also allows supervisors to communicate more effectively to banks their expectations with regards to examinations (FDIC 2012b). As supervisors prefer lower levels of risk than bank managers and shareholders, who do not internalize the cost of negative externalities associated with bank distress in the presence of limited liability and safety net guarantees (e.g., Merton 1977, Keeley 1990, Demirgüç-Kunt and Detragiache 2002, Duchin and Sosyura 2014), we should expect safer bank policies as supervisors become more informed about a bank's portfolio and enforce and communicate their preferences more efficiently.

Alternatively, having regulatory offices that are close to banks may not result in safer bank policies. Supervisors could be "captured" by the banks that they supervise (Stigler 1971). The regulatory capture view states that supervisors may be subject to agency and behavioral conflicts (e.g., career concerns, social relationships, approval, and identification with industry) and thus may cater to the private interests of banks

(Laffont and Tirole 1993, Dal Bo 2006). The risk of regulatory capture could be particularly acute in close proximity. For instance, closer contact with bankers could allow communal relationships to form and undermine monitoring (Mills and Clark 1982). Likewise, supervisors who are rooted in local communities might be more concerned about the local economy and be less reluctant to impose strict measures on banks (Agarwal et al. 2014). The regulatory capture view is echoed by the Office of the Comptroller of Currency in a report on improving supervisory effectiveness:

> … some staff … presently is assigned to the same institution for many years. Examiners may get stale and become too familiar with the mid-management of the institution, giving rise to perceptions of regulatory capture. (OCC 2013)

In testing our hypothesis, we recognize that both explanations—the regulatory capture view and the view that proximate entities are easier to monitor—can be at work simultaneously. Our analysis, therefore, examines which explanation is likely to dominate on average.

Analyzing if bank behavior is influenced by proximity to regulatory offices poses several empirical challenges. In particular, the literature demonstrates how the supervisory setting is endogenously determined when banks can choose to locate risky activities away from geographic environments under strict supervision (Ongena, Popov, and Udell 2013), select their supervisors based on accessibility (Blair and Kushmeider 2006), or shop for supervisors they expect to be softer on them (Rosen 2003, 2005). Furthermore, economic shocks, many local in nature and not directly observable, will affect the conduct of banks and supervisors simultaneously (Acharya and Yorulmazer 2007, Brown and Dinç 2011).

In this paper, we take advantage of some of the unique features of U.S. banking supervision to alleviate some of these issues. We exploit the fact that banks in the same geographic area may be supervised by one of three federal regulators (the Federal Deposit Insurance Corporation [FDIC], the Federal Reserve [Fed], or the Office of the Comptroller of the Currency [OCC]). This decentralized system allows us to devise an empirical strategy that utilizes the closures of regulatory offices as shocks to proximity and observe the effect that changes in proximity have on bank behavior. Key to our strategy is that, within the counties in which a recently closed regulatory office has operated, only banks supervised by the closed office should be affected. Banks that are supervised by a different office should remain unaffected. Since both groups of banks are located in the same county and are exposed to similar local economic conditions, this alleviates concerns that our results could be biased by unobserved local economic shocks that affect the behavior of banks and supervisors at the same time.

Consequently, after an office closes, a more distant office assumes responsibility for the supervision of the banks that were previously supervised by the closed office. If being proximate to banks confers monitoring benefits to supervisors (allowing them to better constrain bank risk), we should observe riskier bank policies after the closure of a bank's regulatory office. Alternatively, if regulatory capture impedes effective monitoring, we should observe safer bank policies after office closures as supervisors are now located further away.

Using a hand-collected data set that maps out the locations of regulatory offices belonging to the FDIC, the Fed, and the OCC, we conduct our analysis in a difference-in-difference (DiD) setting using regulatory office closures from 2002 to 2013. We focus our analysis on community banks as they are tied to local markets and have traveling rather than in-house examiners (Wilson and Veuger 2012, OCC 2020). This allows us to estimate how banks respond after the closure of their regulatory office as examiners would now be housed further away.

We start by showing that office closures are unrelated to the characteristics of the banks under its supervision. Changes in the risk, performance, assets, and loans of banks under the supervision of a regulatory office do not predict the closure of that office. Instead, offices closest to a regional main office that is experiencing decreases in workload (i.e., a decrease in assets under supervision) are shut. Thus, the reasons for office closures reside with banks outside the immediate vicinity of the closed office and appear to be motivated by a need to rebalance supervisory resources within regions. This alleviates concerns that reverse causality or simultaneity, where poor bank performance predicts office closures and higher risk-taking, drive our results.

Moving on to our main analysis, our results show that, following regulatory office closures, banks supervised by the recently closed office altered the composition of their portfolios to riskier assets. Affected banks increase their total lending by $6-10\%$ and increases in lending are directed at riskier loan types (e.g., commercial real estate, CRE). Overall risk-taking, as measured by *Z-Score*, also increases for affected banks by $19-32\%$ as a proportion of the sample mean. Furthermore, we find that larger increases in physical distance to the new regulatory office lead to riskier bank policies. This suggests that geographical proximity alleviates informational frictions in collecting information and communicating with banks. Consequently, a decentralized supervisory structure allows for more efficient monitoring and as a result, safer bank policies.

Our main findings are robust to different model specifications and different sets of fixed effects (FEs). Our tightest specification includes county-quarter, regulatory office-quarter, and bank FEs. Consequently, time-varying omitted variables (e.g., local economic shocks, competition effects, or time-varying preferences in the enforcement of supervision by regulatory offices) are unlikely to bias our results. Diagnostic tests confirm the validity of our DiD design. Treated and control banks do not differ in characteristics preclosure and display similar parallel trends prior to office closures. Using a timing-effects model, we also confirm that increases in lending and risk-taking only occur after office closures, and not before.

The increases in lending and risk-taking after office closures we document might not necessarily be a cause for concern. Lending growth could benefit borrowers and local communities if supervisors were previously too strict. Furthermore, if banks affected by office closures increased their capital ratios alongside risk, lending growth might not undermine financial stability. However, our analysis does not provide clear evidence that treated banks hold more capital after office closures. At the same time, treated banks that were affected by office closures prior to the 2008–09 financial crisis exhibit more bad loans, higher charge-offs, and were more likely to fail during the crisis. Collectively, our findings highlight the benefits linked to a decentralized supervisory structure: supervisors are better able to monitor, leading to banks that are less fragile.

In the last part of our paper, we explore three nonmutually exclusive channels through which proximity to banks facilitates monitoring and leads to less fragile banks: (i) provisioning practices; (ii) payouts, and (iii) supervisory expertise. The first channel—provisioning practices—postulates that proximate supervisors are better able to assess the true quality of a bank's loan portfolio and enforce provisioning practices that are commensurate with loan risk. Loan loss provisions (LLPs) should reflect the expected future losses on a bank's loan portfolio (FDIC 2019). Underprovisioning, while improving liquidity and performance in the short term, can lead to concerns regarding capital adequacy if the accrued LLPs are insufficient to cover losses during economic downturns (Bushman and Williams 2012, 2015, Acharya and Ryan 2016).

However, banks enjoy considerable discretion in provisioning practices (Beatty and Liao 2014). In assessing if the level of provisioning is appropriate, supervisors take into account the bank's internal control systems and information regarding the quality of the bank's portfolio (FDIC 2019). Therefore, if supervisors are better able to assess the quality of a bank's loan portfolio, they should be able to ensure that banks maintain LLPs that are commensurate with expected future losses. In line with our expectation, we find that banks affected by office closures report both lower and less timely LLPs. They also increase their use of income-increasing provisions, which leads to balance sheets that are more opaque.

The second channel—payouts—states that supervisors are better able to restrain aggressive payout policies to bank shareholders. Payouts divert cash to shareholders and leave behind less liquid and riskier assets to repay creditors (Acharya et al. 2011, Acharya, Le, and Shin 2017). Because increases in default risk are subsidized in the presence of safety net guarantees, banks have incentives to increase payouts to shareholders (Ronn and Verma 1986, Hovakimian and Kane 2000, Onali 2014). Proximate supervisors would be in a better position to assess if payouts are commensurate with the true quality of a bank's earning assets and restrict excessive payouts. We find support for this channel; banks affected by office closures significant increase their dividend payouts.

The last channel—supervisory expertise—asserts that supervisors have expertise pertaining to issues such as emerging risks, banking innovations, and the deployment of new technologies that might be useful for banks, in particular, small community banks who are resource-constrained (FDIC 2012b, Wilson and Veuger 2017). From this perspective, we should expect that proximity affords more opportunities for supervisors to share their knowledge in-person with banks. Because supervisory expertise can lead to decreases in cost, increases in revenue or lower risk, we measure its aggregate effects using risk-adjusted returns. Consistent with a supervisory expertise channel, we find that risk-adjusted returns on assets for affected banks decrease after office closures. This suggests that supervisors operating out of a decentralized structure are better able to advise banks, leading to more efficient risk-taking which reduces bank fragility.

1. RELATED LITERATURE AND INSTITUTIONAL BACKGROUND

1.1 Related Literature

Our work is related to the literature on the impact of supervision on bank-level outcomes. Early studies rely on cross-country differences in supervision and produce mixed results. For instance, Barth, Caprio, and Levine (2004) find no evidence that measures of supervisory power are related to bank development, bank efficiency, or loan performance across countries. Later, Delis and Staikouras (2011) find a nonlinear (U-shaped) relationship between onsite supervisory examinations and bank risk. Ongena, Popov, and Udell (2013) show that the effectiveness of regulation on banks' lending standards abroad partly depends on the strength of supervision and regulation in their home market.

Recent work has devised empirical designs that attempt to isolate the effects of various dimensions of supervision on bank behavior. Rezende and Wu (2014) rely on discontinuities in bank size thresholds—that determine the minimum number of onsite bank examinations to be conducted by U.S. regulators—to demonstrate that more frequent examinations are associated with better bank performance. Another group of studies focuses on the effects of formal supervisory enforcement actions on bank behavior. After enforcement actions, banks reduce their holdings of risky assets (Delis, Staikouras, and Tsoumas 2017) and reduce their lending and liquidity creation (Danisewicz et al. 2018). Hirtle, Kovner, and Plosser (2020) use size rankings of bank holding companies within the individual 12 districts of the Fed System as a proxy for regulatory attention. They show that the five largest banks in a Fed district display lower risk compared to a matched sample of similar-sized bank holding companies (BHCs) in a different Fed district that are not among the five largest banks in that district.

To the best of our knowledge, we are the first to make use of office closures to study how geographical networks of offices matter for bank supervision. In a related paper, Kandrac and Schlusche (2021) use the relocation of the ninth district Federal Home Loan Bank from Little Rock to Dallas in 1983 as a shock to supervisory resources and find that affected thrifts become riskier. Our paper differs and complements theirs in several ways. First, our focus is on how physical proximity facilities monitoring in the presence of informational frictions. By contrast, their focus is on how the relocation led to most of the examiners quitting their job. Though related, both mechanisms are distinct and highlight different supervisory dimensions. Second, Kandrac and Schlusche use an abrupt one-time event while we focus on multiple office closures by different federal regulators across fairly normal conditions. This allows us to paint a systematic picture of the effects of supervisors beyond the specific behavior (including potential biases) of any single supervisor or event.[1]

1. In a follow-up paper to ours, Gopalan, Kalda, and Manela (2021) study the effects of OCC office closures and how banks manage capital ratios, while we focus on credit origination and risk-taking.

Our work is also related to studies on regulatory inconsistency and arbitrage within the decentralized structure of U.S. banking supervision. Rosen (2003, 2005) finds that banks show better performance after switching regulators and argues competition between regulators is beneficial. However, Rezende (2014) shows that although banks receive better regulatory ratings after they switch regulators, they also tend to fail more often. Agarwal et al. (2014) exploit supervisory rotation policies, which exogenously assign federal and state regulators to the same bank, to show that different regulators implement identical regulations inconsistently. The authors explain that discrepancies in regulatory behavior are due to differences in incentives with state regulators being more lenient because they are more concerned about local economic conditions than federal regulators. Our paper contributes to this line of work by suggesting that informational frictions can cause inconsistencies in supervisory enforcement.

Our work is also relevant for discussions on the (de)centralization of bank supervision. In late 2014, 130 of the most significant banks in the European Union came under the direct supervision of the European Central Bank (ECB) under its Single Supervisory Mechanism (SSM). Fiordelisi, Ricci, and Lopes (2017) find that banks that came under the direct supervision of the ECB (as compared to banks that remained under the supervision of their national supervisors) reduced lending. Relatedly, Eber and Minoiu (2016) show that banks that fell under the SSM's purview reduced their leverage as compared to banks that were not assigned to the SSM. These findings contrast ours as they suggest that centralization is effective in reducing risky bank behavior.

Differences between our findings and work on the SSM suggest institutional characteristics are important factors behind the effectiveness of supervisory arrangements. For instance, direct supervision by the SSM is restricted to the largest and most complex (primarily international) banks. It is conceivable that central supervision of such banks benefit from scale economies in a way not applicable to the mostly smaller regional banks in our sample. Furthermore, our results point to local supervisors as more attuned to local market conditions. Such local expertise is less relevant (and perhaps unnecessarily complex) in the supervision of large international banks. Finally, our paper also supports the notion that a local supervisory structure is likely to be more efficient when banks are less able to arbitrage differences in regulations. By contrast, the SSM might be more effective in limiting large banks' ability to arbitrage differences in regulations across European countries. Overall, our work highlights the importance of the institutional context when evaluating the effectiveness of local versus centralized supervision.

Finally, we contribute to the literature on the determinants of bank provisioning practices and their consequences. Kanagaretnam, Krishnan, and Lobo (2010) show that independence of auditors matters for banks' provisioning practices. Altamuro and Beatty (2010) find that the FDICIA Act, which improved bank's internal controls, led to more informative and conservative provisioning practices. Delis et al. (2018) document that supervisory enforcement actions lead to improvements in banks' accounting and provisioning practices. Costello, Granja, and Weber (2019) use a regulatory stringency index to show that strict regulators are more likely to enforce income-reducing reporting choices. Consequently, distortions in bank financial reporting can lead to increases in bank and systemic risk (Bushman and Williams 2015, Acharya and Ryan 2016), reductions in the supply of loans (Beatty and Liao 2011), lower bank valuations (Huizinga and Laeven 2012), and higher resolution costs (Granja 2013). We document that the nearby presence of regulatory offices leads to timelier provisioning practices, less use of provisioning to increase income and as a result, more transparent banks that are more financially stable.

1.2 Institutional Background

Banks in the United States operate under a decentralized structure of supervision. Three federal regulators divide the supervision of U.S. commercial banks among them based on the charter of the bank (Board of Governors of the Fed 2017, OCC 2017). The OCC supervises banks with a federal charter, that is, all national banks. By contrast, federal regulatory responsibility for state-chartered banks is divided between the FDIC and the Fed. If a bank is a member of the Fed System, it is supervised by the Fed. If not, it is supervised by the FDIC. Prior to the 1980s, different bank charters implied differences in bank capital requirements, lending limits, and permissible activities (Johnson 1994). However, these differences have diminished over time. More recently, banks select their charters based on the costs that regulators charge them for supervision and their accessibility (Rosen 2005, Blair and Kushmeider 2006, Agarwal et al. 2014).[2]

While a bank's charter determines which federal agency is responsible for supervision, the supervisory unit (or "office") in charge of the bank's day-to-day supervision is determined by its geographic location. The Fed System covers 12 districts, each headed by a Federal Reserve Bank, with multiple local offices. For instance, the Federal Reserve Bank of San Francisco heads the 12th district and oversees four local offices (in Seattle, Portland, Salt Lake City, and Los Angeles). Similarly, the FDIC divides its supervisory activities into eight regions (Atlanta, Boston, Chicago, Dallas, Kansas City, Memphis, New York, and San Francisco). In each region, a regional office heads a network of local offices. Likewise, the OCC divides itself into four districts (Central, Northeastern, Southern, and Western) and relies on a network of offices in each district to supervise banks within their area of jurisdiction.

These decentralized networks are vital for supervisors to carry out their onsite and offsite monitoring of banks. Traveling examiners, who are based in these offices, spend on average 50% of their time traveling to banks under their purview to conduct onsite "safety and soundness" examinations (OCC 2020).[3] These examinations ex-

2. We take several steps to ensure that bank preferences for a certain charter do not bias our results. First, we exclude banks which have changed their charters. Second, we also omit banks which have changed their headquarters. Third, we include bank, regulatory office, and time fixed effects to control for any time-invariant differences. We detail this in Section 2.

3. This figure is calculated from https://careers.occ.gov/careers/explore/bank-supervision/field-examiner/field-examiner-travel-percentages.html.

amine and rate all components of a bank's operations under the CAMELS ratings systems; capital adequacy, asset quality, earnings, management, earnings, liquidity, and sensitivity to market risk. Other than verifying the accuracy of submitted quantitative information, supervisors also use these examinations to gather and assess qualitative aspects of a bank's operations such as banks' risk-management processes and models, internal controls, loan portfolios, and meet with and evaluate the bank's management.

Between onsite examinations, examiners engage in offsite monitoring to assess a bank's financial condition via review of the call reports filed with supervisors on a quarterly basis. Examiners have broad discretion in evaluating both quantitative and qualitative aspects of a bank's operations and performance (Board of Governors of the Fed 2019, FDIC 2020). As such, examiners working out of field offices that are in close proximity to banks are likely to be able to monitor banks more efficiently due to local informational advantages. For instance, the FDIC Risk Management of Examination Policies writes:

> … Prior to assigning an asset quality rating, several factors should be considered. The factors should be reviewed within the context of any local and regional conditions that might impact bank performance. FDIC (2012a)

Furthermore, field offices also allow for more efficient communication with bank executives and personnel before, during, and after examinations, allowing examiners to understand the operations of the bank more clearly (FDIC 2012b). Consequently, being close to banks could lead to more effective supervision. Outside of bank examinations, supervisors also conduct outreaches and dialogues with bankers in their region to better understand their needs and concerns and to communicate and clarify supervisory expectations. Bankers are also strongly encouraged to meet with and engage in discussions with regional supervisory staff should they require regulatory approval in certain transactions and activities such as mergers and acquisitions, investments in real estate, capital retirement, and changes in control (FDIC 2012b). Thus, this network of offices forms a focal point of contact between supervisors and bankers.

2. REGULATORY OFFICE CLOSURES, DATA, AND EMPIRICAL METHODOLOGY

2.1 Regulatory Office Locations

We construct a panel data set to identify the locations of federal regulatory offices to investigate if decentralized supervisory networks matter for bank behavior. Our analysis focuses on federal regulators (rather than state regulators) because they have been shown to enforce supervision more consistently than state regulators. For instance, Agarwal et al. (2014) show that the stringency of state regulators is influenced by their concerns regarding local economic conditions and differences in regulatory resources. Furthermore, Wilson and Veuger (2017) contact state regulators who confirm there have not been significant relocations since the 1980s, a period preceding our sample.

Data on the historical locations of offices are not directly obtainable from regulators. We therefore hand-collect and verify historical office locations from the annual reports of the 12 Federal Reserve banks, obtained from their websites and the FRASER archive maintained by the Federal Reserve Bank of St. Louis. When we cannot identify a past office location in this way, we consult accounts detailing the histories of the Federal Reserve banks. These accounts often include architectural descriptions as well as the locations of the buildings used by the Fed banks and their local offices.[4] We are able to identify the exact geographical locations of all Federal Reserve Banks and their offices from 1984 to 2013.

The FDIC and OCC offer considerably less information on the locations of their offices in their Annual Reports and websites. We therefore rely on a different strategy to identify the historical locations of offices belonging to the FDIC and OCC.[5] We use Wayback Machine, a web archiving site (https://archive.org/web/) to access past versions of the websites of the FDIC and OCC at specific intervals. Since the FDIC and OCC maintain information on office locations on their websites, accessing past versions of these websites allows us to map the historical locations of FDIC and OCC offices over time. We are able to retrieve locations of FDIC offices from 2002 to 2009 and of OCC offices from 2004 to 2013 (differences in data coverage are due to differences in the coverage of Wayback Machine). Using the above steps, we accurately determine the location of 93 unique FDIC offices between 2002 and 2009, 78 OCC offices between 2004 and 2013, and 37 Federal Reserve offices between 1984 and 2013.

2.2 *Regulatory Office Closures and Treatment*

We designate an office as closed the year it disappears from the data set that we have assembled. For example, the Federal Reserve Bank of New York Buffalo Office last appeared in official documents in 2008. In our analysis, we treat 2009 as the closure year. To ensure that offices are not simply renamed following minor relocations, we manually check and compare the addresses of new offices that appear in our data set after an existing office was closed. We exclude closures where a new office is opened in the same county up to 1 year afterward.

To further ascertain if offices are indeed closed, we then cross-check closures with supporting data from federalpay.org, which lists on an annual basis the occupation,

4. For instance, the Federal Reserve Bank of Boston explains in its website: … In 1977, the Boston Fed moved once more to its current site at 600 Atlantic Avenue in Dewey Square. The 1922 Reserve Building was declared a Boston Landmark in the 1980's and now serves as a luxury hotel, The Langham (www.bostonfed.org/about-the-boston-fed/our-history.aspx).

5. Office locations can typically be found under the "contact" or "organizational structure" pages. For instance: https://www.fdic.gov/about/contact/directory/#Field_Offices and http://www.occ.treas.gov/about/who-we-are/district-and-field-offices/index-organization.html.

location, and salary of federal employees in the United States, subject to certain criteria such as having salaries above a certain threshold. Using this, we can observe if bank examiners at the FDIC, Fed, and OCC change their location of work when an office closes. If we are unsure if an office is closed or relocated, we omit it from our sample. Therefore, our sample of closures only includes permanent closures. In total, we identify 11 office closures (five FDIC, one Fed, and five OCC). Panel A of Table 1 shows the years in which offices close. Figure 1 shows office locations, including those we identify as having closed.[6] It is worth noting that we also observe four office openings but do not include them in our analysis. This is due to the small sample size of affected banks after we apply the various criteria to form the sample that we use (as detailed in Section 2.3). Therefore, in our paper, we focus only on closed offices.

To construct our sample, we begin by obtaining a list of commercial U.S. banks with available quarterly financial data from Call Report filings that banks submit to banking regulators. We assume an office is responsible for supervising a bank if it meets two conditions: (i) the office belongs to the federal regulator that the bank indicates in its call reports as responsible for supervision,[7] and (ii) the office is geographically closest to the bank's headquarters.[8] Following a closure, we assume that the next closest office responsible for banks of the same charter takes over the supervision of affected banks. Therefore, our treatment group consists of banks which are supervised by an office that is eventually closed.

Assignment to the treatment group relies on the assumption that the closest regulatory office is responsible for supervising a bank. We make this assumption because we are unable to observe which regulatory office supervises a particular bank. In practice, the supervision of banks is often assigned to regulatory offices based on geography (most commonly, delineated by state, region, or county). Since these geographical markers correlate highly with distance, it is plausible that responsibility for supervision should fall to the office closest to it. However, when banks are equally close to two (or more) regulatory offices, misclassification of treated and control banks

6. Our subsample of OCC closures is smaller compared to Gopalan, Kalda, and Manela (2021) because we do not include closures if another office opens in the same county up to one year later or when offices experience repeated closures and openings in consecutive years. As we are unable to rule out that these types of closure are permanent (and are not due to temporary office relocations or reporting mistakes), we omit them from our sample.

7. As detailed in Section 1.2, nationally chartered banks are supervised by the OCC. State-chartered banks that are not members of the Federal Reserve System are supervised by the FDIC, while state-chartered banks that are members of the Federal Reserve System are supervised by the Fed.

8. We use the distance to banks' headquarters because onsite examinations involve discussion with senior management and risk management units who tend to be based at headquarters. To calculate the geographical distance between the headquarters of each bank and regulatory offices, we obtain the latitude and longitude coordinates corresponding to their zip codes from the U.S. Census Bureau Gazetteer. We then use the Haversine Formula to obtain the km distance between each bank-office pair. The distance between locations 1 and 2 is calculated as follows: $\text{Distance}_{12} = r \times 2 \times \arcsin(\min(1, \sqrt{a}))$ where $a = [\sin(\text{lat2} - \text{lat1})/2]^2 + \cos(\text{lat1}) \times \cos(\text{lat2}) \times [\sin(\text{lon2} - \text{lon1})/2]^2$ and $r \approx 6{,}378$ km (the radius of the earth); lat and lon stand for latitude and longitude, respectively.

TABLE 1

Regulatory Office Closures and Summary Statistics

Panel A: Office closures

Federal regulator	Year closed	# Offices Closed
Fed	2009	1
FDIC	2004	4
FDIC	2008	1
OCC	2006	1
OCC	2007	1
OCC	2010	2
OCC	2011	1

Panel B: Summary statistics	#	Mean	std.	p1	p50	p99
Loan variables:						
Total Loans (to *Total Assets*)	8,321	0.609	0.167	0.163	0.629	0.888
Real Estate Loans (to *Total Loans*)	8,321	0.658	0.167	0.215	0.686	0.944
Agri Loans (to *Total Loans*)	8,321	0.068	0.107	0	0.016	0.481
C&I Loans (to *Total Assets*)	8,321	0.094	0.064	0.003	0.078	0.317
C&I Loans (to *Total Loans*)	8,321	0.153	0.092	0.008	0.134	0.467
Indiv Loans (to *Total Loans*)	8,321	0.105	0.092	0.003	0.079	0.428
Indiv Loans (to *Total Assets*)	8,321	0.057	0.048	0.002	0.045	0.248
RRE Loans (to *Total Assets*)	7,772	0.272	0.125	0.019	0.266	0.639
CRE Loans (to *Total Assets*)	8,321	0.261	0.156	0.018	0.235	0.677
Risk & performance variables:						
Z-Score	7,254	143.45	109.5	14.14	115.99	547.60
σROA	7,254	0.0013	0.0012	0.0002	0.0009	0.0060
σROE	7,254	0.0131	0.0134	0.0019	0.0089	0.0713
$ROA/\sigma ROA$	7,254	3.741	3.255	−0.647	2.956	14.650
$ROE/\sigma ROE$	7,254	38.05	34.77	−7.63	28.66	150.36
ROA	8,321	0.0022	0.0033	−0.0155	0.0026	0.0081
ROE	8,321	0.0235	0.0277	−0.0693	0.0251	0.0905

(*Continued*)

TABLE 1

(CONTINUED)

Panel B: Summary statistics	#	Mean	std.	p1	p50	p99
Financial variables:						
Total Assets (Log)	8,321	11.43	0.99	9.28	11.4	13.44
Total Deposits (to *Total Assets*)	8,321	0.84	0.073	0.534	0.857	0.928
Tier-1 Capital (to *RWA*)	8,321	0.179	0.123	0.086	0.144	0.996
Equity Capital (to *RWA*)	8,321	0.186	0.125	0.089	0.149	1.011
LLA (to *Total Loans*)	8,321	0.0148	0.0074	0.0046	0.013	0.0465
LLP (to *Total Loans*)	8,321	0.0011	0.0024	−0.0012	0.0005	0.0121
BHC	8,321	0.766	0.424	0	1	1
Mandatory Audit	8,321	0.039	0.195	0	0	1
Audit	8,321	0.592	0.492	0	1	1
Bad Loans (to *Total Loans*)	8,321	0.0101	0.0147	0	0.005	0.0699
Loan Charge-Offs (to *Total Loans*)	8,321	0.0009	0.0021	0	0.0002	0.0108
Dividends (to *Total Assets*)	6,998	0.002	0.003	0	0.0007	0.014
Dividends (to *Net Income*)	6,985	0.537	1.185	−1.059	0.253	6.452
Distance variables:						
Distance	8,321	107	74.02	3.78	103.37	371.6
$\%\Delta$*Distance*	8,269	0.225	0.318	0	0.094	1.323
Accounting variables:						
|*ALLP A*|	7,953	0.118	0.136	0.002	0.082	0.916
+*ALLP A*	2,228	0.178	0.366	0.001	0.073	1.648
−*ALLP A*	5,725	−0.103	0.077	−0.402	−0.084	−0.003
County & state variables:						
County Income per Cap (Log)	8,321	3.464	0.24	3.003	3.457	4.067
County Pop (Log)	8,321	4.352	1.737	1.647	3.772	8.211
County HHI	8,321	1,081	795.5	275.9	848.7	3,334
County Pop Density	8,321	0.000181	0.000285	0.0000018	0.0000307	0.000997
Δ*State UR*	8,321	0.0388	0.173	−0.138	0	0.583
Δ*State HPI*	8,321	0.00815	0.00832	−0.0222	0.00928	0.0219

(*Continued*)

TABLE 1

(CONTINUED)

Panel C: Diff. in means preclosure	(1)	(2)	(3)	(4)	(5)	(6)
	Treated		Control		Diff. in means	
Variables	#	mean	#	mean	(Treated-Control)	*p*
Distance (km)	278	96.33	140	95.40	0.93	0.89
Total Loans/Total Assets	278	0.594	140	0.610	−0.016	0.33
Real Estate Loans/Total Loans	278	0.652	140	0.648	0.004	0.82
Agri Loans/Total Loans	278	0.066	140	0.071	−0.005	0.62
C&I Loans/Total Loans	278	0.151	140	0.164	−0.013	0.17
RRE Loans/Total Assets	278	0.262	140	0.254	0.007	0.58
CRE Loans/Total Assets	278	0.243	140	0.259	−0.016	0.28
Indiv Loans/Total Loans	278	0.115	140	0.099	0.016	0.11
Z-Score	264	131.94	128	132.31	−0.37	0.97
σ*ROA*	264	0.0015	128	0.0014	0.0001	0.74
σ*ROE*	264	0.0146	128	0.0146	0.0000	0.98
ROA/σ*ROA*	264	3.322	128	3.489	−0.167	0.62
ROE/σ*ROE*	264	33.646	128	35.539	−1.893	0.59
ROA	278	0.0014	140	0.0012	0.0002	0.70
ROE	278	0.0161	140	0.0162	−0.0001	0.98
Log Total Assets	278	11.364	140	11.415	−0.051	0.62
Total Deposits/Total Assets	278	0.846	140	0.835	0.011	0.18
Tier-1 *Capital/RWA*	278	0.179	140	0.180	−0.001	0.94
Equity Capital/RWA	278	0.187	140	0.184	0.003	0.85
Dividends/Total Assets	240	0.002	117	0.002	0	0.99
Dividends/Net Income	239	0.909	117	0.872	0.037	0.87
LLA/Total Loans	278	0.0155	140	0.0146	0.0009	0.28
LLP/Total Loans	278	0.0015	140	0.0013	0.0002	0.48
Mandatory Audit	278	0.029	140	0.043	−0.014	0.45
Audit	278	0.579	140	0.600	−0.021	0.68
Bad Loans/Total Loans	278	0.0112	140	0.0098	0.0014	0.42
Loan Charge-Offs/Total Loans	278	0.0016	140	0.0014	0.0002	0.43
$\lvert ALLP\ A\rvert$	267	0.138	133	0.124	0.014	0.40
$-ALLP\ A$	176	−0.105	90	−0.091	−0.014	0.19
$+ALLP\ A$	91	0.232	43	0.185	0.047	0.49

(*Continued*)

TABLE 1

(CONTINUED)

Panel D: Diff. in mean changes preclosures

# Quarters prior to office closures	t − 1 Diff. in means	p	t − 2 Diff. in means	p	t − 3 Diff. in means	p	t − 4 Diff. in means	p
Δ(*Total Loans/Total Assets*)	−0.00584	0.21	0.00786	0.17	−0.00004	0.73	0.00082	0.48
Δ*Z-Score*	1.35793	0.53	−5.86073	0.11	−2.19729	0.80	−0.91892	0.59
Δ(*Bad Loans/Total Loans*)	−0.00094	0.44	0.00152	0.31	−0.00162	0.80	−0.00035	0.08*
Δ(*Loan Charge-Offs/Total Loans*)	0.00030	0.29	−0.00033	0.52	0.00014	0.77	0.00014	0.77
Δ(*LLP/Total Loans*)	−0.00018	0.64	0.00024	0.45	0.00000	0.65	0.00001	0.66
Δ−*ALLP A*	0.00822	0.29	0.01355	0.33	−0.00685	0.12	−0.01294	0.29
Δ|*ALLP A*|	−0.01666	0.54	0.00901	0.61	−0.00181	0.30	0.01880	0.39
Δ+*ALLP A*	0.04655	0.36	0.07668	0.76	−0.04470	0.41	−0.01687	0.42
Δ(*Dividends/Net Income*)	0.10731	0.20	−0.15848	0.24	0.32535	0.23	−0.51920	0.17
Δ(*Dividends/Total Assets*)	0.00005	0.44	−0.00019	0.23	0.00023	0.61	−0.00094	0.07*
Δ(*ROA*/σ*ROA*)	0.04471	0.99	−0.15994	0.03**	−0.06932	0.33	−0.01871	0.26
Δ(*ROE*/σ*ROE*)	0.27263	0.84	−1.87635	0.015**	−0.65139	0.31	−0.08305	0.20
Δ(*Tier-1 Capital/RWA*)	0.00314	0.96	−0.00273	0.78	−0.00157	0.60	0.00108	0.98
Δ(*Equity Capital/RWA*)	0.00368	0.76	−0.00280	0.64	−0.00103	0.73	0.00098	0.89
Δ(*Indiv Loans/Total Assets*)	−0.00010	0.054*	−0.00003	0.48	0.00039	0.59	−0.00025	0.32
Δ(*C&I Loans/Total Assets*)	−0.00097	0.91	0.00098	0.79	0.00369	0.51	−0.00111	0.50
Δ(*CRE Loans/Total Assets*)	0.00043	0.73	0.00039	0.36	0.00247	0.13	0.00197	0.99
Δ(*RRE Loans/Total Assets*)	−0.00048	0.85	0.00870	0.099*	−0.00397	0.28	−0.00072	0.34
# quarters prior to 2008–09 financial crisis								
Δ(*Bad Loans/Total Loans*)	0.00057	0.16	−0.00160	0.31	−0.00153	0.63	0.00082	0.93
Δ(*Loan Charge-Offs/Total Loans*)	0.00040	0.68	−0.00021	0.87	0.00017	0.29	0.00017	0.29

(*Continued*)

TABLE 1

(Continued)

# Quarters prior to office closures	$t-5$		$t-6$		$t-7$		$t-8$	
	Diff. in means	p	Diff. in means	p	Diff. in means	p	Diff. in means	p
Δ(*Total Loans/Total Assets*)	−0.00898	0.004***	−0.00157	0.57	0.00105	0.54	−0.01010	0.11
Δ*Z-Score*	0.48645	0.44	−3.26773	1.00	−0.84332	0.59	−0.11298	0.80
Δ(*Bad Loans/Total Loans*)	0.00090	0.11	0.00060	0.38	0.00045	0.30	−0.00039	0.98
Δ(*Loan Charge-Offs/Total Loans*)	−0.00050	0.97	−0.00050	0.97	0.00077	0.13	0.00002	0.84
Δ(*LLP/Total Loans*)	−0.00008	0.41	0.00022	0.13	0.00009	0.89	0.00008	0.24
Δ-*ALLP A*	0.01065	0.29	0.00339	0.48	−0.01238	0.03**	0.00970	0.27
Δ|*ALLP A*|	−0.02272	0.19	0.00824	0.54	0.00934	0.02**	−0.01156	0.24
Δ+*ALLP A*	0.00897	0.89	0.03111	0.20	0.09654	0.13	−0.14363	0.40
Δ(*Dividends/Net Income*)	0.42426	0.22	−0.17995	0.90	0.18933	1.00	−0.13210	0.82
Δ(*Dividends/Total Assets*)	0.00136	0.02**	−0.00066	0.41	0.00073	0.67	−0.00137	0.25
Δ(*ROA*/σ*ROA*)	0.10998	0.56	−0.11805	0.54	−0.03365	0.16	−0.00357	0.93
Δ(*ROE*/σ*ROE*)	1.40185	0.48	−1.38597	0.67	−0.46940	0.18	−0.09243	0.83
Δ(*Tier*-1 *Capital/RWA*)	0.00526	0.98	0.00387	0.08*	0.00295	0.42	0.00864	0.26
Δ(*Equity Capital/RWA*)	0.00639	0.60	0.00397	0.05*	0.00224	0.58	0.00925	0.12
Δ(*Indiv Loans/Total Assets*)	−0.00082	0.24	0.00098	0.35	−0.00034	0.53	−0.00013	0.65
Δ(*C&I Loans/Total Assets*)	−0.00365	0.02**	0.00024	0.98	0.00241	0.11	−0.00489	0.54
Δ(*CRE Loans/Total Assets*)	−0.00304	0.18	−0.00060	0.82	−0.00560	0.07*	−0.00036	0.95
Δ(*RRE Loans/Total Assets*)	−0.00244	0.20	−0.00190	0.33	−0.00305	0.85	0.00085	0.82
# quarters prior to 2008–09 financial crisis								
Δ(*Bad Loans/Total Loans*)	0.00019	0.76	−0.00071	0.13	0.00086	0.54	0.00034	0.64
Δ(*Loan Charge-Offs/Total Loans*)	0.00006	0.38	0.00006	0.38	−0.00022	0.61	−0.00002	0.10

Note: This table reports information on regulatory office closures (Panel A), summary statistics for the variables used (Panel B), differences in means between the treated and control group preclosure (Panel C), and differences in changes of means between the treatment and control group preclosure (Panel D). Treated banks are those supervised by the closed office. Control banks are headquartered in the same county as a treated bank, but not supervised by the closed office. See Section 2 for construction of treatment and control groups. The sample consists of U.S. commercial banks for the years 2002–13 (unbalanced panel). Panel A shows the year of regulatory office closures. Panel B shows summary statistics of variables used. Panel C presents the means of bank characteristics and tests for differences between the treated and control group in the preclosure period ($t-1$). Panel D presents the changes in means for bank characteristics and tests for differences between the treated and control group in the eight quarters prior to office closures or the 2008 financial crisis. Bank balance-sheet variables are winsorized at the 1% and 99% levels. See Table IA1 (Internet Appendix) for definitions of the variables. # is the number of bank-quarter observations, std. is the standard deviation, and p1, p50 and p99 are the 1st, 50th, and 99th percentiles. ***, **, and * significant at 1%, 5%, and 10% level.

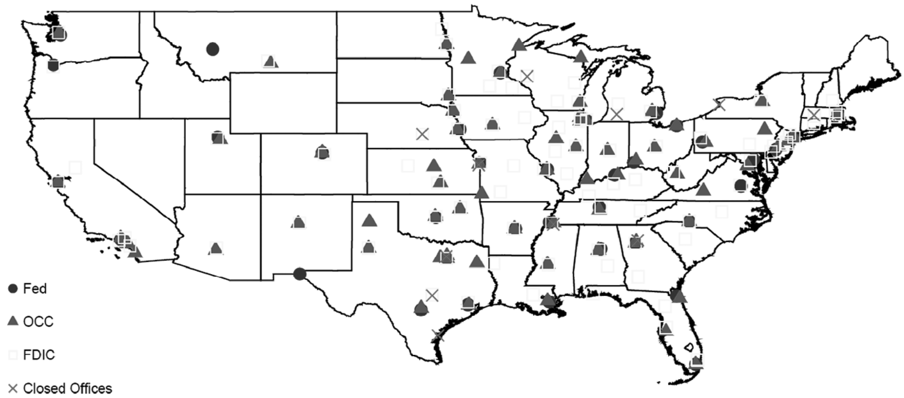

Fig 1. Locations of Regulatory Offices.

NOTE: This figure shows the geographical locations of regulatory office in the United States. The circle denotes Federal Reserve (Fed) offices from 1984 to 2013. The triangle shows locations of offices belonging to the Office of the Comptroller of the Currency (OCC) from 2004 to 2013. The squares are offices belonging to the Federal Deposit Insurance Corporation (FDIC) from 2002 to 2009. The x's show the locations of offices belonging to the OCC, Fed, and FDIC that are closed between 2001 and 2013.

becomes more likely. In Section "Verification of closest regulatory office as bank supervisor," we focus on a subset of banks that are most likely to be misclassified and show that our results remain unchanged. This gives us confidence that the distance assumption we impose is likely to be valid.

2.3 *Empirical Strategy*

We use a DiD analysis to contrast the behavior of banks following the closure of their regulatory office to banks located in the same counties but not affected by the office closure. This is possible because banks in the same geographical areas can be supervised by different offices, depending on their federal regulator (charter). We estimate variants of the following specification:

$$\begin{aligned} Y_{i,k,t} &= \alpha_{i,k,t} + \beta_1 Treated_{i,k,t} + \beta_2 Post_{k,t} + \beta_3 Treated_{i,k,t} \times Post_{k,t} + Z_{i,k,t} \\ &\quad + Fixed\ Effects + \varepsilon_{i,k,t}, \end{aligned} \tag{1}$$

where Y is a bank outcome variable and i, k, and t indicate bank i, regulatory office k, and year-quarter t, respectively, and Z is a vector of control variables. We estimate our DiD regressions with and without control variables as their inclusion could introduce bias if these variables are themselves affected by office closures (Angrist and Pischke 2009). We also rely on various FEs which we detail in the main analysis (Section 3.1).

Treated is equal to 1 if banks are in the treated group, and 0 for banks in the control group. Treated banks are those that were under the supervision of a closed office. Our control group contains banks headquartered in the same counties as treated banks but not affected by regulatory office closures (because they are not supervised by the closed office). By comparing banks that reside in similar geographical regions, we alleviate concerns that demand side shocks (e.g., changes in economic conditions) could bias our results. We estimate the average treatment effect using an estimation window of ± 2 years around office closures; *Post* is equal to 1 for up to 2 years after the closure and 0 for the 2 years before closure. We use a 5-year window as analyzing bank behavior over longer periods risks introducing noise (Bertrand, Duflo, and Mullainathan 2004). Our variable of interest is the coefficient on the interaction term *Treated* $\times$ *Post* that takes a value of 1 for treated banks after closure of its office, and 0 otherwise. *Treated* $\times$ *Post* therefore captures changes in the behavior of banks affected by office closures relative to their preclosure behavior, and relative to unaffected banks in the same county. Standard errors are clustered at the regulatory office level.[9]

We impose several criteria to sharpen our empirical strategy. We require that both treated and control banks have: (i) <\$1bn in assets, (ii) not relocated their headquarters, and (iii) not changed charters during our 5-year DiD window. The main reason we focus our analysis on small community banks is because they are tied to local markets and have traveling rather than in-house examiners. Furthermore, larger banks may be subject to different levels of regulatory and market discipline which could bias our results (e.g., Flannery and Bliss 2019, Hirtle, Kovner, and Plosser 2020). We limit our analysis to banks that have not changed their charters or relocated their headquarters to minimize issues such as "regulatory shopping" to evade supervision (e.g., Rosen 2003, 2005). These filters ensure that any potential reasons banks might have for selecting different charters (which would determine if they would be affected by office closures) do not bias our analysis.[10]

The final sample that we use for our DiD analysis consist of 278 (140) treatment (control) banks with a total of 8,321 bank-quarter observation from 2002 to 2013. Financial variables are winsorized at the 1% and 99% tails. Summary statistics are

9. In unreported analyses, all our findings in the paper remain similar when we cluster standard errors by county.

10. It could be argued that even by restricting our sample to banks that have not changed their charters or headquarters, the assignment of banks to treated and control groups is in fact not random because banks have self-selected to be under a certain regulator and regulatory office, based on their charter and HQ location. Therefore, unobservable differences in bank-level characteristics could exist between the groups and bias our results. We believe that this is unlikely to undermine our findings. First, the inclusion of bank fixed effects in all our estimations means that any time-invariable unobservable bank characteristics that would lead a bank to select a regulator (or office) would be controlled for. Furthermore, we compare treated and control banks to themselves, before and after office closures, thereby differencing out the effects of unobservables. Second, we show in Table 1, Panel C that our group of control and treated banks are similar along balance-sheet variables in the preclosure period. Having similar characteristics across all variables suggests that banks in our analysis are also likely to be similar along unobservable characteristics. For instance, if banks have a preference to be under a particular regulator or office to pursue riskier bank strategies, we should find differences in risk or business strategies between the two groups of banks. Finally, it is not obvious *what* unobservable bank characteristic could bias our results in light of the various fixed effects we employ.

presented in Panel B of Table 1. Table IA1 in the Internet Appendix shows definitions of all variables that we use.

Our empirical strategy requires that the control group represents an adequate counterfactual. We show that banks in both the treated and control group are similar along the 30 main bank-level financial variables used in this study in the preclosure period ($t - 1$). t-Test of differences in level means, which are displayed in Columns (5)–(6) of Panel C in Table 1, reveal both groups to be statistically indistinguishable.

A further identifying assumption in a DiD analysis is the parallel trends assumption. It states that, absent treatment, changes in the outcome variables would have evolved similarly for both treatment and control groups. This assumption cannot be directly validated because we cannot observe how bank policies would have evolved in the absence of office closures. As such, we rely on the conventional approach of plotting the outcome variables for treated and control banks in the preclosure period. If the outcome variables of the two groups of banks follow similar trends prior to office closures, it is more likely then that the parallel trends assumption would hold. Figure IA1 in the Internet Appendix plots the levels of the 18 outcome variables we use in the eight quarters prior to office closures for treated and control banks plus the 2 outcome variables prior to the 2008–09 financial crisis. A visual inspection of the figures suggests similar preshock trends.

To complement the parallel trends graph, we follow Roberts and Whited (2013) and test for differences in the mean growth rates between the treated and control group prior to the shock. As before, we focus on each of the eight quarters prior to office closures for the 20 outcome variables and show the results in Panel D of Table 1. Out of the 160 cells, only 7 (14) have statistically different mean growth rates at the 5% (10%) level. More importantly, 19 out of the 20 outcome variables do not display statistically dissimilar mean growth rates for more than a single quarter. This suggests that the growth rates of treated and control banks are likely to be similar and that the parallel trends assumption is plausible.[11]

2.4 Determinants of Regulatory Office Closures

A natural question that arises is: why are regulatory offices closed? Minimizing the costs of maintaining multiple onsite locations is a likely consideration behind the closure of a regulatory office. For instance, when announcing the closure of its Buffalo Branch, the New York Fed announced that:

> … follows a re-examination of the Bank's regional strategy, which determined that the Second District would be better served if the Bank rebalanced the resources applied to

11. Findings that treated and control banks produce similar pretrends also helps rule out concerns of reverse causality. Should reverse causality be present, we should expect to observe differences in growth rates between treated and control banks in the preclosure period. For instance, if treated banks had increasing nonperforming loans prior to office closures, this might indicate deteriorating performance. Consequently, deteriorating bank performance might lead to reductions in assets under supervision for the regulatory office which then results in its closure. Since we do not observe this, concerns related to reverse causality are alleviated.

its regional efforts to enhance analysis and outreach across the entire District. (Federal Reserve Bank of New York 2008)

Arguably, the viability of an office in a particular location is tied to the amount of supervision that examiners can conduct from that office (i.e., the amount of assets under supervision). Therefore, a potential concern with our empirical strategy is that office closures correlate with bank performance and risk. For instance, if we observe decreases in banking assets under supervision for offices that are eventually closed, this could indicate that treated banks are performing poorly and losing market share. Poor bank performance might then simultaneously predict office closures (as the office becomes less important due to shrinking assets under supervision) and higher bank risk-taking due to moral hazard (e.g., Myers 1977). If so, there could be concerns of reverse causality and simultaneity. Therefore, understanding the determinants of office closures is important to validate our empirical strategy.

We start off by examining if bank characteristics (and their changes) predict regulatory office closures. Panel A in Table 2 shows the results of logit regressions where *Office Closure* is 1 in the office-year in which an office closes. We include two variables to control for the organizational structure of regulators: *Main Office* (equals 1 if the office is a main regional office, and 0 if it is a satellite office) and *Beside Main Office* (equals 1 if the geographically closest office is the main regional office, and 0 if the geographically closest office is another satellite office). Main offices are nonsatellite field offices and serve as the regulatory "headquarters" for that region. As focal points for various regulatory functions, they enjoy economies of scale and scope in their operations, including supervision.

We expect that main offices are less likely to be closed due to their importance while offices that are geographically closest to a main office are more likely to be shut to avoid duplication of regulatory functions and to reduce costs. We also include regulatory district FEs for the 12 Federal Reserve, eight FDIC, and four OCC districts to control for potential differences in closure decisions across regulatory agencies and districts.[12] Finally, we include year FEs to account for time trends and lag all independent variables.

To investigate the determinants of office closures, we calculate several variables at the office-year level to capture the characteristics of banks under the supervision of an office: *Avg Z-Score*, *Avg ROA*, *Avg Total Assets*, and *Avg Total Loans*. These variables are calculated using a lagged 3-year trailing average.[13] We use a 3-year trailing average because the decision to maintain or close an office is unlikely to be

12. It is worth noting that a district could have multiple regional main offices. Recall that our definition of a main regional office is a nonsatellite office.

13. We briefly illustrate with an example on how these variables are calculated. Recall that we lag all independent variables, therefore, given regulatory Office 1 in year 2008, *Avg ROA* is calculated as the average ROA of banks supervised by Office 1 in years 2005–07. Meanwhile, *Avg Total Assets* for Office 1 in year 2008 is calculated first by summing up the total bank assets under its supervision in each of the years from 2005 to 2007 before taking its average.

TABLE 2

Determinants of Regulatory Office Closures

Panel A: Characteristics of banks under supervision of office	(1)	(2)	(3)	(4)
	Office closure			
Avg Z-Score	0.883	0.874		
	[−0.055]	[−0.061]		
Avg ROA	0.515	0.486		
	[−0.121]	[−0.132]		
Avg Total Assets	0.81			
	[−0.251]			
Avg Total Loans		0.793		
		[−0.299]		
Δ*Avg Z-Score*			0.253	0.18
			[−0.190]	[−0.241]
Δ*Avg ROA*			0.00002	0.00007
			[−0.984]	[−0.924]
Δ*Avg Total Assets*			0.798	
			[−1.509]	
Δ*Avg Total Loans*				0.93
				[−0.811]
Main Office	0.126*	0.127*	0.093**	0.106*
	[−1.778]	[−1.777]	[−1.988]	[−1.959]
Beside Main Office	590.742***	593.243***	703.254***	569.961***
	[3.532]	[3.534]	[3.663]	[3.569]
Regulatory District FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Adj. R^2	0.364	0.365	0.393	0.376
Observations	733	733	733	733

Panel B: Characteristics of banks under supervision of main office	(1)	(2)	(3)	(4)
	Office closure			
Avg Z-Score (Main)	0.283	0.296		
	[−0.510]	[−0.488]		
Avg ROA (Main)	0.073	0.068		
	[−0.478]	[−0.482]		
Avg Total Assets (Main)	0.434			
	[−1.059]			
Avg Total Loans (Main)		0.441		
		[−1.128]		
Δ*Avg Z-Score* (Main)			0.252	0.155
			[−0.180]	[−0.257]
Δ*Avg ROA* (Main)			0.01	0.004
			[−0.407]	[−0.524]
Δ*Avg Total Assets* (Main)			0.754*	
			[−1.653]	
Δ*Avg Total Loans* (Main)				0.898
				[−1.107]
Regulatory District FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Adj. R^2	0.138	0.14	0.167	0.138
Observations	203	203	203	203

Note: Results of logistic regressions on the determinants of regulatory office closures. The specification is:
$Office\ Closure_{k,t} = a_{k,t} + Variables_{k,t-1} + Regulatory\ District\ FE + Year\ FE + e_{k,t}$,
where $Office\ Closure_{k,t} = 1$ if office k is closed in a year t, and 0 otherwise. The explanatory variables in Panel A are characteristics of banks supervised by office k, and in Panel B they are characteristics of banks supervised by the regional main office of office k. *Avg Z-Score* = rolling 3-year average Z-Score of supervised banks, and $Z\text{-Score}_t = \text{Log}(ROA+\text{Equity})/(\sigma ROA)$, where ROA = Net Income/Total Assets, Equity = Total Equity/Total Assets, and (σROA) = standard deviation of ROA years $t-2$ to t. *Avg ROA* = rolling 3-year average ROA of supervised banks; *Avg Total Assets* = rolling 3-year average of sum of Log(Total Assets) of supervised banks and *Avg Total Loans* = rolling 3-year average of sum of Log(Total Loans) of supervised banks. *Main Office* = 1 if office k is a main regional office, and 0 if it is a satellite office; *Beside Main Office* = 1 if the geographically closest office to office k is the main regional office, and 0 if the geographically closest office is another satellite office. Bank variables are winsorized at the 1% and 99% levels. See Table IA1 (Internet Appendix) for definitions of the variables. The coefficient shown is the odds ratio. t-Statistics are in parenthesis. ***, **, and * significant at 1%, 5%, and 10% level.

based on the characteristics of banks in a single year.[14] Columns (1)–(2) of Panel A in Table 2 show that the average risk, performance, total assets, and total loans of banks under the supervision of an office do not predict its closure (all coefficients shown are in odds ratios). Likewise, changes in these variables do not predict office closures (Columns (3)–(4)). Thus, office closures do not appear to be caused by reductions in assets under supervision or deteriorating bank performance.

As expected, we find that regulatory offices that are located closest to a regional main office are more likely to be closed (*Beside Main Office* enters significantly). Indeed, in all but one instance in our sample, regulatory offices that were eventually closed were the geographically closest office to the main office. Thus, it is likely that decisions to close an office are dependent on changes in the workload at the main office. To further investigate this, we start by restricting our sample to only offices for which the main office is its nearest office.

We repeat the analysis in Panel A of Table 2 but instead, use the characteristics of banks under the supervision of the main office. That is, we investigate if changes in bank performance or assets under supervision at the main office predict the closure of its nearest satellite office. We show the results in Panel B of Table 2. Despite the small sample size, Column (3) finds some evidence that decreases in the growth rates of assets under the supervision of a main office predict closures of the nearest satellite office. This suggests that when main offices experience a decreasing workload, the closest nonmain offices are more likely to be closed, presumably to consolidate resources.

Jointly, the results in this section show that changes in banking activity at main offices explain office closures—*not* the performance and assets under supervision of closed offices. Thus, the reasons for office closures reside with banks outside the immediate vicinity of the closed office. Consequently, reverse causality explanations are unlikely to explain our results.[15]

3. MAIN RESULTS

3.1 Regulatory Office Closures, Bank Lending, and Risk-taking

We rely on office closures to investigate if the proximity of regulatory office networks influences bank behavior. If the monitoring (regulatory capture) explanation

14. In unreported results, our findings remain consistent when we use a 2-year rolling average.

15. We also conduct additional bank-level analyses to further address reverse causality and simultaneity. For instance, when faced with negative asset shocks (i.e., large declines in assets due to deteriorating performance), moral hazard may cause banks to grow their loan portfolios more aggressively and take on more risk. If these declines in assets also simultaneously lead to office closures (as the workload of regulatory offices declines), there might be concerns of reverse causality and simultaneity. In Table IA2 of the Internet Appendix, we show that: (i) treated banks that experience large decreases in asset growth rates and (ii) treated banks with different asset growth rates do not have higher lending or risk-taking after office closures as compared to control banks. Finally, (iii) the results from a timing-effects model also show that conditional on asset declines and growth rates, treated banks do *not* make more loans or have higher risk pre- and postclosure.

dominates, we should expect to observe riskier (safer) bank policies after office closures as examiners would now be housed in more distant offices. One of the main ways in which banks can increase their risk-taking is to increase their lending (Foos, Norden, and Weber 2010, Fahlenbrach, Prilmeier, and Stulz 2018).

To investigate if office closures affect a bank's lending, we use (Total Loans/Total Assets) in Columns (1)–(4) of Table 3. As observed, the coefficient on *Treated* $\times$ *Post* is positive and statistically significant across all columns. This shows that treated banks grow their loan portfolios more aggressively relative to the control group after office closures. The economic magnitudes are also notable. For instance, banks affected by an office closure increase their total lending by $6-10\%$, depending on specification. Because we scale total loans by total assets, our findings are not simply driven by substitution effects in which banks grow their nonlending assets more aggressively than their lending assets. For robustness, we also show in Table IA3 (Internet Appendix) that our results are similar when using an unscaled measure of lending: *Log Total Loans*.

Our findings are robust to the inclusion of various FEs and specifications. Column (1) is estimated using only bank, year-quarter, and office FEs to alleviate concerns of "bad" controls. We prefer to use a specification without control variables as these variables themselves might be affected by office closures (e.g., capital ratios) and bias our results. Nonetheless, in Column (2), we add a host of additional control variables that might influence bank lending and find similar results.[16]

The inclusion of bank FEs controls for any time-invariant bank-specific variables that differ across banks. This allows us to compare business policies of the same bank around office closures. Therefore, alternative explanations such as affected banks having risk cultures that are inherently different from control banks are less likely to be a cause for concern (e.g., Fahlenbrach, Prilmeier, and Stulz 2012). We also include year-quarter FEs to control for any time effects that could jointly influence bank policies and supervision. The inclusion of regulatory office FEs controls for time-invariant heterogeneity across different offices. Since we include a dummy variable for each office, the regulatory office FEs control for differences *between* and *within* federal regulators. This means that interpretations related to any differences in offices such as the stringency of supervision, style, and preferences are unlikely to drive our results (e.g., Rosen 2003, 2005).

In Columns (3)–(4), we include county-quarter, office-quarter, and bank FEs to address concerns that time-varying omitted variables that arise from local economic shocks or differences in regulatory enforcement practices could bias our results. The inclusion of county-quarter FEs removes any time-varying county-level factors such as local economic shocks or demand-side factors in the form of housing demand (Gilje, Loutskina, and Strahan 2016). Therefore, the estimates in Columns (3)–(4) compare the behavior of banks affected by an office closure with that of banks within the *same county* and within the *same quarter* that are not affected by the closure. The inclusion of office-quarter dummies controls for time-varying intensity of supervision

16. We include *Audit*, *Mandatory Audit*, *ROA*, *Log Total Assets*, *Tier-1 Capital/RWA*, *Total Deposits/Total Assets*, *BHC*, *County Income per Cap*, *County Pop*, *County HHI*, *County Pop Density*, Δ*State UR*, and Δ*State HPI*. See Table IA1 of the Internet Appendix for definition of variables.

TABLE 3

REGULATORY OFFICE CLOSURES, BANK LENDING, AND RISK-TAKING

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Total Loans/Total Assets				*Z-Score*			
Treated × *Post*	0.064***	0.061***	0.102***	0.098***	−0.885***	−1.039***	−1.441***	−1.522***
	[6.367]	[4.164]	[11.939]	[7.647]	[−4.398]	[−4.569]	[−8.590]	[−8.071]
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	No	No	Yes	Yes	No	No
Reg Office FE	Yes	Yes	No	No	Yes	Yes	No	No
County × Year-Quarter FE	No	No	Yes	Yes	No	No	Yes	Yes
Reg Office × Year-Quarter FE	No	No	Yes	Yes	No	No	Yes	Yes
Other controls	No	Yes	No	Yes	No	Yes	No	Yes
Adj. R^2	0.874	0.927	0.869	0.925	0.763	0.77	0.784	0.793
Observations	8,321	8,321	6,570	6,570	7,254	7,254	5,490	5,490

NOTE: Results of difference-in-difference regressions for the effects of regulatory office closures on bank lending and risk-taking. The specification is:

$$Y_{i,k,t} = \alpha_{i,k,t} + \beta_1 Treated_{i,k,t} + \beta_2 Post_{k,t} + \beta_3 Treated_{i,k,t} \times Post_{k,t} + Z_{i,k,t} + Fixed\ Effects + \varepsilon_{i,k,t},$$

where subscripts i, k, and t indicate bank, office, and year-quarter, respectively. Y is either *Total Loans/Total Assets* or *Z-Score*. Z-Score = Log(*ROA* + *Equity*)/(σ*ROA*), where *ROA* = Net Income/Total Assets, *Equity* = Total Equity/Total Assets, and (σ*ROA*) = standard deviation of ROA years $t-2$ to t. *Treated* = 1 if banks are supervised by a closed office k, and 0 if they are in the control group, that is, headquartered in the same county as treated banks but not supervised by the closed office (see Section 2). *Post* = 1 for up to 2 years after closure of office k, and 0 for the 2 years before (5-year diff-in-diff window). The variable of interest is coefficient β_3 on *Treated* × *Post* which = 1 for treated banks for up to 2 years after closure of the office, and 0 otherwise. Z is a vector of control variables. In Columns (2) and (4), Z is: *Audit, Mandatory Audit, ROA, Log Total Assets, Tier-1 Capital/RWA, Total Deposits/Total Assets, BHC, County Income per Cap, County Pop, County HHI, County Pop Density,* Δ*State UR*, and Δ*State HPI*; in Columns (6) and (8), Z is: *Audit, Mandatory Audit, ROA, Log Total Assets, Tier-1 Capital/RWA, Real Estate Loans/Total Loans, Agri Loans/Total Loans, C&I Loans/Total Loans, Indiv Loans/Total Loans, Total Deposits/Total Assets, Total Loans/Total Assets, BHC, County Income per Cap, County Pop, County HHI, County Pop Density,* Δ*State UR*, and Δ*State HPI*. Bank balance-sheet variables are winsorized at the 1% and 99% levels. See Table IA1 (Internet Appendix) for definitions of the variables. Standard errors are clustered at the regulatory office level. t-Statistics are in parenthesis. ***, **, and * significant at 1%, 5%, and 10% level.

specific to each regulatory office. For instance, supervisors might have time-varying forbearance levels due to differing concerns regarding the local economy (e.g., Agarwal et al. 2014). Despite this relatively tight specification, we continue to find similar results as before.[17]

We next turn our attention to investigate if office closures affect a bank's overall risk levels. Because our sample uses unlisted banks, we are unable to use market measures such as tail risk or equity volatility. Therefore, as a measure of overall bank risk, we rely on a bank's *Z-Score*. We follow Laeven and Levine (2009) and Demirgüç-Kunt and Huizinga (2010) and define a bank's *Z-Score* as Log[$(ROA + Equity)/\sigma ROA$]. *ROA* is calculated as (Net Income/Total Assets) while *Equity* is (Total Equity/Total Assets). σROA is the standard deviation of *ROA* measured on a rolling basis from the previous 12 quarters. The *Z-Score* measures the number of standard deviations by which ROA can fall before a bank becomes insolvent. A lower *Z-Score* indicates higher bank risk.

As observed in Columns (5)–(8), the coefficient on $Treated \times Post$ is negative and statistically significant. Consequently, banks affected by an office closure become riskier relative to themselves and relative to a group of control banks headquartered in the same counties. The economic effects are also significant. For instance, the coefficient in Column (5) implies that the *Z-Score* of treated banks is 19% lower after closure, relative to the mean.[18] In Table IA3, we find similar results when we use three alternative measures of risk-taking (an alternative construction of $Z\text{-}Score_2$, σROA, and σROE).

So far, we have shown that banks increase their lending and risk-taking after office closures. Next, we investigate if banks increase (decrease) their lending toward riskier (less risky) types of loans. If so, this would be consistent with our interpretation of riskier banking policies after office closures. We focus on four major loan types and distinguish between lending to businesses (Commercial Real Estate Loans (CRE Loans/Total Assets) and Commercial and Industrial Loans (C&I Loans/Total Assets)) as well as lending to consumers (Residential Real Estate Loans (RRE Loans/Total Assets) and Individual Loans (Indiv Loans/Total Assets)).[19]

17. Another benefit of including bank, county-quarter, and office-quarter FE is that it significantly raises the bar for reverse causality and other explanations behind our results. For instance, by including county x quarter FE, we are comparing treated and control banks located in the same county in the same quarter (and are, therefore, exposed to similar economic conditions). Reverse causality explanations seem unlikely in this setup because they would have to explain why only treated banks should systematically underperform and lead to office closures as a result of decreasing assets (but not other banks operating in the same county-quarter). Furthermore, the inclusion of bank FE also means that any systematic differences in performance and risk of banks are differenced out. As such, any concerns related to reverse causality would need to be unrelated with time-varying factors at the county and office level.

18. It should be noted that since we rely on rolling values of *ROA* over the previous 12 quarters, there is some correlation between *Z-Score* in the pre- and postclosure periods. In unreported results, we conduct the same analysis where we redefine *Post* to equal 1 for year $t + 2$ and 0 for years $t - 2$ and $t - 1$ of the 5-year DiD window. By doing so, we ensure that the constructed rolling values of *ROA* in the preclosure period are not part of the analysis in the postclosure period. Our results remain qualitatively identical.

19. CRE Loans are business loans that are secured by nonresidential real estate. C&I Loans are business loans that are unsecured or are secured with nonreal estate collateral. RRE Loans are family residential

We show the results in Table 4. As observed in Columns (1)–(4), the loan expansion that we previously documented seems to be driven largely by increases in lending to CRE Loans and, to a smaller extent, RRE Loans. For instance, in Column 2 (4), treated banks increase their CRE (RRE) lending by 8.8% (5.3%). This is consistent with our findings of increases in bank risk following office closures as CRE loans are considered a particularly risky loan class by regulators (Rice and Rose 2016) and a key factor behind many bank failures in the banking crises of 1985–92 and 2008–09 (Cole and White 2012). While RRE loans might not traditionally be viewed as riskier, many were originated during 2002–07 (our sample spans 2002–13), a period which coincided with the housing market boom. Various studies have shown that mortgage loans made by banks during that period were of lower quality and therefore riskier (e.g., Dell'Ariccia, Igan, and Laeven 2012, Rajan, Seru, and Vig 2015).

Treated banks can also increase the risk of their portfolios by reducing their exposure to less risky loans. After office closures, treated banks reduce their lending to relative safer C&I and Individual Loans by 2.5% and 5.3% in Columns (6) and (8) (Cole and White 2012). Overall, our results support the earlier findings that treated banks increase their risk-taking by increasing (decreasing) their exposure to riskier (safer) categories of loans. Taken together, the results in this section provide us with evidence that a decentralized supervisory structure is useful in facilitating the monitoring of banks. After closures of offices, we find consistent evidence that affected banks undertake riskier business policies.

3.2 Robustness Tests

Timeline effects. We follow Bertrand and Mullainathan (2003) and estimate a timing-effects model. The benefit of this test is that it allows us to ascertain when treated banks change their behavior surrounding office closures. Increases in lending and risk-taking should only be observable after office closures, not before. We reestimate our lending and risk results in Table 3 by replacing *Post* with a series of dummy variables: *Closure*-1 is a dummy that equals 1 the year before office closures; *Closure* is a dummy that equals 1 for the year of closure; and *Closure*+1 (*Closure*+2) are dummy variables that equals 1 for first (second) year after the closure, respectively. As we omit *Closure*-2 (2 years before office closures), the results are benchmarked against 2 years prior to office closures.

The results are displayed in Panel A of Table 5. The coefficient on the interaction term *Treated* × *Closure*-1 is never significant. The statistically insignificant coefficient on this variable suggests that the behavior of treated banks in the year prior to office closures is not statistically different from their behavior 2 years prior to office closures. Crucially, we also observe that, compared to 2 years prior to office closures, treated banks only increase their lending and risk-taking in the year of office closures (the statistically significant coefficient on *Treated* x *Closure*) and up to 2 years

homes that are secured with residential real estate while Indiv Loans are consumer loans not secured primarily by real estate for nonbusiness reasons (personal expenditure).

TABLE 4

REGULATORY OFFICE CLOSURES AND LOAN PORTFOLIO

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	CRE Loans/Total Assets		*RRE Loans/Total Assets*		*C&I Loans/Total Assets*		*Indiv Loans/Total Assets*	
Treated × *Post*	0.056***	0.088***	0.007	0.053***	−0.017***	−0.025***	−0.062***	−0.053***
	[5.428]	[13.328]	[0.633]	[4.596]	[−5.002]	[−3.277]	[−13.376]	[−20.563]
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	No	Yes	No	Yes	No	Yes	No
Reg Office FE	Yes	No	Yes	No	Yes	No	Yes	No
County × Year-Quarter FE	No	Yes	No	Yes	No	Yes	No	Yes
Reg Office × Year-Quarter FE	No	Yes	No	Yes	No	Yes	No	Yes
Adj. R^2	0.909	0.905	0.886	0.879	0.836	0.841	0.914	0.913
Observations	8,321	6,570	7,772	6,295	8,321	6,570	8,321	6,570

NOTE: Results of difference-in-difference regressions for the effects of regulatory office closures on different loan types. The specification is:
$Y_{i,k,t} = \alpha_{i,k,t} + \beta_1 Treated_{i,k,t} + \beta_2 Post_{k,t} + \beta_3 Treated_{i,k,t} \times Post_{k,t} + Fixed\ Effects + \varepsilon_{i,k,t}$,
where subscripts *i, k*, and *t* indicate bank, office, and year-quarter, respectively. *Y* is either *CRE Loans/Total Assets, RRE Loans/Total Assets, C&I Loans/Total Assets, or Indiv Loans/Total Assets*. *Treated* = 1 if banks are supervised by a closed office *k*, and 0 if they are in the control group, that is, headquartered in the same county as treated banks but not supervised by the closed office (see Section 2). *Post* = 1 for up to 2 years after closure of office *k*, and 0 for the 2 years before (5-year diff-in-diff window). The variable of interest is coefficient β_3 on *Treated* × *Post* which = 1 for treated banks for up to 2 years after closure of the office, and 0 otherwise. Bank balance-sheet variables are winsorized at the 1% and 99% levels. See Table IA1 (Internet Appendix) for definitions of the variables. Standard errors are clustered at the regulatory office level. *t*-Statistics are in parenthesis. ***, **, and * significant at 1%, 5%, and 10% level.

TABLE 5

TIMELINE EFFECTS AND VERIFICATION

Panel A: Timeline	(1)	(2)	(3)	(4)
	Total Loans/Total Assets		Z-Score	
Treated × *Closure*-1	−0.009	−0.002	−0.051	0.019
	[−1.197]	[−0.112]	[−0.933]	[0.034]
Treated × *Closure*	0.064***	0.086***	−0.913***	−1.471***
	[4.386]	[3.104]	[−4.188]	[−4.323]
Treated × *Closure*+1	0.069***	0.132***	−0.864***	−1.459***
	[5.704]	[8.231]	[−4.001]	[−4.288]
Treated × *Closure*+2	0.062***	0.092***	−0.847***	−1.380***
	[3.893]	[3.125]	[−3.849]	[−4.052]
Bank FE	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	No	Yes	No
Reg Office FE	Yes	No	Yes	No
County × Year-Quarter FE	No	Yes	No	Yes
Reg Office × Year-Quarter FE	No	Yes	No	Yes
Adj. R^2	0.874	0.869	0.763	0.784
Observations	8,321	6,570	7,254	5,490

(*Continued*)

TABLE 5

(CONTINUED)

Panel B: Verification	High probability of misclassification preclosure				High probability of misclassification postclosure				High probability of misclassification pre- & postclosure			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Total Loans/Total Assets		*Z-Score*		*Total Loans/Total Assets*		*Z-Score*		*Total Loans/Total Assets*		*Z-Score*	
Treated × *Post*	0.063*	0.091***	−0.950***	−1.257***	0.045***	0.106***	−0.827***	−1.441***	0.035	0.097***	−0.912**	−1.257***
	[1.934]	[4.000]	[−2.750]	[−5.285]	[3.330]	[10.210]	[−4.382]	[−8.247]	[0.995]	[3.568]	[−2.485]	[−5.005]
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
Reg Office FE	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
County × Year-Quarter FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Reg Office × Year-Quarter FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Adj. R^2	0.851	0.862	0.779	0.793	0.858	0.867	0.751	0.784	0.81	0.867	0.765	0.78
Observations	2,006	1,160	1,584	889	4,110	2,810	3,481	2,324	1,225	666	876	454

NOTE: Results of difference-in-difference regressions for the timing effects of regulatory office closures on bank lending and risk-taking (Panel A) and verification test for the distance assumption (Panel B). The timing specification in Panel A is:

$Y_{i,k,t} = \alpha_{i,k,t} + \beta_1 Treated_{i,k,t} + \beta_2 Closure - 1_{k,t} + \beta_3 Closure_{k,t} + \beta_4 Closure + 1_{k,t} + \beta_5 Closure + 2_{k,t} + \beta_6 Treated_{i,k,t} \times Closure - 1_{k,t} + \beta_7 Treated_{i,k,t} \times Closure_{k,t} + \beta_8 Treated_{i,k,t} \times Closure + 1_{k,t} + \beta_9 Treated_{i,k,t} \times Closure + 2_{k,t} + Fixed\ Effects + \varepsilon_{i,k,t}$,

where subscripts *i, k,* and *t* indicate bank, office, and year-quarter, respectively. *Y* is either *Total Loans/Total Assets* or *Z-Score*. Z-Score = Log(ROA + Equity)/(σROA), where ROA = Net Income/Total Assets, Equity = Total Equity/Total Assets, and (σROA) = standard deviation of ROA years $t-2$ to t. *Treated* = 1 if banks are supervised by a closed office *k*, and 0 if they are in the control group, that is, headquartered in the same county as treated banks but not supervised by the closed office (see Section 2). *Closure*-1 = 1 for year before closure of regulatory office *k*, and 0 otherwise. *Closure* = 1 for year of closure of office *k*, and 0 otherwise. *Closure*+1 (+2) = 1 for 1st (2nd) year after closure of office *k*, and 0 otherwise. *Post* = 1 for up to 2 years after closure of office *k*, and 0 for the 2 years before (5-year diff-in-diff window). Panel B estimates equation (1) but restricts the sample to the top 20% of banks that are likely to be misclassified to their regulatory offices: preclosure (Columns (1)–(4)); postclosure (Columns (5)–(8)); and pre- and postclosure (Columns (9)–(12)). Bank balance-sheet variables are winsorized at the 1% and 99% levels. See Table IA1 (Internet Appendix) for definitions of the variables. Standard errors are clustered at the regulatory office level. *t*-Statistics are in parenthesis. ***, **, and * significant at 1%, 5%, and 10% level.

after (the statistically significant coefficient on *Treated* x *Closure*+1 and *Treated* x *Closure*+2).[20]

The findings of the timing-effects model give us additional confidence that the changes in bank behavior we document occurred in *response* to office closures. Furthermore, the timing of when treated banks increase their lending and risk-taking suggests that reverse causality—where changes in bank behavior lead to the closure of offices—is unlikely to be a key cause of concern. Otherwise, we should observe changes in the behavior of treated banks prior to office closures.[21]

Verification of closest regulatory office as bank supervisor. Our assignment of treated and control banks to their relevant regulatory offices (which determines treatment) relies on the assumption that the closest regulatory office is responsible for supervising a bank. While this is likely to be the case for many banks based on our discussion in Section 2.2, the possibility of misclassification remains, particularly if banks are equally close to two (or more) regulatory offices. Therefore, in this section, we design empirical tests to verify this assumption.

To that end, we calculate two new variables. The first variable, (*Preclosure Office Distance* 2 – *Preclosure Office Distance* 1), is the difference in distance between the second nearest preclosure office and the nearest preclosure office (i.e., the original regulatory office). The second variable (*Postclosure Office Distance* 2 – *Postclosure Office Distance* 1) is the difference between the second closest postclosure office and the nearest postclosure office for treated banks (i.e., the new office we assume takes over supervision). It is worth pointing out that the postclosure difference variable can only be calculated for treated banks as control banks do not change their distance. By contrast, the preclosure difference variable is calculated for both treated and control banks.

These two variables capture the possibility that banks are wrongly assigned to treatment based on the distance assumption. The value of these variables is small when the difference in distance between the two nearest offices is small (i.e., this value would be zero if banks were exactly in the middle of two offices). Consequently, smaller values of these variables indicate a larger potential for misclassification. In Panel B of Table 5, we rerun our main results for treated and control banks that fall within the

20. The choice of which year to use as the base year does not affect our interpretation. For instance, if we omit *Treated* x *Closure* (the year of closures) and use it as the base year, we find that the coefficient on *Treated* x *Closure*-1 and *Treated* x *Closure*-2 is statistically significant and negative for lending, and positive for Z-Score (i.e., treated banks have lower levels of lending and risk-taking) while *Treated* x *Closure*+1 and *Treated* x *Closure*+2 are insignificant. This suggests that treated banks have lower levels of lending and risk as compared to the year of closure and beyond. This interpretation is similar to what we find here when we omit *Treated* x *Closure*-2. Treated banks only increase their lending and risk-taking in the year of the closure and after.

21. For instance, if treated banks reduce their lending prior to office closures, this might lead to the loss of importance or significance of an office in that region which might then result in the closure of that office. As such, detecting changes in bank behavior only *after* office closures mitigates concerns over reverse causality.

bottom 20% of these two variables (we find similar results when we use the bottom 25% or 33%). The intuition behind these tests is that if there were misclassification, it would most likely be in the group of banks with small differences in distance between the two nearest offices. If so, banks in this group would be "wrongly assigned to treatment or control" and not produce significant results or produce results that are different from our original findings.

Columns (1)–(4) ((5)–(8)) of Panel B of Table 5 show that treated and control banks that are most likely to be misclassified pre (post) closures continue to increase their lending and risk-taking after office closures. In Columns (9)–(12), we apply both filters and continue to find similar results. Overall, while we cannot unambiguously verify the distance assumption, we are comforted by the fact that when focusing on banks that are most likely to be misclassified, our findings continue to remain robust and largely similar in economic magnitude to those in Table 3.

3.3 *Office Closures and Proximity*

So far, we have shown that banks undertake riskier business policies after office closures. This suggests that proximity to banks is likely to impact supervisory outcomes. Geographical proximity affords local economic agents informational advantages as compared to distant agents (e.g., Coval and Moskowitz 2001, Malloy 2005, Agarwal and Hauswald 2010). Therefore, supervisors that are located physically closer to the banks they supervise should incur lower costs in collecting bank-specific soft information that facilitates monitoring (Kedia and Rajgopal 2011, Chhaochharia, Kumar, and Niessen-Ruenzi 2012, Giroud 2013). In addition, geographical proximity also lowers the cost of in-person communication with banks and allows supervisors to better communicate expectations with regards to examinations (FDIC 2012b).

Following literature, we use physical distance as a measure of informational frictions (e.g., Coval and Moskowitz 2001, Kedia and Rajgopal 2011). We exploit variations in the increase in physical distance between regulatory offices and banks after office closures to test if informational frictions impede supervisory monitoring. Upon closure of a regulatory office, the supervision of affected banks is typically transferred to the next closest regulatory office. Existing examiners will either be relocated to the new office or a group of different examiners will be assigned to affected banks. Regardless of the specific arrangements, supervisory work will be carried out over a longer distance postclosure than before the closure. If informational frictions exist and matter for supervisory monitoring, we should expect to find that larger increases in physical distance to be associated with riskier bank policies as monitoring become more costly.

This is what we find in Table 6. Columns (1)–(4) replaces *Treatment* with $\%\Delta$*Distance* [(distance to new office–distance to old office)/distance to old office]. The statistically significant coefficient on the interaction term $\%\Delta$*Distance* $\times$ *Post* means that larger increases in distance to the new office results in more aggressive loan origina-

TABLE 6

Regulatory Office Closures and Increases in Distance to the New Office

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Total Loans/Total Assets		*Z-Score*		*Total Loans/Total Assets*		*Z-Score*		*Total Loans/Total Assets*		*Z-Score*	
$\%\Delta Distance \times Post$	0.062***	0.043*	−0.234**	−0.178								
	[5.252]	[1.960]	[−2.377]	[−0.684]								
$\%\Delta Distance\ Large \times Post$					0.049***	0.041***	−0.201***	−0.149				
					[5.579]	[2.782]	[−2.863]	[−1.004]				
$\%\Delta Distance\ Small \times Post$									0.004	0.002	−0.044	−0.126
									[0.681]	[0.127]	[−0.382]	[−0.826]
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
Reg Office FE	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
County × Year-Quarter FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Reg Office × Year-Quarter FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Adj. R^2	0.877	0.87	0.763	0.782	0.876	0.869	0.764	0.783	0.874	0.869	0.762	0.782
Observations	8,269	6,530	7,215	5,449	8,321	6,570	7,254	5,490	8,321	6,570	7,254	5,490

Note: Results of difference-in-difference regressions for the heterogeneous effects of the increase in distance to the new regulatory office after existing office closures on bank lending and risk-taking. The specification is: $Y_{i,k,t} = \alpha_{i,k,t} + \beta_1 \%\Delta Distance_{i,k,t} + \beta_2 Post_{k,t} + \beta_3 \%\Delta Distance_{i,k,t} \times Post_{k,t} + Fixed\ Effects + \varepsilon_{i,k,t}$,
where subscripts *i, k,* and *t* indicate bank, office, and year-quarter, respectively. *Y* is either *Total Loans/Total Assets* or *Z-Score*. Z-Score = Log(*ROA* + *Equity*)/(σ *ROA*), where *ROA* = Net Income/Total Assets, *Equity* = Total Equity/Total Assets, and (σ *ROA*) = standard deviation of ROA years $t-2$ to t. $\%\Delta Distance$ = [(Distance to new office − Distance to closed office)/Distance to closed office] for banks that are supervised by a closed office *k*, and 0 if they are in the control group, that is, headquartered in the same county as treated banks but not supervised by the closed office (see Section 2). $\%\Delta Distance\ Large$ ($\%\Delta Distance\ Small$) = 1 if $\%\Delta Distance$ is in the top 20% (bottom 20%) of the sample distribution for affected banks and 0 otherwise. *Post* = 1 for up to 2 years after closure of office *k*, and 0 for the 2 years before (5-year diff-in-diff window). The variable of interest is coefficient β_3 on $\%\Delta Distance \times Post$ which = the % increase in distance to the office taking over the supervision for treated banks for up to 2 years after closure of the office, and 0 otherwise. Bank balance-sheet variables are winsorized at the 1% and 99% levels. See Table IA1 (Internet Appendix) for definitions of the variables. Standard errors are clustered at the regulatory office level. *t*-Statistics are in parenthesis. ***, **, and * significant at 1%, 5%, and 10% level.

tion and riskier banks.[22] In Columns (5)–(8) ((9)–(12)), we investigate if large (small) increases in distance to the new regulatory office have an impact on bank activities. *%ΔDistance Large* (*%ΔDistance Small*) is a dummy that equals 1 if increases in distance for affected banks is in the top (bottom) 20% of the sample distribution. We can see from the interaction term in Columns (5)–(8) that large increases are associated with riskier bank activities. By contrast, smaller increases (Columns (9)–(12)) are not related to increases in risky bank policies.

This nonfinding is important as it rules out concerns that we are picking up an effect due to distractions that are caused by office relocations. Indeed, the results are supportive of our interpretation that informational frictions related to proximity are a key reason behind our findings. Overall, these findings are consistent with the notion that high levels of information asymmetry present a challenge to supervisory efforts. This suggests that a decentralized supervisory structure allows bank supervisors better access to local information and more efficient communication with banks. Consequently, this enables them to monitor banks and reduce bank risk more effectively.[23]

4. ARE REGULATORY OFFICE CLOSURES LINKED TO NEGATIVE BANK OUTCOMES?

Our findings that office closures are linked to riskier business policies by affected banks are not sufficient to argue that closures produce negative bank outcomes. If, for instance, the loans extended by affected banks were of high quality, they might serve to benefit borrowers and the local economy. Furthermore, even if banks undertook riskier business policies, supervisors and other stakeholders might require riskier banks to hold additional (and sufficient) capital to safeguard against failure. We show that this explanation is unlikely to be the case and that office closures are related to worse bank outcomes.

We start by investigating in Columns (1)–(2) ((3)–(4)) of Table 7 the effect of office closures on treated banks' Tier-1 (Total Equity) risk-adjusted capital ratios. While

22. As the accuracy of this test relies on the assumption that the nearest regulatory office bears responsibility for supervision, we could be overestimating the information asymmetry effect because the actual increases in distance might be larger than our estimation (if a more distant office rather than the next-nearest office is chosen). While this is possible, we show in Section "Verification of closest regulatory office as bank supervisor," that the distance assumption is likely to be valid. Nonetheless, we are careful not to overinterpret the economic impact of our findings. Our aim is to ascertain that increases in lending and risk-taking are broadly increasing with distance, in line with our interpretation of informational frictions.

23. In another relevant test, we show that treated banks that were supervised by the OCC increase their lending and risk-taking more than treated banks that were supervised by the FDIC/Fed after office closures. All else constant, the impact of any office closure on bank behavior should be larger for OCC office closures as compared to FDIC/Fed office closures as the OCC is the sole main supervisor for banks with a federal charter while state-chartered banks are jointly supervised by their respective state regulator alongside the FDIC/Fed. Therefore, any losses in supervisory efficacy stemming from more distant regulatory offices should be less acute for FDIC/Fed supervised banks as these banks are jointly supervised by their local state supervisors. We detail this in Table IA4 in the Internet Appendix.

TABLE 7

Regulatory Office Closures and Bank Capital

	(1)	(2)	(3)	(4)
	Tier-1 Capital/RWA		*Equity Capital/RWA*	
Treated × *Post*	0.023***	0.003	0.017***	0.003
	[4.122]	[1.311]	[3.409]	[1.256]
Bank FE	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	No	Yes	No
Reg Office FE	Yes	No	Yes	No
County × Year-Quarter FE	No	Yes	No	Yes
Reg Office × Year-Quarter FE	No	Yes	No	Yes
Adj. R^2	0.711	0.699	0.702	0.682
Observations	8,321	6,570	8,321	6,570

Note: Results of difference-in-difference regressions for the effects of regulatory office closures on bank capital. The specification is:
$Y_{i,k,t} = \alpha_{i,k,t} + \beta_1 Treated_{i,k,t} + \beta_2 Post_{k,t} + \beta_3 Treated_{i,k,t} \times Post_{k,t} + Fixed\ Effects + \varepsilon_{i,k,t}$,
where subscripts *i, k,* and *t* indicate bank, office, and year-quarter, respectively. *Y* is either *Tier-1 Capital/RWA* or *Equity Capital/RWA*. *Treated* = 1 if banks are supervised by a closed office *k*, and 0 if they are in the control group, that is, headquartered in the same county as treated banks but not supervised by the closed office (see Section 2). *Post* = 1 for up to 2 years after closure of office *k*, and 0 for the 2 years before (5-year diff-in-diff window). The variable of interest is coefficient β_3 on *Treated* × *Post* which = 1 for treated banks for up to 2 years after closure of the office, and 0 otherwise. Bank balance-sheet variables are winsorized at the 1% and 99% levels. See Table IA1 (Internet Appendix) for definitions of the variables. Standard errors are clustered at the regulatory office level. *t*-Statistics are in parenthesis. ***, **, and * significant at 1%, 5%, and 10% level.

there is some evidence that treated banks increase their capital ratios in Columns (1) and (3), the statistical and economic significance of the coefficients disappears when we include county-quarter and office-quarter FEs in Columns (2) and (4). This is consistent with previous studies that highlight the importance of local, time-varying demand factors when investigating the effects of bank capital on lending (e.g., Carlson, Shan, and Warusawitharana 2013) as well as the time-varying preferences of regulators (Agarwal et al. 2014). Such local factors are likely to be more salient for the small banks we study in our sample.

An insignificant coefficient on the interaction term suggests that, following office closures, treated banks have capital ratios that are no different from control banks. One interpretation of this is that risk-sensitive capital requirements are working as intended. Increases in capital (the numerator) are offset by increases in the denominator (risk-weighted assets [RWA]). For this interpretation to be correct, RWA would have to accurately reflect the economic risk of a bank's assets. However, this is unlikely to be the case. RWA are widely criticized for being insufficiently attuned to the true risk of bank activities (Vallascas and Hagendorff 2013) and subject to manipulation by institutions (Mariathasan and Merrouche 2014). In addition, treated banks might have more scope to manage capital ratios following office closures as supervisors are located further away and have less information regarding the true risk of their asset portfolios.

Because it is challenging to ascertain if treated banks are holding capital in line with risk, one way to ascertain the adequacy of capital is to observe how treated banks perform in response to a negative shock. We refine our analysis to investigate the performance of treated banks' portfolios and their failure rates during the 2008–09 crisis. In this analysis, we distinguish between treated banks according to whether

TABLE 8

Regulatory Office Closures and the Financial Crisis

	(1)	(2)	(3)	(4)	(5)	(6)
	Bad Loans/Total Loans		*Loan Charge-Offs/Total Loans*		*Fail08–09*	
Treated Crisis × *Crisis*	0.0045*	0.0037**	0.0012***	0.0008***	0.0487**	0.0526**
	[1.8219]	[2.4733]	[9.6688]	[3.3257]	[2.6882]	[2.5337]
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
County × Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Reg Office × Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Other Controls	No	Yes	No	Yes	No	Yes
Adj. R^2	0.533	0.592	0.242	0.411	0.297	0.37
Observations	8,681	8,681	8,681	8,681	8,681	8,681

Note: Results of difference-in-difference regressions for the effects of regulatory office closures on loan performance and bank failure rates during the financial crisis of 2008–09. The specification is:
$Y_{i,k,t} = \alpha_{i,k,t} + \beta_1 TreatedCrisis_{i,k,t} + \beta_2 Crisis_t + \beta_3 TreatedCrisis_{i,k,t} \times Crisis_t + FixedEffects + \varepsilon_{i,k,t}$,
where subscripts *i, k,* and *t* indicate bank, office, and year-quarter, respectively. *Y* is one of: *Bad Loans* = (Total Loans and Receivables 90+days late)/Total Loans; *Loan Charge-Offs* = Total Loan Charge-Offs/Total Loans; and *Fail08–09* = 1 for years 2008–09 if a bank failed in those years. *Treated Crisis* = 1 if banks are supervised by a closed office *k* that occurred up to 2007, and 0 if they are in the control group,that is, headquartered in the same county as treated banks but not supervised by the closed office (see Section 2). *Crisis* = 1 for years 2008–09, and 0 for other years. The variable of interest is coefficient β_3 on *Treated Crisis* × *Crisis* which = 1 for banks that were affected by office closures prior to the crisis during the 2008–09 crisis, and 0 otherwise. Bank balance-sheet variables are winsorized at the 1% and 99% levels. See Table IA1 (Internet Appendix) for definitions of the variables. Standard errors are clustered at the regulatory office level. *t*-Statistics are in parenthesis. ***, **, and * significant at 1%, 5%, and 10% level.

their regulatory office had closed *prior* to the crisis. The intuition behind this analysis is that the true quality of loans is likely to reveal itself in periods of economic downturns (Mian and Sufi 2009, Demyanyk and Van Hemert 2011). Similarly, the effects of bank capital on a bank's survival are also more likely to be salient during times of crisis (Berger and Bouwman 2013, Demirgüç-Kunt, Detragiache, and Merrouche 2013). Therefore, if treated banks had capital sufficiently attuned to the true risk of their portfolio, we should not observe differences in failure rates as compared to control banks. To that end, we restrict our analysis to the six offices that closed in 2004–07, before the crisis, and estimate the following DiD specification for the years up to 2009:

$$Y_{i,k,t} = \alpha_{i,k,t} + \beta_1 Treated\ Crisis_{i,k,t} + \beta_2 Crisis_t + \beta_3 Treated\ Crisis_{i,k,t} \times Crisis_t + Fixed\ Effects + \varepsilon_{i,k,t}. \quad (2)$$

Treated Crisis is a dummy variable that is 1 if banks are affected by one of the six office closures that occurred up to 2007, and 0 otherwise. As before, control banks are banks in the same counties as treated banks, but not affected by the closure. *Crisis* equals 1 for the years 2008–09, and 0 for years 2004–07. The coefficient of interest is β_3 on *Treated Crisis* × *Crisis*, which takes a value of 1 for treated banks in the years 2008–09.

We present our results in Table 8. The positive and statistically significant coefficient on *Treated Crisis* × *Crisis* in Columns (1)–(2) ((3)–(4)) indicate that banks affected by office closures before the crisis had more *Bad Loans* (Loans and receivables which are at least 90 days late/Total Loans) and *Loan Charge-offs* (Loan Charge-Offs/Total Loans) relative to unaffected banks during the crisis. The economic impact is substantial. Relative to the relevant sample mean during the crisis, affected banks have 17% higher bad loans (Column (2)) and 38% higher loan charge-offs (Column (4)). These findings suggest that loans underwritten by banks affected by office closures *prior* to the crisis were of lower quality. This is consistent with Altunbas, Manganelli, and Marques-Ibanez (2017) who find that aggressive credit growth prior to the crisis led to riskier banks during the crisis.

In Columns (5)–(6), we shift our focus to bank failures. Our dependent variable *Fail08-09* is a dummy that equals 1 for 2008–09 for banks that failed during 2008–09, and 0 otherwise.[24] With regards to magnitude, Column (6) shows that banks that were affected by office closures prior to the crisis had a 5.2% higher probability of failing during the crisis than unaffected banks. As the key purpose of bank capital is to absorb losses and prevent failure (Wheelock and Wilson 2000, Berger and Bouwman 2013), we interpret our results as an indication that the reported risk-adjusted capital ratios at treated banks were less attuned to risk as compared to control banks.

Overall, the results in this section imply that office closures lead to negative bank outcomes. Aggressive loan growth strategies by treated banks did not coincide with commensurate increases in the amount of capital held. Consequently, banks affected by office closures incurred larger loan losses in the crisis, leading to higher failure rates.

5. ECONOMIC CHANNELS

In the final portion of our paper, we explore three nonmutually exclusive channels through which proximity to banks facilitates monitoring, leading to more stable banks: (i) provisioning practices, (ii) payouts to shareholders, and (iii) supervisory expertise.

5.1 Provisioning Practices

The provisioning practices channel postulates that proximate supervisors are better able to assess the true quality of a bank's loan portfolio and enforce provisioning practices that are commensurate with its risk. LLP practices are an important objective of supervision to ensure the stability of a bank (FDIC 2019). LLPs are accrued expenses that should reflect expected future losses on a bank's loan portfolio. When banks delay recognition of expected losses by underprovisioning, losses larger than the provisions that were previously made have to be absorbed by bank capital. Therefore, sustained underprovisioning, while freeing up liquidity to support aggressive loan growth and improving performance in the short term, can lead to concerns about bank stability if the accrued LLPs are insufficient to cover losses during economic downturns (Bushman and Williams 2015, Acharya and Ryan 2016).

Supervisors recognize that the estimation of loan losses requires substantial judgment from the bank and its accuracy hinges on a number of factors such as internal

24. Out of the 370 unique treated and control banks, we observe 11 bank failures from 2008 to 2009.

processes, model methodology, and assumptions. In assessing if the level of provisioning is appropriate, supervisors take into consideration the bank's internal systems and controls for identifying, monitoring, and addressing asset quality in a timely manner and whether the bank analyzed all significant factors that affect the collectability of the portfolio (FDIC 2019). If supervisors do not concur that the reported provisioning level is appropriate, recommendations for correcting these problems would be raised and management would be required to increase LLPs to an appropriate level (FDIC 2019). It is important to note that because provisioning is an expense, on average, banks have incentives to underprovision (see Beatty and Liao 2014 for a review). As such supervisors are mainly concerned with *increasing* provisions to appropriate levels.

However, ensuring appropriate levels of LLPs depends on the information set that is available to supervisors. If proximity decreases the cost of collecting and verifying information that is useful in the examination process (e.g., local economic conditions, the bank's internal process, and quality of management), we should expect that after office closures, greater information asymmetry could impair the ability of supervisors to enforce LLP practices that are more in line with expected future losses. Put another way, if supervisors know less about the real quality of the bank's portfolio, banks are more likely to be able to have more discretion in their provisioning practices. We conduct two complementary tests to examine the extent to which affected banks are able to exploit the heightened information asymmetry between themselves and supervisors to: (i) make lower and less timely LLPs, and (ii) increase their discretionary use of LLPs to underprovision.

Size and timeliness of LLPs. If proximity increases supervisor's ability to enforce bank LLPs that are commensurate with bad loans, we should expect less timely provisioning by banks after office closures. That is, we expect to see a decline in how sensitive *current* LLPs are to the actual bad loans that materialize in the *future* among affected banks. We estimate the following model in the spirit of Nichols, Wahlen, and Wieland (2009) and Kanagaretnam, Lim, and Lobo (2014):

$$\begin{aligned} LLP_{i,k,t} &= \alpha_{i,k,t} + \beta_1 Treated_{i,k,t} + \beta_2 Post_{k,t} + \beta_3 Treated_{i,k,t} \times Post_{k,t} \\ &+ \beta_4 Treated_{i,k,t} \times Post_{k,t} \times \Delta Bad\ Loans_{i,k,t+1} + \beta_5 Treated_{i,k,t} \times Post_{k,t} \\ &\times \Delta Bad\ Loans_{i,k,t} + \beta_6 Treated_{i,k,t} \times Post_{k,t} \times \Delta Bad\ Loans_{i,k,t-1} \\ &+ Fixed\ Effects + \varepsilon_{i,k,t}, \end{aligned} \tag{3}$$

where *i, k*, and *t* indicate bank *i*, regulatory office *k*, and year-quarter *t*, respectively. *Treated* and *Post* are as defined in Section 2. *LLP* is calculated as Loan Loss Provisions/Total Loans. *Bad Loans* is constructed as Total Loans and Receivables 90+ days late/Total Loans. The coefficients of interest are β_4, β_5, and β_6; they measure the extent to which office closures affect the sensitivity between bad loans (future, current, and past, respectively) and current LLPs.

We show the estimation results of equation (3) in Panel Columns (1)–(2) of Table 9. We first observe that the coefficients on *Treated* × *Post* × Δ*Bad Loans*$_t$ and *Treated* × *Post* × Δ*Bad Loans*$_{t\text{-}1}$ are insignificant. This implies that there are no differences in

TABLE 9

REGULATORY OFFICE CLOSURES AND BANK ACCOUNTING PRACTICES

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	LLP/Total Loans		*|ALLP A|*		*−ALLP A*		*+ALLP A*
Treated × *Post*	−0.001***	−0.0001	0.055***	0.039***	−0.049***	−0.067***	0.140
	[−3.309]	[−1.116]	[2.931]	[2.962]	[−4.125]	[−4.302]	[1.643]
Treated × *Post* × Δ*Bad Loans*$_{t+1}$	−0.037**	−0.047**					
	[−2.577]	[−2.601]					
Treated × *Post* × Δ*Bad Loans*$_{t}$	−0.028	−0.058					
	[−0.666]	[−0.906]					
Treated × *Post* × Δ*Bad Loans*$_{t-1}$	0.008	0.001					
	[0.301]	[0.024]					
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	No	Yes	No	Yes	No	Yes
Reg Office FE	Yes	No	Yes	No	Yes	No	Yes
County × Year-Quarter FE	No	Yes	No	Yes	No	Yes	No
Reg Office × Year-Quarter FE	No	Yes	No	Yes	No	Yes	No
Adj. R^2	0.204	0.202	0.243	0.23	0.478	0.493	0.168
Observations	8,072	6,330	7,953	6,230	5,725	4,075	2,228

NOTE: Results of difference-in-difference regressions for the effect of regulatory office closures on timeliness of loan loss provisioning (Columns (1)–(2)) and discretionary provisioning practices (Columns (3)–(7)). The specification in Columns (1)–(2) (timeliness of loan loss provisioning) is:

$$Y_{i,k,t} = \alpha_{i,k,t} + \beta_1 Treated_{i,k,t} + \beta_2 Post_{k,t} + \beta_3 Treated_{i,k,t} \times Post_{k,t} + \beta_4 Treated_{i,k,t} \times Post_{k,t} \times \Delta Bad\ Loans_{i,k,t+1} + \beta_5 Treated_{i,k,t} \times Post_{k,t} \times \Delta Bad\ Loans_{i,k,t}$$
$$+ \beta_6 Treated_{i,k,t} \times Post_{k,t} \times \Delta Bad\ Loans_{i,k,t-1} + Fixed\ Effects + \varepsilon_{i,k,t}.$$

The specification in Columns (3)–(7) (discretionary provisioning practices) is:
$Y_{i,k,t} = \alpha_{i,k,t} + \beta_1 Treated_{i,k,t} + \beta_2 Post_{k,t} + \beta_3 Treated_{i,k,t} \times Post_{k,t} + Fixed\ Effects + \varepsilon_{i,k,t}$,
where subscripts *i, k,* and *t* indicate bank, office, and year-quarter, respectively. *Y* is one of: *LLP* = (Loan Loss Provisions/Total Loans); −*ALLP A* (= the negative residuals of equation (4), the discretionary component of LLPs); +*ALLP A* (= the positive residuals of equation (4)): |*ALLP A*| (= absolute value of residuals of equation (4)). *Bad Loans* = (Total Loans and Receivables 90+days late)/Total Loans. *Treated* = 1 if banks are supervised by a closed office *k*, and 0 if they are in the control group, that is, headquartered in the same county as treated banks but not supervised by the closed office (see Section 2). *Post* = 1 for up to 2 years after closure of office *k*, and 0 for the 2 years before (5-year diff-in-diff window). The variable of interest in Columns (1)–(2) is the coefficient on the three triple interaction terms β_4, β_5, and β_6 while the variable of interest in Columns (3)–(7) is the coefficient β_3 of the interaction term *Treated* × *Post*. Bank balance-sheet variables are winsorized at the 1% and 99% levels. See Table IA1 (Internet Appendix) for definitions of the variables. Standard errors are clustered at the regulatory office level. *t*-Statistics are in parenthesis. ***, **, and * significant at 1%, 5%, and 10% level.

the provisioning practices of affected and unaffected banks with regards to bad loans that have materialized. Importantly, current LLPs become less timely in predicting *future* bad loans. This is demonstrated by the negative coefficient on $Treated \times Post \times \Delta Bad\ Loans_{t+1}$. The results are consistent with our argument that one way in which supervisory proximity can lead to safer banks is through the enforcing of timelier provisioning policies.

Abnormal discretionary LLPs. In a complementary second test, we decompose LLPs into its discretionary and nondiscretionary components (e.g., Beatty and Liao 2014, Jiang, Levine, and Lin 2016). We estimate the *discretionary* component in LLPs as the residuals of a regression that predicts firms' expected loan losses. Negative (positive) residuals indicate that banks understate (overstate) their LLPs relative to LLP levels that are commensurate with expected loan losses. We then use the residuals as our dependent variable to investigate if office closures affect the discretionary use of LLPs. The advantage of this approach is that we can observe more cleanly the magnitude and use (direction) of the discretionary component in provisioning practices of affected banks.

Using residual LLPs as proxies for discretionary LLPs relies on the accuracy of the LLP model to predict expected loan losses. Beatty and Liao (2014) assess various models used in the literature and test their validity in predicting earnings restatements and comment letters from the Securities and Exchange Commission (SEC).[25] We follow their choice of best-performing models and estimate Model A as:

$$\begin{aligned} LLP_{i,j,t} &= \alpha_{i,j,t} + \Delta NPA_{i,j,t+t} + \Delta NPA_{i,j,t} + \Delta NPA_{i,j,t-1} + \Delta NPA_{i,j,t-2} \\ &+ Log\ Total\ Assets_{i,j,t-1} + \Delta Loans_{i,j,t} + \Delta State\ GDP_{j,t} + \Delta State\ HPI_{j,t} \\ &+ \Delta State\ HPI_{j,t} + State\ FE + Year\ FE + \varepsilon_{i,j,t}. \end{aligned} \tag{4}$$

Table IA1 lists the variable definitions. $+ALLP\ A$ ($-ALLP\ A$) are the residuals from equation (4) if $\varepsilon_{i,k,t} > 0$ ($\varepsilon_{i,k,t} < 0$). We also calculate the *absolute* values of the residuals ($|ALLP\ A|$) from equation (4). An increase in the absolute values of residuals indicates that banks make more use of discretionary LLPs (have more opaque accounting practices). A decrease (increase) in $-ALLP$ ($+ALLP$) indicates that banks make more use of discretionary LLPs for income-increasing (income-decreasing) reasons, respectively.

We show the results in Columns (3)–(7) of Table 9. In Columns (3)–(4), which examine the absolute value of the residuals, the coefficient on $Treated \times Post$ is positive and statistically significant. This suggests that following office closures, affected banks increase their use of discretionary LLPs, resulting in more opaque provisioning and financial reporting. Crucially, the negative coefficient on our interaction term

25. The model that we use has been identified by Beatty and Liao (2014) as the best in identifying discretionary LLP behavior by banks. Beatty and Liao conduct factor analysis on nine models to understand the importance of the different underlying factors. Based on the results of their factor analysis, the authors identify the "best" models and test the validity of these models in predicting SEC restatements and comment letters.

in Columns (5)–(6) indicates that affected banks increase their understating of LLPs after office closures.[26] We observe no effect in Column (7) in terms of overstating provisions.[27] For robustness, we also show in Table IA5 that the results are similar when relying on the second best-performing model as assessed by Beatty and Liao (2014).

Overall, the two tests on bank LLP practices show that, following office closures, banks are: (i) slower in provisioning for future bad loans, and (ii) increasing their usage of discretionary LLPs to underprovision for expected loan losses. This is consistent with Costello, Granja, and Weber (2019) who find that supervisors that perform well on several dimensions enforce more bank income-reducing restatements, as well as higher levels of LLPs. This suggests that provisioning practices are one channel through which proximate supervisors monitor opportunistic bank behavior that might otherwise result in instability.

5.2 Payouts

The second channel—payouts—posits that proximate supervisors are better able to restrain aggressive payout policies to bank shareholders. Large payouts to shareholders come from a bank's liquid assets and retained earnings, and, as a result, leave behind less liquid and riskier assets to repay a bank's debtors (Acharya et al. 2011, Acharya, Le, and Shin 2017). Subsequently, higher payouts drain a bank's capital and increase its default risk (Hirtle 2014, Onali 2014). Moreover, in the presence of safety net guarantees (both implicit and explicit), higher default risk to shareholders is subsidized (Ronn and Verma 1986, Hovakimian and Kane 2000). This further incentivizes banks to make aggressive payouts to benefit shareholders. Therefore, in evaluating a bank's financial health, supervisors pay attention to the "reasonableness of dividends" with respect to the quality of earnings as excessive payouts can result in a weakened capital positions:

> … Earnings must first be applied … to the establishment of necessary reserves. Thereafter, dividends can be disbursed in reasonable amounts. (FDIC 2015)

Proximate supervisors that are more informed are in a better position to assess if payouts are commensurate with the true quality of a bank's assets and restrict excessive payouts that enrich shareholders at the expense of financial stability. We show results for this channel in Panel A of Table 10. We scale Dividends by Net Income in Columns (1)–(2) and by Total Assets Columns (3)–(4). Throughout the specifications, we find strong evidence that affected banks increase their payouts after office closures. This is particularly striking given that payouts are increasing even as banks

26. Since $-ALLP\ A$ has negative values, a negative coefficient on $Treated \times Post$ means that understating of LLPs increases after office closures.

27. We are unable to run the specification with bank, county-quarter and office-quarter fixed effects as the number of observations of $+ALLP\ A$ is relatively small and the inclusion of these high-dimensional fixed effects absorbs most of the variation.

TABLE 10

Regulatory Office Closures, Dividend Payouts, and Risk-Adjusted Performance

Panel A: Payouts	(1)	(2)	(3)	(4)
	Dividends/Net Income		*Dividends/Total Assets*	
Treated × *Post*	0.427***	0.338**	0.002***	0.002***
	[2.703]	[2.517]	[7.687]	[4.431]
Bank FE	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	No	Yes	No
Reg Office FE	Yes	No	Yes	No
County × Year-Quarter FE	No	Yes	No	Yes
Reg Office × Year-Quarter FE	No	Yes	No	Yes
Adj. R^2	0.0729	0.104	0.226	0.194
Observations	6,985	5,233	6,998	5,245

Panel B: Sharpe ratio	(1)	(2)	(3)	(4)
	ROA/σROA		ROE/σROE	
Treated × *Post*	−4.388***	−5.990***	−64.678***	−74.556***
	[−4.327]	[−5.518]	[−5.366]	[−5.144]
Bank FE	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	No	Yes	No
Reg Office FE	Yes	No	Yes	No
County × Year-Quarter FE	No	Yes	No	Yes
Reg Office × Year-Quarter FE	No	Yes	No	Yes
Adj. R^2	0.713	0.707	0.734	0.724
Observations	7,254	5,490	7,254	5,490

Note: Results of difference-in-difference regressions for the effects of regulatory office closures on dividend payouts (Panel A) and risk-adjusted performance (Panel B). The specification is:
$Y_{i,k,t} = \alpha_{i,k,t} + \beta_1 Treated_{i,k,t} + \beta_2 Post_{k,t} + \beta_3 Treated_{i,k,t} \times Post_{k,t} + Fixed\ Effects + \varepsilon_{i,k,t}$,
where subscripts *i*, *k*, and *t* indicate bank, office, and year-quarter, respectively. *Y* is one of: *Dividends/Net Income*; *Dividends/Total Assets;* (*ROA*/σ*ROA*) or (*ROE*/σ*ROE*). *ROA* = Net Income/Total Assets; *ROE* = Net Income/Total Equity; σROA = standard deviation of *ROA* from years $t-2$ to t and; σROE = standard deviation of *ROE* from years $t-2$ to t. *Treated* = 1 if banks are supervised by a closed office *k*, and 0 if they are in the control group, that is, headquartered in the same county as treated banks but not supervised by the closed office (see Section 2). *Post* = 1 for up to 2 years after closure of office *k*, and 0 for the 2 years before (5-year diff-in-diff window). The variable of interest is coefficient β_3 on *Treated* × *Post* which = 1 for treated banks for up to 2 years after closure of the office, and 0 otherwise. Bank balance-sheet variables are winsorized at the 1% and 99% levels. See Table IA1 (Internet Appendix) for definitions of the variables. Standard errors are clustered at the regulatory office level. *t*-Statistics are in parenthesis. ***, **, and * significant at 1%, 5%, and 10% level.

grow their loan portfolios to riskier customers and are under provisioning for losses. This supports our assertion that supervisory proximity is important in curbing excessive payouts that could destabilize the bank.

5.3 *Supervisory Expertise*

The last channel—supervisory expertise—asserts that supervisors have knowledge related to issues such as innovations in the banking market, implementation of new technologies, or emerging risks that they pass on to banks as part of the supervisory process. This source of expertise could be particular useful for small community banks that tend to have limited resources (FDIC 2012b, Wilson and Veuger 2017). For instance, the FDIC produces videos that address a variety of issues that community banks face which:

> … help institutions economize on the need for consultants or other contractors. (FDIC 2012b)

Therefore, we should expect that being geographically proximate should afford more opportunities for supervisors to have in-person meetings with bankers to share knowledge. Because supervisory expertise can lead to decreases in cost, increases in revenue, or reductions in risk, we measure its aggregate effects using risk-adjusted returns. Consistent with a supervisory expertise channel, we find that risk-adjusted returns, as measured by (*ROA/*σ*ROA*) and (*ROE/*σ*ROE*) in Panel B of Table 10, deteriorates after office closures for affected banks. This suggests that supervisors operating out of a decentralized structure are better able to advise banks, leading to more efficient risk-taking which reduces bank fragility.

6. CONCLUSION

This paper studies whether proximity between regulatory office networks and banks affects supervisory outcomes. We argue that in the presence of informational frictions, having a physical presence that is proximate to banks increases the level and quality of information that bank examiners have on banks and improves monitoring. Our results show that, after an office closes, banks under the supervision of the closed office expand their loan portfolios more aggressively and become riskier as compared to banks located in the same county but not under the supervision of the closed office. Furthermore, we show that regulatory office closures lead to negative bank outcomes. Banks affected by office closures have more bad loans and a higher probability of failure during the 2008–09 financial crisis.

We find support for three nonmutually exclusive channels via which supervisory proximity to banks facilitates monitoring and leads to less fragile banks. First, proximate supervisors enforce more timely provisions for future bad loans and restrict the use of income-increasing accruals. Second, they restrict large payouts by banks. Finally, proximate supervisors are better able to advise banks, improving their risk-adjusted returns.

Our findings are of broad interest to regulators and help inform policy debates regarding regulation and supervision. Our work paints a positive picture of the effectiveness of a decentralized structure of bank supervision where supervisors are located close to the banks which they examine. Given the benefits of being near the banks they supervise, regulators should carefully weigh the cost savings of a more streamlined structure against the possibility of less effective bank supervision that may result from office closures.

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SUPPORTING INFORMATION

Additional supporting information may be found online in the Supporting Information section at the end of the article.

Internet Appendix