Essays on Banking

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Declaration Page

This is to certify that all the work contained in this thesis has been composed by me and is entirely my own, less otherwise specified. No part of this thesis has been submitted for any other degree or professional qualification.

Signed: Ivan Lim

Date: 10th April 2018

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Abstract

This thesis consists of three essays on banking in the U.S. The first two chapters study how supervisors and regulators influence bank behavior. The third chapter explores how bank CEOs allocate credit.

The first chapter uses a quasi-natural experiment, the closure of regulatory offices, to identify the effects of supervision on bank behavior. Under the decentralized structure of U.S. bank supervision, banks in the same geographic area may be supervised by different regulatory offices. The chapter shows that, following the closure of a regulatory office, banks previously supervised by that office increase their solvency risk and lending compared with banks in the same counties that are supervised by a different regulatory office. Further, these banks exhibit lower risk-adjusted returns, lower asset quality, and opportunistic provisioning behavior for loan losses. Information asymmetry between banks and supervisors partly explains the results.

The second chapter documents that nearly 30% of U.S. banks employ at least one board member who currently or previously served on a regulator's advisory council or on the board of a regulator as a form of public service. The chapter shows that connections to regulators undermine regulatory discipline by decreasing the sensitivity of bank risk to capital. Connected banks are able to extract larger public subsidies than non-connected banks by shifting risk to the financial safety-net, resulting in wealth transfers from taxpayers to shareholders of risk-shifting connected

banks. One potential reason for these effects is that connected banks receive preferential treatment in supervision from regulators.

The third chapter uses the birthplace of U.S. bank CEOs to investigate the effect of hometown favoritism on bank business policies. Exploiting within-bank variation in distances to a CEO's hometown, the chapter shows that banks make more mortgage and small business lending as well as branch expansions in counties that are proximate to the hometown of the CEO. This is due to the CEO's altruistic attachment to her hometown; the effects are stronger during economic downturns, among altruistic CEOs, in poorer counties and marginal mortgage applicants. Further, hometown favoritism does not lead to worst bank performance. However, it is associated with positive economic outcomes in counties exposed to greater favoritism.

Introduction

Banks play a pivotal role in the economy. Well-functioning financial systems facilitate efficient allocation of credit from savers to businesses, spurring entrepreneurial growth and capital formation. This is primarily achieved by funding long-term loans with short-term demandable deposits. Because of the fragility of the business model of the bank and its importance in the economy, banks are heavily regulated and supervised, a sui generis feature of the industry. This thesis consists of three independent chapters exploring the themes of supervision and bank behavior. The first two chapters studies how bank supervisors and regulators can influence bank behavior. The third chapter investigates the credit allocation policies of bank CEOs.

The first chapter studies the impact of supervision on bank outcomes. Analyzing whether and how bank supervisors influence bank behavior poses several challenges. First, supervisory scrutiny is endogenously related to the behavior of a bank. Second, changes in local economic conditions could simultaneously influence the behavior of banks as well as supervisors. To overcome these challenges, I make use of the supervisory setting in the U.S. where banks in the same geographic area may be supervised by different regulatory offices. I exploit regulatory office closures — which only affects banks under its supervision— and compare the behavior of these banks to a group of banks residing in the same area that are not affected by these office closures.

Following regulatory office closures, banks supervised by the recently closed office increase their risk-taking and grew their loan portfolios more aggressively as compared to unaffected banks that are located in the same counties as banks affected by the closure. Further, I show that bank supervisors were not too strict during the pre-closure period and that increases in risk-taking and lending by affected banks led to negative consequences. Banks that were affected by regulatory office closures before the 2007-2009 financial crisis had lower risk-adjusted returns, more non-performing loans and a higher probability of failure during the crisis. Regulatory office closures also led to increases in bank resolution costs. Lastly, I highlight one possible channel —the increase in information asymmetry between supervisors and banks— to explain why regulatory office closures decrease the efficacy of bank supervision. The results of this chapter should be of broad interest to supervisors and regulators. It highlights the benefits and costs of maintaining physical offices and the effects local offices have on the quality of supervision.

The second chapter builds on the theme of supervisory efficacy and studies how bank connections to regulators influences supervisory discipline. I highlight an under-explored regulatory arrangement, the representation of bankers in regulatory agencies as a form of public service, and show that nearly 30% of U.S. banks have at least one director who is connected this way. Most notably, the 12 Federal Reserve Banks, which between them supervise all Bank Holding Companies in the U.S., are each overseen by a board of directors that consists of bankers. In addition, the Federal Reserve also relies heavily on bankers to inform policy through their participation in advisory councils.

I show that connected banks —banks that have directors who served in regulatory agencies as a form of public service— undermine supervisory effectiveness. Connected banks have lower risk to capital sensitivities and extract larger public subsidies by shifting risk to the financial safety-net. I use two empirical strategies: (i) the retirement of Federal Reserve Presidents as negative shocks to the efficacy of existing connections and; (ii) the heterogenous effects of the Emergency Economic Stabilization Act of 2008 on risk-shifting incentives, to minimize endogeneity concerns. I further show that preferential treatment in supervision is one reason that explains the risk-shifting behavior of connected banks. The chapter concludes by documenting that wealth is transferred from taxpayers to the shareholders of well-performing connected banks. The findings draws attention to the potential conflicts of interest that exists in allowing bank representation and involvement in regulatory agencies.

The third chapter takes a departure from the subject of supervisory effectiveness and focuses on a determinant of credit allocation policies within banks —CEO hometown favoritism— and investigates its real effects on the economy. Understanding how bank credit is allocated in the presence of behavioural biases and whether such allocations are efficient is an important question given the importance of credit supply on housing outcomes and economic development.

Exploiting within-bank variation in distances to a CEO's place of birth, I show that bank CEOs make more mortgage and small business lending as well as open more branches in counties that are proximate to their place of birth as compared to counties that are located further away. To establish causality and reduce the possibility that CEO-bank matching is driving the results, I use: (i) a subsample of exogenous turnovers and; (ii) exogenous changes in the macroeconomic environment. I further explore various explanations for why bank CEOs favor their hometown and find support for an altruistic motive; the hometown favoritism effect is stronger during economic downturns, among altruistic CEOs, in poorer counties and marginal mortgage applicants. Finally, I document that hometown favoritism is not associated with worst bank performance, but leads to positive economic outcomes for residents located in counties proximate to the hometown of bank CEOs. This suggests that hometown favoritism, while arising out of the altruistic goodwill of the CEO, might inadvertently contribute to economic inequality.

1
Does Bank Supervision Matter? Evidence from Regulatory Office Closures

1.1 Introduction

Banking is one of the most heavily regulated industries. While U.S. banks operate under a unified framework of written rules, the task of ensuring compliance with these rules is divided between different supervisory agencies and their nationwide network of local offices. A particular feature of this supervisory system is that banks in the same geographic area may each be supervised by a different supervisory office. In this chapter, I utilize these decentralized supervisory arrangements to investigate whether supervision has an effect on bank behavior.

Analyzing whether and how bank supervisors affect bank behavior poses a number of identification challenges. Most poignantly, the behavior of banks and supervisors is determined in an endogenous process where risk and other operational bank choices will spur supervisory action. Likewise, economic shocks, many local in nature and not directly observable, will affect the conduct of banks and supervisors simultaneously. Therefore, establishing causal links between bank outcomes and supervision is challenging.

The existing literature highlights some of the difficulties of linking supervision to banking sector variables (Barth, Caprio, and Levine, 2004). In particular, the literature demonstrates how the supervisory setting is endogenously determined when banks locate risky activities away from geographic environments under strict supervision (Ongena, Popov, and Udell, 2013), when the strictness of supervisors is dependent on the state of the economy (Agarwal, Lucca, Seru, and Trebbi, 2014) or when banks shop for supervisors they expect to be softer on them (Rosen, 2003; 2005).

In this chapter, I utilize some of the unique features of the U.S. system of bank supervision to overcome these identification challenges. I exploit the fact that banks in the same geographic area may be supervised by different regulators. Specifically, commercial banks are supervised by one of three main federal regulators (the Federal Deposit Insurance Corporation (FDIC), the Federal Reserve (Fed) or the Office of the Comptroller of the Currency (OCC)).[1] This decentralized supervisory set-up allows me to devise a quasi-natural experiment that utilizes the closures of regulatory offices as negative shocks to the efficacy of supervision.

My identification is based on the rationale that the closure of a regulatory office increases the cost of collecting and verifying bank-specific soft information for supervisors and thus, leads to a decrease in efficacy of bank supervision. However, within the counties in which a recently closed supervisory office has operated, only

[1] A bank's charter and membership to the Federal Reserve System determines the primary federal regulator charged with supervising a bank's activities. Banks with national charters are supervised by the OCC. Banks with state charters that are members of the Fed System are supervised by the Fed while other state-chartered banks are supervised by the FDIC.

banks supervised by the closed regulatory office should be affected by the shock. Banks that are supervised by a different regulatory office should remain unaffected by the closure.

A key advantage of this set-up is that I can compare the behavior of treated banks that were affected by regulatory office closures (because the closed office was responsible for their supervision) to a control group of banks that operate in the same counties as affected banks but are not supervised by the closed office. Since treatment and control banks are located in the same counties, they are exposed to similar local economic conditions. This alleviates concerns that my results could be biased by unobserved local economic shocks that simultaneously affect the behavior of banks and supervisors.[2]

My main hypothesis is that the closure of a regulatory office will impair the efficacy of banking supervision amongst the banks under the supervision of the office that has been closed. To test this hypothesis, I hand-collect a new dataset that maps out the locations of regulatory offices belonging to the FDIC, the Fed and the OCC. I conduct my analysis in a difference-in-difference (DiD) setting using 10 regulatory office closures, 278 (140) treatment (control) banks, and 8,321 bank-quarter observations from 2002 to 2013.

My results show that, following regulatory office closures, banks supervised by the recently closed office increase their risk-taking compared to a control group of banks that are located in the same counties as banks affected by the closure. The

[2] A further advantage of my identification strategy is that there are multiple shocks (i.e., regulatory office closures) affecting different banks in different geographical locations at different times. This minimizes the possibility that potential omitted variables coinciding with a single shock could bias my results (Atanasov and Black, 2016).

economic magnitude of the effects are sizable. For instance, banks whose supervisory office closed increase their solvency risk (*Z-Score*) by approximately 19% (relative to the sample mean) compared to the control group. Further, regulatory office closures are also linked to changes in the asset portfolios of banks under the supervision of a closed office. These banks grew their total loan portfolio by 11% and their real estate loan portfolio by 16% compared to the control group.[3]

My findings are robust to different model specifications as well as the inclusion of various control variables and different sets of fixed effects. The tightest specification includes county x year-quarter, regulatory office x year-quarter and bank fixed effects. Thus, I estimate changes in the behavior of banks vis-à-vis the control group *within* the same county and quarter and *within* the group of banks supervised by a particular regulatory office in each quarter. Consequently, time-varying omitted variables (e.g., local economic shocks or time-varying preferences in the enforcement of supervision by regulatory offices) are unlikely to bias my results.

Further, I conduct several diagnostic and placebo tests to validate my empirical design. I confirm that there are no statistical differences in bank characteristics or trends in the pre-closure period between the treated and control group of banks. I also show that changes in bank behavior are indeed observed *after* (and not before) regulatory office closures. Incidentally, the effects I document remain fairly persistent in each of the post-closure years. This rules out a temporary "distraction" explanation

[3] The exact characteristics of each closure (e.g., what percentage of staff will relocate to the new office or the distance to the new office) could affect the extent to which regulatory office closures impair effective enforcement. Indeed, I show that changes in bank behavior are more pronounced for large increases in distance between banks and the new regulatory office (as caused by the closure of the previously responsible office). However, many other office characteristics are not observable. I therefore interpret the effects I document in this chapter as an aggregate estimate of the effects that office closures have on banks.

following the closure when a new supervisory office is now allocated to a bank and needs to familiarize itself with the institution.

Additionally, I conduct a placebo test that repeats my analysis using only banks in the control group. I find no evidence that banks in the control group increase their risk-taking and lending in the period following regulatory office closures. Finally, I confirm that changes in the performance or risk of banks under the supervision of a regulatory office do not predict the closure of that office. Instead, office closures occur when offices are smaller and experience declines in the assets under its supervision. Thus, the closure of offices is related to the need to rebalance supervisory resources and not the behavior of the *individual* banks under their supervision.

The increases in risk-taking and lending I document following closures of regulatory offices may not necessarily be a cause for concern. For instance, my findings are equally consistent with explanations that regulators may have been too strict in the pre-closure period and that less regulatory attention in the period after a supervisory office closure permits banks to take calculated risks without negative consequences for them. However, the results from various tests do not support this view. While I show that bank profitability increases after regulatory office closures, I also show that less supervisory attention is associated with a number of negative bank outcomes, including lower risk-adjusted performance, more non-performing loans and a higher probability of failure during the 2007-09 financial crisis. Subsequently, office closures are also associated with real costs to the FDIC. I estimate that the closure of a single regulatory office leads to bank failure resolution costs of approximately $15.7 million.

Why do regulatory office closures lead to higher bank risk-taking? In the second part of this chapter, I present evidence that points to information asymmetry issues between regulators and banks as one driving factor behind my results. In a first test, I examine the loan loss provisioning practices of banks. Loan loss provisions (LLPs) should reflect the expected future losses on a bank's loan portfolio. Sustained under-provisioning by banks can lead to capital inadequacy concerns if the accrued LLPs are insufficient to cover losses during economic downturns (Beatty and Liao, 2014; Bushman and Williams, 2015). Ensuring that banks maintain LLPs that are commensurate with the expected future losses on their loans is therefore important from a regulatory perspective (Costello, Granja, and Weber, 2016).

However, since loans are notoriously opaque, banks enjoy considerable discretion in provisioning for expected losses on their loan portfolio (Beatty and Liao, 2014). I therefore expect that office closures impede a supervisor's ability to enforce provisioning practices that are commensurate with the risk profile of a bank's loan portfolio. Specifically, I expect to find that information asymmetry issues permit banks to lever their information advantages over their newly assigned regulatory office to delay provisioning and boost current income.

In line with this expectation, I find that the closure of a regulatory office affects the discretionary provisioning practices of banks previously supervised by the closed office. First, I find that banks previously supervised by a closed office report lower as well as less timely loan loss provisions. Second, using various approaches to determine the discretionary component in banks' LLPs (as used in Kanagaretnam, Krishnan, and Lobo, 2010; Beatty and Liao, 2014; Jiang, Levine, and Lin, 2016), I show that, following supervisory office closures, banks previously supervised by a closed office systematically increase their use of income-increasing provisions which leads to more opaque balance sheets. I interpret this as consistent with banks making opportunistic use of information asymmetry issues between them and their newly assigned regulatory office to make income-increasing accruals.

My second test for information frictions is based on the distance between banks and supervisory offices. Larger physical distances between economic agents are commonly associated with higher levels of information asymmetry due to the increased cost of collecting and verifying of soft information (Coval and Moskowitz, 2001; Malloy, 2005; Agarwal and Hauswald, 2010; Kedia and Rajgopal, 2011). When banks are assigned a new regulatory office, this results in a kilometer-increase in the physical distance between a bank and its regulatory office. My results show that increases of 15 km or more lead to higher levels of bank risk-taking. By contrast, smaller increases in distance have no measurable effect on bank behavior.

1.2 Related Literature and Institutional Background

1.2.1 Related Literature

My work is related to a growing literature on the impact of supervision on bank-level outcomes. Early studies on this topic rely on cross-country differences in supervision and have produced mixed results. Barth et al. (2004) find no evidence that measures of supervisory power are related to bank development, bank efficiency or loan performance across countries. Ongena et al. (2013) find that the effectiveness of regulation on banks' lending standards abroad partly depends on the strength of supervision in their home market.[4]

Recent work has devised empirical set-ups to isolate the effects of supervision on bank behavior within the U.S. Rezende and Wu (2014) use discontinuities in bank size thresholds that determine the minimum number of on-site bank examinations to demonstrate that more frequent examinations are associated with better bank performance. Hirtle, Kovner, and Plosser (2016) use size rankings of Bank Holding Companies (BHCs) within the individual 12 districts of the Federal Reserve 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 BHCs in a different Fed district that are not amongst the five largest banks in that district.

I take this previous work on supervision and bank behavior as a starting point and build on it in the following ways. I devise a new quasi-experimental setting —the closure of regulatory offices— which allows me to use geographically granular data to systematically contrast changes in the behavior of banks supervised by a previously closed office to the behavior of unaffected banks residing in the *same* counties. In a later study by Kandrac and Schlusche (2017), the authors offer an analysis set across the 12 Federal Home Loan Banks (FHLB) that act as the primary supervisor of thrifts in their districts. They show that following the relocation of the 9th district FHLB in 1983, thrifts under its supervision took on more risk compared with thrifts in other districts.

[4] More broadly related to my work are studies that examine the effects of regulatory enforcement actions. For instance, Peek and Rosengren (1995) and Danisewicz, McGowan, Onali, and Schaeck (2016) find that regulatory enforcement actions reduce the supply of credit provided by banks.

My paper differs from Kandrac and Schlusche (2017) in several ways. First, Kandrac and Schlusche (2017) compare thrifts affected by the relocation of the 9th FHLB office to unaffected thrifts from neighboring districts (where economic conditions might differ). Importantly, my analysis is set *within the counties* in which banks affected by an office closure reside. This aids a plausibly causal interpretation of the results as it allows me to compare affected and unaffected banks operating under similar local conditions and thus, hold constant any effects local economic conditions might have on the conduct of banks and supervisors.

Further, my analysis exploits multiple (10) office closures from all three federal regulators spanning years 2003 to 2010 (Kandrac and Schlusche (2017) use only one relocation event in 1983). The use of multiple office closures occurring in different years as well as across different regulators allows me to sharpen inference and generalize my findings by reducing any systematic bias that arise from any specific circumstance surrounding an office closure. Based on this granular geographic analysis, I show that decreases in supervisory efficacy lead to higher bank risk, lower asset quality and higher failures rates in times of economic downturns.[5]

Second, my work is related to studies on regulatory inconsistency and arbitrage within the decentralized structure of U.S. bank supervision. Rosen (2003; 2005) finds that banks show better performance after switching regulators and argues competition

[5] In a paper that entered the public domain after mine, Gopalan, Kalda, and Manela (2017) use a similar research design and present results that are largely complementary. Goplan et al. cite this chapter as inspiration for their identification strategy and for part of their data collection strategy. I find that following regulatory office closures, banks increase their risk-taking and loan expansion while Gopalan et al. report higher leverage ratios. They also find a higher probability of bank failures as I do. However, while Gopalan et al. use OCC regulated banks, I examine the closures of offices of all three federal agencies. This allows me to paint a systematic picture of the effects of supervisors beyond the specific behavior (including biases) of any single supervisor. Additionally, I offer evidence of information asymmetry issues between supervisors and banks by examining bank loan loss provisioning behavior.

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. Agarwal et al. (2014) exploit supervisory rotation policies —which assign federal and state regulators to the same bank exogenously— to show that different regulators implement identical regulations inconsistently. Agarwal et al. (2014) argue that discrepancies in regulatory behavior arises due to differences in incentives, with state regulators being more lenient because they are more concerned about local economic conditions. The chapter contributes to this line of work by offering evidence consistent with information asymmetry issues between examiners and banks as a complementary explanation for discrepancies in supervisory enforcement.

Third, the chapter is also part of the literature on the determinants of bank financial reporting choices and their consequences. Costello et al. (2016) use a regulatory stringency index to show that strict regulators are more likely to enforce income-reducing reporting choices by forcing bank restatements. Further, distortions in bank financial reporting can lead to increases in bank and systemic risk (Bushman and Williams, 2015), reductions in the supply of loans (Beatty and Liao, 2011) and lower bank valuations (Huizinga and Laeven, 2012). I show that the nearby presence of regulatory offices prevents banks from engaging in income-increasing accounting choices which could potentially be destabilizing.

1.2.2 Institutional Background

Banks in the U.S. operate under a decentralized structure of bank supervision. Three federal regulators divide the supervision of U.S. commercial banks based on the charter and geographic location of these institutions (Federal Deposit Insurance Corporation, 2015; Board of Governors of the Federal Reserve System, 2017a; Office of the Comptroller of the Currency, 2017). The OCC supervises banks with a federal charter, i.e. all national banks. By contrast, the federal regulator responsible for state-chartered banks is divided between the Federal Deposit Insurance Corporation (FDIC) and the Federal Reserve (Fed). If a bank is a member of the Federal Reserve System, it is supervised by the Fed. Alternatively, if it is not a member of the Fed, it is then supervised by the FDIC.

Prior to the 1980s, different bank charters implied differences in bank capital requirements, lending limits and permissible activities. However, these differences have diminished over time. Subsequently, banks select their charters based on the cost that supervisors charge for supervision and their accessibility (Rosen, 2005; Blair and Kushmeider, 2006; Agarwal et al., 2014).[6]

While a bank's charter determines which agency is responsible for supervising a bank, the supervisory unit (or "office") in charge of a bank's day-to-day supervision is determined by the geographic location of a bank. The Federal Reserve System covers 12 districts, each headed by a Federal Reserve Bank with multiple local offices that supervise banks located in that district. For instance, the Federal Reserve Bank of San Francisco heads the 12th district and oversees four offices (located 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 offices that are tasked

[6] I take several steps to ensure that bank preferences for a certain charter does not bias my results. First, I exclude banks which have changed their charters. Second, I also omit banks which have changed the location of their headquarters. Third, I include bank, regulatory office and time fixed effects to control for any time-invariant differences. I detail this in Section 1.3.

with supervising banks that fall under its geographic coverage. The OCC divides itself into four districts (Central, Northeastern, Southern and Western) and relies on a network of offices that operate under each district to supervise banks that are headquartered within their area of jurisdiction.

This decentralized network of offices is vital for supervisors to carry out on-site and off-site monitoring of banks. Teams of travelling examiners, who are typically based in the nearest responsible supervisory office, will conduct on-site ("safety and soundness") examinations to verify the accuracy of information contained in regulatory filings made by banks. The examiners will also assess the validity of internal risk management processes and models, review loan portfolios and meet with and evaluate the management of a bank. Between on-site examinations, examiners engage in off-site monitoring to assess a bank's financial condition via monitoring of the Call Reports filed by banks with supervisors on a quarterly basis (Federal Deposit Insurance Corporation, 2015).

For these supervisory activities, knowledge of the environment in which banks and their customers operate is important. Much of this information is soft and of a local nature and proximity to banks is therefore important for effective bank supervision. Consistent with this, the Federal Reserve Bank of St. Louis (2017) argues that "*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. Branches allow not only for a more efficient collection of information, but also for deeper relationships through staff involvement in their local economies, producing a depth and breadth of information not possible from hundreds of miles away*."

1.3 Data and Empirical Methodology

1.3.1 Regulatory Office Closures

I study the effects of supervisory office closures on bank behavior. My analysis focuses on federal supervisors (rather than state supervisors), because federal regulators have been shown to enforce regulations more consistently than state regulators. For instance, Agarwal et al. (2014) show that state regulators are influenced by the potential effects that their actions might have on local economic conditions. Further, Costello et al. (2016) argue that state regulators are more prone to "capture" by the banks they supervise as compared to federal regulators. Therefore, by focusing on federal regulators, I can interpret the results as plausibly due to a reduction in supervisory attention rather than other supervisor-specific factors.

To study the impact of regulatory office closures on bank behavior, I require a comprehensive dataset of the locations of regulatory offices. However, data on the historical locations of offices are not directly obtainable from regulators. I therefore hand-collect data from various sources to construct a novel dataset of regulatory office locations that I then use to identify office closures.

To obtain data on the past locations of Fed offices, I manually collect and verify regulatory office locations from the annual reports of the 12 Federal Reserve Banks. I obtain annual reports from the websites of the respective Federal Reserve Banks and the FRASER archive maintained by the Federal Reserve Bank of St. Louis. When I cannot identify past office locations this way, I consult historical accounts detailing the histories of the Federal Reserve Banks. These accounts often include architectural descriptions as well as the physical location of the buildings used by the Fed Banks and their branches.[7] I am able to identify the exact geographical locations of all Federal Reserve Banks and their branches from 1984 to 2013.

The FDIC and OCC offer considerably less information on the locations of their offices in their Annual Reports or websites. I therefore rely on a different strategy to identify the historical locations of offices belonging to the FDIC and OCC.[8] I 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 time intervals. Since the FDIC and OCC maintain information on office locations on their websites, accessing past versions of these websites allows me to map the historical locations of FDIC and OCC offices across time. I am able to retrieve geographical locations of FDIC offices from 2002 to 2009 and data on OCC office locations from 2004 to 2013 (differences in the data coverage between the two regulators are due to differences in the coverage of WayBack Machine).

Using the above steps, I 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. Having obtained the locations of regulatory offices, I next match banks to the regulatory office that supervise them using information on the charter type and zip codes of a bank's headquarters as published in their Call Reports.

[7] 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)

[8] Office locations can typically be found under the "contact" or organizational structure" pages. E.g., 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

Specifically, I identify a regulatory office as responsible for a bank if it meets two conditions: (i) the regulatory office belongs to the federal regulator that banks indicate in their Call Reports as responsible for overseeing them[9] and (ii) the regulatory office is geographically closest to the headquarters of a bank.[10] To calculate the distance between the headquarters of each bank and regulatory offices, I obtain the latitude and longitude coordinates corresponding to their zip codes from the U.S. Census Bureau Gazetteer. I then use the Haversine Formula to obtain the kilometer distance between each bank-office pair. Specifically, I compute the distance between locations *1* and *2* as follows:

$$Distance_{12} = r \times 2 \times arcsin(min\ (1, \sqrt{a})) \qquad (1\text{-}1)$$

where $a = [sin(lat2\text{-}lat1)/2]^2 + cos(lat1) \times cos(lat2) \times [sin(lon2\text{-}lon1)/2]^2$

and $r \approx 6{,}378\ km$ (the radius of the earth)

where *lat1* and *lon1(lat2* and *lon2*) are the latitudes and longitudes of the headquarters of the bank (regulatory office) respectively.

I identify regulatory offices as closed if the office location ceases to be listed in official documents or on the regulator's website. For example, the Federal Reserve Bank of New York Buffalo Office last appeared in official documents in 2008. In my analysis, I treat 2008 as the year of the closure of this office. Following a closure, I assume that the next closest regulatory office that is responsible for banks of the same charter will take over the supervision of the banks affected by the closure.

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[9] As detailed in Section 1.2.2, nationally-chartered banks are supervised by the OCC. State-chartered banks that are not members of the Federal Reserve System are regulated by the FDIC while state-chartered banks that are members of the Federal Reserve System are regulated by the Fed.

[10] I confirm the validity of the assumption that banks are mostly supervised by the geographically closest regulatory office with senior supervisors. Further, I match banks and regulators based on the bank's headquarters, because on-site examinations involve discussion and evaluations of a bank's senior management and risk management units who tend to be based in the headquarters.

To ensure that regulatory offices are not simply renamed following minor relocations, I manually check and compare the addresses of new offices after an existing office was closed. I exclude closures where a new regulatory office opened in the same county up to one year after an office was closed. I show the years in which offices close in Panel A of Table 1-1. I am able to identify 11 office closures (5 FDIC, 1 Fed and 5 OCC). The locations of both the closed offices as well as of the offices that do not close are shown in Figure 1-1.

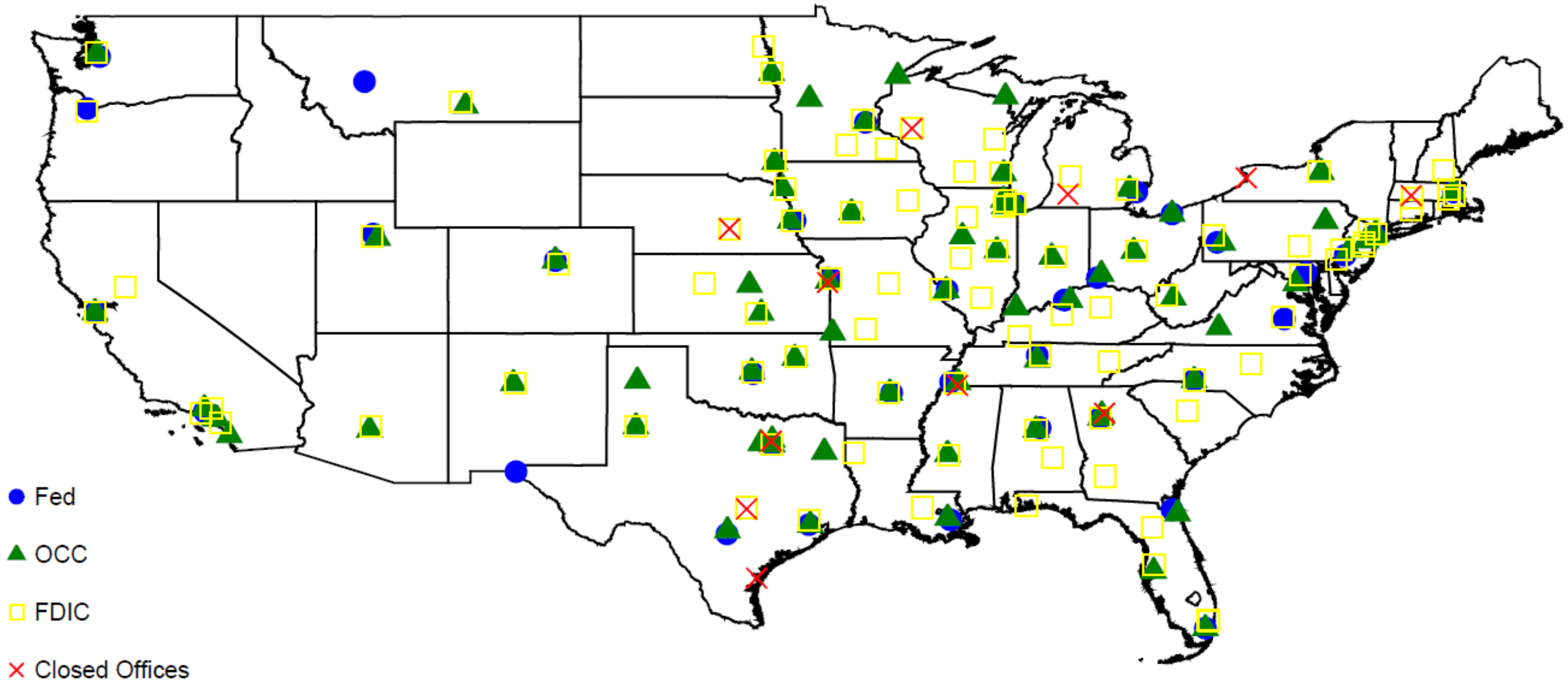
Minimizing the costs of maintaining multiple on-site locations is a likely consideration behind the closure of regulatory offices.[11] For instance, when announcing the closure of its Buffalo Office in 2008, the New York Fed announced "[this] *follows a re-examination of the Bank's regional strategy, which determined that the Second District would be better served if the Bank rebalanced the resources applied to its regional efforts to enhance analysis and outreach across the entire District*" (Federal Reserve Bank of New York, 2008).

As the viability (costs) of maintaining a regulatory office is indirectly tied to the amount of bank assets under its supervision (as the amount of assets under supervision is positively related to the amount of supervisory fees paid), a potential concern that could arise in my analysis is that office closures reflect changing local economic conditions. For instance, if the reason behind decreases in assets under super

[11] For the OCC, regulatory revenues are directly tied to the amount of bank assets under supervision. By contrast, the Fed and FDIC do not derive their revenues from bank supervision. The Fed's income comes primarily from the interest on U.S. Treasury securities that it has acquired through open market operations and fees received for the provision of non-supervisory services provided to banks such as check clearing and funds transfers (Board of Governors of the Federal Reserve System, 2017b). For the FDIC, it is funded by premiums that banks pay for deposit insurance and investments in U.S. Treasury securities (Federal Deposit Insurance Corporation, 2017).

Figure 1-1: Regulatory Office Locations

This figure shows the geographical locations of regulatory offices in the U.S. The blue circle denotes Federal Reserve (Fed) offices from 1984 to 2013. The green triangle shows locations of offices belonging to the Office of the Comptroller of the Currency (OCC) from 2004 to 2013. The yellow squares are offices belonging to the Federal Deposit Insurance Corporation (FDIC) from 2002 to 2009. The red "x" shows the locations of offices belonging to the OCC, Fed and FDIC that were closed between 2001 and 2013.



-vision at a closed office is due to banks under that office performing poorly because of deteriorating local economic conditions, comparing those banks that experience an office closure to better performing banks would bias my analysis. I devise an empirical strategy in the next section that circumvents this issue.

1.3.2 Empirical Methodology

1.3.2.1 Identification

My identification strategy exploits closures of federal regulatory offices to investigate the impact of bank supervision on bank business policies. I use a difference-in-difference (DiD) setting to contrast the behavior of banks following the closure of their responsible regulatory office to banks located in the same counties as affected banks but not affected by the office closure. This is possible as banks located in the same geographical areas can be supervised by different offices depending on their federal regulator (charter) and proximity to regulatory offices.

By exploiting differences in the exposure of banks to regulatory closures *within* the counties affected by the closure, I can rule out the possibility that my results are driven by local factors (e.g., local economic shocks) that could affect both the decision to close a regulatory office and the behavior of banks. Furthermore, there are multiple closures affecting different banks located in different geographical locales across time. This reduces the possibility that omitted variables, which coincide with a single office closure, are correlated with bank outcomes and could thus bias my findings. I estimate variants of the following specification:

$$\begin{aligned} Y_{i,k,t} = \; & \alpha_{i,k,t} + \beta_1 Treated_{i,k,t} + \beta_2 Post_{k,t} + \beta_3 Treated_{i,k,t} \times Post_{k,t} + Z_{i,k,t} \\ & + Bank\ FE + Regulatory\ Office\ FE + Year\text{-}Quarter\ FE + \varepsilon_{i,k,t} \qquad (1\text{-}2) \end{aligned}$$

where i, k and t indicate bank i, regulatory office k and year-quarter t, respectively. Z is a vector of control variables. For robustness, I estimate the DiD regressions with and without control variables as the inclusion of time-varying control variables could introduce bias in the estimation if these variables are affected by regulatory office closures (Angrist and Pischke, 2009).

Treated is equal to 1 if banks are in the treated group (and 0 for banks in the control group). Treated banks are banks that are affected by regulatory office closures (i.e., banks that are under the supervision of a closed office). My control group contains banks that are headquartered in the same counties as treated banks but not affected by regulatory office closures (because they are not supervised by the closed office).

Post equals 1 in the three years after the closure of a regulatory office k (and 0 in the two years before the closure of a regulatory office). For instance, if the last year for which I find a record that an office exists is 2008, *Post* equals 1 in 2009 to 2011 and 0 in 2007 to 2008. I use a five-year DiD window as it is long enough for banks to implement changes in their business policies following the closure of a regulatory office. Analyzing bank behavior over longer periods risks introducing noise to the analysis (Bertrand, Duflo, and Mullainathan, 2004) and would prevent me from including most of the FDIC office closures in my analysis. The latter is due to the relatively shorter period (2002 to 2009) for which I observe FDIC office closures.

My variable of interest is the coefficient on the interaction term *Treated* × *Post* that takes a value of 1 for treated banks in the three years after closure of its regulatory office (and 0 otherwise). *Treated* × *Post* therefore captures changes in the behavior of banks affected by regulatory closures relative to their pre-closure behavior and relative to banks in the same counties that are not affected by office closures.

Finally, I include bank, year-quarter and regulatory office fixed effects to further sharpen the identification. Bank fixed effects control for time-invariant bank specific omitted variables that could differ across banks. For instance, banks which are affected by regulatory office closures could exhibit risk cultures that are inherently different from other banks. The inclusion of year-quarter fixed effects controls for time effects while regulatory office fixed effects controls for time-invariant heterogeneity in supervisory practices across different offices (and different federal regulators). For instance, different federal regulatory agencies and regulatory offices could differ in their stringency of supervision.

I obtain financial variables from Call Reports from the Federal Reserve Bank of Chicago and winsorize them at the 1% and 99% percentile. I require that both treated and control bank have < $1 billion in assets (adjusted using the 2009 GDP deflator) and not have relocated or changed charters during the 5-year DiD window. Larger banks (with >1 billion in assets) may be subject to different levels of depositor, market and regulatory discipline which could bias my results. For instance, many large banks have in-house examiners which makes regulatory offices less relevant for their supervision (Wilson and Veuger, 2016). Further, changes in a bank's charter or the relocation of its headquarters could be due to banks engaging in "regulatory shopping" (e.g., Rosen, 2003; 2005) to evade supervision. These filters ensure that any potential reasons banks might have for selecting different charters (which would determine if they are affected by regulatory office closures) do not affect my analysis.

For the difference-in-difference analysis, I use a total of 10 regulatory office closures (4 FDIC, 1 Fed and 5 OCC) with 278 treatment banks, 140 control banks, and a total of 8,321 bank-quarter observations. Summary statistics of the banks used in the DiD analysis are reported in Panel B of Table 1-1. A full list of balance sheet variables used in this study and their definitions are listed in Appendix 1-A1.

Table 1-1: Office Closures and Summary Statistics

This table reports information on the year of regulatory office closures and summary statistics for the variables used in this chapter which consists of U.S. commercial banks for the years 2002-2013 (unbalanced panel). # is the number of bank-year observations, std. is the standard deviation while p1, p50 and p99 are the 1st, 50th, and 99th percentiles. Panel A report regulatory office closure years while Panel B show summary statistics of treated and control banks used in the difference-in-difference analysis. Treated banks are banks which are affected by regulatory office closures (because they are supervised by the closed office). Control banks are banks which are headquartered in the same counties as treated banks but are not supervised by the closed office. See Section 1.3 for the detailed construction of treatment and control groups. Bank balance sheet variables are winsorized at the 1% and 99% levels. Refer to Appendix 1-A1 for the definition and construction of variables used in this chapter.

Panel A: Office Closures

Federal Regulator	Year Closed	# Offices Closed
Fed	2008	1
FDIC	2003	4
FDIC	2007	1
OCC	2005	1
OCC	2006	1
OCC	2009	2
OCC	2010	1

Panel B: Summary Statistics	Difference-in-Difference Sample					
	#	mean	std.	p1	p50	p99
Risk & Performance Variables						
Z-Score	7,679	-4.684	0.775	-6.312	-4.746	-2.722
σROA	7,681	0.00134	0.00126	0.000224	0.000914	0.00622
σROE	7,681	0.0135	0.0138	0.00187	0.00916	0.0737
ROA/σROA	7,679	3.737	4.438	-0.659	2.903	14.85
ROE/σROE	7,679	38.21	47.72	-7.414	28.11	151.6
ROA	8,321	0.00216	0.00327	-0.0155	0.00256	0.00806
ROE	8,321	0.0235	0.0277	-0.0693	0.0251	0.0905
EBLLP	8,321	0.00289	0.00252	-0.00819	0.00306	0.00877
Loan Variables						
Total Loans	8,321	0.609	0.167	0.163	0.629	0.888
Total Loans (Thousands)	8,321	93,771	103,515	3,609	54,154	496,375
Real Estate Loans (to Total Assets)	8,321	0.409	0.167	0.0490	0.407	0.740
Real Estate Loans (to Total Loans)	8,321	0.658	0.167	0.215	0.686	0.944
Real Estate Loans (Thousands)	8,321	66,729	79,723	1,028	36,190	373,697
Agri Loans	8,321	0.0681	0.107	0	0.0163	0.481
CI Loans	8,321	0.153	0.0918	0.00829	0.134	0.467
Indiv Loans	8,321	0.105	0.0916	0.00260	0.0786	0.428
Financial Variables						
Total Assets (Log)	8,321	11.43	0.990	9.280	11.40	13.44
Total Deposits	8,321	0.840	0.0734	0.534	0.857	0.928
Tier-1 Capital	8,321	0.175	0.0988	0.0859	0.144	0.654
LLA	8,321	0.0148	0.00737	0.00455	0.0130	0.0465
LLP	8,321	0.00113	0.00239	-0.00121	0.000509	0.0121
BHC	8,321	0.766	0.424	0	1	1
Mandatory Audit	8,321	0.0397	0.195	0	0	1
Audit	8,321	0.592	0.492	0	1	1
Bad Loans	8,321	0.0101	0.0147	0	0.00502	0.0699
Loan Charge-Offs	8,321	0.000958	0.00212	0	0.000248	0.0108
Accounting Variables						
|ALLP A|	7,961	0.120	0.163	0.00197	0.0858	0.852
|ALLP B|	7,961	0.117	0.163	0.00175	0.0791	0.816
+ALLP A	1,933	0.182	0.319	0.00116	0.0739	1.836
+ALLP B	2,176	0.167	0.299	0.00112	0.0683	1.717
-ALLP A	6,028	-0.102	0.0723	-0.358	-0.0881	-0.002
-ALLP B	5,785	-0.0997	0.0781	-0.366	-0.0821	-0.002
County & State Variables						
County Income per Cap (Log)	8,321	3.464	0.240	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	0.0002850	0.0000018	0.0000307	0.000997
ΔState UR	8,321	0.0388	0.173	-0.138	0	0.583
ΔState HPI	8,321	0.00815	0.00832	-0.0222	0.00928	0.0219

Second, I confirm that the parallel trends assumption which states that, absent treatment, the average change (trend) in bank behavior should be similar for the treatment and control group prior to regulatory office closures is not violated. I conduct *t*-tests for the difference in means in the *change*s in all dependent variables that I use in this chapter between the treatment and control groups in *t*-1. Columns (7)-(8) of Panel B Table 1-2 show that all variable changes are statistically insignificant. Accordingly, there are no differences in trends between the treatment and control group in the pre-office closure period.

Importantly, Table 1-2 also validates my choice of using banks that are located in the same counties as treated banks. Columns (9)-(10) in Panel A of Table 1-2 show that in *t*-1 (pre-office closure), treated banks are systematically different from a general control group of banks that is distributed across the U.S.[12] Likewise, Columns (9)-(10) in Panel B test the parallel trends assumption for the treatment group and the U.S.-wide control group. I find that the trend (change) in the pre-closure period for eight out of eighteen dependent variables is statistically significant (at least at the 10% level) which violates the parallel trends assumption.

[12] This general group of control banks contains all banks that I am able to match to their relevant supervisory office and are unaffected by regulatory office closures.

Table 1-2: Pre-closure Covariate Balance and Parallel Trends

This table shows the results of diagnostic tests for the difference-in-difference regressions. Panel A reports pre-shock (1 quarter before regulatory office closures) means (Columns (1)-(6)) and differences in means and their p-values (Columns (7)-(10)) of covariates between the treated and control groups as well as the full sample. Panel B reports pre-shock (1 quarter before regulatory office closures) *changes* in the mean of dependent variables (Columns (1)-(6)) and differences in *changes* of the means and their p-values (Columns (7)-(10)) between the treated and control groups as well as the full sample. Treated banks are banks which are affected by regulatory office closures (because they are supervised by the closed office). Control banks are banks which are headquartered in the same counties as treated banks but not supervised by the closed office. See Section 1.3 for the detailed construction of treatment and control groups. Bank balance sheet variables are winsorized at the 1% and 99% levels. Refer to Appendix 1-A1 for the definition and construction of variables used in this chapter.

Panel A: Covariate Balance	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Treated		Control		All Possible Controls		Diff. in Means		Diff. in Means	
Variables	#	mean	#	mean	#	mean	(Treated-Control)	p	(Treated-Full Sample)	p
Z-Score	264	-4.6100	128	-4.6119	32889	-4.6232	0.0019	0.98	0.0132	0.80
σROA	264	0.0015	128	0.0014	32914	0.0015	0.0000	0.74	-0.0001	0.39
σROE	264	0.0148	128	0.0148	32914	0.0158	0.0000	0.98	-0.0011	0.34
ROA/σROA	264	3.3219	128	3.4891	32889	3.6742	-0.1671	0.62	-0.3522	0.74
ROE/σROE	264	33.6462	128	35.5393	32889	37.6020	-1.8931	0.59	-3.9559	0.71
ROA	278	0.0014	140	0.0012	34860	0.0009	0.0002	0.70	0.0004	0.11
ROE	278	0.0161	140	0.0162	34860	0.0094	-0.0001	0.98	0.0067	0.02**
EBLLP	278	0.0024	140	0.0022	34860	0.0026	0.0002	0.55	-0.0002	0.22
Total Loans	278	0.5936	140	0.6103	34860	0.6465	-0.0168	0.33	-0.0529	0.00***
Total Loans (Thousands)	278	85711	140	93080	34860	217177	-7368	0.46	-131466	0.00***
Real Estate Loans (to Total Assets)	278	0.3948	140	0.4033	34860	0.4406	-0.0085	0.61	-0.0458	0.00***
Real Estate Loans (to Total Loans)	278	0.6523	140	0.6484	34860	0.6698	0.0039	0.82	-0.0176	0.12
Real Estate Loans (Thousands)	278	59405	140	65524	34860	142611	-6119	0.41	-83206	0.00***
Agri Loans	278	0.0655	140	0.0710	34860	0.0777	-0.0055	0.62	-0.0123	0.11
CI Loans	278	0.1508	140	0.1641	34860	0.1518	-0.0134	0.17	-0.0011	0.86
Indiv Loans	278	0.1145	140	0.0992	34860	0.0804	0.0153	0.11	0.0341	0.00***
Total Assets (Log)	278	11.3639	140	11.4145	34860	11.8038	-0.0506	0.62	-0.4399	0.00***
Total Deposits	278	0.8456	140	0.8353	34860	0.8262	0.0103	0.18	0.0195	0.00***
Tier-1 Capital	278	0.1777	140	0.1727	34860	0.1591	0.0050	0.64	0.0186	0.00***
LLA	278	0.0155	140	0.0146	34860	0.0153	0.0009	0.28	0.0001	0.78
LLP	278	0.0015	140	0.0013	34860	0.0024	0.0002	0.48	-0.0009	0.00***

Mandatory Audit	278	0.0288	140	0.0429	34860	0.1297	-0.0141	0.45	-0.1010	0.00***
Audit	278	0.5791	140	0.6000	34860	0.6337	-0.0209	0.68	-0.0546	0.06*
Bad Loans	278	0.0112	140	0.0098	34860	0.0159	0.0014	0.42	-0.0047	0.00***
Loan Charge-Offs	278	0.0016	140	0.0014	34860	0.0021	0.0002	0.43	-0.0005	0.05*
|ALLP A|	267	0.1425	133	0.1312	33837	0.2063	0.0112	0.57	-0.0639	0.00***
|ALLP B|	267	0.1381	133	0.1303	33837	0.2038	0.0079	0.68	-0.0656	0.00***
-ALLP A	188	-0.1056	99	-0.0958	20277	-0.1113	-0.0098	0.31	0.0057	0.40
-ALLP B	186	-0.1017	91	-0.0999	19771	-0.1133	-0.0018	0.86	0.0116	0.11
+ALLP A	79	0.2334	34	0.2344	13560	0.3595	-0.0010	0.99	-0.1261	0.04**
+ALLP B	81	0.2254	42	0.1960	14066	0.3409	0.0294	0.61	-0.1155	0.04**

Panel B: Parallel Trends	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Treated		Control		All Possible Controls		Diff. in Means		Diff. in Means	
Variables	#	mean	#	mean	#	mean	(Treated-Control)	p	(Treated-Full Sample)	p
ΔZ-Score	264	-0.0111	126	-0.0071	32719	0.0388	-0.0040	0.87	-0.0499	0.00***
ΔσROA	264	0.0000	126	0.0000	32746	0.0001	0.0000	0.69	-0.0001	0.00***
ΔσROE	264	0.0000	126	-0.0003	32746	0.0011	0.0003	0.62	-0.0011	0.00***
ΔROA/σROA	264	0.0070	126	-0.0377	32719	-0.1253	0.0447	0.67	0.1322	0.36
ΔROE/σROE	264	-0.0559	126	-0.3286	32719	-1.3706	0.2726	0.81	1.3147	0.39
ΔTotal Loans	276	0.0009	138	0.0067	34673	-0.0044	-0.0058	0.23	0.0053	0.04**
ΔTotal Loans (Thousands)	276	311	138	1671	34673	2904	-1360	0.42	-2593	0.12
ΔReal Estate Loans (to Total Assets)	276	0.0047	138	0.0059	34673	0.0002	-0.0012	0.76	0.0045	0.02**
ΔReal Estate Loans (Thousands)	276	1420	138	1428	34673	2243	-7	0.99	-823	0.47
ΔLLP	276	0.0001	138	0.0002	34673	0.0007	-0.0002	0.54	-0.0007	0.00***
ΔBad Loans	276	-0.0007	138	0.0003	34673	0.0006	-0.0009	0.27	-0.0012	0.05*
ΔLoan Charge-Offs	276	0.0007	138	0.0004	34673	0.0007	0.0003	0.32	-0.0001	0.78
Δ|ALLP A|	267	0.0089	133	0.0322	33644	0.0399	-0.0233	0.33	-0.0310	0.13
Δ|ALLP B|	267	0.0059	133	0.0355	33644	0.0397	-0.0295	0.19	-0.0338	0.09*
Δ-ALLP A	155	0.0119	88	0.0018	17338	0.0174	0.0101	0.25	-0.0055	0.38
Δ-ALLP B	153	0.0139	78	-0.0011	16671	0.0173	0.0150	0.12	-0.0034	0.59
Δ+ALLP A	36	0.1115	20	0.0798	6830	0.1197	0.0317	0.74	-0.0083	0.93
Δ+ALLP B	39	0.0558	23	0.0989	7279	0.1110	-0.0431	0.57	-0.0552	0.48

1.3.2.2 Determinants of Regulatory Office Closures

After showing that no differences exist between my treatment and control group, I next analyze the determinants of regulatory office closures. One concern is that if regulatory office closures were systematically related to banks under the supervision of the closed office performing poorly, comparing these poorly performing banks to a control group of better performing banks could bias my results.

I run a logit regression and show results in Table 1-3 to explore the determinants of office closures. I create several variables in the form of the ROA, Bad Loans and *Z*-Score of the average bank under the supervision of each regulatory office as well as the total bank assets under the supervision of each regulatory office. Refer to Appendix 1-A1 for the definition of these variables. The dependent variable is defined as *Office Closure* (which is equal to 1 if a regulatory office closes in a year and 0 otherwise). I also include regulator (FDIC, Fed or OCC) and year fixed effects. I include regulator fixed effects to control for any potential differences that different regulators might have in their organizational structure. Columns (1)-(2) (all coefficients are in odds ratios) show that the average level or changes in ROA, Bad Loans and Z-Score of banks under the supervision of a regulatory office do not predict its closure. Incidentally, this confirms my finding in Table 1-2 that treated banks are similar to control banks in the pre-shock period and that bank characteristics do not predict office closures.

Columns (3)-(4) show that smaller offices (i.e., offices with fewer total assets under supervision, Column (3)) and offices that experience a reduction in the assets under supervision (Column (4)) are more likely to be closed. A decrease in assets under supervision at a particular regulatory office means that it is less cost efficient to maintain a physical presence in the area due to fixed costs incurred. Jointly, my results suggest that the need to make efficiency gains and to rebalance supervisory resources are a reason for the closure of offices. Importantly, banks under the supervision of a closed office are not underperforming and riskier than other banks.[13]

Table 1-3: Determinants of Office Closures

This table reports estimates of a logistic regression on the determinants of regulatory office closures. I report estimates of the following equation:

$$Office\ Closure_{k,t} = \alpha_{k,t} + Z_{k,t} + Regulator\ FE + Year\ FE + \varepsilon_{k,t}$$

where subscripts k and t indicate regulatory office and year respectively. *Office Closure* = 1 if a regulatory office is closed in a particular year and 0 if otherwise while Z is a vector of control variables. *ROA under Sup.* (defined as Net Income/Total Assets), *Bad Loans under Sup*. (defined as Total Loans and Receivables 90+days late/Total Loans), and *Z-Score under Sup.* (computed as [Log (ROA+Equity) / σROA)] x (-1). ROA is defined as Net Income/Total Assets, Equity as Equity/Total Assets and σROA is the standard deviation of ROA over the past 3 years) are the average ROA, Bad Loans and Z-Score of the banks under the supervision of the regulatory office. *Log Total Assets under Sup*. is defined as the Log (Sum of Total Assets under the regulatory office supervision). *Main Office* = 1 if a regulatory office is a non-satellite field office and *Beside Main Office* = 1 if a regulatory office is the closest office to *Main Office*. Bank variables are winsorized at the 1% and 99% levels. Refer to Appendix 1-A1 for the definition and construction of variables used in this chapter. The coefficient is in odds ratio. The constant is suppressed. Standard errors are clustered at the regulatory office-level. t-statistics are reported in parenthesis. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

	(1) Office Closure	(2) Office Closure	(3) Office Closure	(4) Office Closure
ROA under Sup.$_{t-1}$	40.0277			
	[1.59362]			
ROA under Sup.$_{t-2}$	3.48214			
	[0.34183]			
Bad Loans under Sup.$_{t-1}$	0.58998			
	[-0.37441]			
Bad Loans under Sup.$_{t-2}$	5.60484			
	[1.38817]			
Z-Score under Sup.$_{t-1}$	1.80254			
	[0.34221]			
Z-Score under Sup.$_{t-2}$	0.23967			
	[-0.91853]			
ΔROA under Sup.$_{t-1}$		1.73445		
		[0.29109]		
ΔROA under Sup.$_{t-2}$		0.18477		

[13] One reason that could explain the joint findings of a decrease in banking assets under supervision at a regulatory office predicting closure and banks under the supervision of a closing office not under-performing is as follows. A decrease in banking assets could arise if banks choose to switch charters. This could be either as a result of consolidation or other reasons (e.g., the accessibility of supervisors). I do not distinguish between these reasons. However, more importantly for my analysis, I show that bank performance under a closed office does not predict office closures and that office closures lead to worse bank outcomes.

		[-0.39642]		
ΔBad Loans under Sup.$_{t-1}$		0.52199		
		[-0.47411]		
ΔBad Loans under Sup.$_{t-2}$		2.13191		
		[1.11038]		
ΔZ-Score under Sup.$_{t-1}$		0.44342		
		[-0.38133]		
ΔZ-Score under Sup.$_{t-2}$		0.09901		
		[-0.77603]		
Log Total Assets under Sup.$_{t-1}$			0.39810**	
			[-1.97580]	
Log Total Assets under Sup.$_{t-2}$			1.61231	
			[0.74867]	
ΔLog Total Assets under Sup.$_{t-1}$				0.3920**
				[-2.20363]
ΔLog Total Assets under Sup.$_{t-2}$				1.29185
				[0.41494]
Main Office	0.22505		0.21365	
	[-1.25549]		[-1.20322]	
Beside Main Office	21.258***	6.135**	20.553***	7.421**
	[2.71381]	[2.18653]	[2.68292]	[2.21055]
Year FE	Yes	Yes	Yes	Yes
Regulator FE	Yes	Yes	Yes	Yes
Pseudo R-squared	0.206	0.15	0.173	0.138
Observations	973	415	973	415

1.4 Main Results

1.4.1 Regulatory Office Closures and Bank Risk

I begin my main analysis by investigating the effects of regulatory office closures on bank risk-taking in Table 1-4. As proxies for risk, I employ the *Z-Score* (Columns (1)-(2)), *σROA* (Columns (3)-(4)) and *σROE* (Columns (5)-(6)). The *Z-Score* measures a bank's distance to default as the number of standard deviations by which *ROA* can fall before a bank becomes insolvent. Following Laeven and Levine (2009) and Demirgüç-Kunt and Huizinga (2010), I calculate *Z-Score* as the logarithm of [(*ROA*+*Equity*)/*σROA*] and inverse it. A higher *Z-Score* therefore indicates higher bank risk. *σROA* and *σROE* are the logarithmic values of the standard deviation of *ROA* and *ROE*, respectively. I calculate the risk measures over a 12-quarter (*t-11* to *t*) window.

Table 1-4 shows bank risk increases after regulatory office closures. The coefficient on *Treated* × *Post* is positive and statistically significant for all risk proxies I examine. Further, the economic effects are large. For instance, Column (1) reports that banks affected by the closure of a supervisory office increase their *Z-Score* by about 19% (relative to the mean) compared to banks in the same counties that are not affected by the office closure.[14] This confirms my hypothesis that more effective bank supervision curtails bank risk-taking.[15]

1.4.2 Regulatory Office Closures and Bank Lending

Loans are typically the most important assets that banks issue. I therefore investigate the impact of bank supervision on bank lending, in particular, if regulatory office closures are linked to banks aggressively growing their loan portfolios. To measure the loan origination activities of banks, Table 1-5 examines the effects of supervisory office closures on *Log Total Loans* (Columns (1)-(2)), *Total Loans/TA* (Columns (3)-(4)), *Log Real Estate Loans* (Columns (5)-(6)) and *Real Estate Loans/TA* (Columns (7)-(8)). *Log Total Loans* and *Log Real Estate Loans* are the logarithmic values of total loans and real estate loans, respectively. *Total Loans/TA* and *Real Estate Loans/TA* are total loans and real estate loans scaled by total assets.

[14] This is calculated as (0.88474/4.684). Mean values are obtained from Panel 1B in Table 1-1.

[15] It should be noted that I include regulatory office fixed effects, and therefore, control for any differences in regulatory office closures that affect national banks (supervised by the OCC) and state banks (supervised by the FDIC or Fed, alongside their respective state banking supervisor). In unreported results, I find that OCC office closures lead to larger changes in bank behavior than FDIC and Fed office closures. This is unsurprising as the OCC functions as the primary supervisor for nationally-chartered banks while state-chartered banks are primarily supervised by both the state and the FDIC or Fed. Therefore, the effects of OCC office closures (as compared to FDIC and Fed office closures) on bank behavior is likely to be greater.

Table 1-4: Office Closures and Bank Risk

This table reports estimates of a difference-in-difference regression which estimates the effect of regulatory office closures on bank risk. I report estimates of the following equation:

$$Y_{i,k,t} = \alpha_{i,k,t} + \beta_1 Treated_{i,k,t} + \beta_2 Post_{k,t} + \beta_3 Treated_{i,k,t}\ x\ Post_{k,t} + Z_{i,k,t} + Bank\ FE + Regulatory\ Office\ FE + Year\text{-}Quarter\ FE + \varepsilon_{i,k,t}$$

where subscripts *i*, *k* and *t* indicate bank, regulatory office and year-quarter respectively. *Y* is either the *Z-Score* (Columns (1)-(2)), computed as [Log (ROA+Equity) / σROA)] x (-1). ROA is defined as Net Income/Total Assets, Equity as Equity/Total Assets and σROA is the standard deviation of ROA over the past 3 years)], *σROA* (Columns (3)-(4), computed as Log (σROA)) or *σROE* (Columns (5)-(6), computed as Log (σROE)). ROE is defined as Net Income/Total Equity. σROE is the standard deviation of ROE over the past 3 years. *Treated* is a dummy variable that = 1 if banks are in the treated group (and 0 if in the control group). Treated banks are banks which are affected by regulatory office closures (supervised by the closed office). Control banks are banks which are headquartered in the same counties as treated banks but are not supervised by the closed office. See Section 1.3 for the detailed construction of treatment and control groups. *Post* is a variable that = 1 for the 3 years after the closure of a regulatory office *k* and 0 for the 2 years before (5-year diff-in-diff window). The variable of interest is the coefficient β_3 on the interaction term *Treated x Post* which takes a value of 1 for treated banks in the 3 years after closure of its regulatory office and 0 otherwise. *Z* is a vector of control variables and includes *Audit, Mandatory Audit, ROA, Log Total Assets, Tier-1 Capital, Real Estate Loans, Agri Loans, CI Loans, Indiv Loans, Total Deposits, Total Loans, 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. Refer to Appendix 1-A1 for the definition and construction of variables used in this chapter. The constant is suppressed. Standard errors are clustered at the bank-level. t-statistics are reported in parenthesis. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively

	(1) Z-Score	(2) Z-Score	(3) σROA	(4) σROA	(5) σROE	(6) σROE
Treated x Post	0.88474***	1.04435***	0.00084**	0.00101***	0.00497*	0.00799**
	[6.38229]	[6.67965]	[2.35018]	[2.89023]	[1.71593]	[2.47198]
Post	-0.0294	-0.02156	-0.00002	0	-0.00009	0.00021
	[-0.2601]	[-0.1925]	[-0.1245]	[0.0008]	[-0.036]	[0.0926]
Audit		-0.05814		-0.00006		-0.00063
		[-1.02380]		[-0.65997]		[-0.61139]
Mandatory Audit		0.09483		0.00029***		0.00282**
		[0.95962]		[2.63896]		[2.10404]
ROA		-26.896***		-0.057***		-0.589***
		[-5.76037]		[-5.66165]		[-4.91650]
Log Total Assets		-0.15349		-0.00035		-0.00222
		[-0.99902]		[-1.38763]		[-0.89001]
Tier-1 Capital		-1.12054		-0.00039		-0.02348
		[-1.33262]		[-0.20789]		[-1.06360]
Real Estate Loans		-0.27728		-0.00032		0.00037
		[-0.56303]		[-0.38219]		[0.04641]
Agri Loans		-0.95622		-0.00092		-0.00827
		[-1.53541]		[-0.75568]		[-0.67386]
CI Loans		0.35934		0.0008		0.00808
		[0.67018]		[0.77098]		[0.93148]
Indiv Loans		-0.32819		-0.00146		-0.01012
		[-0.54058]		[-1.38961]		[-0.93199]
Total Deposits		0.48689		0.00013		0.01813
		[0.91121]		[0.13615]		[1.50221]
Total Loans		-0.35245		-0.00016		-0.00358
		[-1.20806]		[-0.29218]		[-0.56544]
BHC		-0.07195		0.00026		0.00177
		[-0.47778]		[1.29759]		[1.04567]
County Income per Cap		-0.0572		-0.00044		-0.00445

		[-0.11483]		[-0.63222]		[-0.55326]
County Pop		-0.94134		-0.0002		-0.01068
		[-0.96715]		[-0.13885]		[-0.64810]
County HHI		0.00003		0		0
		[0.74628]		[1.43353]		[1.48960]
County Pop Density		-42.46308		-0.22695		-23.74901
		[-0.01351]		[-0.04132]		[-0.34164]
∆State UR		0.16591		0.00016		0.00134
		[1.07216]		[0.64933]		[0.46063]
∆State HPI		-1.46702		-0.00224		-0.03505
		[-0.87243]		[-0.72197]		[-0.90527]
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Reg Office FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-squared	0.108	0.135	0.0935	0.128	0.0928	0.131
Observations	7,254	7,254	7,274	7,274	7,274	7,274

Table 1-5 reports the estimation results. The coefficient on *Treated* × *Post* is positive and statistically significant in all eight columns. This indicates that banks whose responsible supervisory office has closed grow their loan portfolios more aggressively relative to the control group. The economic impact of this effect is non-trivial. Relative to the control group, treated banks increase their total loans by approximately 11% (in Column (1)) and their real estate loans by 16% (Column (5)). Further, the proportion of loans to total assets also increases, indicating that my findings are not simply driven by substitution effects when banks grow their non-lending assets more aggressively than their lending assets. The ratios of total loans to assets (Column (3)) and real estate loans to total assets (Column (7)) also increase by approximately 6% and 7%, respectively.

Table 1-5: Office Closures and Bank Lending

This table reports estimates of a difference-in-difference regression which estimates the effect of regulatory office closures on bank lending. I report estimates of the following equation:

$$Y_{i,k,t} = \alpha_{i,k,t} + \beta_1 Treated_{i,k,t} + \beta_2 Post_{k,t} + \beta_3 Treated_{i,k,t} \text{ x } Post_{k,t} + Z_{i,k,t} + \textit{Bank FE} + \textit{Regulatory Office FE} + \textit{Year-Quarter FE} + \varepsilon_{i,k,t}$$

where subscripts *i*, *k* and *t* indicate bank, regulatory office and year-quarter respectively. *Y* is either *Log Total Loans* (Columns (1)-(2)), *Total Loans/Total Assets* (Columns (3)-(4)), *Log Real Estate Loans* (Columns (5)-(6)) or *Real Estate Loans/Total Assets* (Columns (7)-(8)). *Treated* is a dummy variable that = 1 if banks are in the treated group (and 0 if in the control group). Treated banks are banks which are affected by regulatory office closures (supervised by the closed office). Control banks are banks which are headquartered in the same counties as treated banks but are not supervised by the closed office. See Section 1.3 for the detailed construction of treatment and control groups. *Post* is a variable that = 1 for the 3 years after the closure of a regulatory office *k* and 0 for the 2 years before (5-year diff-in-diff window). The variable of interest is the coefficient β_3 on the interaction term *Treated x Post* which takes the value of 1 for treated banks the 3 years after closure of its regulatory office and 0 otherwise. *Z* is a vector of control variables and includes *Audit*, *Mandatory Audit*, *ROA*, *Log Total Assets*, *Tier-1 Capital*, *Total Deposits*, *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. Refer to Appendix 1-A1 for the definition and construction of variables used in this chapter. The constant is suppressed. Standard errors are clustered at the bank-level. t-statistics are reported in parenthesis. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

	(1) Log Total Loans	(2) Log Total Loans	(3) Total Loans/ TA	(4) Total Loans/ TA	(5) Log Real Estate Loans	(6) Log Real Estate Loans	(7) Real Estate Loans/TA	(8) Real Estate Loans/TA
Treated x Post	0.11091*	0.08526***	0.06375***	0.06269***	0.16294*	0.13729***	0.07694***	0.07345***
	[1.65728]	[2.79060]	[3.47526]	[3.87428]	[1.69674]	[2.69768]	[3.86230]	[3.97454]
Post	0.00077	0.02138	0.00411	0.00889	-0.00436	0.02157	0.00931	0.01377
	[0.02073]	[1.19446]	[0.33735]	[0.90684]	[-0.10336]	[0.72873]	[0.72732]	[1.13819]
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Reg Office FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other Controls	No	Yes	No	Yes	No	Yes	No	Yes
Adj. R-squared	0.26	0.883	0.1	0.449	0.251	0.781	0.144	0.402
Observations	8,321	8,321	8,321	8,321	8,321	8,321	8,321	8,321

1.5 Are Regulatory Office Closures Linked to Negative Bank Outcomes

My finding that a reduction in regulatory attention is linked to riskier banks that expand their loan portfolios is not sufficient to argue that regulatory office closures produce negative outcomes. By contrast, it could be argued that supervisors may have been unduly strict in the period preceding the closure and may have constrained lending and other risky bank activities. If so, the increase in risk and lending that I observe in the period following an office closure could result from easing credit constraints that may well benefit borrowers without producing negative bank outcomes. This section examines changes in risk-adjusted returns and the performance of banks during the 2007-09 financial crisis to conclude that less regulatory attention is indeed linked to negative bank outcomes.

1.5.1 Evidence from Risk-adjusted Performance

Table 1-6 shows that banks affected by regulatory office closures increase their performance after office closures (as measured by *ROA* in Columns (1)-(2) and *ROE* in Columns (3)-(4)). Jointly interpreted with analysis in the previous section, this indicates that affected banks make business policy choices to increase profitability by increasing risk.

However, when examining risk-adjusted returns (measured using the logarithmic values of *ROA/σROA* (Columns (5)-(6)) and *ROE/σROE* (Columns (7)-(8)) in Columns (5)-(8) of Table 1-6, it is clear that that office closures are linked to negative outcomes. The coefficient on *Treated* × *Post* is negative and statistically significant at the 1% level.

Table 1-6: Regulatory Office Closures and Risk-adjusted Bank Performance

This table reports estimates of a difference-in-difference regression which estimates the effect of regulatory office closures on risk-adjusted performance. I report estimates of the following equation:

$$Y_{i,k,t} = \alpha_{i,k,t} + \beta_1 Treated_{i,k,t} + \beta_2 Post_{k,t} + \beta_3 Treated_{i,k,t}\, x\, Post_{k,t} + Z_{i,k,t} + Bank\ FE + Regulatory\ Office\ FE + Year\text{-}Quarter\ FE + \varepsilon_{i,k,t}$$

where subscripts *i*, *k* and *t* indicate bank, regulatory office and year-quarter respectively. *Y* is either *ROA* (Columns (1)-(2), ROA is defined as Net Income/Total Assets), *ROE* (Columns (3)-(4), ROE is defined as Net Income/Total Equity), (ROA/σROA (Columns (5)-(6), ROA is defined as Net Income/Total Assets and σROA is the standard deviation of ROA over the past 3 years) or ROE/σROE (Columns (7)-(8), ROE is defined as Net Income/Total Equity and σROE is the standard deviation of ROE over the past 3 years). *Treated* is a dummy variable that = 1 if banks are in the treated group (and 0 if in the control group). Treated banks are banks which are affected by regulatory office closures (supervised by the closed office). Control banks are banks which are headquartered in the same counties as treated banks but are not supervised by the closed office. See Section 1.3 for the detailed construction of treatment and control groups. *Post* is a variable that = 1 for the 3 years after the closure of a regulatory office *k* and 0 for the 2 years before (5-year diff-in-diff window). The variable of interest is the coefficient β_3 on the interaction term *Treated x Post* which takes the value of 1 for treated banks the 3 years after closure of its regulatory office and 0 otherwise. *Z* is a vector of control variables and includes *Audit, Mandatory Audit, Log Total Assets, Tier-1 Capital, Real Estate Loans, Agri Loans, CI Loans, Indiv Loans, Total Deposits, Total Loans, 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. Refer to Appendix 1-A1 for the definition and construction of variables used in this chapter. The constant is suppressed. Standard errors are clustered at the bank-level. t-statistics are reported in parenthesis. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

	(1) ROA	(2) ROA	(3) ROE	(4) ROE	(5) ROA/σROA	(6) ROA/σROA	(7) ROE/σROE	(8) ROE/σROE
Treated x Post	0.00258***	0.00154***	0.02802***	0.01782***	-0.70590***	-0.88987***	-0.94181***	-1.04770***
	[10.79505]	[4.49616]	[11.76359]	[5.16875]	[-3.81854]	[-4.61868]	[-3.87038]	[-4.06278]
Post	-0.00013	-0.00006	-0.00045	0.00021	0.01326	0.01968	0.11114	0.09308
	[-0.46013]	[-0.20023]	[-0.15043]	[0.06593]	[0.13935]	[0.21093]	[0.73214]	[0.62419]
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Reg Office FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other Controls	No	Yes	No	Yes	No	Yes	No	Yes
Adj. R-squared	0.0639	0.0773	0.057	0.0703	0.0983	0.134	0.0901	0.12
Observations	7,254	7,254	7,254	7,254	7,250	7,250	6,957	6,957

Consequently, following a supervisory office closure, the risk-adjusted performance of banks deteriorates compared with a control sample of local banks. Less regulatory attention means that banks become less profitable per unit of risk. Consequently, a negative shock to the efficacy of bank supervision is associated with negative bank outcomes on average.

1.5.2 Evidence from the 2007-09 Financial Crisis

The 2007 to 2009 financial crisis presents a significant shock to the asset quality of banks. Since this shock is plausibly exogenous to individual banks and office closures, contrasting bank outcomes during the crisis by whether banks were previously exposed to supervisory office closures presents me with a second test of whether regulatory office closures lead to negative outcomes for banks. Put succinctly, if more supervisory attention was beneficial for banks, I should observe that banks affected by regulatory office closures prior to the crisis should perform better in the crisis.

I restrict my analysis to the 6 regulatory office closures that occurred before the crisis (between 2003 and 2006) and estimate the following difference-in-difference specification for the years up to 2009:

$$\begin{aligned} Y_{i,k,t} = {} & \alpha_{i,k,t} + \beta_1 \mathit{Treated\ Crisis}_{i,k,t} + \beta_2 \mathit{Crisis}_t + \beta_3 \mathit{Treated\ Crisis}_{i,k,t} \times \mathit{Crisis}_t + Z_{i,k,t} \\ & + \mathit{Bank\ FE} + \mathit{Regulatory\ Office\ FE} + \mathit{Year\text{-}Quarter\ FE} + \varepsilon_{i,k,t} \qquad (1\text{-}3) \end{aligned}$$

Variable definitions are as before with the exception of *Treated Crisis* which is a dummy variable that is 1 if banks are affected by one of the six regulatory office closures that occurred *before* 2007 (and 0 otherwise). As previously, control banks include banks in the same counties as treated banks that are not affected by regulatory office closures. *Crisis* is 1 for the years 2007 to 2009 (and 0 otherwise).

The variable of interest is the coefficient β_3 on the interaction term *Treated Crisis* × *Crisis*. The interaction term takes a value of 1 for treated banks in years 2007 to 2009 (and 0 otherwise). Consequently, the set-up compares the crisis performance of treated banks (that were affected by regulatory office closures *prior* to the crisis) to themselves in the pre-crisis period and to a set of control banks.

Table 1-7 employs several variables to investigate bank performance during the crisis. Specifically, I examine *Bad Loans* (defined as loans and receivables which are 90+ days late scaled by total loans in Columns (1)-(2)), *Loan Charge-offs* (bad loans that are charged off scaled by total loans in Columns (3)-(4)), and bank failures during the financial crisis (Columns (5)-(8)). As regards the latter, I construct *Fail07-10* and *Fail07-09* (defined as a dummy variable that equals 1 for years 2007-10 (2007-09) for banks that failed during 2007-10 (2007-09) and 0 otherwise). I focus on bank failures up to 2010 because not all bank closures and resolutions occurred at the outset of the crisis (Ng and Roychowdhury, 2014).[16]

I find that the coefficient on the interaction term *Treated Crisis* × *Crisis* is positive and statistically significant in all columns. This suggests that banks which are affected by regulatory office closures prior to the crisis display worse performing loan portfolios and were more likely to fail during the crisis relative to the group of control banks. The economic impact is meaningful. Relative to the sample mean, banks

[16] In my analysis, no banks in the treatment and control group failed before the crisis (years 2002-2006). Thus, I focus on failures during the 2007 to 2010 crisis period. Out of the 351-unique treated and control banks, I observe 11 bank failures from 2007 to 2009 and 15 bank failures from 2007 to 2010.

affected by a regulatory office closure prior to the crisis have 40% more bad loans (Column (1)), 50% more loan charge-offs (Column (3)) and are 6% (Column (5)) more likely to fail.

Using this test, I am also able to estimate one specific cost —cost of bank failures— to the FDIC that arises due to regulatory office closures. In Table 1-7 (Columns (5)-(6)), I find that 15 banks failed from years 2007 to 2010. The failure of these 15 banks generated losses of approximately $1.57 billion to the FDIC. Given that office closures in the pre-crisis period increases the probability of bank failure in the crisis by 6%, this implies losses of $15.7 million for a single regulatory office closure.[17]

It should be noted that this estimate is likely to form a lower bound of *total costs* associated with regulatory office closures to the taxpayer for at least three reasons. First, these losses *only* include bank failure costs. Given that banks receive other forms of assistance during the crisis (e.g., discount window lending, increased insurance coverage limits and explicit bailout funds), the total cost to taxpayers is likely to be higher (Berger, Black, Bouwman, and Dlugosz, 2015; Duchin and Sosyura, 2012; 2014; Lambert, Noth, and Schüwer, 2016). Second, regulators are likely to increase forbearance in the crisis and delay the resolution of weak banks (Brown and Dinç, 2011). Therefore, the bank failure cost I estimate is likely to be underestimated. Lastly, in my analysis, I limit my sample to only small banks (< $1 billion in assets) which excludes losses from larger banks.

[17] Expected losses arising from bank failures are obtained from the FDIC failed bank list. I calculate $15.7 million as ($1.57 billion x 6%)/6. I divide by 6 because there were 6 regulatory office closures in the pre-crisis period in my sample.

Taken together, the results in this section do not support the view that supervisors are unduly strict or that the riskier portfolio choices that banks make following a reduction in supervisory attention are without negative consequences for banks. By contrast, I show that less supervisory attention is associated with a number of negative outcomes, including lower risk-adjusted performance, more non-performing loans and a higher probability of failure during the crisis.

1.6 Information Asymmetry and Supervisory Effectiveness

In this section, I provide evidence on the role of information asymmetry as one mechanism that impedes supervisory effectiveness. I offer two tests that show evidence of increased information frictions between banks and supervisors. First, I show evidence from the loan loss provisioning practices of banks. Specifically, I examine if supervisors are less able to accurately evaluate the expected losses on a bank's portfolio following a supervisory office closure. Second, I use variation in the increase in the physical distance between banks and their newly assigned supervisory office as a measure of information asymmetry between banks and supervisors.

1.6.1 Evidence from Loan Loss Provisioning Practices

In the process of lending to firms, banks acquire firm-specific soft information on borrowers that is largely unavailable to outside parties. Since loans are notoriously opaque and difficult to value for supervisors and other outsiders, they afford banks some discretion over how to provision for expected losses on their loan portfolios (see Beatty and Liao (2014) for a survey). This presents me with an indirect test of the effects of information asymmetry on supervisory effectiveness.

Table 1-7: Regulatory Office Closures and the Financial Crisis

This table reports estimates of a difference-in-difference regression which estimates the effect of regulatory office closures on loan performance and bank failure rates during the 2007-2009 financial crisis. I report estimates of the following equation:

$$Y_{i,k,t} = \alpha_{i,k,t} + \beta_1 Treated\ Crisis_{i,k,t} + \beta_2 Crisis_t + \beta_3 Treated\ Crisis_{i,k,t}\ x\ Crisis_t + Z_{i,k,t} + Bank\ FE + Regulatory\ Office\ FE + Year\text{-}Quarter\ FE + \varepsilon_{i,k,t}$$

where subscripts *i*, *k* and *t* indicate bank, regulatory office and year-quarter respectively. *Y* is either *Bad Loans* (Columns (1)-(2), Bad Loans is defined as Total Loans and Receivables 90+days late/Total Loans), *Loan Charge-Offs* (Columns (3)-(4), Loan Charge-Offs is defined as Total Loan Charge-Offs/Total Loans), *Fail07-10* (Columns (5)-(6)), *Fail07-10* is a dummy variable that = 1 for years 07 to 09 if a bank failed in years 07 to 10) or *Fail07-09* (Columns (7)-(8), Fail07-09 is a dummy variable that = 1 for years 07 to 09 if a bank failed in years 07 to 09). *Treated Crisis* is a dummy variable that = 1 if banks are in the treated group (and 0 if in the control group). Treated banks are banks which are affected by regulatory office closures in the pre-crisis period (supervised by the closed office). Control banks are banks which are headquartered in the same counties as Treated Crisis banks but are not supervised by the closed office. See Section 1.5 for the detailed construction of treatment crisis and control groups. Only Treated Crisis (and corresponding control) banks which experience regulatory office closures before the financial crisis are included in this analysis to investigate the impact of office closures on performance during the crisis. The variable of interest is the coefficient β_3 on the interaction term *Treated Crisis x Crisis* which takes the value of 1 for treated banks during the financial crisis (years 2007 to 2009) and 0 otherwise. *Z* is a vector of control variables and includes *Audit, Mandatory Audit, Log Total Assets, Tier-1 Capital, Real Estate Loans, Agri Loans, CI Loans, Indiv Loans, Total Deposits, Total Loans, BHC, County Income per Cap, County Pop, County HHI, County Pop Density,* Δ*State UR* and Δ*State HPI* for Columns (1)-(4) and *Audit, Mandatory Audit, ROA, Log Total Assets, Tier-1 Capital, Real Estate Loans, Agri Loans, CI Loans, Indiv Loans, Total Deposits, Total Loans, BHC, County Income per Cap, County Pop, County HHI, County Pop Density,* Δ*State UR* and Δ*State HPI* for Columns (5)-(8). Bank balance sheet variables are winsorized at the 1% and 99% levels. Refer to Appendix 1-A1 for the definition and construction of variables used in this chapter. The constant is suppressed. Standard errors are clustered at the bank-level. t-statistics are reported in parenthesis. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

	(1) Bad Loans	(2) Bad Loans	(3) Loan Charge-Offs	(4) Loan Charge-Offs	(5) Fail07-10	(6) Fail07-10	(7) Fail07-09	(8) Fail07-09
Treated Crisis x Crisis	0.00397*	0.00383**	0.00053**	0.00052**	0.05746***	0.05306***	0.03057*	0.03063*
	[1.80299]	[1.98642]	[2.22975]	[2.47512]	[2.61043]	[2.76225]	[1.70093]	[1.86407]
Crisis	0.01250***	0.01226**	0.00275***	0.00363***	-0.00631	-0.11455**	-0.00774	-0.08501***
	[4.12184]	[2.11628]	[7.06763]	[5.52382]	[-1.07260]	[-2.33267]	[-1.28878]	[-2.90283]
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Reg Office FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other Controls	No	Yes	No	Yes	No	Yes	No	Yes
Adj. R-squared	0.151	0.23	0.101	0.144	0.0784	0.193	0.0451	0.165
Observations	9,055	9,055	9,055	9,055	9,055	9,055	9,055	9,055

Loan loss provisions (LLPs) are accrued expenses that reflect expected *future* losses on a bank's loan portfolio. When banks delay recognition of expected losses by under-provisioning for loan losses, loan losses above previously made provisions have to be absorbed by bank capital. This can raise solvency concerns if capital cushions are insufficient to cover losses during economic downturns. For instance, Beatty and Liao (2011) find that delays in provisioning (a form of under-provisioning) lead to decreases in bank lending in recession times while Bushman and Williams (2015) show that delays in provisioning for expected losses are associated with higher bank risk during economic downturns.

As banks have some discretion over how to expense for expected bad loans, supervisors charged with maintaining the safety and soundness of the system will require banks to hold levels of LLPs that are commensurate with the expected losses arising from their loan portfolio. However, effective enforcement of LLP rules depends on the information set that is available to regulators. Consistent with this, Costello et al. (2016) show supervisors that perform well on several dimensions enforce more bank income-reducing restatements as well as higher levels of LLPs. I expect that, after regulatory office closures, the cost of collecting and verifying soft information increases for examiners. I then use this intuition to infer that information asymmetry issues following regulatory office closures will negatively impact the ability of supervisors to enforce appropriate loan loss provisioning practices of banks.

I conduct two complementary, but related tests to examine the extent to which banks affected by regulatory office closures are able to exploit the heightened information asymmetry issues between them and supervisors to: (i) make lower and less timely loan loss provisions; and (ii) increase their discretionary use of loan loss provisions to under-provision for expected loan losses.

1.6.1.1 Magnitude and Timeliness of Loan Loss Provisions

If heightened information asymmetry issues following regulatory office closures decrease a supervisor's ability to enforce bank loan loss provisions that are commensurate with bad loans, I expect less timely provisioning for banks that are affected by these closures. That is, I expect to see a decline in how sensitive *current* LLPs are to the actual bad loans that materialize in the *future* amongst affected banks.

To analyze whether information asymmetry affects the provisioning behavior of banks, we estimate the following model in the spirit of Nichols, Wahlen, and Wieland (2009) and Kanagaretnam, Lim, and Lobo (2014):

$$LLP_{i,k,t} = \alpha_{i,k,t} + \beta_1 Treated_{i,k,t} + \beta_2 Post_{k,t} + \beta_3 Treated_{i,k,t} \times Post_{k,t} + \beta_4 Treated_{i,k,t} \times Post_{k,t} \times \Delta Bad\ Loans_{i,k,t+1} + \beta_5 Treated_{i,k,t} \times Post_{k,t} \times \Delta Bad\ Loans_{i,k,t} + \beta_6 Treated_{i,k,t} \times Post_{k,t} \times \Delta Bad\ Loans_{i,k,t-1} + Z_{i,k,t} + Bank\ FE + Regulatory\ Office\ FE + Year\text{-}Quarter\ FE + \varepsilon_{i,k,t} \qquad (1\text{-}4)$$

where *i*, *k* and *t* indicate bank *i*, regulatory office *k* and year-quarter *t* respectively while *Z* is a vector of control variables. *Treated* and *Post* are as previously defined in Section 1.3.2.1. *LLP* is defined as Loan Loss Provisions scaled by Total Loans. *Bad Loans* is constructed as Total Loans and Receivables 90+ days late scaled by Total Loans.

The variables of interest are the coefficients on β_4, β_5 *and* β_6. These interaction terms measure the extent to which regulatory office closures affect the sensitivity between bad loans (past, current and future) and current LLPs. I first estimate Equation

(1-4) with only *Treated* × *Post* in Columns (1)-(2) and then include the full Equation in Columns (3)-(4) of Table 1-8.

In Columns (1)-(2) of Table 1-8, the coefficient on *Treated* × *Post* is negative and statistically significant. This shows that banks that were affected by a regulatory office closures decrease their LLPs compared to the group of control banks. Further, provisioning practices become less timely for banks affected by office closures. This is demonstrated by a negative coefficient on *Treated* × *Post* × Δ*Bad Loans*$_{t+1}$ (statistically significant at the 1% level in Columns (3)-(4)). This is consistent with the argument that information asymmetry impedes effective supervision. Following office closures, the newly assigned supervisory office is less effective when enforcing levels of LLPs that are appropriate for future bad loans.

1.6.1.2 Abnormal Discretionary Loan Loss Provisions

After demonstrating that LLPs become less sensitive to future loan losses following the closure of a supervisory office, I next show results from banks' use of discretionary provisions to under-provision for bad loans. While the earlier test show that the timeliness of provisioning for bad loans decreases for banks that are affected by office closures, they do not demonstrate that affected banks increase their use of the *discretionary* component of LLPs to *under*-provision for expected loan losses.

Table 1-8: Office Closures and Loan Loss Provisions

This table reports estimates of a difference-in-difference regression which estimates the effect of regulatory office closures on loan loss provisioning practices. I report estimates of the following equation in Columns (3)-(4):

$$Y_{i,k,t} = \alpha_{i,k,t} + \beta_1 Treated_{i,k,t} + \beta_2 Post_{k,t} + \beta_3 Treated_{i,k,t}\ x\ Post_{k,t} + \beta_4 Treated_{i,k,t}\ x\ Post_{k,t}\ x\ \Delta Bad\ Loans_{i,k,t+1} + \beta_5 Treated_{i,k,t}\ x\ Post_{k,t}\ x\ \Delta Bad\ Loans_{i,k,t} + \beta_6 Treated_{i,k,t}\ x\ Post_{k,t}\ x\ \Delta Bad\ Loans_{i,k,t-1} + Z_{i,k,t} + Bank\ FE + Regulatory\ Office\ FE + Year\text{-}Quarter\ FE + \varepsilon_{i,k,t}$$

where subscripts *i, k* and *t* indicate bank, regulatory office and year-quarter respectively. *Y* is *LLP* (Columns (1)-(4), LLP is defined as Loan Loss Provisions/Total Loans). *Treated* is a dummy variable that = 1 if banks are in the treated group (and 0 if in the control group). Treated banks are banks which are affected by regulatory office closures (supervised by the closed office). Control banks are banks which are headquartered in the same counties as treated banks but are not supervised by the closed office. See Section 1.3 for the detailed construction of treatment and control groups. *Post* is a variable that = 1 for the 3 years after the closure of a regulatory office *k* and 0 for the 2 years before (5-year diff-in-diff window). The variable of interest for Columns (1)-(2) is the coefficient β_3 of the interaction term *Treated x Post* which takes the value of 1 for treated banks the 3 years after closure of its regulatory office and 0 otherwise. The variable of interest for Columns (3)-(4) is the coefficient on the three triple interaction terms β_4, β_5 and β_6. *Z* is a vector of control variables and includes *EBLLP*, *Log Total Assets*, *LLA*, *Audit*, *Mandatory Audit*, *Real Estate Loans*, *Agri Loans*, *CI Loans*, *Indiv Loans*, *Total Deposits*, *Total Loans*, *Loan Growth*, *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. Refer to Appendix 1-A1 for the definition and construction of variables used in this chapter. The constant is suppressed. Standard errors are clustered at the bank-level. t-statistics are reported in parenthesis. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

	(1) LLP	(2) LLP	(3) LLP	(4) LLP
Treated x Post	-0.00079***	-0.00075**	-0.00061***	-0.00070*
	[-4.35262]	[-1.97893]	[-3.76394]	[-1.83867]
Treated x Post x ΔBad $Loans_{t+1}$			-0.04141***	-0.04758***
			[-2.69609]	[-3.15270]
Treated x Post x ΔBad $Loans_t$			-0.00778	-0.0083
			[-0.18322]	[-0.19332]
Treated x Post x ΔBad $Loans_{t-1}$			0.00602	0.0063
			[0.28855]	[0.28283]
Bank FE	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes
Reg Office FE	Yes	Yes	Yes	Yes
Other Controls	No	Yes	No	Yes
Adj. R-squared	0.058	0.128	0.0744	0.139
Observations	8,321	7,214	7,214	7,214

I use a two-step approach following the literature (e.g., Kanagaretnam et al., 2010; Beatty and Liao, 2014; Jiang et al., 2016). In the first-step, I estimate the discretionary component in LLPs as the residuals of a regression that predicts loan defaults. Negative (positive) residuals indicate that banks understate (overstate) their LLPs relative to a level of LLPs that is commensurate with expected loan losses. I then use the residuals as dependent variables in the second-step to investigate if regulatory office closures affect the discretionary uses of LLPs. The advantage of this approach is that I am able to separate the component of LLPs that are nondiscretionary and observe if supervisors are less able to enforce the under-provisioning of expected loan losses for affected banks.

Interpreting residual LLPs as proxies for discretionary uses of 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.[18]

I follow their choice of the two best performing models and estimate Models A and B as:

$$\textit{Model A: } LLP_{i,j,t} = \alpha_{i,j,t} + \Delta NPA_{i,j,t+t} + \Delta NPA_{i,j,t} + \Delta NPA_{i,j,t-1} + \Delta NPA_{i,j,t-2} + \textit{Log Total Assets}_{i,j,t-1} + \Delta \textit{Loans}_{i,j,t} + \Delta \textit{State GDP}_{j,t} + \Delta \textit{State HPI}_{j,t} + \Delta \textit{State UR}_{j,t} + \textit{State FE} + \textit{Year-Quarter FE} + \varepsilon_{i,j,t} \qquad (1\text{-}5)$$

[18] While I acknowledge that using residuals as proxies for discretionary loan loss provisioning behavior relies heavily on the model assumptions and variables included in the first-step, I follow the “best” models specified in Beatty and Liao (2014) to minimize the issues of “non-standard arbitrary” models. Specifically, Beatty and Liao (2014) review 9 different models used in the banking literature to identify discretionary loan loss provisioning behavior. They conduct factor analysis on these 9 models to understand the importance of the different underlying factors. Based on the results of their factor analysis, the authors specify “best” models and test the validly of these models in predicting SEC restatements and comment letters.

$$Model\ B: LLP_{i,j,t} = \alpha_{i,j,t} + \Delta NPA_{i,j,t+t} + \Delta NPA_{i,j,t} + \Delta NPA_{i,j,t-1} + \Delta NPA_{i,j,t-2} + LLA_{i,j,t-1} + Log\ Total\ Assets_{i,j,t-1} + \Delta Loans_{i,j,t} + \Delta State\ GDP_{j,t} + \Delta State\ HPI_{j,t} + \Delta State\ UR_{j,t} + State\ FE + Year\text{-}Quarter\ FE + \varepsilon_{i,j,t} \quad (1\text{-}6)$$

where *i, j* and *t* indicate bank *i,* state *j* and year-quarter *t* respectively. I also include state and year-quarter dummies. Appendix 1-A1 lists the variable definitions. Standard errors are clustered at the bank-level. Results of Equations (1-5) and (1-6) are reported in Appendix 1-A2. *+ALLP A* (*-ALLP A*) are the residuals from Equation (1-5) if $\varepsilon_{i,k,t} > 0$ ($\varepsilon_{i,k,t} < 0$). I also calculate the *absolute* value of the residuals (|*ALLP A*|) from Equation (1-5). An increase in the absolute value 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. For robustness, I also use the residuals from Equation (1-6) in my analysis.

I show the results in Table 1-9. When examining the results for *-ALLP*, the coefficients on the interaction term *Treated* × *Post* in Columns (1)-(4) is negative and statistically significant at the 1% level. Following regulatory office closures, banks that were affected by closures increase their understating of LLPs.[19] I observe no effect in Columns (5)-(8) in terms of banks overstating their provisions. Finally, in Columns (9)-(12), which examine the absolute value of residuals, the coefficient on *Treated* × *Post* is mostly 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 practices.

[19] Note that *-ALLP A* and *-ALLP B* have negative values while *+ALLP A* and *+ALLP B* have positive values. Thus, a negative coefficient on *Treated x Post* in Columns (1)-(4) means that understating uses of LLPs increases after regulatory office closures by affected banks.

Table 1-9: Office Closures and Abnormal Loan Loss Provisions

This table reports estimates of a difference-in-difference regression which estimates the effect of regulatory office closures on earnings management via loan loss provisioning practices. I report estimates of the following equation:

$$Y_{i,k,t} = \alpha_{i,k,t} + \beta_1 Treated_{i,k,t} + \beta_2 Post_{k,t} + \beta_3 Treated_{i,k,t}\, x\, Post_{k,t} + Z_{i,k,t} + Bank\ FE + Regulatory\ Office\ FE + Year\text{-}Quarter\ FE + \varepsilon_{i,k,t}$$

where subscripts *i*, *k* and *t* indicate bank, regulatory office and year-quarter respectively. *Y* is either *-ALLP A* (Columns (1)-(2), -ALLP A is the negative residuals of Equation (1-5), the discretionary component of LLPs), *-ALLP B* (Columns (3)-(4), -ALLP B is the negative residuals of Equation (1-6), the discretionary component of LLPs), *+ALLP A* (Columns (5)-(6), +ALLP A is the positive residuals of Equation (1-5), the discretionary component of LLPs), *+ALLP B* (Columns (7)-(8), +ALLP B is the positive residuals of Equation (1-6), the discretionary component of LLPs), *|ALLP A|* (Columns (9)-(10), |ALLP A| is the absolute value of residuals of Equation (1-5), the discretionary component of LLPs) and *|ALLP B|* (Columns (11)-(12), |ALLP B| is the absolute value of residuals of Equation (1-6), the discretionary component of LLPs). *Treated* is a dummy variable that = 1 if banks are in the treated group (and 0 if in the control group). Treated banks are banks which are affected by regulatory office closures (supervised by the closed office). Control banks are banks which are headquartered in the same counties as treated banks but are not supervised by the closed office. See Section 1.3 for the detailed construction of treatment and control groups. *Post* is a variable that = 1 for the 3 years after the closure of a regulatory office *k* and 0 for the 2 years before (5-year diff-in-diff window). The variable of interest is the coefficient β_3 on the interaction term *Treated x Post* which takes the value of 1 for treated banks the 3 years after closure of its regulatory office and 0 otherwise. *Z* is a vector of control variables and includes *Audit, Mandatory Audit, Lag LLP, EBLLP, Log Total Assets, Tier-1 Capital, Real Estate Loans, Agri Loans, CI Loans, Indiv Loans, Total Deposits, Total Loans, 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. Refer to Appendix 1-A1 for the definition and construction of variables used in this chapter. The constant is suppressed. Standard errors are clustered at the bank-level. t-statistics are reported in parenthesis. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	-ALLP A	-ALLP A	-ALLP B	-ALLP B	+ALLP A	+ALLP A	+ALLP B	+ALLP B	|ALLP A|	|ALLP A|	|ALLP B|	|ALLP B|
Treated x Post	-0.0336***	-0.0453***	-0.0470***	-0.0684***	0.2681	0.371	-0.0489	-0.0754	0.0320**	0.0715***	0.0283	0.0659**
	[-4.161]	[-4.891]	[-3.051]	[-4.280]	[0.837]	[1.187]	[-0.507]	[-0.644]	[2.117]	[2.735]	[1.4680]	[2.375]
Post	-0.0274*	-0.021	-0.0126	-0.0088	-0.007	0.0179	0.0379	0.045	0.000	0.002	-0.003	-0.000
	[-1.79]	[-1.62]	[-0.772]	[-0.57756]	[-0.040]	[0.116]	[0.19516]	[0.25998]	[0.008]	[0.08463]	[-0.10787]	[-0.031]
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Reg Office FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Adj. R-squared	0.502	0.531	0.426	0.457	0.0819	0.109	0.081	0.101	0.103	0.118	0.0946	0.108
Observations	5,042	5,041	4,868	4,867	1,486	1,486	1,660	1,660	6,528	6,527	6,528	6,527

Overall, the evidence presented in this section shows that, following supervisory office closures, banks engage more in their use of discretionary LLPs and do so in way that under-provisions for expected loan losses. I interpret this finding as evidence that regulatory office closures increase information asymmetry issues in supervision and facilitate opportunistic provisioning behavior by banks.

1.6.2 Evidence from Changes in Distance

Physical distance is commonly used as a proxy for information asymmetry between economic agents. That is because increases in physical distance increase the cost of collecting and verifying the type of soft information that facilitates monitoring and enforcement (Kedia and Rajgopal, 2011; Wilson and Veuger, 2016).

Upon closure of a regulatory office, the supervision of affected banks is typically transferred to the next closest regulatory office. Therefore, regulatory office closures generate an increase in the physical distance between a treated bank and its supervising regulatory office. I exploit variation in the increase in distance for banks which are affected by regulatory office closures and hypothesize that for larger increases in physical distance, information asymmetry issues between banks and supervisors should become more pronounced. The latter should result in larger increases in bank risk for banks with larger increase to the supervisory office taking over the supervision. I estimate the following equation:

$$Y_{i,k,t} = \alpha_{i,k,t} + \beta_1 Post_{k,t} + Z_{i,k,t} + Bank\ FE + Regulatory\ Office\ FE + Year\text{-}Quarter\ FE + \varepsilon_{i,k,} \quad (1\text{-}7)$$

where subscripts i, k and t indicate bank, regulatory office and year-quarter respectively, while Z is the same vector of control variables used in Section 1.4.1. In this analysis, I only include banks affected by regulatory office closures (banks that are not affected do not change their distance to supervisors). I thus run the analysis for groups of treated banks that differ in terms of how large the increase in distance between the previous and the new regulatory office is.

I show the results in Table 1-10. Columns (1)-(2) and (3)-(4) analyze changes in risk when the increases in distance to the new office are in the lowest 20% and 50% of the sample distribution respectively. Columns (5)-(6) and (7)-(8) analyze changes in risk for increases that are in the top 50% and 20% of the sample distribution respectively. The results show that *Post* is mostly insignificant in Columns (1)-(4) and positive and significant at the 1% level in Columns (5)-(8). Thus, increases of 15 km or more (the median increase in distance in the sample) are positively related to risk-taking, while smaller increases are not related to increases in bank risk.

Further, I also observe that larger increases in distance lead to higher risk-taking post-closures. Columns (7)-(8) show larger coefficient estimates for *Post* when increases in distance are 42 km or more (the 80th percentile of the sample distribution) as compared to when increases in distance are 15 km or more in Columns (5)-(6) (the 50^{th} percentile of the sample distribution). These findings are consistent with explanations that higher levels of information asymmetry present a challenge to effective supervisory enforcement.

1.7 Robustness

This section conducts various robustness tests on my main findings that banks affected by regulatory office closures become riskier and more aggressively grow their loan portfolios.

1.7.1 Time-varying Local Economic Shocks and Supervisory Effectiveness

To address remaining concerns that time-varying omitted variables that arise from local economic shocks or differences in regulatory enforcement practices could bias my results, Table 1-11 offers a particularly tight specification that includes bank fixed effects and 2 two-way fixed effects (County x Year-Quarter FE and Reg Office x Year-Quarter FE).

A within-county and -quarter specification is feasible because banks residing in the same county may be supervised by different regulatory offices (with some affected by closures and others are not) which allows me to control for two-way fixed effects. Reg Office x Year-Quarter dummies control for the time-varying intensity of supervision specific to each regulatory office. Table 1-11 shows the coefficients on *Treated* × *Post* remains statistically significant for the risk and lending outcomes. This gives me confidence that my results are not biased by time-varying unobservable factors.

1.7.2 Dynamic Timing Effects of Regulatory Office Closures

To confirm that my findings are indeed caused by regulatory office closures, I follow Bertrand and Mullainathan (2003) and estimate a dynamic timing effects model. I re-estimate the risk and lending results in Section 1.4.1 and 1.4.2 by replacing *Post* with four dummy variables (*Post Closure-1*, *Post Closure*, *Post Closure+1* and *Post Closure+2*) which are equal to 1 (and 0 otherwise) in the year of a regulatory office closure, the year immediately after the closure of a regulatory office and two or three years after the closure of a regulatory office, respectively.

Table 1-10: Office Closures and Increases in Distance to the New Office

This table reports regression estimates of the heterogeneous effects of the change (increase) in distance to a treated bank's new regulatory office after closure of its existing regulatory office. Only treated banks are used in this analysis. I report estimates of the following equation:

$$Y_{i,k,t} = \alpha_{i,k,t} + \beta_1 Post_{k,t} + Z_{i,k,t} + Bank\ FE + Regulatory\ Office\ FE + Year\text{-}Quarter\ FE + \varepsilon_{i,k,t}$$

where subscripts *i*, *k* and *t* indicate bank, regulatory office and year-quarter respectively *Y* is the *Z-Score* (Columns (1)-(8), computed as [Log (ROA+Equity) / σROA)] x (-1). ROA is defined as Net Income/Total Assets, Equity as Equity/Total Assets and σROA is the standard deviation of ROA over the past 3 years). Treated banks are banks which are affected by regulatory office closures (supervised by the closed office). *Post* is a variable that = 1 for the 3 years after the closure of a regulatory office *k* and 0 for the 2 years before (5-year diff-in-diff window). The variable of interest is the coefficient β_1 on the term *Post* which takes the value of 1 for treated banks the 3 years after closure of its regulatory office and 0 otherwise. *Z* is a vector of control variables and includes *Audit, Mandatory Audit, ROA, Log Total Assets, Tier-1 Capital, Real Estate Loans, Agri Loans, CI Loans, Indiv Loans, Total Deposits, Total Loans, 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. Refer to Appendix 1-A1 for the definition and construction of variables used in this chapter. The constant is suppressed. Standard errors are clustered at the bank-level. t-statistics are reported in parenthesis. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Increase in Distance							
	Bottom 20% (7km)		Bottom 50% (15km)		Top 50% (15km)		Top 20% (42km)	
	Z-Score	Z-Score	Z-Score	Z-Score	Z-Score	Z-Score	Z-Score	Z-Score
Post	0.0674	0.3995	0.16629*	0.2157	0.37721***	0.30145***	0.40408***	0.99578***
	[0.3164]	[1.0374]	[1.6717]	[1.1194]	[3.5365]	[2.64656]	[2.78245]	[4.6678]
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Reg Office FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other Controls	No	Yes	No	Yes	No	Yes	No	Yes
Adj. R-squared	0.169	0.274	0.106	0.153	0.173	0.247	0.182	0.364
Observations	1,067	1,067	2,473	2,473	2,429	2,429	951	951

I interact the four *Post Closure* variables with *Treated*. This allows me to assess the timing effects of regulatory office closures. The results are displayed in Table 1-12. The coefficient on the interaction term *Treated* × *Post Closure-1* is never significant.[20]

Further, the effects of increased risk-taking and lending for banks affected by regulatory office closures occurs only *after* regulatory office closures. *Treated* × *Post Closure, Treated* × *Post Closure+1* and *Treated* × *Post Closure+2* are mostly positive and significant. Therefore, the results of the dynamic timing effects model gives me additional confidence that the changes in bank behavior I document occurred in *response* to regulatory office closures.

Another advantage of the dynamic timing test is that I am able to rule out a "distraction" interpretation, where regulators are temporarily distracted as they supervise new institutions. In most columns, the economic and statistical magnitude of my results (increased risk-taking or loan expansion) remains relatively similar for each of the three years after an office closes. Should my results arise due to a temporary distraction effect, I would observe a gradual decrease in risk-taking and loan expansion in the later post regulatory office closure years.

[20] If *Treated* × *Post Closure-1* was statistically significant, this would indicate that banks which are affected by regulatory office closures differed in terms of their risk from the control group *before* regulatory office closures occur. This would give rise to concerns over reverse causality or that some unobservable shock is driving both office closures and bank behavior and therefore bias my results.

Table 1-11: Time-varying Economic Shocks and Supervisory Intensity

This table reports estimates of a difference-in-difference regression that controls for time-varying economic shocks and time-varying supervisory intensity and estimates the effect of regulatory office closures on bank risk and lending. I report estimates of the following equation:

$$Y_{i,k,t} = \alpha_{i,k,t} + \beta_1 Treated_{i,k,t} + \beta_2 Post_{k,t} + \beta_3 Treated_{i,k,t}\, x\; Post_{k,t} + Bank\ FE + County\ x\ Year\text{-}Quarter\ FE + Reg\ Office\ x\ Year\text{-}Quarter\ FE + \varepsilon_{i,k,t}$$

where subscripts *i*, *k* and *t* indicate bank, regulatory office and year-quarter, respectively. *Y* is either *Z-Score* (Column (1), computed as [Log (ROA+Equity) / σROA)] x (-1). ROA is defined as Net Income/Total Assets, Equity as Equity/Total Assets and σROA is the standard deviation of ROA over the past 3 years), *σROA* (Column (2), computed as Log (σROA)), *σROE* (Column (3), computed as Log (σROE)). ROE is defined as Net Income/Total Equity. σROE is the standard deviation of ROE over the past 3 years), *Log Total Loans* (Column (4)), *Total Loans/Total Assets* (Column (5)), *Log Real Estate Loans* (Column (6)) or *Real Estate Loans/Total Assets* (Column (7)). *Treated* is a dummy variable that = 1 if banks are in the treated group (and 0 if in the control group). Treated banks are banks which are affected by regulatory office closures (supervised by the closed office). Control banks are banks which are headquartered in the same counties as treated banks but are not supervised by the closed office. See Section 1.3 for the detailed construction of treatment and control groups. *Post* is a variable that = 1 for the 3 years after the closure of a regulatory office *k* and 0 for the 2 years before (5-year diff-in-diff window). The variable of interest is the coefficient β_3 on the interaction term *Treated x Post* which takes the value of 1 for treated banks the 3 years after closure of its regulatory office and 0 otherwise. Bank balance sheet variables are winsorized at the 1% and 99% levels. Refer to Appendix 1-A1 for the definition and construction of variables used in this chapter. The constant is suppressed. Standard errors are clustered at the bank-level. t-statistics are reported in parenthesis. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

	(1) Z-Score	(2) σROA	(3) σROE	(4) Log Total Loans	(5) Total Loans/TA	(6) Log Real Estate Loans	(7) Real Estate Loans/TA
Treated x Post	1.441***	0.001***	0.00682***	0.352***	0.102**	0.471***	0.11664**
	[12.467]	[19.690]	[10.38542]	[3.316]	[2.460]	[2.879]	[2.15439]
Post	0.734*	0	-0.00282	0.407**	0.118***	0.483***	0.07511**
	[1.775]	[-0.030]	[-0.76395]	[2.431]	[4.334]	[2.625]	[2.16379]
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County x Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Reg Office x Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other Controls	No	No	No	No	No	No	No
Adj. R-squared	0.24	0.243	0.358	0.289	0.109	0.221	0.132
Observations	7,254	7,274	7,274	8,321	8,321	8,321	8,321

1.7.3 Placebo Tests

I repeat my analysis in Section 1.4.1 and 1.4.2 using *only* the group of control banks (defined in Section 1.3.2.1). If local economic conditions (rather than office closures) explain my results, I should also observe changes in bank behavior in the control group in the period following office closures. After all, these banks reside in the same counties as the banks affected by regulatory office closures and are thus exposed to the same local economic shocks.

The results of this placebo test are reported in Table 1-13. The variable of interest is *Post* which is 1 in the three years following a regulatory office closure (and 0 otherwise). The coefficient on this variable is never significant for the risk and lending regressions. Therefore, the increase in bank risk and lending I observe for banks affected by a regulatory office closure is plausibly due to regulatory office closures, rather than unobserved local economic shocks.

1.8 Conclusion

This chapter studies the effects of supervision on bank business polices. I use a novel quasi-natural experiment, the closing of regulatory offices, as negative shocks to the efficacy of bank supervision. My results show that, after a regulatory office closes, banks under the supervision of the closed office become riskier and expand their loan portfolios more aggressively than banks located in the same counties but not under the supervision of the closed office. Further, banks affected by regulatory office closures exhibit lower risk-adjusted returns, lower asset quality and a higher probability of failure during the 2007-2009 financial crisis.

Table 1-12: Dynamic Timing Effects of Office Closures

This table reports estimates of a difference-in-difference regression which estimates the timing effects of regulatory office closures on bank risk and lending to show that bank behavior only changes after office closures. I report estimates of the following equation:

$$Y_{i,k,t} = \alpha_{i,k,t} + \beta_1 Treated_{i,k,t} + \beta_2 Post\ Closure\text{-}1_{k,t} + \beta_3 Post\ Closure_{k,t} + \beta_4 Post\ Closure{+}1_{k,t} + \beta_5 Post\ Closure{+}2_{k,t} + \beta_6 Treated_{i,k,t}\ x\ Post\ Closure\text{-}1_{k,t} + \beta_7 Treated_{i,k,t}\ x\ Post\ Closure_{k,t} + \beta_8 Treated_{i,k,t}\ x\ Post\ Closure{+}1_{k,t} + \beta_9 Treated_{i,k,t}\ x\ Post\ Closure{+}2_{k,t} + Bank\ FE + Regulatory\ Office\ FE + Year\text{-}Quarter\ FE + \varepsilon_{i,k,t}$$

where subscripts *i, k* and *t* indicate bank, regulatory office and year-quarter respectively. *Y* is either *Z-Score* (Column (1), computed as [Log (ROA+Equity) / σROA)] x (-1). ROA is defined as Net Income/Total Assets, Equity as Equity/Total Assets and σROA is the standard deviation of ROA over the past 3 years), *σROA* (Column (2) computed as Log (σROA)), *σROE* (Column (3), computed as Log (σROE). ROE is defined as Net Income/Total Equity. σROE is the standard deviation of ROE over the past 3 years), *Log Total Loans* (Column (4)), *Total Loans/Total Assets* (Column (5)), *Log Real Estate Loans* (Column (6)) or *Real Estate Loans/Total Assets* (Column (7)). *Treated* is a dummy variable that = 1 if banks are in the treated group (and 0 if in the control group). Treated banks are banks which are affected by regulatory office closures (supervised by the closed office). Control banks are banks which are headquartered in the same counties as treated banks but are not supervised by the closed office. See Section 1.3 for the detailed construction of treatment and control groups. *Post Closure-1* is a variable that = 1 for the year of the closure of regulatory office *k* and 0 otherwise. *Post Closure* is a variable that = 1 for the year immediately after the closure of regulatory office *k* and 0 otherwise. *Post Closure+1* is a variable that = 1 for the 2nd year after closure of regulatory office *k* and 0 otherwise. *Post Closure+2* is a variable that = 1 for the 3rd year after the closure of regulatory office *k* and 0 otherwise. The variable of interest is the coefficient $\beta_6, \beta_7, \beta_8$ and β_9 on interaction terms *Treated x Post Closure-1, Treated x Post Closure, Treated x Post Closure+1 and Treated x Post Closure+2*. Bank balance sheet variables are winsorized at the 1% and 99% levels. Refer to Appendix 1-A1 for the definition and construction of variables used in this chapter. The constant is suppressed. Standard errors are clustered at the bank-level. t-statistics are reported in parenthesis. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

	(1) Z-Score	(2) σROA	(3) σROE	(4) Log Total Loans	(5) Total Loans/TA	(6) Log Real Estate Loans	(7) Real Estate Loans/TA
Treated x Post Closure-1	0.05144	0.0001	0.00075	-0.02659	-0.00858	-0.03931	-0.0079
	[1.11611]	[1.28686]	[0.86995]	[-0.96471]	[-1.32662]	[-1.23277]	[-1.36721]
Treated x Post Closure	0.91264***	0.00087**	0.00473	0.17720**	0.06410***	0.21791**	0.07484***
	[6.21637]	[2.35133]	[1.54742]	[2.37429]	[3.19017]	[2.08471]	[3.52153]
Treated x Post Closure+1	0.86395***	0.00082**	0.00435	0.15179**	0.06914***	0.19741**	0.01572*
	[5.85610]	[2.26003]	[1.44764]	[2.16909]	[3.60098]	[1.97872]	[1.75542]
Treated x Post Closure+2	0.84695***	0.00085**	0.00515*	0.04678	0.06183***	0.08975	0.07865***
	[5.58759]	[2.36912]	[1.71508]	[0.63816]	[3.13182]	[0.87596]	[3.82711]
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Reg Office FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Other Controls	No	No	No	No	No	No	No
Adj. R-squared	0.11	0.0944	0.094	0.266	0.101	0.255	0.144
Observations	7,254	7,274	7,274	8,321	8,321	8,321	8,321

Table 1-13: Placebo Group and Office Closures

This table reports estimates of a regression which estimates the effect of regulatory office closures on bank risk and lending for a group of control banks. Only control banks are used in this analysis. I report estimates of the following equation:

$$Y_{i,k,t} = \alpha_{i,k,t} + \beta_1 Post_{k,t} + Bank\ FE + Regulatory\ Office\ FE + Year\text{-}Quarter\ FE + \varepsilon_{i,k,t}$$

where subscripts *i*, *k* and *t* indicate bank, regulatory office and year-quarter respectively. *Y* is either *Z-Score* (Column (1), computed as [Log (ROA+Equity) / σROA)] x (-1). ROA is defined as Net Income/Total Assets, Equity as Equity/Total Assets and σROA is the standard deviation of ROA over the past 3 years), *σROA* (Column (2), computed as Log (σROA)), *σROE* (Column (3), computed as Log (σROE). ROE is defined as Net Income/Total Equity. σROE is the standard deviation of ROE over the past 3 years), *Log Total Loans* (Column (4)), *Total Loans/Total Assets* (Column (5)), *Log Real Estate Loans* (Column (6)) or *Real Estate Loans/Total Assets* (Column (7)). Treated banks are banks which are affected by regulatory office closures (supervised by the closed office). Control banks are banks which are headquartered in the same counties as treated banks but are not supervised by the closed office. See Section 1.3 for the detailed construction of treatment and control groups. Post is a variable that = 1 for the 3 years after the closure of a regulatory office *k* and 0 for the 2 years before (5-year diff-in-diff window). The variable of interest is the coefficient β_1 on the term *Post* which takes the value of 1 for control banks the 3 years after closure of regulatory office of treated banks that are residing in the same counties as themselves and 0 otherwise. Bank balance sheet variables are winsorized at the 1% and 99% levels. Refer to Appendix 1-A1 for the definition and construction of variables used in this chapter. The constant is suppressed. Standard errors are clustered at the bank-level. t-statistics are reported in parenthesis. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Z-Score	σROA	σROE	Log Total Loans	Total Loans/TA	Log Real Estate Loans	Real Estate Loans/TA
Post	-0.143	-0.00013	-0.00058	-0.03835	0.01323	-0.02905	0.0242
	[-1.17353]	[-0.80232]	[-0.33416]	[-0.63925]	[0.76755]	[-0.42705]	[1.25590]
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Reg Office FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other Controls	No	No	No	No	No	No	No
Adj. R-squared	0.0859	0.0755	0.0814	0.229	0.0518	0.223	0.0911
Observations	2,352	2,352	2,352	2,947	2,947	2,947	2,947

Office closures are also associated with real costs to the FDIC. The closure of a single regulatory office leads to bank failure resolutions costs of approximately $15.7 million.

I argue that regulatory office closures reduce the level and quality of information that bank examiners have on banks. I show evidence that information asymmetry issues between banks and supervisors is one mechanism that impedes supervision. Specifically, I demonstrate that, following regulatory office closures, banks affected by closures exploit heightened information frictions between them and regulators to make lower and less timely provisions for future bad loans. I also show the post-closure changes in bank risk become more pronounced as the physical distance between banks and the newly assigned regulatory office increases.

My findings are of broad interest to regulators and help inform policy debates regarding regulations and supervision. Most importantly, my work paints a positive picture of the effectiveness of a decentralized structure of bank supervision where supervisory offices are located close to the banks which they examine. The findings are timely because of continued pressures on bank supervisors to deliver their services in a cost-effective way and with as few offices as feasible. Government and regulators should carefully weigh the cost savings of a more centralized organizational structure against the possibility of less effective bank supervision that may result from office closures.

A case in point is the ongoing implementation of the Single Supervisory Mechanism (SSM) in Europe where the supervision of systemically important banks is transferred from national regulators to the European Central Bank in Frankfurt.

However, the results I present in this chapter suggest that the informational advantages of being close to the institutions under supervision may well outweigh the other benefits of a more centralized supervisory structure.

Appendix 1-A1: Definition of Variables

Variables	Definition	Source
Risk & Performance Variables		
Z-Score	[Log (ROA+Equity) / σROA)] x (-1). σROA calculated is calculated using a 3-year rolling window	Call Report
σROA	Log (Standard deviation of ROA). σROA is calculated using a 3-year rolling window	Call Report
σROE	Log (Standard deviation of ROE). σROE is calculated using a 3-year rolling window	Call Report
ROA/σROA	Log (ROA / σROA). σROA is calculated using a 3-year rolling window	Call Report
ROE/σROE	Log (ROE / σROE). σROE is calculated using a 3-year rolling window	Call Report
ROA	Net Income / Total Assets	Call Report
ROE	Net Income / Total Equity	Call Report
EBLLP	(Net Income before Extraordinary Items + Loss Loan Loss Provisions) / Total Assets	Call Report
Fail07-09	Dummy variable = 1 if a bank failed from 07-09 and 0 otherwise	FDIC Failed Bank List
Fail07-10	Dummy variable = 1 if a bank failed from 07-10 and 0 otherwise	FDIC Failed Bank List
Loan Variables		
Total Loans	Total Loans / Total Assets	Call Report
Log Total Loans	Log (Total Loans in Thousands)	Call Report
Real Estate Loans	Real Estate Loans / Total Loans	Call Report
Log Real Estate Loans	Log (Real Estate Loans in Thousands)	Call Report
Real Estate Loans/TA	Real Estate Loans / Total Assets	Call Report
Agri Loans	Agricultural Loans / Total Loans	Call Report
CI Loans	Commercial and Industrial Loan / Total Loans	Call Report
Indiv Loans	Individual Loans / Total Loans	Call Report
Financial Variables		
Log Total Assets	Log (Total Assets)	Call Report
Total Deposits	Total Deposits / Total Assets	Call Report
Tier-1 Capital	Tier-1 Capital / Risk Weighted Assets	Call Report
LLA	Loan Loss Allowances / Total Loans	Call Report
LLP	Loan Loss Provisions / Total Loans	Call Report
BHC	Dummy variable = 1 if a bank is part of a Bank Holding Company and 0 otherwise	Call Report
Audit	Dummy variable = 1 if a bank receives an external audit and 0 otherwise	Call Report
Mandatory Audit	Dummy variable = 1 if total assets >$500m and 0 otherwise	Call Report
Bad Loans	Total Loans and Receivables 90+ days late Total Loans / Total Loans	Call Report
Loan Charge-Offs	Total Loan Charge-Offs / Total Loans	Call Report
Equity	Total Equity / Total Assets	Call Report
Accounting Variables		
|ALLP A|	Abs. value of the residuals of Model A (Equation 1-5)	Author's calculation
|ALLP B|	Abs. value of the residuals of Model B (Equation 1-6)	Author's calculation
-ALLP A	Negative residuals of Model A (Equation 1-5)	Author's calculation
-ALLP B	Negative residuals of Model B (Equation 1-6)	Author's calculation
+ALLP A	Positive residuals of Model A (Equation 1-5)	Author's calculation
+ALLP B	Positive residuals of Model B (Equation 1-6)	Author's calculation

NPA	Total Loans and Receivables 90+ days late / Lag Total Loans	Call Report
County & State Variables		
∆State GDP	Change in state GDP	Bureau of Economic Analysis
∆State HPI	Change in the return of the House Price Index (all transactions index)	Federal Housing Finance Agency
∆State UR	Change in State Unemployment Rate	Bureau of Labor
County Income per	Log (Income per capita of the county)	Bureau of Economic Analysis
County HHI	HHI index using the deposits of banks headquartered in the county	Call Report
County Pop Density	Population of the county / area of the county	U.S. Census Bureau
County Pop	Log (Population of the county)	U.S. Census Bureau
Regulatory Office Variables		
ROA under. Sup.	Mean ROA of banks under the regulatory office's supervision	Call Report
Bad Loans under Sup.	Mean Bad Loans of banks under the regulatory office's supervision	Call Report
Z-Score under Sup.	Mean Z-Score of banks under the regulatory office's supervision	Call Report
Log Total Assets under Sup.	Log (Sum of Total Assets of banks under the regulatory office's supervision)	Call Report
Main Office	Dummy variable that = 1 if a regulatory office is a non-satellite field office and 0 otherwise	Author's calculation
Beside Main Office	Dummy variable that = 1 if a regulatory office is the nearest office to a *Main Office* and 0 otherwise	Author's calculation

Appendix 1-A2: Determinants of Non-Discretionary Loan Loss Provisions

This table reports estimates of the determinants of loan loss provisions (where these determinants are the non-discretionary components) for which the residuals are used as proxies for the discretionary component of loan loss provisioning practices used in Table 1-9. I report estimates of the following equation in Column (1) (Equation 1-5):

$$LLP_{i,j,t} = \alpha_{i,j,t} + \beta_1 \Delta NPA_{i,j,t+1} + \beta_2 \Delta NPA_{i,j,t} + \beta_3 \Delta NPA_{i,j,t-1} + \beta_4 \Delta NPA_{i,j,t-2} + \beta_5 Log\ Total\ Assets_{i,j,t-1} + \beta_6 \Delta Total\ Loans_{i,j,t} + \beta_7 \Delta State\ GDP_{j,t} + \beta_8 \Delta State\ HPI_{j,t} + \beta_9 \Delta State\ UR_{j,t} + State\ FE + Year\text{-}Quarter\ FE + \varepsilon_{i,k,t}$$

I report estimates of the following equation in Column (2) (Equation 1-6):

$$LLP_{i,j,t} = \alpha_{i,j,t} + \beta_1 \Delta NPA_{i,j,t+1} + \beta_2 \Delta NPA_{i,j,t} + \beta_3 \Delta NPA_{i,j,t-1} + \beta_4 \Delta NPA_{i,j,t-2} + \beta_5 LLA_{i,j,t-1} + \beta_6 Log\ Total\ Assets_{i,j,t-1} + \beta_7 \Delta Total\ Loans_{i,j,t} + \beta_8 \Delta State\ GDP_{j,t} + \beta_9 \Delta State\ HPI_{j,t} + \beta_{10} \Delta State\ UR_{j,t} + State\ FE + Year\text{-}Quarter\ FE + \varepsilon_{i,k,t}$$

where subscripts i, j and t indicate bank, state and year-quarter respectively. Bank balance sheet variables are winsorized at the 1% and 99% levels. Refer to Appendix 1-A1 for the definition and construction of variables used in this chapter. The constant is suppressed. Standard errors are clustered at the bank-level. t-statistics are reported in parenthesis. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

	(1) Model A LLP	(2) Model B LLP
ΔNPA_{t+1}	0.00870**	0.00940**
	[2.03799]	[2.19948]
ΔNPA_{t}	0.01770*	0.01844**
	[1.90483]	[1.98542]
ΔNPA_{t-1}	0.01908***	0.01925***
	[3.80754]	[3.83945]
ΔNPA_{t-2}	0.01821***	0.01830***
	[3.99261]	[4.00174]
Log Total Assets$_{t-1}$	0.00002	0.00003
	[0.49810]	[0.74532]
ΔTotal Loans$_t$	-0.00034	-0.0004
	[-0.77181]	[-0.91821]
ΔState GDP$_t$	0.00831***	0.00819***
	[3.34693]	[3.30327]
ΔState HPI$_t$	-0.01970***	-0.01981***
	[-3.09660]	[-3.10118]
ΔState UR$_t$	-0.0008	-0.00081
	[-1.59034]	[-1.59383]
LLA_{t-1}		0.01084*
		[1.70958]
Year-Quarter FE	Yes	Yes
State FE	Yes	Yes
Adj. R-squared	0.07652	0.07782
Observations	7,953	7,953

2
Is the Fox Guarding the Henhouse? Regulatory Connections and Public Subsidies in Banks

2.1 Introduction

In 2012, JPMorgan Chase incurred a multi-billion dollar trading loss that subsequently led to a Senate Congressional hearing and a $920 million fine for 'unsafe and unsound practices'. At the time JPMorgan incurred the loss, Jamie Dimon, the bank's CEO, served on the board of the New York Fed, JPMorgan's regulator.[21] In a similar case, Mary Pugh who was the chair of Washington Mutual Bank's finance committee when the bank incurred large losses that led to its bankruptcy in 2008, previously held a directorship position at the San Francisco Fed.[22] Are these isolated cases where regulators appear less effective when supervising banks whose senior management hold positions in these agencies? This chapter focuses on a previously unexplored institutional setting. I examine connections between banks and regulators that result when members of a bank's board serve regulatory agencies in an advisory

[21] "Dimon's Role on N.Y. Fed Board Sparks Fierce Debate", *American Banker*, 18 May 2012. See also, "Dimon and the Fed's Legitimacy", *New York Times Blog*, 24 May 2012.
[22] "WaMu board director forced out", *Financial Times*, 16 April 2008.

or other public service position. I find that these connections are widespread. Consequently, connected banks have lower risk to capital sensitivities which facilitates risk-shifting to the financial safety-net. One possible explanation for these findings is that connected banks receive preferential treatment in supervision.

Recent studies document the effects of financial firms hiring former regulatory employees (e.g., Lucca, Seru, and Trebbi, 2014; Shive and Forster, 2016). In contrast, this chapter focuses on members of bank boards who serve regulatory agencies by undertaking public service positions. In the U.S., members of bank boards routinely take up directorship and advisory positions with regulatory agencies. Most notably, the 12 Federal Reserve Banks, which between them supervise all Bank Holding Companies (BHCs) in the U.S., are each overseen by a board of directors that consists of representatives from the private sector, including representatives from banks that the Fed supervises. In addition, the Federal Reserve (as well as other agencies) also rely on advisory councils consisting of bankers to inform policy.[23] Thus, public service roles constitute a unique setting in which bankers, which are intended to be regulated

[23] Involvement of bankers in regulatory agencies and central banks are not limited to the U.S. For instance, Austria, France, Germany, Italy and Switzerland amongst other countries have bankers involved in some official capacity at regulatory agencies or central banks. For e.g. see

https://www.oenb.at/en/About-Us/Organization/Decision-Making-Bodies/General-Council.html,

https://acpr.banquefrance.fr/en/acpr/organisation/the-consultative-committees-and-the-scientific-committee.html,

https://www.bafin.de/EN/DieBaFin/GrundlagenOrganisation/Gremien/Fachbeirat/fachbeirat_node_en.html,

http://www.bancaditalia.it/chisiamo/organizzazione/filiali/index.html?com.dotmarketing.htmlpage.language=1

http://www.snb.ch/en/iabout/snb/bodies/id/snb_bodies_council#t9.

by regulatory agencies, are given opportunities to serve in various positions in these very agencies.

I propose two competing hypotheses on the effects of public service connections. On the one hand, such connections may not undermine, and could even improve the supervisory process. The ‘public interest view’ states that regulators derive utility and a sense of duty when contributing to society (Shiller, 2012; Bond and Glode, 2014). If so, public service connections should not influence the supervisory process. Additionally, these interactions could even improve the regulatory process by providing regulators with timely information on industry trends to support the formulation and implementation of monetary policy and regulations (Federal Reserve Bank of New York, 2015).

On the other hand, having connected directors on boards may allow banks to shift risk to the safety-net for two main reasons. Firstly, connected banks could receive preferential treatment by regulators. The ‘private interest view’ put forth by Stigler (1971) argues that regulators are frequently captured by the industry they regulate and seek to further their own private interests. For instance, the prospect of future employment in the banking sector could incentivize a regulator to be less stringent in her supervision in an attempt to promote relationships with the industry. Public service connections would then function as a conduit for supervisors to offer favors to banks. Further, personal connections could undermine supervisory monitoring by making the relationship between supervisors and banks more communal (Mills and Clark, 1982). This encourages supervisors to “go easy” on connected banks for fear of “rocking the boat”. Second, connections between bank directors and regulators could allow bank directors to acquire expertise on supervision and enforcement that could help banks evade regulatory discipline (Dal Bo, 2006; Lucca et al., 2014; Shive and Forster, 2016).

To investigate if connections established via public service positions undermine regulatory efficacy, I use detailed data from the CVs of members of the boards of U.S. Bank Holding Companies (BHCs) between 2001 and 2013.[24] I identify what I call *public service connections* if board members currently or previously served either on a regulator's advisory council or on a Fed board.[25] Following, I construct bank-level measures of connectedness to the main regulatory agencies: the Federal Reserve (Fed), the Federal Deposit Insurance Corporation (FDIC), the Office of the Comptroller of the Currency (OCC), the Office of Thrift Supervision (OTS), the U.S. Securities and Exchange Commission (SEC) and state banking regulators. Such connections are widespread. Nearly 30% of banks in my sample employ at least one board member who is connected to a regulatory agency in this way. Further, 13% of banks have at least one director who currently serves in a public service position while sitting on the board of the bank.

This chapter explores if connections allow banks to access larger public subsidies. In the presence of deposit insurance, the value of public subsidies afforded to shareholders of banks can be modelled as the value of a put option that is underwritten by the FDIC (Merton, 1977). To prevent banks from shifting risk to the safety-net (that is, to prevent banks from increasing the value of the put option),

[24] Throughout the chapter, I use the term banks to refer to BHCs unless I need to distinguish between a BHC and banks operating under the BHC.

[25] I include previous as well as current connections because many of the effects of connections that the analysis is meant to identify (personal familiarity with regulators and the transfer of supervision-related skills to banks) outlast currently active connections. Some of my analysis is based on current connections only and finds similar results.

regulators must monitor and discipline bank risk-taking. They do so by requiring a commensurate increase in capital when bank risk increases (Duan, Moreau, and Sealey, 1992; Hovakimian and Kane, 2000).[26] If the capital discipline imposed by regulators is insufficient to offset increases in risk, banks can increase the value of public subsidies by increasing risk. I measure the extent to which connected banks are able to enjoy greater public subsidies by increasing risk, which operates through the decrease in the sensitivity of capital to risk.

My baseline results show that public service connections increase the gain to banks from shifting risk to the financial safety-net. This effect is economically large. An average-sized board of twelve with one connected director increases the gain from risk-shifting by 36% (compared to banks with no connected directors). This gain is achieved through a decrease in the sensitivity of the bank's capital to asset risk. The sensitivity of a bank's capital base to asset risk decreases by almost 39% with one connected director on an average-sized board. This indicates that connected banks are able to increase risk without holding a commensurate amount of capital compared to their unconnected peers. My results are robust to the inclusion of bank fixed effects, which helps rule out concerns related to time-invariant omitted variables, and to a range of other robustness tests.

To support a causal interpretation of my results, I exploit two identification strategies. The first strategy relies on the retirements of Federal Reserve Presidents to

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[26] The intuition follows basic option theory. As standard in option theory, gains to one party implies losses to the opposite party. Thus, increases in the value of the put to banks comes at the expense of the FDIC. The objective of regulators is to prevent bank shareholders from increasing the value of the put option (shifting risk to the safety-net). As the value of the put option is dependent on risk and leverage, increases in risk must be met with decreases in leverage for the value of the put option to remain constant. I discuss the model in greater detail in Section 2.2.

generate plausibly exogenous shocks to the quality of existing bank connections to regulators. In the U.S., BHCs are overseen by 1 of the 12 Reserve Banks, each headed by a President. Each Reserve Bank has their own board of directors as well as their own advisory councils. I employ a difference-in-difference method (DiD) that measures variation in banks' gains from risk-shifting within Fed districts. This set-up controls for potential within-district changes in bank behavior that may be related to the retirements of Fed Presidents. The group of treated banks are banks with board members who have served alongside the outgoing Fed President in advisory or directorship positions. I show that the gain from risk-shifting of banks with connections decreases if the President of a Fed retires, as compared to a control group of banks without connections that are located in the same Federal Reserve district.

A key advantage of this empirical set-up is that there are multiple instances of retirements in my data that affect different banks in different years and geographic areas. This rules out the possibility that omitted variables coinciding with a single retirement could bias my findings. Further, to ensure that retirements are plausibly exogenous to bank risk-shifting, I check newspaper reports and articles for the reason behind retirements as well as exclude unplanned retirements (that could be dismissals dressed up as retirements and thus potentially be related to regulatory effectiveness). Various placebo and robustness tests report consistent results.

The second identification strategy is based on the heterogeneous effects of the Emergency Economic Stabilization Act (EESA) on gains from risk-shifting at connected versus unconnected banks. The EESA, the largest federal investment program in U.S. history, was signed into law in 2008 in response to the crisis, and contained a number of provisions to support and recapitalize the banking industry.

Various studies have demonstrated that EESA increased moral hazard and risk-taking by banks (Duchin and Soysura, 2014; Lambert, Noth, and Schüwer, 2016). Further, the EESA signalled the willingness of regulators to engage in forbearance during crisis times (Archarya and Yorulmazer, 2007; Brown and Dinç 2009). I predict that the effects of regulatory forbearance would be more evident at connected banks and lead to larger gains in risk-shifting following EESA as compared to non-connected banks.

I provide evidence of the exogeneity of the timing of the shock to bank connections by showing that the proportion of directors with public service connections remains constant before and during the enactment of the Act. Additionally, I also consider only banks with directors who currently hold public service positions at the time (and after) the implementation of the Act. This reduces the possibility that banks establish connections in anticipation of the Act.[27] The results are consistent with the prediction that connected banks increase the gains to risk-shifting as compared to non-connected banks following the enactment of EESA. They are also robust to numerous tests such as controlling for the receipt of TARP funds and the exclusion of too-big-to-fail banks. Taken together, the results of the two identification strategies reinforce the baseline results and suggest a causal link between connections and gains from risk-shifting.

Having shown that connected banks risk shift more to the financial safety-net, I proceed to disentangle between the two principle channels of why they are able to do so: 1) preferential treatment by supervisors or; 2) through the transfer of skills. I conduct two tests which present evidence that preferential treatment by regulators is

[27] Fahlenbrach and Stulz (2011) also provide evidence that suggest bank CEOs were unable to anticipate the timing of the financial crisis. This suggests that bankers were unable to “time” their current public service connections to coincide with the financial crisis and the Act.

one reason for gains from risk-shifting by connected banks. It is empirically challenging to disentangle the two explanations, mainly because preferential treatment is publicly unobservable. I circumvent this challenge by exploiting the public attention generated by JPMorgan Chase's large trading loss in 2012 as a negative shock to the ability of regulators to afford preferential treatment to connected banks. The publication of the trading loss heightened public scrutiny of the Fed's connections to banks and stoked suspicion over conflicts of interest in the supervisory process after it emerged that JPMorgan's CEO was sitting on the board of the New York Fed at the time of the loss.

Consistent with this, I find a reduction in gains from risk-shifting at connected banks in the aftermath of the trading loss. Additionally, I find that the gains to connected banks decrease most when banks are regulated by the New York Fed, under whose supervision the trading loss occurred. Crucially, the use of the trading loss shock backs the view that preferential treatment (rather than technical expertise or skills) explains my results, because any skills which bank directors may have developed from dealings with connected regulators are not plausibly affected by the trading loss. Bank directors will be no less skilled following the trading loss, but the loss (and the heightened public scrutiny that followed it) will have curtailed the ability of the Fed to extend preferential treatment to connected banks.

The second test for preferential treatment exploits the charter type of the main commercial bank operating under a BHC. While all BHCs are regulated by the Fed, commercial banks operating under the umbrella of a BHC are regulated by either the Fed, the FDIC or the OCC depending on their charter. If there is evidence of preferential treatment, my results should be strongest when connections exist to the regulator in charge and weakest when connections exist to regulators not responsible for a particular bank. However, if connections are related to skills, I should observe that connections lead to risk-shifting irrespective of the regulator in charge, due to the uniformity of regulations. Consistent with explanations of preferential treatment, I find that banks that are connected to the Fed while also being regulated by the Fed are able to access larger subsidies from the financial safety-net.

The last set of results show that gains from risk-shifting at connected banks occurs when banks are not performing poorly, and that wealth is transferred to shareholders. Risk-shifting by well-performing connected banks is positively associated with higher stock and accounting performance and an increased probability of larger payouts to shareholders. Overall, the results show that connections facilitate risk-shifting and that wealth is transferred from taxpayers to the shareholders of connected banks.

My research contributes to several strands of literature. This chapter is the first to document the scale of public service connections in banking and how they undermine regulatory discipline. In doing so, I identify a previously undocumented channel through which regulatory capture manifests itself. Previous work identifies campaign contributions (Mian, Sufi, and Trebbi, 2010), lobbying (Igan, Mishra, and Tressel, 2011; Lambert, 2015), and the hiring of ex-regulators (Shive and Forster, 2016) as vehicles through which the financial industry seeks to influence regulation and bank outcomes.[28] I contribute to this work by demonstrating that public service connections also acts as a conduit for regulatory capture.

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[28] Mian et al. (2010) find that higher campaign contributions from the financial industry influence politicians' voting behavior regarding financial regulations. Igan et al. (2011) show that lobbying by

Second, the chapter contributes to the literature on financial firms hiring former regulatory employees. This literature remains inconclusive on the motivations behind revolving door hires. Shive and Forster (2016) and Lucca et al. (2014) suggest that former regulators are hired for their technical skills and expertise while Duchin and Sosyura (2012) show that the presence of connected board members leads to preferential treatment by regulators in times of crisis.[29] My evidence suggests that connected banks benefit from preferential treatment rather than from expertise acquired via connections. Public service roles do not involve decision-making or direct participation in supervision, whereas ex-regulators have experience in the supervision of banks. Given this, my finding of preferential treatment at banks with public service connections complements existing evidence that hiring ex-regulators brings benefits of greater expertise about the regulatory process and highlights the heterogeneity of the various channels of influence.

Finally, I document that bank connections to regulators facilitate a wealth transfer from taxpayers to shareholders. The results of previous work on connections are suggestive but not conclusive of a wealth transfer. For instance, extant studies report shareholder wealth gains linked to regulatory and political connections (Acemoglu, Johnson, Kwak, and Mitton, 2016; Adams, 2013) but do not show that connections are detrimental to taxpayer interests. Further, existing studies show that

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financial institutions is positively associated with risk-taking leading up to the crisis, while Lambert (2015) finds that lobbying banks are less likely to be subject to severe enforcement actions. Shive and Forster (2016) show that hiring ex-regulators leads to a reduction in risk-taking in U.S. banks in support of the skills channel. The authors explain that if preferential treatment were present, risk-taking should increase after the recruitment of ex-regulators.

[29] Duchin and Sosyura (2012) define connected directors as having current or former positions at the Treasury, the firm's banking regulator or Congress. They also use an index of political connections that includes connected directors, bank representation on the House Financial Services Committee, lobbying expenditures and campaign contributions for the majority of their tests.

politically connected banks that received taxpayer-funded bailouts underperformed as compared to non-receivers, suggesting a misallocation of government funds in the 2008/09 crisis period (Duchin and Soysura, 2012). Since crisis periods are typically characterized by heightened policy discretion and a high chance of forbearance by regulators (cf. Brown and Dinç, 2011), it is unclear whether this result holds more generally. By contrast, I present evidence that risk-shifting by well-performing banks with public service connections consistently facilitate access to public subsidies.

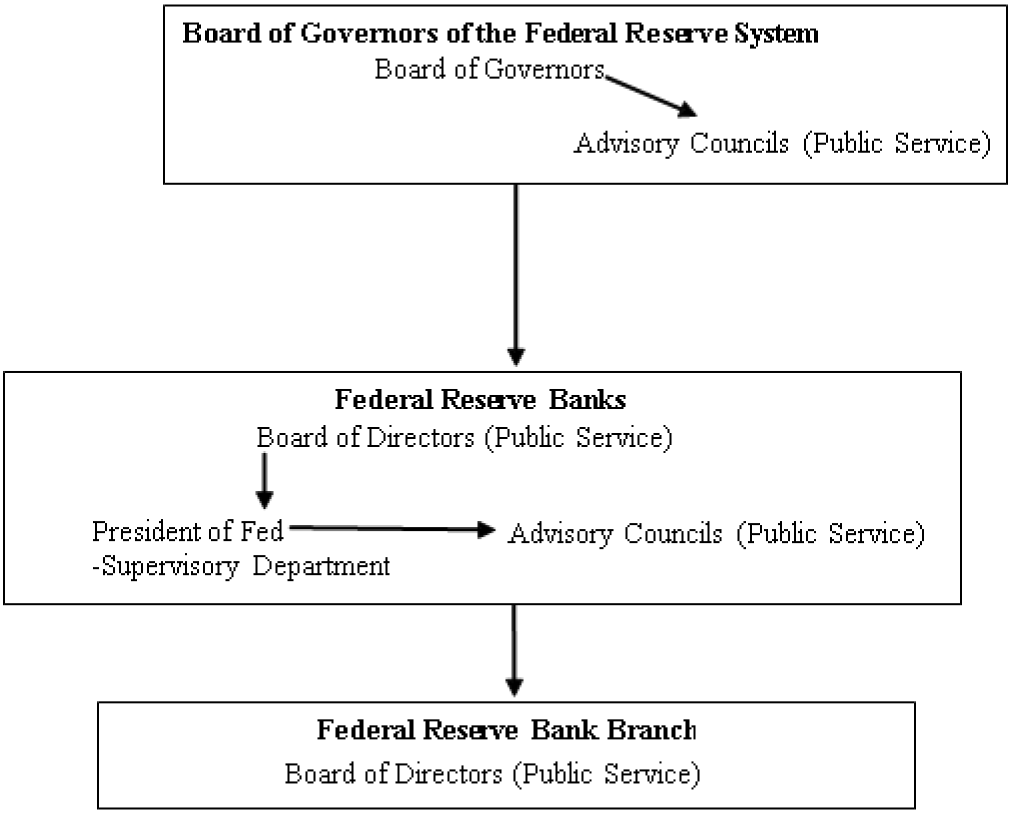
2.2 Institutional Setting and Hypothesis Development

2.2.1 Institutional Setting

Depending on their charter, commercial banks are supervised by one of three federal regulators. The Fed supervises all Bank Holding Companies as well as state-chartered banks that are members of the Fed. The FDIC supervises non-member state-chartered banks, while the OCC supervises nationally-chartered banks. Additionally, the SEC regulates all publicly listed firms.

The chapter focuses on bank directors who serve regulatory agencies in a public service capacity. I define public service positions as positions that members of a bank's board presently hold or previously held in regulatory agencies. Since many of the public service positions (91%) in my sample are with the Fed, Figure 2-1 provides illustrations relating the structure of the Fed with emphasis on public service positions.

Figure 2-1: Federal Reserve Structure and Public Service Positions



There are two main types of public service positions that members of a bank board (as well as other private sector employees) can take up with the Fed. First, bank directors can take up directorship positions on the board of Federal Reserve Banks. The 12 Federal Reserve Banks (and their 24 branches) have three classes of directors; A, B and C. Class C directors are appointed by the Board of Governors to represent the public. Class A and B directors are elected by member banks in each Fed district to represent member banks and the public, respectively. Class A directors are typically bank directors of commercial banks residing in the Fed district. Four of the seven Fed branch directors must be Class A and B directors, and the rest constitute Class C directors.[30] All of the 12 Fed Banks have their own board of directors as well as advisory councils.

Members of the board of directors of a Fed Bank are responsible for supervising the Fed's operations and its internal auditing procedures, and for holding the President and First Vice-President to account. Crucially, Fed directors are not involved in matters of supervision (Board of Governors of the Federal Reserve System, 2013, p. 2). That is, "[Fed] directors may not be consulted regarding bank examination ratings, potential enforcement actions, application/approval matters and other such supervisory matters" (pg. 41). Thus, they are not in a position to favor a particular bank directly, nor to acquire detailed knowledge pertaining to bank supervision.

The second type of public service position is membership to an advisory council. The range of advisory councils that members of a bank's board may serve in is extensive (see Panel C of Table 2-1). For instance, "The New York Fed meets regularly with small business leaders, community bankers, financial market participants, economists and others through external committees and outreach programs to obtain essential perspectives on the economy from both Main Street and Wall Street. These interactions help the New York Fed to provide timely information to the Federal Reserve System and to support the formulation and implementation of monetary policy effectively" (Federal Reserve Bank of New York, 2015).

Advisory councils typically meet two to four times a year with their respective Federal Reserve Presidents (or the Board of Governors), consist of ten to twelve

[30] Class B and C directors cannot be employees of the bank while serving Fed directorship positions. They are however not prohibited from joining banks after their tenures as Federal Reserve Bank directors.

members, have two- to three-year terms (with the possibility of extension) and varied responsibilities. For instance, the Federal Advisory Council (FAC) which advises the Board of Governors is "…composed of twelve representatives of the banking industry, consults with and advises the Board on all matters within the Board's jurisdiction…. [Each] year, each Reserve Bank chooses one person to represent its district on the FAC, and members customarily serve three one-year terms" (Board of Governors of the Federal Reserve System, 2016).[31]

2.2.2 Modelling Bank Subsidies

The deposit insurance premium model pioneered by Merton (1977) and later developed by Duan et al. (1992) and Hovakimian and Kane (2000) offers a tool to estimate the subsidies afforded to banks. Merton (1977) models safety-net subsidies as the value of a put option underwritten by the FDIC (and by extension, the taxpayer). On a conceptual level, deposit insurance permits banks to put the assets back to the FDIC at the face value of its debt whenever the value of assets falls below the value of liabilities. It follows that bank shareholders can extract higher public subsidies by increasing the value of the put option if they increase asset risk and leverage.[32]

This model is widely used to test for risk-shifting by banks to the financial safety-net (e.g., Duan et al. 1992; Hovakimian and Kane, 2000; Wagster, 2007; Bushman and Williams, 2012; Carbo-Valverde, Kane, and Rodriguez-Fernandez, 2008; 2013).[33] Risk-shifting is distinct from risk-taking in that the former arises when

[31] The descriptions of the objectives of individual Advisory Councils can be obtained from the individual Federal Reserve Banks' websites.
[32] The idea corresponds to the valuation of a put option. The value of a put option increases in volatility (bank asset risk) and leverage (the strike price). It follows that losses to one party imply gains to the counterparty.
[33] Duan et al. (1992) test if U.S. banks are able to risk shift to the safety-net. Hovakimian and Kane (2000) show that capital regulations in the U.S. were not effective in controlling risk-shifting by U.S.

a contractual counterparty (in this case the taxpayer) is inadequately compensated for the risks to which they are exposed. The model permits me to investigate if public service connections to regulators impedes the supervisory process and allow banks to extract larger benefits from the financial safety-net.

This chapter adopts the quasi-reduced form equations developed by Duan et al. (1992) and Hovakimian and Kane (2000):

$$\Delta(B/V) = \alpha_0 + \alpha_1\Delta\sigma_v + \varepsilon_1 \qquad (2\text{-}1)$$

$$\Delta IPP = \beta_0 + \beta_1\Delta\sigma_v + \varepsilon_2 \qquad (2\text{-}2)$$

where B is the book value of debt, V the market value of bank assets, B/V the leverage ratio and σ_v the volatility of the bank's assets. IPP is the per-period flow of subsidies to bank shareholders, defined as the actuarially fair insurance premium percentage per dollar of debt. Δ is the first-difference operator. The estimation of V, σ_v and IPP is described in Appendix 2-A2.

The slope coefficients of Equations (2-1) and (2-2) have the following interpretations:

$$\alpha_1 = d(B/V) / d\sigma_v \qquad (2\text{-}3)$$

$$\beta_1 = dIPP / d\sigma_v = (\partial IPP / \partial\sigma_v) + \partial IPP / \partial(B/V)\alpha_1 \qquad (2\text{-}4)$$

Equation (2-1) describes the notion that regulators (and also bank creditors) restrict banks to certain combinations of leverage and volatility. Accordingly, Equation (2-1) reflects outside discipline to reduce (increase) bank leverage as an institution's asset

banks. Wagster (2007) shows that the adoption of explicit deposit insurance expanded risk-shifting incentives for Canadian banks and trusts. Bushman and Williams (2012) show that accounting discretion can influence risk-shifting incentives by banks. Carbo-Valverde et al. (2013) show that too-big-to-fail banks are more able to extract subsidies from the safety-net.

risk increases (decreases). Equation (2-2) measures if banks are able to increase the value of public subsidies by increasing risk after overcoming the effects of discipline imposed by regulators and creditors.

For regulatory and market forces to fully neutralize risk-shifting incentives, two joint conditions have to be satisfied:

Leverage decreases with volatility: $\alpha_1 < 0$.

The value of public subsidies (IPP) does not rise with volatility: $\beta_1 \leq 0$.

A negative α_1, while indicative of disciplinary forces imposed on a bank, is insufficient to show that outside discipline mitigates the incentives to shift risk. To fully neutralize risk-shifting incentives, a decline in leverage must be sufficiently large to offset increases in the value of public subsidies that would be generated by increasing asset volatility ($\beta_1 \leq 0$). If so, banks would not find it advantageous to increase risk.

2.2.3 Hypotheses

I propose two competing hypotheses about the effect of public service connections. On the one hand, such connections may not undermine, and could improve the supervisory process if regulators are motivated by a sense of duty (Bond and Glode, 2014) or social purpose (Shiller, 2012). The 'public interest view' implies that regulators may derive utility and a sense of purpose from contributing to society and that they work hard to achieve these ideals (Predergast, 2007). As connections between regulators and bank directors are publicly observable, public scrutiny could thwart the ability of banks to influence supervisory stringency. In fact, regulators may safeguard themselves against allegations of preferential treatment and limit the ability of connected banks to shift risk to the safety-net. Subsequently, connections could even improve the regulatory process by providing regulators with timely information on industry insights to support the efficient formulation and implementation of monetary policy and regulations.

On the other hand, having connected directors on boards may allow banks to shift risk to the safety-net. I put forth two reasons; preferential treatment and the transfer of skills through connections. Firstly, connected banks could receive preferential treatment by regulators. This is the 'private interest view', first put forth by Stigler (1971), which argues that regulators are frequently captured by the industry they regulate.[34] Information asymmetry surrounding supervision grants discretionary powers to regulatory staff who might not necessarily work to promote societal welfare, but might seek to further their own private interests (Baron and Myerson, 1982; Laffont and Tirole, 1993). One example of this is the prospect of future employment in the banking sector which could incentivize a regulator to be less stringent in her supervision to promote relationships with the industry (Dal Bo, 2006). A member of staff might be more tempted to curry favor or avoid conflict with banks with connections with her employer, than with unconnected banks. Additionally, personal connections could undermine monitoring by making the relationship between supervisors and supervisees more communal (Mills and Clark, 1982) and by tempting supervisors to socially identify with the banking sector (Barth, Caprio, and Levine 2012). For instance, bank examiners might be less willing to imposed regulatory discipline and scrutiny on connected banks to minimize conflicts and retain communal relationships with "friends" of the agency.

[34] Dal Bo (2006) surveys the extensive literature on regulatory capture.

Secondly, connected directors could acquire relevant expertise on supervision and enforcement from their dealings with regulators. Any expertise acquired from regulators could help banks evade regulatory discipline (Lucca et al., 2014; DeHaan, Koh, Kedia, and Rajgopal, 2015; Cornaggia, Cornaggia, and Xia, 2015).

To empirically analyze if regulatory connections are linked to regulatory laxity, and subsequently, larger public subsidies, I modify Equations (2-1) and (2-2) as:

$$\Delta(B/V)_{i,t} = \alpha_0 + \alpha_1 \Delta\sigma_{Vi,t} + \alpha_2(Public\ Service_{i,t}\ x\ \Delta\sigma_{Vi,t}) + \alpha_3 Public\ Service_{i,t} + Bank\ Controls + Year\ FE + Regulator\ FE + \varepsilon_{i,t} \quad (2\text{-}5)$$

$$\Delta IPP_{i,t} = \beta_0 + \beta_1 \Delta\sigma_{Vi,t} + \beta_2(Public\ Service_{i,t}\ x\ \Delta\sigma_{Vi,t}) + \beta_3 Public\ Service_{i,t} + Bank\ Controls + Year\ FE + Regulator\ FE + \varepsilon_{i,t} \quad (2\text{-}6)$$

where *i* indexes bank, *t* indexes year, and *Public Service* is my main measure of regulatory connections, defined in detail in Section 3.2. Regulator FE are dummy variables that control for the main federal regulator (Fed, FDIC or OCC) of the largest commercial bank operating under the BHC. Carbo-Valverde, Kane, and Rodriguez-Fernandez (2008) explain that the dialectical nature of the process in which banks devise new strategies to conceal or understate risk to avoid capital requirements makes it advisable to estimate *B/V*, σ_v and *IPP* in first-difference form.

The coefficient α_2 captures the effect of public service connections on regulatory discipline imposed on banks in response to increasing risk (sensitivity of changes in capital or leverage to changes in risk).

Hypothesis 1: Connected banks have lower levels of risk-leverage sensitivities ($\alpha_2 > 0$).

If $\alpha_2 > 0$, and the bank's risk increases, its capital ratio does not increase by as much as the capital ratio of unconnected banks.

The interaction term β_2 measures the extent to which connected banks gain access to larger public subsidies by evading regulatory discipline imposed on them for higher asset risk.

Hypothesis 2: Connected banks extract higher public subsidies if bank risk increases ($\beta_2 > 0$).

If $\beta_2 > 0$, a connected bank has more to gain by increasing its risk than an unconnected bank.

2.3 Data and Descriptive Statistics

2.3.1 Sample Construction

The initial sample consists of all public U.S. banks from 2001 to 2013 covered in BoardEx, a database maintained by Management Diagnostics Limited. BoardEx provides me with detailed biographical and employment (current and historical) data on all members of the board which allows me to identify public service positions held in regulatory agencies. BoardEx began populating their database on corporate directors in 2000 from various sources, including, but not limited to SEC filings, company press releases, corporate websites and news outlets. I retain deposit-taking banks with SIC codes starting 602 (commercial banks) and 603 (savings institutions).

Table 2-1: Summary Statistics

This table contains summary statistics of key variables (Panel A and B) and description of the public service positions bank directors undertaken while in regulatory agencies (Panel C). # is the number of bank-year observations, std. is the standard deviation while p1, p50 and p99 are the 1st, 50th and 99th percentiles. Panel A show summary statistics of the variables used in this chapter. Panel B presents detailed information on public service connections. Panel C shows the positions as well public service committees that connected directors have or is currently serving in at regulatory agencies. Refer to Appendix 2-A1 for construction and definition of these variables. The sample period is 2001 to 2013.

Panel A: Summary Statistics	#	mean	std.	p1	p50	p99
Connection Variables						
Public Service	3,011	0.0306	0.0552	0	0	0.250
Current Public Service	3,011	0.011	0.03	0	0	0.125
Fed Public Service	3,011	0.0289	0.0542	0	0	0.250
Politically Connected	3,011	0.0174	0.043	0	0	0.182
Top Politician	3,011	0.0088	0.284	0	0	0.143
Lobby%	3,011	0.405	1.545	0	0	8.199
Financial Variables						
IPP (%)	3,011	0.285	1.131	0	0.00034	4.456
ΔIPP (%)	3,011	0.049	1.288	-2.235	0	2.879
σ_V (%)	3,011	3.333	2.495	0.579	2.849	12.20
$\Delta\sigma_V$ (%)	3,011	-0.172	2.893	-7.453	-0.178	7.562
(B/V)%	3,011	89.80	6.853	73.16	89.67	103.1
Δ(B/V)%	3,011	0.619	3.844	-8.378	0.349	10.594
Tier-1 Capital	3,011	0.124	0.039	0.061	0.118	0.233
ΔTier-1 Capital	3,011	0.00052	0.0235	-0.06	0.0001	0.0588
Bad Loans	3,011	0.0131	0.0162	00004	0.0075	0.0796
Enforcement Actions	3,011	0.093	0.43	0	0	2
ROA (%)	3,011	0.586	0.0132	-5.69	0.863	2.14
Total Deposits	3,011	0.757	0.0899	0.464	0.774	0.898
Market Risk	3,011	0.108	0.175	-0.366	0.106	0.529
Total Assets	3,011	6.480	0.687	5.460	6.320	9.102
Asset Growth	3,011	0.0854	0.171	-0.158	0.0547	0.726
Total Loans	3,011	0.673	0.123	0.316	0.690	0.890
Stock Rets.	3,011	-0.0156	0.3575	-0.95	0.0161	0.8415
Pr Div↑	3,011	0.42	0.49	0	0	1
Pr Net Payout↑	3,011	0.43	0.49	0	0	1
Noninterest Income	3,011	0.1827	0.1183	-0.0062	0.163	0.6047
Leverage	3,011	0.904	0.0299	0.835	0.906	0.9618
Book-to-Market Ratio	3,011	1.46	0.838	0.146	1.348	3.916
Core Deposits	2,958	0.789	0.129	0.259	0.820	0.955
Sub Debt	3,011	0.0399	0.0859	0	0	0.3421
Variables in Appendix 2-A2						
σ_E (Annualized)	3,011	0.302	0.225	0.0718	0.230	1.297
E (Millions)	3,011	4,083	20,632	8.500	270.1	120,049
B (Millions)	3,011	31,110	182,698	265.5	1,900	1157816
V (Million)	3,011	34,207	195,248	294.7	2,125	1273114
Board & Bank Structure Variables						
Board Size	3,011	11.82	3.442	6	11	22
Board Independence	3,011	0.780	0.120	0.438	0.800	0.933
CEO Tenure (Years)	3,011	6.63	5.988	0	4.9	25.9
Duality	3,011	0.496	0.500	0	0	1
Reg by FED	3,011	0.217	0.412	0	0	1

Reg by FDIC	3,011	0.493	0.500	0	0	1
Reg by OCC	3,011	0.290	0.454	0	0	1
State Economic Variables						
ΔState GDP	2,998	0.038	0.049	-0.043	0.0385	0.1
ΔState Housing Index	2,998	0.021	0.073	-0.222	0.009	0.253
ΔState Unemployment	3,011	0.247	1.303	-1.5	-0.199	4.5

Panel B: Connection Statistics	bank-years (% of total bank-years)	Max # connected dir.	# bank-years with connected director 1	2	3	4	5
Connection Type							
At least 1 Public Service dir.	870 (28.9%)	4	688	145	46	11	0
At least 1 Current Public Service dir.	380 (12.6%)	2	360	20	0	0	0
Public Service Regulatory Agency							
At least 1 Fed Public Service dir.	822 (27.3%)	4	629	141	43	9	0
At least 1 FDIC Public Service dir.	5 (0.17%)	1	5	0	0	0	0
At least 1 SEC Public Service dir.	27 (0.9%)	2	26	1	0	0	0
At least 1 State Public Service dir.	52 (1.7%)	1	52	0	0	0	0

Panel C: Public Service Positions

Regulatory Agency	Position	Position Served in:
Federal Reserve	Director	Federal Reserve Bank Board of directors
Federal Reserve	Advisor	Federal Advisory Council, New England Advisory Council, Community Depository Institution Advisory Council, Business and Community Advisory Council, Industry Councils Committee, Economic Advisory Council, Business Advisory Council, Small Business Advisory Council, Small Business and Agriculture Advisory Council, US Treasury and the Foreign Exchange Committee, International Advisory Committee, Investors Advisory Committee on Financial Markets, Community Depository Advisory Council, Community Bank Advisory Council, Small Bank Advisory Council, Labor, Education and Healthcare Advisory Council, Agriculture, Small Business and Labor Advisory Council, Thrift Institution Advisory Council, Consumer Advisory Council
FDIC	Advisor	Advisory Committee on Economic Inclusion, Advisory Committee on Community Banking
SEC	Advisor	Market Oversight and Financial Services Advisory Committee, Consumer Affairs Advisory Committee, Advisory Committee on Smaller Public Companies
State	Director	Board of Directors
State	Advisor	Bankers Advisory Board, Commissioner's Council

I next match the list of BoardEx banks to 4th Quarter FR Y9-C consolidated accounting information reported by banks to the Federal Reserve. Market information is obtained from CRSP. The final sample contains 3,011 bank-year observations consisting of 448 unique banks. Definitions of the variables used in this chapter are described in Appendix 2-A1. Summary statistics of the variables are reported in Table 2-1. The mean $\Delta\sigma_v$ and ΔIPP is -0.17% and 0.049% respectively.

2.3.2 Bank Regulatory Connections

The main proxy of a bank's regulatory connectedness is the proportion of board members who are currently serving or have previously served the Fed, FDIC, OCC, OTS, SEC or state regulators in a public service capacity (*Public Service*). I include previous as well as current connections because the potential benefits of connections —personal familiarity with regulators— outlast the period during which connections exist. Parts of the analysis are based on currently active positions only, and the results are similar.

I define public service positions as positions that are held as a form of public service (and not as a full-time occupation). They consist of directorship positions on the boards of Fed Banks or membership in various advisory councils in either the Fed system, the FDIC, OCC, OTS, SEC or state regulators. Information on each director's employment history is from BoardEx. I manually supplement missing information from regulators' annual reports, legal documents, LinkedIn, Marquis Who's Who and news sources such as Bloomberg, the Wall Street Journal and the Financial Times.

Panel B of Table 2-1 show some descriptive statistics for the sample of connected banks. In total, almost 30% (28.9%) of bank-years in the sample have directors with formal public service connections to regulators. Additionally, connected directors are not limited to a small subset of banks; 135 out of 448 banks have at least one director with public service connections to regulatory agencies in the sample. The majority (91%) of public service positions held by directors in the sample are with the Fed and typically involve Fed board directorships and advisory roles on various councils. Public service connections between banks and regulators, which are the focus of this chapter, are widespread.

2.3.3 Control Variables

The control variables correspond to the CAMELS ratings system. CAMELS —an acronym for capital adequacy, asset quality, management quality, earnings, liquidity and sensitivity to market risk— is a composite supervisory rating system used by bank regulators to assess the safety and soundness of a bank.[35] Bank safety and soundness are likely to affect the gains from risk-shifting, because banks in poor financial condition may either have more to gain (Eisdorfer, 2008; Bushman and Williams, 2012) or, alternatively, these banks may receive more regulatory attention and thus be less able to shift risk. As CAMELS ratings are confidential, I employ proxies for each component.

I measure capital adequacy using *Tier-1 Capital* and proxy for asset quality using *Bad Loans*. Enforcement actions proxy for management quality (Duchin and Sosyura, 2012). Enforcement actions (i.e., Formal Agreements, Cease and Desist Orders, Prompt Corrective Actions and Civil Money Penalties) are collected from

[35] See https://www.fdic.gov/regulations/examinations/

websites of the three main federal regulators.[36] When enforcement actions are issued to the commercial bank, I attribute it to the holding company. I control for earnings and liquidity using *ROA* and *Total Deposits*, respectively. I proxy for *Market Risk* using the gap between short-term assets and short term-liabilities scaled by total assets. The gap approximates the net amount of assets and liabilities that will be repriced within one year, reflecting the bank's sensitivity to interest rate risk. Further, I control for firm size using the log of *Total Assets*. Carbo-Valverde et al. (2013) show that too-big-to-fail banks are able to access larger public subsidies. *Asset Growth* and *Total Loans* measure the aggressiveness of expansion and the lending focus of a bank, which could both affect risk.

Finally, I include as controls a number of board level corporate governance variables that could influence bank risk policies. I proxy for the effectiveness of monitoring and advisory functions of the board using *Board Size* and *Board Independence* (e.g., Adams and Mehran, 2012; Minton, Taillard, and Williamson, 2014). Finally, I also control for the power of a CEO in affecting risk policies using *CEO Tenure* and *Duality*, a dummy variable that equals 1 if the CEO is also the Chairman of the board (e.g., Ellul and Yerramilli, 2013).

2.4 Baseline Results

2.4.1 Connections and the Risk-leverage Sensitivity

Panel A of Table 2-2 show how public service connections impact the risk-leverage sensitivity of banks (Hypothesis 1). Panel B investigates the same for the sensitivity of Tier-1 capital to risk. By definition, regulatory Tier-1 capital is meant to

[36] Federal Reserve: http://www.federalreserve.gov/, FDIC: https://www5.fdic.gov/edo/ and OCC: http://apps.occ.gov/EnforcementActions/

be risk sensitive. If connected banks were to receive preferential treatment by supervisors, I should expect decreases in leverage (and increases in Tier-1 capital) to be less sensitive to risk increases for connected banks as compared to banks without connections. Columns (1)-(4) of both panels in Table 2-2 estimate a baseline model, with different specifications. Column (5) report results for only banks with at least one public service director during the sample period.

The coefficient on the main interaction term of interest *Public Service x* $\Delta\sigma_v$ is positive and statistically significant at the 5% level across different specifications used in Columns (1)-(5) of Panel A Table 2-2. This shows that, as the proportion of public service connections increases, connected banks reduce leverage less for a given level of asset risk increase compared to non-connected banks.

The results are similar when investigating the sensitivity of Tier-1 capital to risk at connected banks as reported in Panel B of Table 2-2. The coefficient on *Public Service x* $\Delta\sigma_v$ is negative and statistically significant at the 5% significance level or lower. Thus, a given change in risk is associated with a smaller change in Tier-1 capital at banks with public service connections as compared to non-connected banks.

The results are economically meaningful. For instance, in Column (4) Panel B, a one standard deviation increase in the proportion of the board with public service experience (the equivalent of a 5% increase) decreases the sensitivity of Tier-1 capital to changes in risk by 32% at the mean.[37] Alternatively, the addition of a single connected director to an average-sized board of 12 directors decreases the sensitivity

[37] I calculate this figure using coefficient estimates from Column (4) Panel B of Table 2-2 and the corresponding means and standard deviation in the summary statistics. At the mean *Public Service* (0.0306), Δ*Tier-1* increases by 0.11% [(0.129)-(0.623*0.0306)] and by 0.075% [(0.129)-(0.623*0.086)] in banks with high *Public Service* (one standard deviation above the mean, which is equal to

Table 2-2: Sensitivity of $\Delta B/V$ and $\Delta Tier\text{-}1$ to $\Delta\sigma_v$ at Connected Banks

This table reports estimates of Equation (2-5) using panel OLS regressions (with different specifications) and examines the sensitivity of changes in leverage (*B/V* in Panel A) and Tier-1 Capital (*Tier-1* in Panel B*)* to changes in σ_v at connected banks. I estimate the following regression:

$$\Delta Y_{i,t} = \alpha_0 + \alpha_1 \Delta\sigma_{Vi,t} + \alpha_2(Public\ Service_{i,t}\ x\ \Delta\sigma_{Vi,t}) + \alpha_3 Public\ Service_{i,t} + Bank\ Controls_{i,t} + Year\ FE + Regulator\ FE + \varepsilon_{i,t}$$

where subscripts *i* and *t* indicate bank and year respectively. *Y* is (*B/V*)*,* defined as the book value of leverage divided by market value of assets in Panel A and (*Tier-1*), defined as (Tier-1 Capital/Risk-weighted assets) in Panel B. *Public Service* is defined as: (*number of Public Service directors/board size*). σ_v is the volatility of asset returns. *Bank Controls* is the vector of variables in each column. The coefficient α_2 on (*Public Service x* $\Delta\sigma_v$) is the variable of interest. Refer to Appendix 2-A1 for description of variables. The sample period is from 2001 to 2013. Standard errors are clustered at the bank-level and t-statistics are reported in parenthesis. The constant is suppressed for brevity. ***, ** and * indicate significance level at the 1, 5 and 10% respectively.

Panel A: ΔLeverage	(1) ΔB/V	(2) ΔB/V	(3) ΔB/V	(4) ΔB/V	(5) ΔB/V
Public Service x $\Delta\sigma_v$	2.477**	2.437**	2.532**	2.371**	3.431**
	[2.000]	[2.096]	[2.088]	[2.040]	[2.406]
Public Service	0.949	3.344	0.277	1.119	1.49
	[1.120]	[1.345]	[0.323]	[0.521]	[0.643]
$\Delta\sigma_v$	-0.157	-0.113	-0.15	-0.097	-0.266
	[-1.457]	[-1.123]	[-1.411]	[-1.011]	[-1.564]
Tier-1 Capital			-7.501	-18.716***	-25.460***
			[-1.303]	[-4.461]	[-3.999]
Bad Loans			19.883**	26.883***	34.775*
			[2.507]	[2.714]	[1.758]
Lag Enforcement Actions			0.434***	0.335*	0.504**
			[2.600]	[1.926]	[2.525]
ROA			-38.815***	-39.634***	-30.91
			[-3.541]	[-3.402]	[-1.233]
Total Deposits			-0.898	-1.252	0.055
			[-1.149]	[-0.753]	[0.021]
Market Risk			-0.336	-1.06	-1.617
			[-1.129]	[-1.603]	[-1.500]
Total Assets			0.008	4.631***	3.311**
			[0.071]	[5.066]	[2.391]
Asset Growth			3.344***	3.059***	2.609***
			[6.688]	[7.221]	[2.910]
Total Loans			-0.439	0.813	1.345
			[-0.548]	[0.628]	[0.599]
Board Size			0.009	0.013	0.038
			[0.625]	[0.368]	[0.739]
Board Independence			0.46	-1.368	-0.656
			[0.903]	[-1.366]	[-0.395]
CEO Tenure			0.011	0.027	0.032
			[1.090]	[1.640]	[1.276]
Duality			0.211*	0.373*	0.444
			[1.848]	[1.693]	[1.381]
Bank FE	No	Yes	No	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Regulator FE	Yes	Yes	Yes	Yes	Yes

0.0306+0.055=0.086). Thus, banks with high public service connections are able to decrease the sensitivity of risk increases by (0.075-0.11)/(0.11) = -32%.

Observations	3,011	3,011	3,011	3,011	1,175
Adj. R-squared	0.368	0.371	0.429	0.451	0.457
Observations	3,011	3,011	3,011	3,011	1,175

Panel B: ΔTier-1 Capital	(1) ΔTier-1	(2) ΔTier-1	(3) ΔTier-1	(4) ΔTier-1	(5) ΔTier-1
Public Service x $\Delta\sigma_v$	-0.831**	-0.788**	-0.841**	-0.623***	-1.223***
	[-1.983]	[-1.979]	[-2.275]	[-2.710]	[-3.058]
Public Service	0.389	-3.734	0.01	-2.164	-1.985
	[0.605]	[-1.532]	[0.018]	[-1.557]	[-1.483]
$\Delta\sigma_v$	0.181***	0.152***	0.176***	0.129***	0.215***
	[4.475]	[4.919]	[4.264]	[5.834]	[3.181]
Tier-1 Capital			13.882	53.335***	63.183***
			[1.258]	[8.002]	[6.785]
Bad Loans			1.073	-6.12	-8.904
			[0.111]	[-0.950]	[-0.627]
Lag Enforcement Actions			-0.08	-0.022	0.014
			[-0.878]	[-0.322]	[0.167]
ROA			38.361***	15.327**	6.563
			[2.816]	[2.088]	[0.418]
Total Deposits			0.975	-1.988	-5.292**
			[0.835]	[-1.159]	[-2.179]
Market Risk			0.309	0.354	0.069
			[1.192]	[0.851]	[0.109]
Total Assets			0.330***	-1.321*	-0.92
			[2.938]	[-1.710]	[-0.898]
Asset Growth			-2.715***	-1.948***	-1.343**
			[-4.271]	[-6.022]	[-2.531]
Total Loans			0.696	1.488	1.805
			[0.492]	[1.389]	[1.097]
Board Size			0.004	0.001	-0.034
			[0.346]	[0.031]	[-0.895]
Board Independence			-0.398	0.188	0.962
			[-1.022]	[0.281]	[1.060]
CEO Tenure			-0.004	-0.006	-0.027
			[-0.473]	[-0.541]	[-1.606]
Duality			-0.256**	0.109	0.512**
			[-2.402]	[0.720]	[2.378]
Bank FE	No	Yes	No	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Regulator FE	Yes	Yes	Yes	Yes	Yes
Adj. R-squared	0.0746	0.0725	0.202	0.428	0.571
Observations	3,011	3,011	3,011	3,011	1,175

of Tier-1 capital to risk by 39%. This supports Hypothesis 1 and show that banks with public service connections are able to engage in capital arbitrage compared to non-connected banks by holding less capital as asset risk increases.

2.4.2 Regulatory Connections and Risk-shifting

As outlined in Section 2.2, a lower sensitivity of risk to leverage is a necessary but insufficient condition for banks with public service connections to extract subsidies from the financial safety-net. For banks to be able to shift risk to the safety-net via regulatory connections, they need to be able to increase the value of the taxpayer put.

Table 2-3 reports results from estimating Equation (2-6) (Hypothesis 2). The coefficient on $\Delta\sigma_v$ is positive and significant at the 1% level in all models (Columns (1)-(5)). This means that by increasing asset risk, banks are able to extract larger public subsidies from the safety-net. The key coefficient of interest, on *Public Service x* $\Delta\sigma_v$, is positive and statistically significant at the 1% to 5% level depending on the specifications used in Columns (1)-(5). Column (5) only use the sample of banks with at least 1 public service director. The economic significance is sizable. For instance, a one standard deviation (0.055) increase in public service connections from the mean increases the gain from extracting safety-net benefits by 22% in Column (4).[38] An increase in connectedness of a single board member at an average-sized board increases the gain by 36%.

[38] I calculate this figure using coefficient estimates from Column (4) of Table 2-3 and the corresponding means and standard deviation in the summary statistics. At the mean *Public Service* (0.0306), ΔIPP increases by 0.313% [(0.275)+(1.236*0.0306)] and by 0.381% [(0.275)+(1.236*0.086)] in banks with high (one standard deviation above the mean, which is equal to 0.0306+0.055=0.086). Thus, banks with high public service connections are able to increase risk-shifting to the safety-net by (0.381-0.313)/(0.313) = 21.7%

Table 2-3: Sensitivity of Δ*IPP* to $\Delta\sigma_v$ at Connected Banks

This table reports estimates of Equation (2-6) using panel OLS regressions (with different specifications) and examines the sensitivity of changes in the value of public subsidies (*IPP)* to changes in σ_v at connected banks. I estimate the following regression:

$$\Delta(IPP)_{i,t} = \beta_0 + \beta_1 \Delta\sigma_{Vi,t} + \beta_2(Public\ Service_{i,t}\ x\ \Delta\sigma_{Vi,t}) + \beta_3 Public\ Service_{i,t} + Bank\ Controls_{i,t} + Year\ FE + Regulator\ FE + \varepsilon_{i,t}.$$

where subscripts *i* and *t* indicate bank and year respectively. *IPP* is the fair value of the deposit insurance premium. *Public Service* is defined as: (*number of Public Service directors/board size*). σ_v is the volatility of asset returns. *Bank Controls* is the vector of variables in each column. The coefficient β_2 on (*Public Service x* $\Delta\sigma_v$) is the variable of interest. Refer to Appendix 2-A1 for description of variables. The sample period is from 2001 to 2013. Standard errors are clustered at the bank-level and t-statistics are reported in parenthesis. The constant is suppressed for brevity. ***, ** and * indicate significance level at the 1, 5 and 10% respectively.

	(1) ΔIPP	(2) ΔIPP	(3) ΔIPP	(4) ΔIPP	(5) ΔIPP
Public Service x $\Delta\sigma_v$	1.247**	1.227**	1.276**	1.236**	2.021***
	[2.150]	[2.146]	[2.246]	[2.188]	[2.861]
Public Service	0.075	0.205	-0.059	-0.143	-0.166
	[0.329]	[0.299]	[-0.257]	[-0.219]	[-0.263]
$\Delta\sigma_v$	0.262***	0.273***	0.263***	0.275***	0.156*
	[5.775]	[6.130]	[5.871]	[6.337]	[1.746]
Tier-1 Capital			0.227	-1.102	-0.25
			[0.247]	[-0.779]	[-0.134]
Bad Loans			8.391***	13.225***	12.535**
			[4.372]	[4.746]	[1.992]
Lag Enforcement Actions			0.270***	0.245***	0.168*
			[4.160]	[3.481]	[1.712]
ROA			-7.754**	-7.775*	-7.117
			[-2.265]	[-1.725]	[-0.882]
Total Deposits			-0.036	-0.037	-0.783
			[-0.244]	[-0.079]	[-1.260]
Market Risk			-0.056	-0.117	-0.168
			[-0.817]	[-0.691]	[-0.793]
Total Assets			0.008	0.799***	0.348
			[0.341]	[3.487]	[1.120]
Asset Growth			0.211***	0.096	0.054
			[2.856]	[1.027]	[0.436]
Total Loans			0.009	0.076	0.465
			[0.059]	[0.217]	[0.885]
Board Size			0.006*	0.020**	0.012
			[1.913]	[2.084]	[0.918]
Board Independence			0.083	-0.227	0.087
			[0.798]	[-0.845]	[0.214]
CEO Tenure			0.004*	0.008	0.001
			[1.670]	[1.334]	[0.195]
Duality			0.064***	0.169**	0.246**
			[2.619]	[2.119]	[2.087]
Bank FE	No	Yes	No	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Regulator FE	Yes	Yes	Yes	Yes	Yes
Adj. R-squared	0.562	0.583	0.601	0.624	0.671
Observations	3,011	3,011	3,011	3,011	1,175

The potential to extract higher public subsidies is achieved through the risk-leverage mechanism shown in Section 2.2. Subsequently, regulatory efforts to impose discipline on banks by making them hold additional capital against increasing asset risk are less effective in connected banks.

2.5 Endogeneity

The empirical results so far show a relationship between connections and the extraction of public subsidies by connected banks. However, I recognize that the baseline results could be endogenous. In this section, I address the different potential sources of endogeneity, specifically time-invariant and variant omitted variable bias and reverse causality.

2.5.1 Time-invariant Omitted Variables

Unobserved time-invariant omitted variables could confound causal inference between connections and risk-shifting when such variables correlate with both public service connections and risk-shifting. For instance, a particular risk culture might simultaneously attract directors with regulatory connections, and be related to risk-shifting. The results in Section 4 are robust to the inclusion of bank fixed effects (Columns (2), (4), and (5) of Table 2-3) which helps alleviate concerns pertaining to time-invariant omitted variables. Additionally, I include year fixed effects in all specifications. The latter absorb time related trends that could influence both connections and risk-shifting, such as the business cycle.

I further reduce concerns related to time-invariant omitted variables by running the baseline regressions (Equations (2-5) and (2-6) with bank fixed effects) on a sample of banks that have at least one public service director during the sample period

(Columns (5) of Tables 2-2 and 2-3). By excluding banks that do not have any public service directors, I estimate the *within*-connected-bank variation of public service connections on gains from risk-shifting. This ameliorates concerns that unobservable time-invariant differences between banks with and without connections are driving my results.

2.5.2 Identification: Retirements of Federal Reserve Presidents

Time-variant unobservable variables could jointly affect both connections and risk-shifting. For example, a change in the business strategy of the bank could simultaneously increase risk-shifting and regulatory connectedness if connections are established to give the appearance of regulatory approval of the bank's activities.[39] To help establish causality, I exploit a quasi-natural experiment in a difference-in-difference (DiD) setting, the retirements of Federal Reserve Presidents, as exogenous shocks to the quality of existing bank connections. The retirements of Fed Presidents should decrease the ability of connected banks to shift risk. Fed Presidents are arguably the single most powerful Fed officer and if personal familiarity between Fed Presidents gives rise to preferential treatment, the quality of these connections will be different and less conducive to risk-shifting following the retirement.

I therefore predict that banks with connections will experience a decrease in gains from risk-shifting if the President of the Fed district that they belong to retires, as compared to a control group of banks without connections that are located in the same Federal Reserve district. I rely on within-district variation to minimize the

[39] Reverse causality would not necessarily be inconsistent with my explanations. Banks that engage in risk-shifting could establish regulatory connections to attract regulatory leniency. However, this explanation would be consistent with my interpretation of preferential treatment from regulators at connected banks, and need not bias my results.

possibility that geographical economic shocks could influence risk-shifting and bias the results. The following example illustrates my empirical approach. Consider two banks (Bank A and Bank B) both supervised by the Richmond Fed under President Broaddus. Bank A has board members that served the Richmond Fed as a form of public service during Broaddus' tenure while Bank B does not. Broaddus retired in 2004, causing a negative shock to the quality of Bank A's connections to the Richmond Fed. I compare risk-shifting gains of the connected Bank A (the treatment bank) before and after the President's retirement to the non-connected Bank B (the control bank). The control bank absorbs changes associated with the retirement of the Fed President (if any) and allows me to investigate if a decrease in the efficacy of connections to the President leads to a reduction in risk-shifting gains at Bank A.

The DiD strategy has several characteristics that aid in the inference of a causal effect. The cross-sectional comparison between treated and control banks avoids the problem of omitted trends while the time-series difference alleviates issues related to unobserved differences between the treated and control groups before and after the event (Roberts and Whited, 2012); i.e., I am comparing risk-shifting of treated banks after the shock (to itself, before the shock) and to a group of control banks. An additional advantage of this identification strategy is that there are multiple retirements (11 in total) affecting different banks that reside in different Federal Reserve districts across time. This alleviates concerns that omitted variables coinciding with a single retirement are correlated with risk-shifting (Atanasov and Black, 2016).

I begin by identifying the years in which Federal Reserve Presidents retired from their positions during the sample period from Federal Reserve documents and their corresponding websites. I identify 11 Fed President retirements (listed in

Appendix 2-A3).[40] To ascertain the exogeneity of the shock, I conduct searches on Factiva and various news websites to confirm that the retirements are not related to Fed Presidents being seen as too close to banks or risk-shifting (and thus possibly linked to risk-shifting). I use a five-year window (t-2, t-1, t, t+1, t+2) around the year the Fed President retires (year t). Years' t-2 and t-1 are the pre-shock years while years t+1 and t+2 are the post-shock years. The DiD strategy requires me to identify the treatment and control groups.

I allocate banks to the treatment group (*Treated Public Service* = 1) if they have at least one director with public service experience throughout the 5-year DiD window surrounding the shock and if the bank is located in the Fed district that the President is retiring from. Additionally, the public service director must have served alongside the retiring President and thus have/had an opportunity to establish connections. The group of control banks (*Treated Public Service* = 0) are banks which are in the same district of the outgoing Fed President but do not have any directors with regulatory connections in the 5-year window.

I show summary statistics for both the treated and control groups in t-1 in Panel A of Table 2-4. In total, there are 34 banks in the *Treated Public Service* group and 81 banks in the control group. Importantly, the mean of ΔIPP, the dependent variable of interest, is statistically insignificant between treated and control banks. This satisfies the parallel trends assumption which requires trends in outcomes (ΔIPP) to be similar for both the control and treatment groups prior to the shock. I further observe that there

[40] The original sample consists of 14 retirements. I exclude Timothy Geithner (New York Fed President in 2008 who later took up the position of Secretary of the Treasury), Janet Yellen (San Francisco Fed President in 2010 who took up positions as Vice Chairman, then Chairman of the Board of Governors of the Federal Reserve System) and Thomas Hoenig (Kansas City Fed President in 2011 who later become Vice Chairman of the FDIC) as they constitute "reassignments" within regulatory agencies.

are covariate differences between treated and control groups prior to the shock. Covariate imbalances could lead to biasness in the estimations as they could produce heterogeneous effects of the shock to the treatment and control group. I address this issue later in the robustness tests.

I perform the DiD tests using different variants of the following model:

$$\Delta IPP_{i,k,t} = \alpha_0 + \beta_1 \Delta\sigma_{Vi,k,t} + \beta_2 \textit{Treated Public Service}_{i,k,t} + \beta_3 Post_{k,t} + \beta_4 \textit{Treated Public Service}_{i,k,t} \times Post_{k,t} + \beta_5 \textit{Treated Public Service}_{i,k,t} \times \Delta\sigma_{Vi,k,t} + \beta_6 Post_{k,t} \times \Delta\sigma_{Vi,k,t} + \beta_7 \textit{Treated Public Service}_{i,k,t} \times Post_{k,t} \times \Delta\sigma_{Vi,k,t} + \textit{Bank Controls} + \textit{Year FE} + \textit{Regulator FE} + \varepsilon_{i,t} \quad (2\text{-}7)$$

where k indexes Fed districts, and *Post* is a dummy variable that equals 0 for t_{-2}, t_{-1}, t and 1 for t_{+1} and t_{+2} by Fed district. The main variable of interest is the triple interaction term *Treated Public Service x Post x* $\Delta\sigma_V$ which shows how the marginal effects of Fed retirements on ΔIPP at connected treated banks (relative to the non-connected control banks) varies with $\Delta\sigma_V$.

Columns (1)-(4) of Table 2-4 Panel B show the results for the DiD analysis. The coefficient on *Treated Public Service x Post x* $\Delta\sigma_V$ is consistently negative and statistically significant at the 1% level throughout different specifications (inclusion of baseline controls and bank fixed effects). The results show gains from risk-shifting decrease at connected banks relative to the group of non-connected control banks, after the retirement of Federal Reserve Presidents. This is consistent with my predictions that connections facilitate risk-shifting and that a negative shock to the efficacy of existing connections reduces opportunities to shift risk.

Next, I discuss the exogeneity of Fed President retirements as well as run a number of placebo and robustness tests for the DiD analysis. The results are presented in Panel C of Table 2-4.

Dynamic timing effects: I ascertain that the treatment effects of the DiD estimation are due to retirements as opposed to some other omitted variable or events that might coincide with the timing of the shock in Column (1) of Panel C Table 2-5. I follow Bertrand and Mullainathan (2003) and replace the *Post* variable with four indicator variables (Shock_{t-1}, $Shock_t$, $Shock_{t+1}$, $Shock_{t+2}$). These variables equal 1 for the year before retirement, the year of retirement, the year following retirement and two years after retirement (and 0 otherwise). I interact the variables with *Public Service x* $\Delta\sigma_V$. As observed, the reduction in risk-shifting gains at connected banks is only detectable in the second year after the Fed President's retirement (significant at the 1% level). This suggests that a reduction in the gains to risk-shifting at connected banks manifests itself only after the shock, and with a short delay, as banks adjust their asset risk and capital structure following a retirement.

Placebo test: I maintain the actual timing of retirements in the DiD analysis but randomly assign these shocks to banks. For example, if the data indicates that a shock occurred in 2003 in Fed Districts 2 and 4, I now randomly allocate banks located in Fed districts that did not experience a shock to be shocked. If unobservable shocks occurring in the year of the actual shock are correlated with the actual timing of the shock, I should observe similar results to those in Panel B of Table 2-4 (as these unobservable shocks would still reside in the DiD framework and drive my results).[41]

[41] However, this scenario is unlikely as my research design uses multiple shocks in different geographic markets and years. For the inference to be biased there would have to be a time-varying omitted variable

Table 2-4: DiD Analysis: Fed Reserve Bank Retirements as Shocks to Connections

This table reports summary statistics (Panel A), estimates (Panel B), placebo and robustness tests (Panel C) of the difference-in-difference (DiD) estimation (with different specifications) as described in Equation (2-7) and examines the sensitivity of changes in the value of public subsidies (*IPP*) to changes in σ_v at treated banks as compared to a group of control banks following shocks to the efficacy of public service connections. The DiD analysis is carried out over a 5-year window (*t-2, t-1, t, t+1 and t+2)* where *t* is the year of Fed President retirements (11 shocks). *Post* is a dummy variable that = 1 for years *t+1 and t+2* and 0 otherwise. I define banks to be in the treatment group (*Treated Public Service* = 1) if banks: 1) have at least one public director with public service experience throughout the 5-year DiD window and; 2) the public service position held by the director overlaps with the tenure and Fed district of the outgoing Federal Reserve President. Banks in the control group (*Treated Public Service* = 0) are banks that do not have any directors with public service experience in the 5-year DiD window and are in the district of the outgoing Federal Reserve President. Panel A reports difference in means (and p-values) of the characteristics of treated and control banks in the pre-shock (*t-1*) year. Panel B presents estimates of the DiD regression:

$$\Delta IPP_{i,k,t} = \alpha_0 + \beta_1 \Delta\sigma_{Vi,k,t} + \beta_2 Treated\ Public\ Service_{i,k,t} + \beta_3 Post_{k,t} + \beta_4\, Treated\ Public\ Service_{i,k,t}\ x\ Post_{k,t} + \beta_5 Treated\ Public\ Service_{i,k,t}\ x\ \Delta\sigma_{Vi,k,t} + \beta_6 Post_{k,t}\ x\ \Delta\sigma_{Vi,k,t} + \beta_7 Treated\ Public\ Service_{i,k,t}\ x\ Post_{k,t}\ x\ \Delta\sigma_{Vi,k,t} + Bank\ Controls_{i,t} + Year\ FE + Regulator\ FE + \varepsilon_{i,t}$$

where subscripts *i, k* and *t* indexes bank, Fed district and year respectively. *IPP* is the fair value of the deposit insurance premium, σ_v is the volatility of asset returns while *Bank Controls* is the vector of variables in each column. The variable of interest is the coefficient β_7 on (*Treated Public service* x *Post* x $\Delta\sigma_v$). Refer to Appendix 2-A1 for a description of variables. The sample period is from 2001 to 2013. Standard errors are clustered at the bank-level and t-statistics are reported in parenthesis. The constant is suppressed for brevity. ***, ** and * indicate significance level at the 1, 5 and 10% respectively.

Panel A: Diagnostics	Pre-Shock Treated		Pre-Shock Control		Diff. in Means
Variables	# Treated	Mean	# Control	Mean	p(Treated-Control)
ΔIPP	34	0.0100	81	0.00200	0.00800
$\Delta\sigma_v$	34	-0.184	81	-0.12	-0.0680
Tier1 Capital	34	0.108	81	0.113	-0.005
Bad Loans	34	0.00600	81	0.00500	0
Lag Enforcement Actions	34	0.147	81	0.0370	0.110*
ROA	34	0.0130	81	0.0100	0.003***
Total Deposits	34	0.731	81	0.746	-0.0150
Market Risk	34	0.172	81	0.102	0.070**
Total Assets	34	6.966	81	6.416	0.550***
Asset Growth	34	0.0890	81	0.135	-0.046*
Total Loans	34	0.643	81	0.703	-0.060**
Board Size	34	14	81	12.07	1.926**
Board Independence	34	0.781	81	0.765	0.0160
CEO Tenure	34	5.75	81	7.277	-1.527
Duality	34	0.765	81	0.556	0.209**

related to the timing of each retirement that only affects connected banks. Nonetheless, I formally address this issue.

Panel B: DiD Analysis	(1) ΔIPP	(2) ΔIPP	(4) ΔIPP	(4) ΔIPP
Treated Public Service x Post x $\Delta\sigma_v$	-0.327***	-0.326***	-0.280***	-0.287***
	[-3.181]	[-3.008]	[-3.085]	[-2.975]
Treated Public Service x Post	-0.128	-0.126	-0.039	-0.104
	[-1.640]	[-1.656]	[-0.700]	[-1.645]
Treated Public Service x $\Delta\sigma_v$	-0.001	0.013	-0.003	0.005
	[-0.081]	[0.552]	[-0.150]	[0.211]
Treated Public Service	-0.003		0.013	
	[-0.093]		[0.417]	
Post x $\Delta\sigma_v$	0.336***	0.329***	0.300***	0.319***
	[3.346]	[3.253]	[3.574]	[3.649]
Post	0.099	0.088	0.035	0.084
	[1.622]	[0.956]	[0.727]	[1.011]
$\Delta\sigma_v$	0.018	0.011	0.030**	0.009
	[1.610]	[0.839]	[2.433]	[0.581]
Tier-1 Capital			2.249	6.089**
			[1.595]	[2.605]
Bad Loans			15.389**	15.367**
			[2.831]	[2.054]
Lag Enforcement Actions			0.033	-0.014
			[0.370]	[-0.128]
ROA			-25.83***	-32.90***
			[-3.224]	[-2.785]
Total Deposits			0.475*	0.932
			[1.970]	[1.182]
Market Risk			0.011	0.174
			[0.106]	[0.428]
Total Assets			0.107**	0.511
			[2.271]	[0.477]
Asset Growth			0.121	0.069
			[0.851]	[0.256]
Total Loans			0.027	1.439
			[0.118]	[1.305]
Board Size			-0.002	0.002
			[-0.469]	[0.100]
Board Independence			0.169	-0.432
			[1.034]	[-1.091]
CEO Tenure			0.003	-0.003
			[0.742]	[-0.554]
Duality			0.049	-0.165**
			[0.979]	[-2.113]
Bank FE	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes
Regulator FE	Yes	Yes	Yes	Yes
Adj. R-squared	0.42	0.43	0.612	0.597
Observations	575	575	575	575

Panel C: Placebo & Robustness	(1) Dynamic Timing ΔIPP	(2) Placebo Year ΔIPP	(3) Exclude Fed 1,3,4,11 ΔIPP	(4) Control Macro. ΔIPP	(5) Control Macro. ΔIPP	(6) Control Pre-shock ΔIPP
Treated Public Service x Post x $\Delta\sigma_v$		0.079	-0.341***	-0.215***	-0.203***	-0.274***
		[0.809]	[-3.221]	[-3.240]	[-3.190]	[-3.165]
Treated Public Service x $Shock_{t-1}$ x $\Delta\sigma_v$	0.056					
	[0.97]					
Treated Public Service x $Shock_t$ x $\Delta\sigma_v$	0.021					
	[0.37]					
Treated Public Service x $Shock_{t+1}$ x $\Delta\sigma_v$	-0.05					
	[-0.69]					
Treated Public Service x $Shock_{t+2}$ x $\Delta\sigma_v$	-0.343***					
	[-3.16]					
ΔState GDP x $\Delta\sigma_v$				2.985*	0.026	
				[1.826]	[0.051]	
ΔState Housing Index x $\Delta\sigma_v$				-0.634**	-0.389**	
				[-1.997]	[-2.063]	
ΔState Unemployment x $\Delta\sigma_v$				0.136***	0.008	
				[4.518]	[0.469]	
ΔState GDP x Post x $\Delta\sigma_v$					5.323*	
					[1.844]	
ΔState Housing Index x Post x $\Delta\sigma_v$					-0.852	
					[-1.229]	
ΔState Unemployment x Post x $\Delta\sigma_v$					0.163***	
					[3.449]	
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Regulator FE	Yes	Yes	Yes	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes	Yes	Yes	Yes
Other Controls x Treated x Post	No	No	No	No	No	Yes
Adj. R-squared	0.64	0.496	0.629	0.735	0.759	0.603
Observations	575	640	440	570	570	575

However, if no such omitted variables exist, the coefficient on *Public Service x Post x* $\Delta\sigma_v$ should become statistically insignificant. When repeating the DiD analysis with the placebo retirement year shocks in Column (2) of Panel C, the coefficient on the interaction term of interest *Public Service x Post x* $\Delta\sigma_v$ is statistically insignificant. This alleviates concerns that findings are driven by omitted variables relating to the timing of the shock.

Mandatory vs unexpected retirements: A natural question that arises relates to the reason for the retirements of Federal Reserve Presidents to satisfy the identifying assumption that retirements are not related to connected banks, risk-shifting or any time-varying omitted variables. As mentioned above, I read news reports pertaining to retirements and do not find any reason linking retirements to risk-shifting or being too close to banks. In addition, I use only retirements that are mandatory and, thus, planned. I exclude unplanned retirements as these could be dismissals dressed up as retirements (and be potentially related to regulatory effectiveness which would not appear in news articles and regulatory reports). Specifically, I exclude retirements occurring more than one year before the mandatory retirement age of 65, or less than ten years from an appointment that commenced after the age of 55.[42] I repeat the DiD analysis, dropping observations from Fed Districts 1, 3, 4 and 11 as these retirements constitute "unplanned" retirements, and obtain similar results (Column (3) of Panel C in Table 2-4).

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[42] "Reserve Bank Presidents are subject to mandatory retirement upon becoming 65 years of age. However, Presidents initially appointed after age 55 can, at the option of the board of directors, be permitted to serve until attaining ten years of service in the office or age 75, whichever comes first". https://www.federalreserve.gov/aboutthefed/bios/banks/default.htm

Macroeconomic conditions: I allow macroeconomic conditions to influence risk-shifting (by interacting three economic proxies, Δ*State GDP,* Δ*State Housing Index* and Δ*State Unemployment* with $\Delta\sigma_v$) in Column (4) of Panel C in Table 2-5 as macroeconomic conditions could influence the timing of Fed President retirements. Presidents could choose to retire in certain states of the economy. Thus, changes in the local economic environment might be correlated with risk-shifting at connected banks, instead of the shock. In Column (5) of Panel C Table 2-5, I interact the three economic proxies with $\Delta\sigma_v$ and *Post* to control for macroeconomic trends in the post-retirement period. As Fed Presidents have information about the forecasts of the economy, they could choose the timing of their retirements. By including the triple interaction term, I control for that possibility. The coefficient of the interaction term of interest *Public Service x Post x* $\Delta\sigma_v$ continues to remain statistically significant and negative at the 1% level in both these specifications.

Pre-shock covariate imbalances: I note in Panel A of Table 2-4 that the covariates are not balanced in the pre-shock period between the treatment and control groups. I would ideally like to conduct matching exercises to ensure that the treatment and control groups are similar along most observable characteristics in the pre-shock period to ensure that the shock does not produce heterogeneous treatment effects that arise from differences in bank characteristics. Unfortunately, due to the relatively small number of potential control banks in the sample, I am unable to do so. I take a different approach and interact all the control variables with *Treated Public Service x Post* in Column (6) of Panel C in Table 2-4. By interacting the vector of covariates with *Treated x Post,* I control for the possibility that the covariates of treated banks produce heterogeneous effects to the shock as compared to the group of control banks.

My results remain statistically significant at the 1% level, consistent with estimates in the DiD analysis.

2.5.3 Alternative Identification: The Emergency Economic Stabilization Act of 2008

An alternative identification exploits the heterogeneous effects of the Emergency Economic Stabilization Act (EESA) of 2008 as a positive shock to the incentives of banks to risk shift. EESA was enacted in response to the recent financial crisis, the largest shock to the U.S. banking system to date (Fahlenbrach, Prilmeier, and Stulz, 2012). To stabilize the economy, the EESA contained various programs including the largest taxpayer-financed capital infusion in U.S. history (cf. Duchin and Sosyura, 2012; 2014), increased insurance coverage limits for depositors (cf. Lambert et al., 2016) as well as short-term lending to banks (cf. Berger, Black, Bouwman, and Dlugosz, 2015).

I hypothesize that following the enactment of EESA, connected banks would increase risk-shifting *more* than non-connected banks. The rationale underlying the identification strategy follows the well-documented moral hazard effect of public guarantees on increased bank risk-taking (Merton, 1977; Hovakimian and Kane, 2000; Gropp, Hakenes, and Schnabel, 2011; Dam and Koetter; 2012; Gropp, Kakenes, and Guettler, 2013). Importantly, the literature has demonstrated that EESA has indeed led to an increase in moral hazard and risk-taking at U.S. banks (Duchin and Soysura, 2014; Lambert et al., 2016). Additionally, EESA signalled the willingness of regulators to engage in forbearance during crisis times (Archarya and Yorulmazer, 2007; Brown and Dinç 2009). This leads me to predict that connected banks would engage in *more* risk-shifting as compared to non-connected banks in the presence of heightened regulatory forbearance.

EESA can be viewed as plausibly exogenous to public service connections for a number of reasons. Firstly, public service positions fulfil specific roles as determined by regulators and are taken up by bank directors periodically, independent of the timing of financial crises or specific pieces of legislation. Second, to further mitigate concerns that banks hire directors with public service experience in anticipation of the crisis and EESA, I use *Current Public Service,* the proportion of board members who *currently* hold public service positions with a regulator, as well as *Public Service*.[43]

I present summary statistics for *Public Service* and *Current Public Service* by year in Panel A of Table 2-5 and *t*-tests for the difference in means between these two variables during the pre-crisis and crisis period in Panel B to provide some evidence for argument two above. The mean value of *Current Public Service* remains constant throughout the sample period. For instance, 1% of board members have current public service connections in 2006-2008 compared with 1.1% in 2009 and 1% in 2010. I further show differences for *Public Service* and *Current Public Service* between the pre-crisis and crisis periods. There are no statistical differences between the means for both periods, presenting further evidence that banks do not attempt to hire in anticipation of the crisis. Lastly, connected banks could not have anticipated the magnitude of government support or the crisis.

[43] Fahlenbrach and Stulz (2011) show evidence that CEOs of banks whose incentives were better aligned with shareholders suffered larger losses in their compensation during the crisis. This suggests that bank CEOs were not able to anticipate the crisis.

I estimate variants of the following equation with and without bank fixed effects and baseline controls to test my hypothesis:

$$\Delta IPP_{i,t} = \alpha_0 + \beta_1 \Delta\sigma_{Vi,t} + \beta_2 Connected_{i,t} + \beta_3 Post\text{-}EESA_{08\text{-}13} + \beta_4 Connected_{i,t}\ x\ Post\text{-}EESA_{08\text{-}13} + \beta_5 Connected_{i,t}\ x\ \Delta\sigma_{Vi,t} + \beta_6 Post\text{-}EESA_{08\text{-}13}\ x\ \Delta\sigma_{Vi,t} + \beta_7 Connected_{i,t}\ x\ Post\text{-}EESA_{08\text{-}13}\ x\ \Delta\sigma_{Vi,t} + Bank\ Controls + Year\ FE + Regulator\ FE + \varepsilon_{i,t} \qquad (2\text{-}8)$$

where $Post\text{-}EESA_{08\text{-}13}$ is a dummy variable that equals 1 for years 2008 to 2013 and 0 otherwise, and *Connected* is either *Public Service* or *Current Public Service*. The main variables of interest are the triple interaction terms *Public Service x* $Post\text{-}EESA_{08\text{-}13}\ x\ \Delta\sigma_v$ and *Current Public Service x* $Post\text{-}EESA_{08\text{-}13}\ x\ \Delta\sigma_v$ which investigate if connected banks increase their gains from risk-shifting after EESA with respect to banks without connections.

The results of the estimation are shown in Panel C of Table 2-5. The first 4 columns show different specifications for the banks with public service connections while Columns (5)-(8) show banks with current public service connections. The coefficient on $Post\text{-}EESA_{08\text{-}13}\ x\ \Delta\sigma_v$ is positive and statistically significant at the 1% level in all columns, lending support to the moral hazard hypothesis of government subsidies. *Public Service x* $Post\text{-}EESA_{08\text{-}13}\ x\ \Delta\sigma_v$ and *Current Public Service x* $Post\text{-}EESA_{08\text{-}13}\ x\ \Delta\sigma_v$ are both positive and statistically significant at the 5% level or better, regardless of the specifications used. These results support the predictions that the regulatory climate of enhanced forbearance following EESA allowed greater gains from risk-shifting at connected banks relative to banks without connections.

Table 2-5: Heterogeneous Effects of EESA on Risk-shifting at Connected Banks

This table reports the annual mean % of *Public Service* and *Current Public Service* (Panel A), results of the t-test of the difference in means between these connection variables in the pre-crisis and crisis period (Panel B) and estimates of Equation (2-8) using panel OLS regressions (with different specifications) which examines the sensitivity of changes in the value of public subsidies (*IPP)* to changes in σ_v at connected banks (Panel C) following the enactment of the Emergency Economic Stabilization Act of 2008. I estimate the following regression:

$$\Delta(IPP)_{i,t} = \alpha_0 + \beta_1\Delta\sigma_{Vi,t} + \beta_2 Z_{i,t} + \beta_4 Post\text{-}EESA_t + \beta_5(Z_{i,t}\, x\, Post\text{-}EESA_t) + \beta_6(Z_{i,t}\, x\, \Delta\sigma_{Vi,t}) + \beta_7(Post\text{-}EESA_t\, x\, \Delta\sigma_{Vi,t}) + \beta_8(Z_{i,t}\, x\, Post\text{-}EESA_t\, x\Delta\sigma_{Vi,t}) + Bank\ Controls_{i,t} + Year\ FE + Regulator\ FE + \varepsilon_{i,t}.$$

where subscripts *i* and *t* indicate bank and year respectively. *IPP* is the fair value of the deposit insurance premium while *Post-EESA is* a dummy variable that equals 1 for years 2008-2013 and 0 otherwise. *Z* is either *Public Service* (*number of Public Service directors/board size*) or *Current Public Service* (*Number of Public Service directors who are currently holding public service roles/board size*). σ_v is the volatility of asset returns. *Bank Controls* is the vector of variables in each column and includes *Tier-1 Capital, Bad Loans, Lag Enforcement Actions, ROA, Total Deposits, Market Risk, Total Assets, Asset Growth, Total Loans, Board Size, Board Independence, CEO Tenure* and *Duality*. The variable of interest is the coefficient β_8 on (*Z x Post-EESA x* $\Delta\sigma_v$). Refer to Appendix 2-A1 for description of variables. The sample period is from 2001 to 2013. Standard errors are clustered at the bank-level and t-statistics are reported in parenthesis. The constant is suppressed for brevity. ***, ** and * indicate significance level at the 1, 5 and 10% respectively.

Panel A: Summary Statistics	Years													
	01	02	03	04	05	06	07	08	09	10	11	12	13	All
Mean (# Public Service Dir./Board Size) %	3.9	4.2	4.4	2.7	2.4	2.6	2.6	2.8	2.9	3.0	3.6	3.6	3.9	3.1
Mean (# Current Public Service Dir./Board Size) %	1.4	1.5	1.4	1.0	0.9	1.0	1.0	1.0	1.1	1.0	1.2	1.1	1.3	1.1

Panel B: Difference in Means	Pre-Crisis %	Crisis %	Diff in Means (Pre-Crisis - Crisis) %	Diff p-value
Public Service: Pre-Crisis (04-07) & crisis (08-10)	2.58	2.88	-0.30	0.20
Public Service: Pre-Crisis (05-07) & crisis (08-10)	2.55	2.88	-0.33	0.19
Public Service: Pre-Crisis (06-07) & crisis (08-10)	2.61	2.88	-0.27	0.35
Public Service: Pre-Crisis (06-07) & crisis (08-09)	2.61	2.83	-0.22	0.49
Public Service: Pre-Crisis (07) & crisis (08-09)	2.63	2.83	-0.20	0.61
Public Service: Pre-Crisis (07) & crisis (08)	2.63	2.76	-0.13	0.77
Public Service: Pre-Crisis (05-07) & crisis (08)	2.55	2.76	-0.21	0.55
Public Service: Pre-Crisis (06-07) & crisis (08)	2.61	2.76	-0.15	0.70
Current Public Service: Pre-Crisis (04-07) & crisis (08-10)	0.97	1.01	-0.04	0.73
Current Public Service: Pre-Crisis (05-07) & crisis (08-10)	0.97	1.01	-0.04	0.77

Current Public Service: Pre-Crisis (06-07) & crisis (08-10)	0.99	1.01	-0.02	0.88
Current Public Service: Pre-Crisis (06-07) & crisis (08-09)	0.99	1.01	-0.02	0.88
Current Public Service: Pre-Crisis (07) & crisis (08-09)	1.00	1.01	-0.01	0.96
Current Public Service: Pre-Crisis (07) & crisis (08)	1.00	0.95	0.05	0.83
Current Public Service: Pre-Crisis (04-07) & crisis (08)	0.97	0.95	0.02	0.94
Current Public Service: Pre-Crisis (05-07) & crisis (08)	0.97	0.95	0.02	0.92
Current Public Service: Pre-Crisis (06-07) & crisis (08)	0.99	0.95	0.04	0.86

Panel C: EESA 2008	(1) ΔIPP	(2) ΔIPP	(3) ΔIPP	(4) ΔIPP	(5) ΔIPP	(6) ΔIPP	(7) ΔIPP	(8) ΔIPP
Public Service x Post-$EESA_{08\text{-}13}$ x $\Delta\sigma_v$	1.300***	1.303***	1.307***	1.343***				
	[2.695]	[2.712]	[2.760]	[2.821]				
Current Public Service x Post-$EESA_{08\text{-}13}$ x $\Delta\sigma_v$					2.467***	2.466***	2.374**	2.391***
					[2.597]	[2.679]	[2.506]	[2.630]
Public Service x Post-$EESA_{08\text{-}13}$	-0.094	-0.660*	-0.043	-0.029				
	[-0.296]	[-1.714]	[-0.120]	[-0.066]				
Public Service x $\Delta\sigma_v$	-0.062**	-0.067*	-0.043	-0.1				
	[-2.032]	[-1.944]	[-0.733]	[-1.202]				
Public Service	0.032	0.942*	0.006	0.207				
	[0.740]	[1.869]	[0.063]	[0.391]				
Current Public Service x Post-$EESA_{08\text{-}13}$					0.701	0.385	0.721	1.254
					[1.034]	[0.393]	[0.921]	[1.223]
Current Public Service x $\Delta\sigma_v$					-0.131*	-0.078	-0.028	0.021
					[-1.730]	[-1.207]	[-0.209]	[0.111]
Current Public Service					0.009	0.839*	-0.1	-0.381
					[0.115]	[1.949]	[-0.525]	[-0.740]
Post-$EESA_{08\text{-}13}$ x $\Delta\sigma_v$	0.313***	0.318***	0.313***	0.314***	0.332***	0.337***	0.334***	0.335***
	[8.100]	[8.222]	[8.095]	[8.400]	[9.490]	[9.504]	[9.567]	[9.749]
Post-$EESA_{08\text{-}13}$	-0.262***	-0.183**	-0.287***	-0.438***	-0.274***	-0.201***	-0.287***	-0.440***
	[-4.736]	[-2.581]	[-4.241]	[-3.214]	[-5.196]	[-3.135]	[-4.478]	[-3.388]
$\Delta\sigma_v$	0.008***	0.009**	0.009**	0.014***	0.008***	0.008**	0.008**	0.011***
	[3.458]	[2.479]	[2.224]	[3.591]	[3.779]	[2.317]	[2.018]	[2.816]
Bank FE	No	Yes	No	Yes	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Regulator FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other Controls	No	No	Yes	Yes	No	No	Yes	Yes
Adj. R-squared	0.647	0.662	0.686	0.701	0.643	0.659	0.681	0.699
Observations	3,011	3,011	3,011	3,011	3,011	3,011	3,011	3,011

I present and discuss a number of robustness tests for my results and show them in Panels A (for *Public Service)* and B (for *Current Public Service*) of Appendix 2-A4, respectively.

TARP bailout funds: I control for additional risk-shifting incentives of banks that receive TARP bailout funds. Duchin and Soysura (2014) show that banks that receive bailout funds make riskier loans as compared to banks that did not receive these funds. I include an indicator variable, *TARP Dummy*, which equals 1 if years are from 2008-2013 and the bank receives bailout funds in 2008 and similarly, 1 if years are from 2009-2013 and the bank received bailout funds in 2009. I further interact *TARP Dummy* with $\Delta\sigma_v$. Estimates are shown in Columns (1) of Panel A and B in Appendix 2-A4. The coefficient on the interaction term *TARP Dummy x* $\Delta\sigma_v$ is positive and statistically significant at the 5% level indicating that gains from risk-shifting increase at banks that receive these funds, consistent with evidence of increased risk-taking by Duchin and Soysura (2014). Importantly, the variables of interest *Public Service x Post-EESA*$_{08\text{-}13}$ *x* $\Delta\sigma_v$ and *Current Public Service x Post-EESA*$_{08\text{-}13}$ *x* $\Delta\sigma_v$ remains positive and statistically significant at the 1% level.

Excluding too-big-to-fail: Large systematically important banks could receive regulatory laxity in the crisis due to their importance in the economy. I remove the largest 10% and 20% of banks (by assets) and re-run Equations (2-8) in Columns (2) and (3) of Panel A and B, respectively. The triple interaction terms of interest continue to remain positive and statistically significant at least at the 5% level.

Alternate post-EESA windows: I use different post-EESA windows (2008-2010), (2008-2011) and (2008-2012) in Columns (4)-(6) of Panel A and B in Appendix

2-A4 respectively for robustness. *Public Service x Post-EESA x* $\Delta\sigma_v$ and *Current Public Service x Post-EESA x* $\Delta\sigma_v$ continue to remain positive and statistically significant at the 1% level.

2.6 Do connected Banks Receive Preferential Treatment by Regulators?

The previous section presents evidence of a link between public service connections and risk-shifting. One explanation for my findings is that connected banks receive preferential treatment from regulators. An alternative explanation, which is partly consistent with the results so far, is that connected banks obtain the technical expertise and skills needed to evade regulatory discipline. In this section, I present evidence that backs the view that connected banks benefit from preferential treatment. I find no evidence that connected banks display superior skills in terms of evading regulatory discipline.

2.6.1 Testing for Preferential Treatment 1: JPMorgan Chase Trading Loss

My first test exploits a widely publicized trading loss at JPMorgan Chase in 2012. In May 2012, JPMorgan reported a $2 billion-dollar trading loss (which subsequently increased to $6 billion), leading to a senate congressional hearing and a $920 million fine.[44] Crucially, JPMorgan's CEO Jamie Dimon was on the board of directors of the New York Fed at the time of the trading loss. Following the publication of the trading loss, there were calls for Dimon's resignation from the Fed Board as well as for reforms to the Federal Reserve System.[45]

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[44] http://money.cnn.com/2013/09/19/investing/jpmorgan-london-whale-fine/

[45] See for example: http://money.cnn.com/2012/05/21/news/economy/jamie-dimon-new-york-fed/, http://business.time.com/2012/06/19/jamie-dimon-isnt-the-only-or-even-the-worst-problem-plaguing-the-federal-reserve/, http://www.seattletimes.com/business/dimons-place-on-fed-board-renews-conflict-of-interest-concerns/ and http://www.theatlantic.com/business/archive/2012/06/simon-johnson-rips-into-jamie-dimons-conflicts-of-interest/259258/

The media attention and public scrutiny surrounding this event and how regulators failed to effectively supervise JPMorgan's risk management provides me with an exogenous shock to regulators' stringency in monitoring banks, especially those with connections. If preferential treatment was one explanation behind my results, I expect to observe a decrease in gains from risk-shifting at connected banks after 2012. Any technical expertise which bank directors may have obtained from their connections to regulators is not plausibly affected by this event. Therefore, if a transfer of skills is the reason why banks can shift risk following the establishment of connections, I should find risk-shifting to continue following the JPMorgan trading loss.

I test this using both *Public Service* and *Current Public Service* definitions of connectedness. I estimate the model as:

$$\Delta IPP_{i,t} = \alpha_0 + \beta_1 \Delta\sigma_{Vi,t} + \beta_2 Connected_{i,t} + \beta_3 Post\text{-}JPMorgan\ Loss_{12\text{-}13} + \beta_4 Connected_{i,t}\ x\ Post\text{-}JPMorgan\ Loss_{12\text{-}13} + \beta_5 Connected_{i,t}\ x\ \Delta\sigma_{Vi,t} + \beta_6 Post\text{-}JPMorgan\ Loss_{12\text{-}13}\ x\ \Delta\sigma_{Vi,t} + \beta_7 Connected_{i,t}\ x\ Post\text{-}JPMorgan\ Loss_{12\text{-}13}\ x\ \Delta\sigma_{Vi,t} + Bank\ Controls + Bank\ FE + Year\ FE + Regulator\ FE + \varepsilon_{i,t} \qquad (2\text{-}9)$$

where *Connected* is either *Public Service* or *Current Public Service*, and *Post-JPMorgan* $Loss_{12\text{-}13}$ is a dummy variable that equals 1 for years 2012 to 2013 and 0 otherwise. The main variables of interest are the triple interaction terms *Public Service x Post-JPMorgan* $Loss_{12\text{-}13}$ *x* $\Delta\sigma_v$ and *Current Public Service x Post-JPMorgan* $Loss_{12\text{-}13}$ *x* $\Delta\sigma_v$ which investigate if connected banks increase their risk-shifting after the JPMorgan trading loss in 2012, in relation to banks without connections.

The results are displayed in Panel A of Table 2-6. Estimations without baseline controls and bank fixed effects show similar results but are omitted for brevity. In Columns (1)-(2), I observe that the triple interaction terms of interest *Public Service x Post-JPMorgan Loss*$_{12\text{-}13}$ *x* $\Delta\sigma_v$ and *Current Public Service x Post-JPMorgan Loss*$_{12\text{-}13}$ *x* $\Delta\sigma_v$ is negative and significant at the 5% level. These findings suggest that gains from risk-shifting is reduced at banks with public service connections in the immediate aftermath of the JPMorgan trading loss, presumably due to heightened regulatory scrutiny of banks with public service connections. If skills were related to connections, I would continue to observe risk-shifting at public service connected banks following the shock.

Interestingly, Columns (3)-(4) show that gains from risk-shifting at connected banks decrease most when banks are regulated by the New York Fed, as shown by the negative quadruple interaction term (at the 1% significance level). One plausible explanation for this finding is that regulators in the New York Fed district, which oversaw JPMorgan's trading loss and had Jamie Dimon as a director on their board, increased the stringency of their monitoring of connected banks as compared to the other districts, to safeguard themselves from further negative publicity. Overall, the results indicate that risk-shifting is due to preferential treatment rather than skills

Table 2-6: Preferential Treatment or Skills

This table reports estimates using panel OLS regressions (with different specifications) and examines the sensitivity of changes in the value of public subsidies (*IPP)* to changes in σ_v at connected banks to investigate why connections facilities risk-shifting to the safety-net. Panel A show results of Equation (2-9) using the JPMorgan Trading Loss Shock in 2012 while Panel B investigates if risk-shifting occurs under different regulators. I estimate the following regression in Panel A:

$$\Delta(IPP)_{i,t} = \alpha_0 + \beta_1 \Delta\sigma_{Vi,t} + \beta_2 Z_{i,t} + \beta_4 Post\text{-}JPMorgan\ Loss_t + \beta_5(Z_{i,t}\ x\ Post\text{-}JPMorgan\ Loss_t) + \beta_6(Z_{i,t}\ x\ \Delta\sigma_{Vi,t}) + \beta_7(Post\text{-}JPMorgan\ Loss_t\ x\ \Delta\sigma_{Vi,t}) + \beta_8(Z_{i,t}\ x\ Post\text{-}JPMorgan\ Loss_t\ x\ \Delta\sigma_{Vi,t}) + Bank\ Controls_{i,t} + Bank\ FE + Year\ FE + Regulator\ FE + \varepsilon_{i,t}.$$

where subscripts *i* and *t* indicate bank and year respectively. *IPP* is the fair value of the deposit insurance premium while *Post-JPMorgan Loss is* a dummy variable that equals 1 for years 2012-2013 and 0 otherwise. *Z* is either *Public Service* (*number of Public Service directors/board size*) or *Current Public Service* (*Number of Public Service directors who are currently holding public service roles/board size*). *New York Fed* is a dummy variable that equals 1 if BHC is supervised by the New York Fed and 0 otherwise. σ_v is the volatility of asset returns. *Bank Controls* is the vector of variables in each column and includes *Tier-1 Capital*, *Bad Loans*, *Lag Enforcement Actions*, *ROA*, *Total Deposits*, *Market Risk*, *Total Assets*, *Asset Growth*, *Total Loans*, *Board Size*, *Board Independence*, *CEO Tenure* and *Duality*. The variable of interest is the coefficient β_8 on (*Z x Post-JPMorgan Loss x* $\Delta\sigma_v$) in Columns (1)-(2). Panel B is estimated using variants of Equation (2-6). *Reg by FDIC* and *Reg by OCC* are dummy variables that equals 1 if the main commercial bank under the BHC is supervised by the FDIC and OCC respectively and 0 otherwise. The coefficient on (*Public Service x* $\Delta\sigma_v$) is the variable of interest. Refer to Appendix 2-A1 for description of variables. The sample period is from 2001 to 2013. Standard errors are clustered at the bank-level and t-statistics are reported in parenthesis. The constant is suppressed for brevity. ***, ** and * indicate significance level at the 1, 5 and 10% respectively.

Panel A: JPMorgan Trading Loss Shock 2012	(1)	(2)	(3)	(4)
	ΔIPP	ΔIPP	ΔIPP	ΔIPP
Public Service x Post-JPMorgan Loss$_{12\text{-}13}$ x $\Delta\sigma_v$	-1.855**		-0.832	
	[-2.529]		[-1.195]	
Current Public Service x Post-JPMorgan Loss$_{12\text{-}13}$ x $\Delta\sigma_v$		-2.764**		-0.459
		[-2.236]		[-0.430]
Public Service x Post-JPMorgan Loss$_{12\text{-}13}$ x New York Fed x $\Delta\sigma_v$			-6.108***	
			[-3.462]	
Current Public Service x Post-JPMorgan Loss$_{12\text{-}13}$ x New York Fed x $\Delta\sigma_v$				-8.966***
				[-3.083]
Public Service x Post-JPMorgan Loss$_{12\text{-}13}$	0.199		0.847	
	[0.330]		[1.566]	
Public Service x $\Delta\sigma_v$	1.247**		0.354	
	[2.309]		[0.738]	
Public Service	-0.05		-0.416	
	[-0.070]		[-0.614]	
Current Public Service x Post-JPMorgan Loss$_{12\text{-}13}$		-0.724		0.224
		[-0.719]		[0.238]
Current Public Service x $\Delta\sigma_v$		2.223**		-0.015

		[1.981]		[-0.017]
Current Public Service		0.448		0.06
		[0.681]		[0.091]
Public Service x Post-JPMorgan Loss$_{12\text{-}13}$ x New York Fed			-4.407***	
			[-3.450]	
Public Service x New York Fed			2.867	
			[1.306]	
Public Service x New York Fed x $\Delta\sigma_v$			3.366***	
			[6.793]	
Current Public Service x Post-JPMorgan Loss$_{12\text{-}13}$ x New York Fed				-6.835**
				[-2.143]
Current Public Service x New York Fed				4.494**
				[2.265]
Current Public Service x New York Fed x $\Delta\sigma_v$				7.147***
				[7.754]
Post-JPMorgan Loss$_{12\text{-}13}$ x New York Fed x $\Delta\sigma_v$			0.254***	0.220***
			[3.428]	[3.281]
Post-JPMorgan Loss$_{12\text{-}13}$ x New York Fed			0.165**	0.096*
			[2.316]	[1.666]
New York Fed x $\Delta\sigma_v$			-0.304***	-0.294***
			[-6.628]	[-7.188]
Post-JPMorgan Loss$_{12\text{-}13}$ x $\Delta\sigma_v$	-0.229***	-0.260***	-0.243***	-0.265***
	[-3.363]	[-4.191]	[-3.561]	[-4.253]
Post-JPMorgan Loss$_{12\text{-}13}$	-0.931***	-0.895***	-0.891***	-0.843***
	[-4.837]	[-4.602]	[-4.778]	[-4.662]
$\Delta\sigma_v$	0.289***	0.308***	0.307***	0.316***
	[6.860]	[8.284]	[7.764]	[8.627]
Bank FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Regulator FE	Yes	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes	Yes
Adj. R-squared	0.643	0.636	0.675	0.671
Observations	3,011	3,011	3,011	3,011

Panel B: Relevant Regulators	(1) All ΔIPP	(2) Reg by Fed ΔIPP	(3) Reg by OCC ΔIPP	(4) Reg by FDIC ΔIPP
Fed Public Service x $\Delta\sigma_v$		2.228***	-0.303	0.647
		[8.367]	[-0.437]	[1.179]
Fed Public Service		2.259**	-1.422*	0.208
		[2.250]	[-1.833]	[0.143]
Reg by FDIC x $\Delta\sigma_v$	-0.129			
	[-1.485]			
Reg by OCC x $\Delta\sigma_v$	-0.077			
	[-0.781]			
Reg by FDIC	0.057			
	[0.577]			
Reg by OCC	0.016			
	[0.130]			
$\Delta\sigma_v$	0.393***	0.258***	0.322***	0.236***
	[5.023]	[6.193]	[4.788]	[4.739]
Bank FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Regulator FE	No	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes	Yes
Adj. R-squared	0.604	0.8	0.641	0.492
Observations	3,011	652	874	1,485

2.6.2 Testing for Preferential Treatment 2: Relevant Regulators

The second test that points to preferential treatment exploits the charter types of the main commercial banks operating under BHCs. While all BHCs in the sample are regulated by the Fed, commercial banks operating under the BHC umbrella are regulated by either the Fed, the FDIC or OCC depending on their charter. For there to be evidence of preferential treatment, the results should be strongest when connections exist to the regulator in charge and weakest when connections exist to regulators not responsible for regulating a particular bank.

By contrast, if I were to find uniform risk-shifting behaviour irrespective of the relevance of the connections to regulatory agencies, this would back the skills channel. That is because bank regulations are identical regardless of the regulator responsible (Agarwal, Lucca, Seru, and Trebbi, 2014).[46] In the presence of uniform regulations and no preferential treatment by connected regulators, banks with Fed connections should be able to extract larger subsidies irrespective of whether the commercial banks operating under their holding company are regulated by the FDIC or the OCC. As BHCs could have multiple commercial entities operating under them, I rely on the charter of the largest commercial bank (by assets) operating under the BHC to perform this test.

I first show that the three different federal regulators show similar levels of stringency towards supervision in Column (1) Panel B of Table 2-6. I re-run the baseline regression (Equation 2-6) without the connections variable but with a dummy

[46] Arguably, regulatory enforcement may differ across agencies. Agarwal et al. (2014) find differences in enforcement between state and federal regulators but not across federal regulators. They explain that state banking regulators are more lenient to banks when there are concerns over the local economy, while federal regulators are harsher as a result of their emphasis on systemic stability. They say little regarding differences between federal agencies.

variable for the regulator in charge of the main commercial bank under the BHC and interact it with $\Delta\sigma_v$. The coefficients on *Reg by FDIC x* $\Delta\sigma_v$ and *FDIC x* $\Delta\sigma_v$ are both insignificant, suggesting that enforcement is similar across the federal agencies.

Next, I re-run the regression using the number of directors who have served in public service positions at the Federal Reserve scaled by board size (*Fed Public Service*). I do this because the Fed is the most common regulator in which directors hold public service positions. Note that the Fed oversees banks at the BHC level. If the main commercial bank operating under a BHC is also regulated by the Fed, connections to the Fed would be undoubtedly more important compared to a BHC whose main commercial bank is regulated by the OCC or FDIC.

I show estimates of the baseline regression as in Equation (2-6), but with *Fed Public Service x* $\Delta\sigma_v$ as the main interaction term of interest in Columns (2)-(4) of Panel B Table 2-6. Columns (2)-(4) are sub-samples where the main commercial bank is regulated by the Fed, the OCC and the FDIC, respectively. The interaction term of interest *Fed Public Service x* $\Delta\sigma_v$ is positive and significant at the 1% level in only Column (2), indicating that banks with public service connections to the Fed which are also regulated by the Fed are able to access larger subsidies from the financial safety-net.[47] Importantly, the results show that public service connections to seemingly less relevant regulators do not result in detectable gains from risk-shifting by connected banks. If connections were related to technical expertise, such gains should be observable regardless of the responsible regulator.

[47] I would ideally like to show that FDIC/OCC connections should only result in risk-shifting when regulated by the FDIC/OCC but am unfortunately unable to do so due insufficient connections established with the FDIC/OCC. For instance, there are 0 FDIC connections under FDIC regulated banks.

2.7 Additional Analysis

2.7.1 When Do Connected Banks Risk Shift?

I next investigate *when* banks use their connections to shift risk to the safety-net. Eisdorfer (2008) show that risk-shifting incentives are higher for poorly performing firms. I partition banks into three groups based on whether a bank has low, medium or high Tier-1 capital in Columns (1)-(3) and low, medium or high ROA in (4)-(6) of Table 2-7 Panel A.

I re-run the baseline regressions (Equation 2-6) by the level of Tier-1 capital and ROA (calculated on a yearly basis) and display the results in Table 2-7 Panel A. I observe that the coefficient on *Public Service x* $\Delta\sigma_v$ is only positive and statistically significant at the 1% level when banks are in the medium group of Tier-1 capital (Column (2)). When splitting the sample by ROA, risk-shifting by public service banks is only observable for banks with high and medium levels of ROA (Columns (5)-(6)). Therefore, the results indicate that risk-shifting at connected banks occurs only when banks are not undercapitalized or underperforming. One explanation for this is that connected banks risk shift when regulatory scrutiny is likely to be low and when regulators may exhibit more leniency.

Table 2-7: When do Connected Banks Shift Risk and Benefits to Shareholders

Panel A of this table reports estimates of Equation (2-6) using panel OLS regressions (with different specifications) and examines the sensitivity of changes in the value of public subsidies (*IPP*) to changes in σ_v at connected banks at three terciles (low, medium and high) of Tier-1 Capital and ROA. I estimate the following regression:

$$\Delta(IPP)_{i,t} = \alpha_0 + \beta_1\Delta\sigma_{Vi,t} + \beta_2(Public\ Service_{i,t}\ x\ \Delta\sigma_{Vi,t}) + \beta_3 Public\ Service_{i,t} + Bank\ \ Controls_{i,t} + Bank\ FE + Year\ FE + Regulator\ FE + \varepsilon_{i,t}.$$

where subscripts *i* and *t* indicate bank and year respectively. *IPP* is the fair value of the deposit insurance premium. *Public Service* is defined as: (*number of Public Service directors/board size*). σ_v is the volatility of asset returns. *Bank Controls* is the vector of variables in each column and includes *Tier-1 Capital, Bad Loans, Lag Enforcement Actions, ROA, Total Deposits, Market Risk, Total Assets, Asset Growth, Total Loans, Board Size, Board Independence, CEO Tenure* and *Duality*. The coefficient β_2 on (*Public Service x* $\Delta\sigma_v$) is the variable of interest. Panel B and C of this table reports estimates of the panel OLS and logit regressions and examines if larger public subsidies at banks with public service connections is beneficial for shareholders of these banks at three terciles (low, medium and high) of Tier-1 Capital (Panel B) and ROA (Panel C). Panel B and C estimates the following regression in Equation (2-10):

$$(P)_{i,t} = \alpha_0 + \beta_1\Delta IPP_{i,t} + \beta_2(Public\ Service_{i,t}\ x\ \Delta IPP_{i,t}) + \beta_3 Public\ Service_{i,t} +\ \ Bank\ Controls_{i,t} + Bank\ FE + Year\ FE + Regulator\ FE + \varepsilon_{i,t}.$$

where subscripts *i* and *t* indicate bank and year respectively. *P* is one of the following 4 variables: *Stock Rets* (the annualized monthly log buy-hold returns), *ROA* (the Income before extraordinary items divided by total assets), *Pr Div*↑ (a dummy variable that = 1 if the change in (common dividends/total equity) increases from the previous year and 0 otherwise) or *Pr Net Payout*↑ (a dummy variable that = 1 if the change in total net payout [common dividends + (treasury stock purchase – sales)]/(total equity) increases from the previous year and 0 otherwise). *Bank Controls* in Columns ((1), (2), (5), (6), (9) and (10)) of Panel B and C includes: *Tier-1 Capital*, *Bad Loans*, *Total Deposits*, *Total Assets*, *Total Loans*, *Noninterest Income*, *Asset Growth*, *Leverage*, *Board Size*, *Board Independence* and *CEO Tenure*. *Bank Controls* in Columns ((3), (4), (7), (8), (11) and (12)) of Panel B and C includes: *Tier-1 Capital, Bad Loans, Total Assets, ROA, Asset Growth, Leverage, Book-to-Market Ratio, Board Size, Board Independence*, *Duality* and *CEO Tenure.* Columns ((3), (4), (7), (8), (11) and (12)) reports odd ratios from a logit model. The coefficient β_2 on (*Public Service x* Δ*IPP*) is the variable of interest. Refer to Appendix 2-A1 for description of variables. The sample period is from 2001 to 2013. Standard errors are clustered at the bank-level and t-statistics are reported in parenthesis. The constant is suppressed for brevity. ***, ** and * indicate significance level at the 1, 5 and 10% respectively.

Panel A: Tier-1 & ROA	(1)	(2)	(3)	(4)	(5)	(6)
	Tier-1 Capital			ROA		
	Low ΔIPP	Mid ΔIPP	High ΔIPP	Low ΔIPP	Mid ΔIPP	High ΔIPP
Public Service x $\Delta\sigma_v$	-0.147	2.674***	-0.176	0.034	1.599***	1.999**
	[-0.335]	[8.638]	[-0.239]	[0.054]	[3.289]	[2.560]
Public Service	0.058	-2.259	-2.107**	2.441	-0.201	-1.529
	[0.069]	[-1.487]	[-2.297]	[1.043]	[-0.248]	[-1.314]
$\Delta\sigma_v$	0.306***	0.165***	0.309***	0.337***	0.196***	0.224**
	[7.204]	[3.331]	[4.501]	[8.534]	[3.969]	[2.275]
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Regulator FE	Yes	Yes	Yes	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-squared	0.543	0.773	0.693	0.624	0.586	0.739
Observations	1,009	1,003	999	1,009	1,003	999

Panel B: Tier-1 Capital	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Low Tier-1 Capital				Mid Tier-1 Capital				High Tier-1 Capital			
	Stock Rets.	ROA	Pr Div↑	Pr Net Payout↑	Stock Rets.	ROA	Pr Div↑	Pr Net Payout↑	Stock Rets.	ROA	Pr Div↑	Pr Net Payout↑
Public Service x ΔIPP	-0.191	-0.647	0.974**	0.985	0.297**	1.031***	1.047**	1.032*	-0.051	-0.235	0.889*	0.966
	[-1.248]	[-0.878]	[-2.065]	[-1.043]	[2.175]	[2.850]	[2.410]	[1.680]	[-0.287]	[-0.530]	[-1.822]	[-1.350]
Public Service	-0.111	-0.73	1.051	0.97	-0.139	-1.282	0.969	0.955	0.293	0.061	1.135**	1.037
	[-0.388]	[-0.700]	[1.256]	[-1.071]	[-0.407]	[-0.949]	[-0.869]	[-1.190]	[0.537]	[0.046]	[2.231]	[0.721]
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regulator FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-squared	0.6	0.628	0.23	0.206	0.562	0.38	0.234	0.16	0.417	0.306	0.182	0.139
Observations	1,009	1,009	640	668	1,003	1,003	673	683	999	999	677	704

Panel C: ROA	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Low ROA				Mid ROA				High ROA			
	Stock Rets.	ROA	Pr Div↑	Pr Net Payout↑	Stock Rets.	ROA	Pr Div↑	Pr Net Payout↑	Stock Rets.	ROA	Pr Div↑	Pr Net Payout↑
Public Service x ΔIPP	-0.048	-0.55	0.951*	0.967**	0.468***	0.221**	1.070**	1.049**	0.014	0.026	1.052**	1.021**
	[-0.446]	[-0.730]	[-1.872]	[-2.287]	[3.825]	[2.089]	[2.300]	[2.450]	[0.078]	[0.292]	[2.255]	[2.012]
Public Service	0.008	-0.36	1.041	0.988	-0.316	-0.263	1.034	0.981	0.32	-0.77	1.037	0.999
	[0.017]	[-0.182]	[0.573]	[-0.208]	[-1.027]	[-1.429]	[0.912]	[-0.612]	[1.167]	[-1.223]	[1.107]	[-0.037]
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regulator FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-squared	0.637	0.593	0.288	0.198	0.581	0.845	0.251	0.221	0.44	0.358	0.222	0.166
Observations	1,009	1,009	470	539	1,003	1,003	651	681	999	999	778	801

2.7.2 Do Shareholders Benefit When Connected Banks Shift Risk?

I next investigate if risk-shifting to the safety-net results in tangible benefits to the shareholders of a bank. I have shown that an increase in risk is associated with an increase in IPP at banks with public service connections. As explained, IPP can be viewed as a risk subsidy or a put option held by the shareholders of the bank. Therefore, an increase in gains from IPP at connected banks should produce benefits for shareholders. To investigate this hypothesis, I use four measures of shareholder benefits and estimate the following model in Panel B of Table 2-7:

$$\textit{Stock Returns}_{i,t}\textit{, or ROA}_{i,t}\textit{, or Pr Div}\uparrow_{i,t} \textit{ or Pr Net Payout}\uparrow_{i,t} = \alpha_0 + \beta_1 \Delta IPP_{i,t} + \beta_2 \textit{Public Service}_{i,t} + \beta_3 \textit{Public Service}_{i,t} \times \Delta IPP_{i,t} + \textit{Bank Controls} + \textit{Bank FE} + \textit{Year FE} + \textit{Regulator FE} + \varepsilon_{i,t} \quad (2\text{-}10)$$

where *Stock Returns* is the annualized monthly logarithmic buy-and-hold returns for the calendar year and it is used as a dependent variable in Columns (1), (5) and (9). *ROA* is Total Income/Total Assets and is used in Columns (2), (6) and (10). The probability of a dividend increase *Pr Div*↑ is a dummy variable that equals 1 if the ratio of common dividends to the book value of equity increases from the previous year and is used in Columns (3), (7) and (11). Lastly, the probability of a total net payout increase *Pr Net Payout*↑ is an indicator variable that equals 1 if total net payout to shareholders, defined as[(common dividends + treasury stock repurchase – treasury stock sales)/book value of equity] increases from the previous year and is used in Columns (4), (8) and (12). For the payout variables, I exclude banks that do not pay dividends or make share repurchases during the sample period and use a logit model. Odds ratios are reported.

Because Section 2.7 show that gains from risk-shifting by connected banks occurs predominately when banks are not undercapitalized or underperforming, I partition the analysis and run the regressions by Tier-1 capital in Panel B of Table 2-7 and by ROA in Panel C. The main variable of interest are the interaction terms *Public Service x* Δ*IPP* which investigates if an increase in IPP at banks with public service connections is linked to shareholder benefits.

The results of the estimations are displayed in Panel B and C of Table 2-7. The control variables are suppressed for brevity. When partitioning the sample by Tier-1 capital, I observe that the coefficient on the interaction term of interest *Public Service x* Δ*IPP* is positive and statistically significant in Columns (5)-(8) for connected banks with medium levels of Tier-1 capital. This is consistent with the results in Panel A of Table 2-7 which shows that gains from risk-shifting are largest for such banks. I repeat the analysis using *ROA* in Panel C and find similar results. The coefficient on *Public Service x* Δ*IPP* is statistically significant in 6 out of 8 columns in the medium and high *ROA* partitions. Overall, I show that gains from risk-shifting to the safety-net results in real benefits to the shareholders of connected banks (that are not financially weak) in the form of higher stock returns, better accounting performance and a higher probability of dividend increases and or other payouts.[48]

[48] It is worth noting that the transfer of wealth effects I document are not contingent on if these banks receive bailout funds (an explicit form of wealth transfer). Because well-performing connected banks are able to access larger public subsidies by risk-shifting, the increase in shareholder benefits that I document constitutes a wealth transfer because these banks are "underpaying" for their deposit insurance. Therefore, the "loss" of fees that the FDIC collects is transferred as benefits to shareholders of well-performing banks.

2.8 Robustness Tests

This section reports the results of various robustness tests. First, an alternative explanation for some of my findings is that regulatory connections do not lead to a reduction in monitoring by regulators, but to less discipline exerted by market participants. For instance, investors may assume that connected banks are more likely to be bailed out when distressed. To control for the effects of market discipline, I follow the literature and use subordinated debt and core deposits as proxies for creditor discipline (Ashcraft, 2008; Schaeck, Cihak, Maechler, and Stolz, 2012). I rerun the baseline regressions and show the results in Appendix 2-A5. The interaction term of interest *Public Service x* $\Delta\sigma_v$ continues to remain statistically significant.

Second, large banks could find it easier to recruit directors with public service positions. Subsequently, large banks could also be more likely to shift risk onto the safety-net as a result of being too-big-to-fail (Carbo-Valverde et al., 2013). I conduct a number of tests and show estimation results in Appendix 2-A6. I first exclude banks with assets >$10 Billion (Peer group 1), >$3 Billion (Peer group 1 and 2) and the top 20% of assets as ranked in 2004 in Columns (1)-(3).[49] I next control for risk-shifting for these size groups in Columns (4)-(6). In all estimations, *Public Service x* $\Delta\sigma_v$ continues to remain statistically significant.

Third, a number of recent studies focus on the effects lobbying and political connections have in affecting legislation and bank outcomes (Mian et al., 2010; Igan et al., 2011; Duchin and Sosyura, 2012; Acemoglu et al., 2016; Lambert, 2015). As

[49] Refer to https://www.ffiec.gov/nicpubweb/content/BHCPRRPT/BHCPR_Peer.htm for information on BHC peer groups. The year of ranking is arbitrarily chosen. Multiple ways of ranking banks by assets are tested and produce similar results.

banks could also wield various forms of influence that could influence regulatory outcomes, I control for a bank's lobbying and political connections. Appendix 2-A1 details the construction of these variables (*Politically Connected, Top Politician* and *Lobby%*). The results are shown in Appendix 2-A7. I find that even after controlling for various forms of political influence, the main variable of interest *Public Service x* $\Delta\sigma_v$ continues to remain statistically significant.

Fourth, BoardEx began populating data on boards from 2000 and initially only covered the largest firms. It began to reach full capacity only in 2003. I re-estimate the main regressions using data starting from 2004 to address any concerns of sample selection. The results are shown in Column (1) of Appendix 2-A8. The results remain consistent and robust.

Finally, I re-estimate the main regression using *IPP* and σ_v derived from Duan (1994) Maximum likelihood estimations (ML). Estimations of *IPP* and σ_v are provided by Carbo-Valverde et al. (2013).[50] I match data from their paper to my sample and re-run the baseline estimations. The results for the regression using values obtained by ML estimates are reported in Column (2) of Appendix 2-A8. The interaction *Public Service x* $\Delta\sigma_v$ remains positive and statistically significant.

2.9 Conclusions

This chapter investigates if connections established while bank directors hold public service positions in regulatory agencies allow banks to access larger subsidies from the financial safety-net. I demonstrate that banks with public service connections hold less capital for a given increase in risk than non-connected banks. As a result,

[50] I thank Santiago Carbo, Francisco Fernandez and Ed Kane for sharing their data.

connected banks are able to shift risk to the financial safety-net and extract larger public subsidies. The analysis also shows that preferential treatment by regulators is one reason why connected banks are able to risk shift. Lastly, I find that risk-shifting is primarily concentrated among well-performing connected banks and that wealth is transferred from taxpayers to the shareholders of these banks.

The chapter draws attention to the darker side of interactions between regulators and bank directors and suggests that connections between regulators and bankers warrant more scrutiny. The fact that connected banks can shift risk on the back of connections established through public service roles —which carry no formal decision-making powers over matters of supervision and enforcement— is notable. It suggests that regulators do not treat banks equally and are subject to a degree of bias in their dealings with certain banks. My findings also suggest that attempts to restrict the brief of advisory directors are unlikely to be effective. This is because my findings find that risk-shifting is linked to preferential regulatory treatment that continues to persist even after bank directors have ended their tenure in public service roles and not by banks wielding formal influence over decision-making.

Appendix 2-A1: Definition of Variables

Variables	Definition	Source
<u>Connection Variables</u>		
Public Service	Total number of directors of the board that have current or former experience in public service positions at the Fed, FDIC, OCC, OTS, SEC or State regulators / Board Size. A position is defined as public service if the position is to be held by private sector individuals as a form of service to the public and not as a full-time position at the regulatory agencies	Various sources
Current Public Service	Total number of directors of the board that are currently serving in public service positions at the Fed, FDIC, OCC, OTS, SEC or State regulators / Board Size	Various sources
Fed Public Service	Total number of directors of the board that have current or former experience in public service positions at the Fed / Board Size	Various sources
Politically Connected	(Total number of directors of the board that hold current or former positions in the U.S. Congress, the U.S. Department of the Treasury, the White House, are Deputy Secretary, Secretary of U.S. Departments, are U.S. State Lieutenant Governors/Governor/U.S. City Mayors) / Board Size	Various sources
Top Politician	(Total number of directors of the board that have been Congressman (U.S. Senators and U.S. House Representatives), Deputy Secretary/Secretary of U.S. Departments, U.S. State Lieutenant Governors/Governors or U.S. City Mayors) / Board Size	Various sources
Lobby%	(Lobbying / Total Assets) in %	Center for Responsive Politics
<u>Financial Variables</u>		
IPP%	Fair value of the deposit insurance premium in % as described in Appendix 2-A2	CRSP, FRY-9C
σ_V%	Volatility of asset returns (annualized) in % as described in Appendix 2-A2	CRSP, FRY-9C
(B/V)%	(Book value of Liabilities / Market value of Assets) in %	FRY-9C
Tier-1 Capital	Tier-1 Capital / Risk Weighted Assets	FRY-9C
Bad Loans	Sum of loans past due 90 days or more and nonaccrual loans / Total Assets	FRY-9C
Enforcement Actions	Total number of enforcement actions issued by the Fed, FDIC, OCC and State regulators to a bank and its subsidiaries	Regulatory websites
ROA	Return on Assets defined as the Income before extraordinary items / Total Assets	FRY-9C
Total Deposits	Total Deposits / Total Assets	FRY-9C
Market Risk	(Short term interest earning assets - Short term interest earning liabilities) / Total Assets	FRY-9C
Total Assets	Natural logarithm of the book value of Total Assets	FRY-9C
Asset Growth	Change in Total Assets from previous year	FRY-9C
Total Loans	Total Loans / Total Assets	FRY-9C
Stock Rets.	∑ [1+Log Monthly Buy-hold Returns for the 12 months of the year] -1	CRSP
Noninterest Income	Noninterest Income / (Interest income + Noninterest Income)	FRY-9C
Leverage	Book value of liabilities / Book value of assets	FRY-9C
Book-to-Market Ratio	Market value equity at year end / Book value of equity	CRSP FRY-9C
Core Deposits	(Core Deposits / Total Assets). Core Deposits include deposits held in domestic offices of the subsidiaries of the bank, excluding all time deposits of over $100,000 USD and any brokered deposits	FRY-9C
Sub Debt	Book value of Subordinated debt / (Subordinated debt + Tier-1 Capital)	FRY-9C
<u>Financial Variables (used in Appendix 2-A2)</u>		
σ_E	Volatility of monthly equity returns (annualized)	CRSP

E	Number of shares outstanding times the share price on the last day of the trading year	CRSP
B	Book value of Total Liabilities	FRY-9C
V	Market value of Total Assets	CRSP, FRY-9C
<u>TARP & Payout Variables</u>		
TARP Dummy	Dummy variable that = 1 if the bank receives TARP bailout funds and 0 if otherwise	Center for Responsive Politics
Common Div	Common dividends / Book value of equity	FRY-9C
Pr Div↑	Dummy variable that = 1 if the change in Common Div from the previous year is positive and 0 if otherwise	FRY-9C
Net Repo	(Treasury stock purchase – Treasury stock sales) / Book value of equity	FRY-9C
Total Net Payout	(Common dividends + Net Repo) / Book value of equity	FRY-9C
Pr Net Payout ↑	Dummy variable that = 1 if the change in Net Payout from the previous year is positive and 0 if otherwise	FRY-9C
<u>Board & Bank Structure Variables</u>		
Board Size	Total number of directors on the board of the bank	BoardEx
Board Independence	Total number of directors that are classified as independent / Board Size	BoardEx
CEO Tenure	Total number of years the CEO has served in this position	BoardEx
Duality	Dummy variable that = 1 if the CEO is also the Chairman of the board and 0 if otherwise	BoardEx
New York Fed	Dummy variable that = 1 if the bank is under the supervision of the New York Fed and 0 if otherwise	FRY-9C
Reg by Fed	Dummy variable that = 1 if the main bank subsidiary (as defined by proportion of total assets) under the BHC is regulated by the Fed and 0 if otherwise	Call Reports
Reg by FDIC	Dummy variable that = 1 if the main bank subsidiary (as defined by proportion of total assets) under the BHC is regulated by the FDIC and 0 if otherwise	Call Reports
Reg by OCC	Dummy variable that = 1 if the main bank subsidiary (as defined by proportion of total assets) under the BHC is regulated by the OCC and 0 if otherwise	Call Reports
Peer Group 1	Dummy variable that = 1 if the assets of the bank is larger than $10 billion U.S. and 0 if otherwise	FRY-9C
Peer Group 1 & 2	Dummy variable that = 1 if the assets of the bank is larger than $3billion U.S. and 0 if otherwise	FRY-9C
Top 20% Assets 2004	Dummy variable that = 1 if the bank has Total Assets in the top 20% of banks in the sample as ranked in year 2004 and 0 if otherwise	FRY-9C
<u>State & Economic Variables</u>		
State GDP	GDP of the state in which bank is headquartered	Bureau of Economic Analysis
State Housing Index	Return of the House Price Index of the state in which bank is headquartered (All Transactions Index)	Federal Housing Finance Agency
State Unemployment	Unemployment rate of the state in which bank is headquartered	Bureau of Labor Statistics

Appendix 2-A2: Estimation of σ_V, *V* and *IPP*

I follow Ronn and Verma (1986), Duan, Moreau, and Sealey (1992) and Bushman and Williams (2012) in estimating the two unobservables (σ_v and *V)* required as inputs to compute the insurance premium percentage (*IPP*). I obtain values for both σ_v (volatility of asset returns) and *V* (market value of assets) by through solving an iterative process of two non-linear equations based on the Black-Scholes-Merton option pricing model.

The first Equation (2-A1) models the market value of a bank's equity as a call option on the unobservable market value of a bank's total assets:

$$E = VN(X) - pBN(X - \sigma_V\sqrt{T}) \quad (2\text{-}A1)$$

$$\text{where } X = (\ln(V/pB) + \sigma^2_V T/2)/(\sigma_V\sqrt{T}) \qquad (2\text{-}A2)$$

where *E* is the market value of equity, *B* is the book value of liabilities, *T* is the time to maturity of the option and is set to one on the assumption that the next audit occurs in one year when the option is re-priced following changes in the financial parameters, *N*() is the cumulative density of a standard normal variable, and *p* is a regulatory forbearance parameter introduced by Ronn and Verma (1986) that accounts for regulatory delays in exercising the option due to dissolution costs. *p* is set to 0.97 following previous research (Ronn and Verma, 1986; Hovakimian, Kane, and Laeven, 2003; Bushman and Williams, 2012) which allows the asset value of a bank to deteriorate to 97% of debt before the option is exercised.

Using Ito's lemma, it can be shown that:

$$\sigma_E = (VN(X)\sigma_V)/(E) \qquad (2\text{-}A3)$$

where σ_E is the standard deviation of the returns of equity volatility (annualized using monthly equity returns). Equation (2-A3) is the optimal hedge equation that relates the volatility of bank equity returns to bank asset returns. A Newton search algorithm obtains annual estimates of σ_v and V by simultaneously solving Equations (2-A1) and (2-A3) in an iterative process.

After obtaining estimates of σ_v and V, I am then able to compute the fair value of IPP, derived by Merton (1977) as:

$$IPP = N(y + \sigma_V\sqrt{T}) - (1 - \delta)^n(V/B)N(y) \qquad (2\text{-}A4)$$

$$\text{where } y = ((\ln(B/V(1 - \delta)^n) - (\sigma^2_V T/2))/(\sigma_V\sqrt{T}) \qquad (2\text{-}A5)$$

where δ is the dividend per dollar of market value of assets and n is the number of times per period the dividend is paid per annum. Dividends are included in the *IPP* valuation equation since the writer of the put option, the FDIC, is not dividend-protected.

Appendix 2-A3: Federal Reserve Presidents Retirement Years

This table reports the years and Federal Reserve Districts in which the Federal Reserve President retired and is used in the difference-in-difference (DiD) tests presented in Table 2-4. Information on retirements are obtained from the Federal Reserve websites.

Federal Reserve Banks	Year President Stepped Down
Federal Reserve Bank of New York, District 2	2003
Federal Reserve Bank of Cleveland, District 4	2003
Federal Reserve Bank of Richmond, District 5	2004
Federal Reserve Bank of Dallas, District 11	2004
Federal Reserve Bank of San Francisco, District 12	2004
Federal Reserve Bank of Philadelphia, District 3	2006
Federal Reserve Bank of Atlanta, District 6	2006
Federal Reserve Bank of Boston, District 1	2007
Federal Reserve Bank of Chicago, District 7	2007
Federal Reserve Bank of St. Louis, District 8	2008
Federal Reserve bank of Minneapolis, District 9	2009

Appendix 2-A4: Heterogeneous Effects of EESA on Risk-shifting at Connected Banks

This table reports robustness tests for Table 2-5 Panel C. I examine the sensitivity of changes in the value of public subsidies (*IPP*) to changes in σ_v at connected banks (*Public Service* in Panel A) and (*Current Public Service* in Panel B) following the enactment of the Emergency Economic Stabilization Act of 2008. Refer to Appendix 2-A1 for description of variables. The sample period is from 2001 to 2013. Standard errors are clustered at the bank-level and t-statistics are reported in parenthesis. The constant is suppressed for brevity. ***, ** and * indicate significance level at the 1, 5 and 10% respectively.

Panel A: Public Service EESA Robustness Tests	(1) TARP Receipt	(2) Excl. Top10% Assets	(3) Excl. Top20% Assets	(4) EESA 08-10	(5) EESA 08-11	(6) EESA 08-12
	ΔIPP	ΔIPP	ΔIPP	ΔIPP	ΔIPP	ΔIPP
Public Service x Post-EESA$_{08\text{-}13}$ x $\Delta\sigma_v$	1.351***	1.228**	1.737***			
	[3.470]	[2.059]	[3.088]			
Public Service x Post-EESA$_{08\text{-}10}$ x $\Delta\sigma_v$				2.739***		
				[3.757]		
Public Service x Post-EESA$_{08\text{-}11}$ x $\Delta\sigma_v$					1.440***	
					[3.125]	
Public Service x Post-EESA$_{08\text{-}12}$ x $\Delta\sigma_v$						1.523***
						[3.115]
TARP Dummy x $\Delta\sigma_v$	0.154**					
	[2.382]					
TARP Dummy	-0.168***					
	[-3.442]					
Public Service x Post-EESA$_{08\text{-}13}$	0.185	0.679	0.652			
	[0.408]	[1.569]	[1.233]			
Post-EESA$_{08\text{-}13}$ x $\Delta\sigma_v$	0.188***	0.301***	0.241***			
	[2.950]	[7.184]	[5.917]			
Post-EESA$_{08\text{-}13}$	-0.356**	-0.481***	-0.467***			
	[-2.578]	[-3.236]	[-2.710]			
Public Service x Post-EESA$_{08\text{-}10}$				-1.356**		
				[-2.196]		
Post-EESA$_{08\text{-}10}$ x $\Delta\sigma_v$				0.177**		
				[2.148]		
Post-EESA$_{08\text{-}10}$				-0.671***		
				[-3.796]		
Public Service x Post-EESA$_{08\text{-}11}$					-0.41	
					[-0.896]	
Post-EESA$_{08\text{-}11}$ x $\Delta\sigma_v$					0.328***	
					[8.403]	
Post-EESA$_{08\text{-}11}$					-0.055	
					[-0.478]	
Public Service x Post-EESA$_{08\text{-}12}$						0.008
						[0.018]
Post-EESA$_{08\text{-}12}$ x $\Delta\sigma_v$						0.307***
						[7.320]
Post-EESA$_{08\text{-}12}$						-0.15
						[-1.116]
Public Service x $\Delta\sigma_v$	-0.097	-0.057	-0.105	-1.366**	-0.207	-0.297**
	[-1.205]	[-0.559]	[-1.016]	[-2.051]	[-1.495]	[-1.968]
Public Service	0.158	0.028	-0.236	0.37	0.557	0.347
	[0.294]	[0.040]	[-0.364]	[0.699]	[1.133]	[0.668]
$\Delta\sigma_v$	0.016***	0.013***	0.012***	0.174**	0.023	0.032*

	[4.038]	[3.124]	[3.219]	[2.047]	[1.462]	[1.734]
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Regulator FE	Yes	Yes	Yes	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-squared	0.718	0.678	0.635	0.706	0.729	0.71
Observations	3,011	2,676	2,386	3,011	3,011	3,011

Panel B: Current Public Service EESA Robustness Tests	(1) TARP Receipt	(2) Excl. Top10% Assets	(3) Excl. Top20% Assets	(4) EESA 08-10	(5) EESA 08-11	(6) EESA 08-12
	ΔIPP	ΔIPP	ΔIPP	ΔIPP	ΔIPP	ΔIPP
Current Public Service x Post-EESA$_{08-13}$ x $\Delta\sigma_v$	2.271***	2.669**	3.944***			
	[2.868]	[2.431]	[4.338]			
Current Public Service x Post-EESA$_{08-10}$ x $\Delta\sigma_v$				3.786***		
				[3.278]		
Current Public Service x Post-EESA$_{08-11}$ x $\Delta\sigma_v$					2.456***	
					[2.788]	
Current Public Service x Post-EESA$_{08-12}$ x $\Delta\sigma_v$						2.487***
						[2.721]
TARP Dummy x $\Delta\sigma_v$	0.138**					
	[2.162]					
TARP Dummy	-0.17***					
	[-3.305]					
Current Public Service x Post-EESA$_{08-13}$	1.592*	1.79	1.787			
	[1.706]	[1.431]	[1.004]			
Post-EESA$_{08-13}$ x $\Delta\sigma_v$	0.223***	0.307***	0.250***			
	[3.769]	[8.263]	[6.753]			
Post-EESA$_{08-13}$	-0.35***	-0.47***	-0.44***			
	[-2.652]	[-3.393]	[-2.672]			
Current Public Service x Post-EESA$_{08-10}$				0.93		
				[0.711]		
Post-EESA$_{08-10}$ x $\Delta\sigma_v$				0.228***		
				[2.648]		
Post-EESA$_{08-10}$				-0.78***		
				[-3.873]		
Current Public Service x Post-EESA$_{08-11}$					1.767	
					[1.569]	
Post-EESA$_{08-11}$ x $\Delta\sigma_v$					0.351***	
					[9.409]	
Post-EESA$_{08-11}$					-0.078	
					[-0.696]	
Current Public Service x Post-EESA$_{08-12}$						1.58
						[1.583]
Post-EESA$_{08-12}$ x $\Delta\sigma_v$						0.331***
						[8.414]
Post-EESA$_{08-12}$						-0.183
						[-1.384]
Current Public Service x $\Delta\sigma_v$	0.028	-0.042	-0.198	-1.476*	-0.128	-0.134
	[0.158]	[-0.175]	[-0.799]	[-1.791]	[-0.613]	[-0.577]
Current Public Service	-0.518	-0.382	0.273	-0.276	-0.015	-0.419
	[-0.914]	[-0.521]	[0.246]	[-0.491]	[-0.038]	[-0.989]
$\Delta\sigma_v$	0.013***	0.012***	0.011***	0.153*	0.018	0.025
	[3.289]	[3.140]	[3.194]	[1.885]	[1.297]	[1.511]
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes

Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Regulator FE	Yes	Yes	Yes	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-squared	0.712	0.683	0.649	0.692	0.726	0.708
Observations	3,011	2,676	2,386	3,011	3,011	3,011

Appendix 2-A5: Market Discipline

This table reports estimates of Equation (2-6) using panel OLS regressions (with different specifications) and examines the sensitivity of changes in the value of public subsidies (*IPP)* to changes in σ_v at connected banks and tests for the effects of market discipline. I estimate variants of the following regression:

$$\Delta(IPP)_{i,t} = \alpha_0 + \beta_1 \Delta\sigma_{Vi,t} + \beta_2\ (Public\ Service_{i,t}\ x\ \Delta\sigma_{Vi,t}) + \beta_3 Public\ Service_{i,t} + Bank\ Controls_{i,t} + Bank\ FE + Year\ FE + Regulator\ FE + \varepsilon_{i,t}.$$

where subscripts *i* and *t* indicate bank and year respectively. *IPP* is the fair value of the deposit insurance premium. *Public Service* is defined as: (*number of Public Service directors/board size*). σ_v is the volatility of asset returns. *Bank Controls* is the vector of variables in each column and includes *Tier-1 Capital, Bad Loans, Lag Enforcement Actions, ROA, Total Deposits, Market Risk, Total Assets, Asset Growth*, *Total Loans*, *Board Size, Board Independence*, *CEO Tenure* and *Duality*. The coefficient β_2 on (*Public Service x* $\Delta\sigma_v$) is the variable of interest. Refer to Appendix 2-A1 for description of variables. The sample period is from 2001 to 2013. Standard errors are clustered at the bank-level and t-statistics are reported in parenthesis. The constant is suppressed for brevity. ***, ** and * indicate significance level at the 1, 5 and 10% respectively.

	(1) ΔIPP	(2) ΔIPP	(3) ΔIPP
Public Service x $\Delta\sigma_v$	1.313**	1.192*	1.267**
	[2.359]	[1.944]	[2.083]
Core Deposits x $\Delta\sigma_v$	-0.131		-0.127
	[-0.780]		[-0.779]
Sub Debt x $\Delta\sigma_v$		0.278	0.283
		[0.686]	[0.670]
Core Deposits	-0.904***		-0.929***
	[-2.837]		[-2.760]
Sub Debt		0.097	0.12
		[0.166]	[0.205]
Public Service	-0.154	-0.168	-0.18
	[-0.214]	[-0.254]	[-0.247]
$\Delta\sigma_v$	0.373***	0.266***	0.361***
	[3.033]	[5.931]	[2.902]
Bank FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Regulator FE	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes
Adj. R-squared	0.628	0.626	0.63
Observations	2,958	3,011	2,958

Appendix 2-A6: Size Effects

This table reports estimates of Equation (2-6) using panel OLS regressions (with different specifications) and examines the sensitivity of changes in the value of public subsidies (*IPP)* to changes in σ_v at connected banks and tests for the effects of bank size. I estimate variants of the following regression:

$$\Delta(IPP)_{i,t} = \alpha_0 + \beta_1\Delta\sigma_{Vi,t} + \beta_2(Public\ Service_{i,t}\ x\ \Delta\sigma_{Vi,t}) + \beta_3 Public\ Service_{i,t} + Bank\ Controls_{i,t} + Bank\ FE + Year\ FE + Regulator\ FE + \varepsilon_{i,t}.$$

where subscripts *i* and *t* indicate bank and year respectively. *IPP* is the fair value of the deposit insurance premium. *Public Service* is defined as: (*number of Public Service directors/board size*). σ_v is the volatility of asset returns. *Bank Controls* is the vector of variables in each column and includes *Tier-1 Capital, Bad Loans, Lag Enforcement Actions, ROA, Total Deposits, Market Risk, Total Assets, Asset Growth, Total Loans, Board Size, Board Independence, CEO Tenure* and *Duality*. The coefficient β_2 on (*Public Service x* $\Delta\sigma_v$) is the variable of interest. Refer to Appendix 2-A1 for description of variables. The sample period is from 2001 to 2013. Standard errors are clustered at the bank-level and t-statistics are reported in parenthesis. The constant is suppressed for brevity. ***, ** and * indicate significance level at the 1, 5 and 10% respectively.

	(1) Excl. Peer Group 1 > $10b ΔIPP	(2) Excl. Peer Group 1&2 > $3b ΔIPP	(3) Excl. Top20% Assets ΔIPP	(4) Ctrl. Peer Group 1 > $10b ΔIPP	(5) Ctrl. Peer Group 1&2 > $3b ΔIPP	(6) Ctrl. Top20% Assets ΔIPP
Public Service x $\Delta\sigma_v$	1.349**	1.881***	1.458**	1.146*	1.266**	1.238**
	[1.990]	[3.329]	[2.468]	[1.733]	[2.174]	[2.101]
Peer Group 1 x $\Delta\sigma_v$				0.058		
				[0.747]		
Peer Group 1				0.189		
				[1.251]		
Peer Group 1 & 2 x $\Delta\sigma_v$					0.052	
					[0.805]	
Peer Group 1 & 2					0.220***	
					[2.833]	
Top 20 Assets 2004 x $\Delta\sigma_v$						-0.002
						[-0.019]
Public Service	-0.314	-0.216	0.307	-0.121	-0.09	-0.144
	[-0.376]	[-0.214]	[0.340]	[-0.191]	[-0.136]	[-0.222]
$\Delta\sigma_v$	0.261***	0.215***	0.266***	0.266***	0.246***	0.275***
	[5.147]	[4.682]	[5.539]	[5.523]	[6.143]	[6.160]
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Regulator FE	Yes	Yes	Yes	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-squared	0.611	0.623	0.633	0.627	0.628	0.624
Observations	2,460	1,836	2,457	3,011	3,011	3,011

Appendix 2-A7: Political Connections & Lobbying

This table reports estimates of Equation (2-6) using panel OLS regressions (with different specifications) and examines the sensitivity of changes in the value of public subsidies (*IPP)* to changes in σ_v at connected banks and tests for various forms of influence. I estimate variants of the following regression:

$$\Delta(IPP)_{i,t} = \alpha_0 + \beta_1 \Delta\sigma_{Vi,t} + \beta_2\ Public\ Service_{i,t}\ x\ \Delta\sigma_{Vi,t}) + \beta_3 Public\ Service_{i,t} + \ Bank\ Controls_{i,t} + Bank\ FE + Year\ FE + Regulator\ FE + \varepsilon_{i,t}.$$

where subscripts *i* and *t* indicate bank and year respectively. *IPP* is the fair value of the deposit insurance premium. *Public Service* is defined as: (*number of Public Service directors/board size*). σ_v is the volatility of asset returns. *Bank Controls* is the vector of variables in each column and includes *Tier-1 Capital, Bad Loans, Lag Enforcement Actions, ROA, Total Deposits, Market Risk, Total Assets, Asset Growth, Total Loans, Board Size, Board Independence, CEO Tenure* and *Duality*. The coefficient β_2 on (*Public Service x* $\Delta\sigma_v$) is the variable of interest. Refer to Appendix 2-A1 for description of variables. The sample period is from 2001 to 2013. Standard errors are clustered at the bank-level and t-statistics are reported in parenthesis. The constant is suppressed for brevity. ***, ** and * indicate significance level at the 1, 5 and 10% respectively.

	(1) ΔIPP	(2) ΔIPP	(3) ΔIPP
Public Service x $\Delta\sigma_v$	1.275**	1.250**	1.236**
	[2.404]	[2.272]	[2.193]
Politically Connected x $\Delta\sigma_v$	-0.528		
	[-1.141]		
Top Politician x $\Delta\sigma_v$		-1.165	
		[-1.588]	
Lobby% x $\Delta\sigma_v$			-5.369
			[-0.136]
Politically Connected	0.43		
	[0.487]		
Top Politician		0.753	
		[0.874]	
Lobby%			10.364
			[0.314]
Public Service	-0.214	-0.082	-0.14
	[-0.324]	[-0.127]	[-0.215]
$\Delta\sigma_v$	0.285***	0.279***	0.275***
	[6.257]	[6.446]	[6.259]
Bank FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Regulator FE	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes
Adj. R-squared	0.627	0.627	0.624
Observations	3,011	3,011	3,011

Appendix 2-A8: Data Start Date and Alternative IPP Calculation

This table reports estimates of Equation (2-6) using panel OLS regressions (with different specifications) and examines the sensitivity of changes in the value of public subsidies (*IPP*) to changes in σ_v at connected bank. Column (1) uses data from years 2004-2013 while Column (2) uses *IPP* and σ_v calculated by the Duan (1994) Maximum likelihood method. I estimate the following regression:

$$\Delta(IPP)_{i,t} = \alpha_0 + \beta_1 \Delta\sigma_{Vi,t} + \beta_2(Public\ Service_{i,t}\ x\ \Delta\sigma_{Vi,t}) + \beta_3 Public\ Service_{i,t} + Bank\ Controls_{i,t} + Bank\ FE + Year\ FE + Regulator\ FE + \varepsilon_{i,t}.$$

where subscripts *i* and *t* indicate bank and year respectively. *IPP* is the fair value of the deposit insurance premium. *Public Service* is defined as: (*number of Public Service directors/board size*). σ_v is the volatility of asset returns. *Bank Controls* is the vector of variables in each column and includes *Tier-1 Capital, Bad Loans, Lag Enforcement Actions, ROA, Total Deposits, Market Risk, Total Assets, Asset Growth*, *Total Loans*, *Board Size*, *Board Independence*, *CEO Tenure* and *Duality*. The coefficient β_2 on (*Public Service x* $\Delta\sigma_v$) is the variable of interest. Refer to Appendix 2-A1 for description of variables. Standard errors are clustered at the bank-level and t-statistics are reported in parenthesis. The constant is suppressed for brevity. ***, ** and * indicate significance level at the 1, 5 and 10% respectively.

	(1) Sample Period 2004-2013 ΔIPP	(2) Duan (1994) Max. Likelihood Estimations of IPP & σ_v ΔIPP
Public Service x $\Delta\sigma_v$	1.354***	0.012**
	[2.608]	[2.125]
Public Service	-0.292	-0.561
	[-0.388]	[-0.781]
$\Delta\sigma_v$	0.286***	-0.0003
	[6.773]	[-0.997]
Bank FE	Yes	Yes
Year FE	Yes	Yes
Regulator FE	Yes	Yes
Other Controls	Yes	Yes
Adj. R-squared	0.652	0.261
Observations	2,718	299

3
Is Home Where the Heart Is? CEO Hometown and Bank Policies

3.1 Introduction

How bank credit is allocated and whether this allocation is efficient is a fundamental question given the importance of credit supply on housing outcomes and local economic development (Mian, Rao, and Sufi, 2013; Mian and Sufi, 2014). In this chapter, I investigate how bank CEO hometown favoritism affects credit allocation policies *within* banks and its real effects on the local economy.

I define the hometown of the CEO as the county or state that the CEO was born. I hypothesize that CEOs emotional attachment to their hometown community makes them more likely to favor their hometown area over other geographical areas when making credit allocation decisions.[51] This is grounded in the psychological concept of place attachment, which argues that people tend to gravitate towards familiar places such as their hometown (e.g., Hernandez, Hidalgo, Salazar-Laplace, and Hess, 2007; Low and Altman, 1992). Specifically, I ask four questions. Do bank

[51] I recognize that CEOs do not personally make local lending decisions. However, there are many ways CEOs could influence the process. For instance, CEOs could open more branches (shown later) or set targets to encourage local branch managers to lend more in relevant regions.

CEOs favor their hometown in mortgage and small business lending? Is hometown favoritism efficient for shareholders? Why do CEOs favor their hometown? Who benefits from bank CEO favoritism and are there real effects?

To conduct the analysis, I use hand-collected data on the birthplace of CEOs from multiple sources, including NNDB, Marquis Who's Who, ancestry.com, CEO appointment announcements and obituaries.[52] The data allows me to precisely identify birthplace information up to the county-level of nearly 55% of CEOs of all publicly listed U.S. banks between 1999 and 2014.[53]

My empirical strategy takes advantage of the fact that most banks lend in multiple geographic locations. Therefore, I am able to exploit within-bank variation in the proximity between the bank CEO's birthplace county and the county where lending decisions take place. In my main results, I find that bank CEOs favor their hometown in lending and branching decisions. Within the same bank, banks have higher mortgage approval rates and mortgage origination growth rates in counties that are located nearer to the hometown of the CEO as compared to counties that are located further away. The effects are economically meaningful. For instance, a one standard deviation increase in proximity to the CEO's hometown is associated with a 2.1% (28% as compared to the mean) increase in mortgage origination growth rates. The results from higher bank growth rates are particularly notable given that branch decisions are real business choices. This reflects a conscious choice by bank CEOs to expand the

[52] I thank Louis Nguyen for providing the data on CEO birthplaces.
[53] This is a significant improvement relative to prior studies. For instance, Bernile, Bhagwat, and Rau, (2017) are only able to identify the birthplace of 30% of CEOs in their S&P1500 sample.

operations of the bank to counties proximate to their hometown, possibly to facilitate increases in lending or to create local jobs.

Importantly, I control for the proximity to the headquarters (HQ) of the bank, indicating that the CEO hometown favoritism effect I identify is conceptually distinct from the HQ effect (Giroud, 2013). Time-varying controls that could influence loan decisions, such as the riskiness of the loan as well as bank-level characteristics are also included. I also include bank fixed effects and county-year fixed effects in all of the estimations. The inclusion of bank fixed effects means that I hold constant any time-invariant bank omitted variables and identify lending and branching decisions at the *same bank* in different counties, conditional on the distance to the CEO's hometown of that same bank. Incorporating county-year fixed effects ensures that I control for any time-varying changes in local economic conditions or changes in state laws or regulations that could bias my results.

I employ several identification strategies to increase the confidence of a causal interpretation that CEOs favor counties closer to their hometown over others. The main endogeneity challenge is endogenous CEO-bank matching. Unobserved bank heterogeneity (both time-invariant and variant) could simultaneous determine the matching between CEOs to a bank and the bank's mortgage and branching decisions. For example, banks that have traditionally lent to counties proximate to the CEO's hometown (or have time-invariant characteristics that would explain this, such as the mission statement of the bank) could attract CEOs whose preference is to operate in these counties.

To address the possibility that CEO-bank matching is due to time-invariant bank factors, I rely on within-bank variation. The inclusion of bank FE means that any time-invariant bank characteristics that would be correlated with a CEO's preference to join any particular bank would be differenced out (i.e., effects are partially identified following CEO turnovers at the same bank) and the findings can be interpreted as causal.

However, if changes in the characteristics of the bank or bank business strategies (time-varying CEO-bank matching) are the main reasons behind CEO turnovers, relying on within-bank variation would not sufficiently address this issue. If changes in underlying business strategies were driving the turnover, the hometown favoritism effect I document following turnovers would be irrelevant as the board would select *any* CEO willing to implement the new business strategy of the bank (Fee, Hadlock, and Pierce, 2013). I use two identification strategies to address this concern.

In the first identification strategy, I focus on a subsample of banks where CEO turnovers are likely to be exogenous (turnovers arising from natural causes (death or illness), planned retirements, or scheduled succession plans). While the choice of the incoming CEO is not random, the timing of the turnover is likely to be exogenous to changes to the business strategy of the bank (Custodio and Metzger, 2014; Bushman, Davidson, Dey, and Smith, Forthcoming). Therefore, exogenous turnovers produce a shock to the proximity to the bank CEO's hometown while being orthogonal to local lending and branching decisions. I also use an alternate CEO turnover subsample; internal CEO turnovers. Internal CEO turnovers are succession events where the incoming CEO was already an employee of the bank for a period of time before taking over the role of CEO. In this turnover setting, the choice of an internal CEO successor is likely to reflect continuity in the business strategy of the bank and less likely to reflect major changes in bank policies (Dittmar and Duchin, 2016). In both subsamples, the results remain consistent with the baseline findings that CEOs favor their hometown with additional mortgage origination and branching decisions.

The second identification strategy exploits exogenous shocks in macroeconomic conditions, i.e., boom and bust periods, to show that CEO-bank endogenous matching does not bias the results. This approach is similar in spirit to Opler and Titman (1994) and Yonker (2017b) and has several advantages. First, boom and bust periods are largely exogenous to the CEO-bank matching process as firms do not frequently change their CEOs in anticipation of business cycles.[54] Second, it allows me to observe if a CEO's hometown favoritism persists in changing business environments, where decisions are likely to be more complex and unstructured, and thus, more likely to be influenced by the CEO's characteristics. Further, the use of both boom and bust periods allows me to *contrast* the hometown favoritism effect of the CEO in differing credit conditions; when credit is most crucial to borrowers (bust), and when additional credit is unlikely to matter (boom).

Consistent with expectations, I find that CEO hometown favoritism is particularly salient during bust periods (when credit conditions are tight and an extra favor from the CEO would make a large difference to their hometown communities)

[54] Fahlenbrach and Stulz (2011) show that CEOs of banks whose incentives were better aligned with shareholders suffered larger losses in their compensation during the crisis, suggesting the inability of bank CEOs to anticipate the crisis.

but not during boom periods (when credit is abundant and additional credit is unlikely to matter).

Beyond endogenous CEO-bank matching, I also control for a host of CEO observable characteristics to mitigate issues related to omitted variables that might be correlated with CEO hometown favoritism. Specifically, I control for CEO educational background (MBA and Ivy League*)*, age, overconfidence, military and Great Depression experience and if the CEO started her career in a recession. My findings of CEO hometown favoritism on bank policies remains robust to the inclusion of these other CEO-level characteristics.

To further give validity to my findings of hometown favoritism and sharpen inference, I condition the main results on the CEO's degree of attachment to her hometown. For example, individuals who spend longer periods of time in their place of birth should develop deeper connections to their hometown. I show that CEOs who undertake their undergraduate degree in their birth state —and therefore are more likely to spend most of their formative years there— show stronger hometown favoritism in mortgage lending and branching decisions. Overall, the results from various tests for endogeneity related interpretations supports the findings of a causal link between CEO hometown favoritism and bank policies. Alternate endogeneity driven interpretations would need to persist through the different identification strategies and various fixed effects.

After showing that CEOs favor their hometown with mortgage lending and branching decisions, I next seek to disentangle the reasons behind these effects. The three main reasons that could explain why CEOs favor their hometown in business decisions are: (1) informational advantages; (2) private benefits due to agency conflicts and; (3) altruistic hometown attachment.

The first reason states that CEOs favor their hometown due to informational advantages in conducting business (Coval and Moskowitz, 1999; 2001; Ivkovic and Weisbenner, 2005; Malloy, 2005). Local contacts could still reside and work in the CEO’s hometown and provide them with information regarding local economic conditions and trends (Cohen, Frazzini, and Malloy, 2008). CEOs could also be better informed about the local culture (Fisman, Paravisini, and Vig, 2017) and have accessibility and connections to key politicians and regulators (Mian, Sufi, and Trebbi, 2010; Duchin and Sosyura, 2012).

A second reason that could explain hometown favoritism is the pursuit of private benefits due to the presence of agency conflicts (Jensen and Meckling, 1976; Shleifer and Vishny, 1997). By conducting business in her hometown, a CEO could obtain local awards, local directorship positions, speaking arrangements, popularity and status. Local lending could also be seen as a form of corporate philanthropy to increase the private utility of the CEO at the expense of bank shareholders (Masulis and Reza, 2014).

Finally, the last reason why CEOs favor their hometown in lending and business decisions is due to an altruistic hometown attachment. Place attachment theory suggests that people develop deep attachments to places where they are familiar with, such as their hometown, and that these attachments forms a key portion of their personal identity (Low and Altman, 1992; Manzo, 2003; Gieryn, 2000; Hernandez et al., 2007; Lewicka, 2011). Further, place attachment theory suggests that individuals are more likely to invest their time and money, as well as care more about the welfare of people that reside in these places (Vaske and Kobrin, 2001; Manzo and Perkins, 2006).

Importantly, these three explanations offer different predictions. If information advantages were driving the results, the hometown favoritism effect that I document would materialize as an optimal business strategy of the bank. This predicts that CEO hometown favoritism should result in positive bank-level outcomes. However, if hometown favoritism were motivated by agency conflicts at the expense of bank shareholders, this implies that hometown favoritism are agency costs and should result in negative bank outcomes. Lastly, while hometown attachment could manifest as private benefits accrued to the CEO in the presence of agency conflicts, what distinguishes the altruistic hometown interpretation from the agency argument is that shareholders of the bank are *not* harmed by this altruistic hometown attachment. If the reason behind hometown favoritism were altruistic motives, the resources of the bank would simply be relocated to counties that are proximate to the hometown of the CEO and have no effects on bank performance.

I show strong empirical support for the altruistic hometown attachment interpretation. First, using bank-level analysis, I find that banks that lend more in the CEO's birth state do *not* have better baseline performance (ROA) nor have better performance in their loan portfolio (bad loans) as compared to other banks. These banks also do not increase their lending (the proportion of total loans to assets remains consistent). Taken together, these results suggest that CEOs relocate credit from counties located further away to favor counties proximate to their birthplace for altruistic reasons, and is inconsistent with predictions from both the information (agency) argument which predicts positive (negative) bank outcomes.

Second, I also show that hometown favoritism effects are stronger for CEOs whose cultural heritage places a greater emphasis on patriotism, selflessness, humane-orientation, and collectivism.[55] These attributes are related to altruism and aligns well with the altruistic hometown explanation which suggests that CEOs invest in their hometown as a way to contribute back to their community.[56]

Third, the hometown favoritism effects are more salient amongst struggling counties (i.e., higher unemployment and lower home ownership rates) and marginal applicants (i.e., poorer, riskier, and non-white applicants). These applicants typically face a higher barrier in accessing bank credit and would benefit the most from a CEO's desire to help. Given that home ownership has been a hallmark of the "American dream" (Laeven and Popov, 2017), my findings that CEOs increase mortgage lending near their hometowns to weaker applicants and counties supports the idea that CEOs aim to help their hometown residents achieve their aspirations, in-line with the altruistic attachment interpretation.

Finally, I also document that CEO hometown favoritism extends to small business lending. CEOs make more small business loans (loans that do not exceed $250,000) to counties that are closer to their hometown as compared to counties that are further away. However, there are no changes to small business loans exceeding

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[55] As I am unable to directly observe a CEO's degree of altruism, I infer a CEO's altruistic values based on her cultural heritage. This is based on Nguyen, Hagendorff and Eshraghi (Forthcoming) who find that bank CEOs exhibit distinct behavior based on the country from which their ancestors immigrate from. Hence, I infer a CEO's level of altruism based on their inherited cultural values.

[56] These findings also rule out the agency explanation. If agency reasons prevail, I should observe *opposite* results in the analysis. For example, the agency argument would predict that hometown lending should be more prevalent when the CEO is individualistic, not collectivistic.

$250,000. This supports my interpretation of a hometown altruistic driven motive. If CEOs where motivated by other reasons (such as agency conflicts), I should find that origination of larger loan amounts to be equally probable, or, even likelier. Larger firms are likely to take out larger loan amounts, and approving these loans would help increase the reputation and visibility of the CEO more as compared to smaller loans. All of these tests consistently support the altruistic hometown attachment interpretation, indicating that CEOs lend more near their birthplace because they want to help their hometown communities.

I conclude by showing that hometown favoritism is likely to be beneficial to residents near the bank's CEO birthplace beyond the receipt of loans. I find correlational evidence suggesting that counties with a greater exposure to CEO hometown favoritism have higher income levels as well as lower unemployment rates. Intriguingly, a different way to interpret my results is that, if a county is unlucky (lucky) enough to have a lower (higher) exposure to hometown favoritism, it would have to unfairly experience lower economic development. Thus, hometown favoritism may contribute to the deepening of economic inequality.

3.2 Related Literature and Contributions

This chapter is related to three streams of literature: the economic effects of home bias, behavioral factors that influence economic decisions, and the idiosyncratic style of CEOs. The home bias literature focuses largely on investor behavior and generally finds that investors prefer proximate stocks over others. However, it offers conflicting explanations on the economic mechanisms behind the effect. For instance, while Coval and Moskowitz (1999) and Ivkovic and Weisbenner (2005) argue that home bias reflects informational advantages to investors, Pool, Stoffman, and Yonker

(2012) find no such advantages to local investing. More recently, the home bias literature investigates firm-level business outcomes, including employment policies (Yonker, 2017b), mergers and acquisitions (Chung, Green, and Schmidt, 2017; Jiang, Qian, and Yonker, 2017) and show evidence for the differing motives of hometown favoritism. Most related to this chapter is a study by Yonker (2017b) who finds that, following periods of industry distress, CEOs are less likely to fire employees working in establishments proximate to their hometown and concludes that such favoritism is suboptimal.

The key difference in this chapter is that I identify the effect of hometown favoritism on a firm's production *outputs* (i.e., bank credit) as opposed to its production inputs (e.g., employees). I further show that the hometown favoritism effect extends beyond internal favoritism to benefit the wider community where the CEO grew up in. Furthermore, focusing on outputs allows me to estimate the economic effects of home bias on the real economy. Finally, the richness of my tests enables me to disentangle between the different explanations behind the hometown effect. I propose a new explanation, an altruistic attachment motive, and find that it better explains the findings as compared to the information and agency arguments.

This chapter also contributes to the literature that studies how behavioral factors influence credit allocation. The prior literature shows that loan applications may be rejected due to: the loan applicant appearing less physically trustworthy (Duarte, Siegel, and Young, 2012); negative moods induced by the weather (Cortes, Duchin, and Sosyura, 2016); behavioural biases that follow sequential streaks of approvals (Chen, Moskowitz, and Shue, 2016). This chapter extends this literature by uncovering a new factor —CEO geographical origin— that leads to bias in credit allocation decisions.

Finally, this chapter is related to the literature that studies the impact of CEO attributes on corporate outcomes. Various studies have found that CEO's life (Bernile, Bhagwat, and Rau, 2017; Cronqvist and Yu, 2017) and career experience (Custodio and Metzger, 2014; Benmelech and Frydman, 2015; Dittmar and Duchin, 2016; Schoar and Zuo, 2017) matters for corporate decisions. While these studies focus on firm-level outcomes, I show how a CEO's geographic origin explains heterogeneity in the production outputs *within* the firm.

3.3 Sample Construction and Data

3.3.1 Sample Construction

My sample is constructed using public listed U.S. Bank Holding Companies and commercial banks from 1999 to 2014. Balance sheet data on these banks are obtained from Call Reports (forms FFIEC 031/041 and FR Y-9C) maintained by the Federal Reserve Bank of Chicago. I then merge this sample of banks with data from BoardEx to retrieve demographic information on the CEO. BoardEx's coverage is fairly comprehensive for U.S. public firms and provides detailed biographical and employment data of board members and executives. The sample begins in 1999 because it is the first year in which BoardEx started collecting executive-level information.

To determine the hometown of the CEO, the county and state of the birth place of the CEO is manually collected from various sources, starting with NNDB.com and Marquis Who's Who, which have birth data for CEOs of the largest banks. If birth data cannot be obtained this way, extensive Google searches are performed using keywords of "CEO full name + native of" and/or "CEO full name + born". This process allows the identification of birth information for a large number of CEOs from multiple sources, including CEO appointment announcements, SEC filings, school donations, charity events, biographies, interviews and obituaries. For the remaining CEOs, searches are conducted on ancestry.com, the world's largest genealogy database, for a CEO's birth and marriage certificates that occasionally list the CEO's location of birth. In total, CEO birth county and state data is available for 485 out of 906 CEOs (54%) in the sample between 1999 and 2014 (from 369 out of 783 banks). This is an improvement over Bernile et al. (2017), who can only identify the birth locations of approximately 31% of CEOs in the S&P1500 sample. Appendix 3-A2 lists, by birth state, the number and proportion of bank CEOs in my sample.

An advantage of this approach is that it contains information on the exact location of the birthplace of the CEO. In contrast, other studies (e.g., Yonker, 2017a) rely on the CEO's Social Security Number (SSN) to infer their state of birth.[57] As most SSNs are obtained at the ages between 14-17, inference of a CEO's place of birth using SSNs introduces noise to the accuracy of the data due to the possibility of family relocations. However, the drawback of targeting such a high level of accuracy in determining a CEO's birth location is the loss of a large proportion of CEOs whose birthplace cannot be precisely identified.[58]

[57] Using the SSN approach, Yonker (2017a) is able to identify the *state* of origin of approximately 89% of S&P1500 CEOs.
[58] As I am unable to identify the birthplace of all bank CEOs, it could be argued that there exists a sample selection bias that could influence the interpretation of my results. For instance, if CEOs are not proud of their hometown, they could be less likely to publicize this information, causing me to overstate my findings when generalizing to the entire population of CEOs. However, this concern is unlikely to influence the interpretation of my findings for several reasons. First, I use a larger proportion of CEO

3.3.2 Mortgage Loan Data

The data on mortgage loans comes from the Home Mortgage Disclosure Act (HMDA) database collected by the Federal Financial Institutions Examination Council (FFIEC). The HMDA database covers all mortgage applications that have been reviewed by qualified financial institutions. Specifically, an institution is required to disclose any mortgage lending under HMDA if it has at least one branch office in any metropolitan statistical area and meets the minimum size threshold. For instance, in 2002, this reporting threshold is $32 million in book assets.[59] Because of this low reporting threshold, almost all banks are included in the dataset.[60]

Each loan application in the dataset provides borrower demographic characteristics (e.g., income, gender, and race), loan characteristics (e.g., loan amount applied for and its purpose), property characteristics (e.g., type and geographical location), decision on the loan application (e.g., approved, denied, or withdrawn) and the year the application of the loan was made. The HMDA data also contains a lender identifier which allows the matching of loan data to the sample of banks for which there is information on CEO birthplace.

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birthplace locations than previous studies, which reduces the possibility of this bias. Second, and more importantly, the results I document holds when I use the *state* of birth of the CEO as a measure of hometown. As there exist *multiple* CEOs born to the same state, it is less obvious why some CEOs would be less proud of their home state and choose to hide it and some do not. For example, I find that 27 bank CEOs were born in California. If there were something inherently less ideal about being born in California, it is not obvious why these 27 CEOs would make known this information and other Californian CEOs would choose to hide it. Further, specifying hometown at the state level (a larger unit of geographical disaggregation as compared to the county) also reduces the possibility that CEOs deliberate hide their hometown of origin because they are less proud of it, as their state of birth is likely to be less "personal" than the county that they were born, and therefore, should have less negative feelings about it (if any).

[59] HMDA reporting criteria can be found at https://www.ffiec.gov/hmda/reporterhistory.htm

[60] See Cortes et al. (2016) for a more detailed description of the HMDA dataset.

The sample includes all mortgage loan applications reviewed by my sample of U.S. listed banks between 1999 and 2014. I drop applications that were closed for incompleteness or withdrawn by the applicant before a decision was made. Following Agarwal, Benmelech, Bergman, and Seru (2012), loan amount and applicant income are winsorized at the 1% level.

3.3.3 Dependent Variables

In this chapter, I use three outcome variables: (1) *Approval rate*; (2) *Δln($originated loan)* and; (3) *Δbranches.* All of these measures are aggregated at the bank-county-year level. The data used to construct *Δbranches* is obtained from the Summary of Deposits (SOD) dataset, maintained by the Federal Deposit Insurance Corporation (FDIC). HMDA mortgage loan data described in the previous section is used to construct *Approval rate* and *Δln($originated loan).*

The first measure, *Approval rate*, is defined as the number of approved mortgage loan applications divided by the total number of applications received by a bank, in a particular county, in a specific year. The key advantage of this dependent variable is that it normalizes the number of approved applications by the number of loan application a bank receives in a county-year, and thus, partially accounts for significant demand related variations in loan applications (Gilje, Loutskina, and Strahan, 2016). Holding other loan and applicant characteristics constant, *Approval rate* measures a bank's willingness to supply mortgage credit in a county-year.

The second measure, *Δln($originated loan),* is defined as the logarithmic originated mortgage loans a bank makes relative to the prior year divided by logarithmic originated loans in the prior year, again, measured at the bank-county-year level. In contrast to *Approval rate*, this measure looks at logarithmic changes in the nominal dollar amount of loans that were originated. Estimating the model in growth rates allows me to difference out lending for a bank-county relative to the prior year which, again, partially controls for fluctuations in demand for mortgages over the sample period.

The last measure, *Δbranches*, is defined as the number of branches a bank has in a county minus the number of branches in the prior year, scaled by the number of branches in the prior year. It measures the percentage change in annual growth rates of branches of the bank in a county from the previous year.

Table 3-1 provides summary statistics on these dependent variables as well as other variables used in this chapter. Overall, the summary statistics are in line with those reported in the literature (e.g., Agarwal et al., 2012; Cortes et al., 2016). The average approval rate is 69.8%, meaning 7 out of 10 mortgage applications are approved in an average bank-county-year. The average borrower earns about $102,300 per year and applies for a $155,100 mortgage loan. The average growth rate in mortgage originations is –7%, which is perhaps driven by the large lending reduction during the 2007-2009 financial crisis. Finally, branch growth rates are also decreasing at a –2% rate for a bank-county-year, consistent with the overall trend of bank branch consolidation in the U.S.

3.3.4 CEO Hometown Measure

Using data on a CEO's birth county, I create a *within-bank* measure to investigate if CEO hometown affects a bank's mortgage lending and branching decisions. To illustrate, consider Mr. James E. Rohr, the former CEO of PNC Financial

Services Group Inc. He was born in Cleveland, a major city located in Cuyahoga County in the state of Ohio. Thus, it is unclear if Mr. Rohr would consider the city of Cleveland, the county of Cuyahoga or the state of Ohio, or all of these, as his hometown. That is, the geographical measure of 'hometown' is not clear ex-ante and can be different across CEOs. Additionally, Cuyahoga County is approximately 50 km away from Lake County. Both counties are located in the state of Ohio and share very similar demographic and economic characteristics. Therefore, it is equally likely that Mr. Rohr also considers Lake County as part of his hometown identity.

Based on these considerations, I create a continuous, *within*-bank variable to measure a CEO's degree of hometown attachment. Specifically, *Ln(dist. hometown)* is the natural logarithm of the physical distance (in kilometers) between a CEO's birth county and the county in which the mortgage originations and branching decisions take place.[61] As an example, PNC Financial Service Group Inc. is headquartered in Allegheny County (PA) and has operations in multiple counties across the U.S., including Lake County (OH) and King County (WA). While Lake County (OH) is only 50 km away from the hometown of PNC CEO (James Rohr), King County (WA) is more than 3,000 km away. The measure therefore measures bank outcomes in Lake County (proximate to his hometown) as compared to King County (located further away).

[61] Geographic coordinates (longitude and latitude) are obtained from the U.S. Census (2014) Gazetteer.

Table 3-1: Summary Statistics

This table reports summary statistics for bank and loan characteristics in the sample. std. is the standard deviation while p1, p50 and p99 are the 1st, 50th, and 99th percentiles. The sample covers the period 1999–2014 for which data on CEO birthplace is available. Refer to Appendix 3-A1 for the definition and construction of variables used in this chapter.

	#	mean	std.	p1	p50	p99
Hometown Variables						
Ln(dist. hometown)	558,932	6.658	1.105	3.682	6.832	8.301
Ln(dist. HQ)	558,932	6.530	1.213	3.504	6.723	8.318
Dist. hometown	558,932	1,219	1,038	38.71	925.6	4,028
Dist. HQ	558,932	1,162	1,065	32.25	829.9	4,094
Key Dependent Variables						
Approval rate	558,932	0.698	0.303	0.000	0.759	1.000
Δln($originated loan)	408,184	-0.074	0.309	-1.000	-0.007	0.415
Δbranches	85,086	-0.027	0.139	-1.000	0.000	0.143
Loan Characteristics						
%minor applicants	558,932	0.329	0.333	0.000	0.231	1.000
%female applicants	558,932	0.199	0.229	0.000	0.167	1.000
Loan	558,932	155.100	670.500	6.000	97.960	1000.000
Income	558,932	102.300	210.400	20.000	69.410	683.000
Bank Characteristics						
Assets	5,357	14.940	1.789	12.240	14.550	20.950
Leverage	5,357	0.908	0.026	0.826	0.910	0.954
ROA (%)	5,357	0.783	1.077	-4.510	0.958	2.167
Lending	5,357	0.662	0.122	0.303	0.674	0.890
Deposits	5,357	0.751	0.104	0.385	0.769	0.898
%mortgage loan in home state	5,357	0.528	0.421	0.000	0.645	1.000
%small business loan in home state	3,913	0.532	0.431	0.000	0.637	1.000
CEO Characteristics						
Out-of-state CEO	485	0.412	0.487	0.000	0.000	1.000
Hometown UG	474	0.640	0.481	0.000	1.000	1.000
Ivy	474	0.136	0.342	0.000	0.000	1.000
MBA	474	0.231	0.422	0.000	0.000	1.000
County Characteristics						
Unemployment rate (%)	22,741	6.188	2.505	2.328	5.636	13.600
Non-ownership (%)	22,741	26.820	7.709	14.000	25.400	52.300

For robustness, I also create *Hometown state*, a dummy that equals 1 if the CEO's birth state and the state in which the mortgage originations and branch decisions occur is similar. I obtain consistent results using this alternative definition of CEO hometown favoritism.

3.4 Empirical Results

3.4.1 Methodology

To investigate if CEO hometown matters in the bank's mortgage origination and branching decisions, I estimate the following equation at the bank-county-year level:

$$Y_{i,k,t} = \alpha_{i,k,t} + \beta_1 Ln(dist.\ hometown)_{i,k,t} + Loan\ Controls_{i,k,t} + Bank\ Controls_{i,t} + Bank\ FE + County\text{-}Year\ FE + \varepsilon_{i,k,t} \qquad (3\text{-}1)$$

where *i, k* and *t* indicate bank *i*, county *k* and year *t*, respectively. *Y* is either *Approval Rate, Δln($originated loan)* or *Δbranches,* measured at the bank-county-year level. The key variable of interest, *Ln(dist. hometown)*, is the natural logarithm of the physical distance (in kilometers) between a CEO's birth county and the county in which the mortgage originations and branching decisions occur. If CEOs favor counties proximate to their hometown, the coefficient β_1 should be significantly negative.

All the estimations include bank fixed effects as well as county-year fixed effects. The inclusion of bank fixed effects means that I hold constant any time-invariant bank omitted variables and identify mortgage lending and branching decisions at the *same bank* in different counties conditional on the county's distance to the CEO's hometown. Furthermore, the inclusion of bank FE also means that unobserved time-invariant bank characteristics that simultaneously explain the matching between CEOs, banks and business policies (see for e.g., Custodio and Metzger, 2014) are controlled for. I address this in greater detail in the next section.

The inclusion of county-year fixed effects removes any time-varying county-level factors such as demographic, social, economic as well as demand-side factors related to local business cycles, industry consumption, and housing demand (Gilje et al., 2016). In addition, it also controls for the possibility that the results are driven by changes in state foreclosure or anti-predatory lending laws that could affect bank origination behavior in different geographical locations (Agarwal, Amromin, Ben-David, Chomsisengphet, and Evanoff, 2014; Di Maggio and Kermani, 2017).

With these two sets of fixed effects, the estimations are identified by two sources of variation: (1) varying distances between a CEO's hometown to different counties in the same bank; and (2) changes in the distance between the CEO's hometown and a given county in the same bank that arises because of CEO turnovers. Therefore, the coefficient of interest β_1 compares the mortgage origination and branching decisions of the same bank in two identical counties, which varies only by distance to the CEO's hometown.

I also include a number of time-varying control variables, the most important of which is *Ln(dist. HQ)*. *Ln(dist. HQ)* is the natural logarithm of the physical distance between a bank's headquarters (HQ) to the counties where mortgage and branching decisions take place. I include this control variable as Landier, Nair, and Wulf (2009) and Giroud (2013) show that proximity to the firm's HQ influences how managers allocate labor resources and monitor non-HQ establishments. To further isolate the hometown effect from the HQ effect, I follow Yonker (2017b) and include the interaction term between *Ln(dist. HQ*) and *Ln(dist. hometown)* as an additional control variable.[62] Finally, controls for loan (% *female applicants*, *% minor applicants* and *Loan/Income*) and bank (*Assets, Leverage, ROA, Lending* and *Deposit*) characteristics are also included. Importantly, the inclusion of the borrower's loan-to-income ratio controls for the riskiness of the loan (a higher ratio implies that the loan is riskier as borrowers are less able to use their income to repay the loan). See Appendix 3-A1 for the definition and construction of these variables.

3.4.2 Baseline Results

In this section, I examine how a bank's mortgage origination (Columns (1)-(2)) and branching decisions (Column (3)) vary with distance to its CEO's hometown. Table 3-2 presents the baseline results.

Across all dependent variables, the coefficient on *Ln(dist. hometown)* is negative and statistically significant at the 1% level. This indicates that, within the same bank, counties located nearer to the CEO's hometown enjoy higher mortgage approvals (Column (1)), higher mortgage origination growth rates (Column (2)), and higher branch growth rates (Column (3)) as compared to counties located further away. The effects are economically meaningful. For instance, the magnitude of the coefficient estimate in Column (2) indicates that a one standard deviation decrease in ln distance to the CEO's hometown is associated with a 2.1% increase in mortgage origination growth (28% as compared to the mean).[63]

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[62] The results in this chapter remains qualitatively similar even when I do not include the interaction term.

[63] These values are calculated using the mean ln distance to HQ (6.53). I calculate 2.1% as [-0.006 + (-0.002*6.53)]*(1.105) while 28% is calculated as [(-0.006 + (-0.002*6.53))*(1.105)] / (0.074).

When looking at bank branching decisions in Column (3), I also show that being close to the CEO's hometown matters. This is noteworthy as the opening of bank branches constitutes real business decisions, therefore reflecting a conscious choice by the CEO to expand the operations of the bank to counties proximate to her hometown, possibly to facilitate increases in lending. This also rules out interpretations that credit officers located nearer to the hometown of the CEO are pursuing aggressive lending practices without the knowledge of the CEO.

In Appendix 3-A3, I show that the results are robust to using an alternate definition of CEO hometown favoritism. I create *Hometown state*, a dummy that equals 1 if the state in which the CEO was born and the state in which the mortgage originations and branch decisions take place is the same. Consistent with the main results, I find higher mortgage approval rates, higher mortgage origination and branch growth rates in the state of the CEO's birth as compared to other states. In sum, I find that banks lend more, and, open more branches in counties proximate to the CEO's hometown, suggesting that the CEO's hometown matters for bank business policies.

Table 3-2: CEO Hometown Favoritism and Mortgage Lending

This table reports estimates of an OLS regression which estimates the effect of CEO hometown favoritism on bank business policies. I report estimates of the following equation:

$$Y_{i,k,t} = \alpha_{i,k,t} + \beta_1 Ln(dist.\ hometown)_{i,k,t} + Loan\ Controls_{i,k,t} + Bank\ Controls_{i,t} + Bank\ FE + County\text{-}Year\ FE + \varepsilon_{i,k,t}$$

where subscripts *i*, *k* and *t* indicate bank, county and year respectively. *Y* is either: (1) *Approval rate*, defined as the number of approved mortgage loan applications divided by the total number of applications received; (2) *Δln($originated loan)*, defined as the logarithmic originated mortgage loans relative to the prior year divided by logarithmic originated loans in the prior year; or (3) *Δbranches*, defined as the number of branches minus the number of branches in the prior year scaled by number of branches in the prior year. *Ln(dist. hometown)* is the logarithmic distance between the bank CEO's hometown county and the county in which lending or branching decisions take place. The coefficient β_1 on *Ln(dist. hometown)* is the variable of interest. Standard errors are clustered at the bank-level. The sample covers the period 1999–2014 for which data on CEO birthplace is available. Refer to Appendix 3-A1 for the definition and construction of variables used in this chapter. The constant is suppressed. t-statistics are reported in parentheses. ***, **, and * indicate significance at the 1, 5 and 10% level, respectively.

	(1) Approval rate	(2) Δln($originated loan)	(3) Δbranches
Ln(dist. hometown)	-0.008***	-0.006***	-0.003***
	(-8.919)	(-4.813)	(-3.248)
Ln(dist. hometown) x Ln(dist. HQ)	0.000	-0.002***	0.001***
	(0.400)	(-8.596)	(3.173)
Ln(dist. HQ)	-0.004***	-0.023***	-0.007***
	(-4.591)	(-17.359)	(-5.751)
Assets	-0.021***	0.065***	0.023***
	(-9.626)	(20.675)	(5.473)
Leverage	-0.755***	-0.936***	0.029
	(-20.076)	(-16.790)	(0.429)
ROA	0.011***	0.007***	0.002*
	(15.417)	(7.252)	(1.889)
Lending	0.004	0.191***	0.102***
	(0.464)	(13.938)	(5.574)
Deposit	0.352***	0.082***	0.033
	(36.103)	(5.743)	(1.416)
%female applicants	-0.085***	-0.049***	
	(-34.595)	(-11.759)	
%minor applicants	-0.142***	-0.152***	
	(-70.978)	(-48.258)	
Loan/Income	0.000	-0.001*	
	(0.697)	(-1.835)	
Bank FE	Yes	Yes	Yes
County-Year FE	Yes	Yes	Yes
Adj. R-squared	0.178	0.097	0.058
Observations	558,932	408,184	85,086

3.5 Endogeneity

In this section, I employ multiple strategies to establish a causal link between CEO hometown favoritism and mortgage origination and branching decisions. The first and main endogeneity concern is the endogenous CEO-bank matching problem. The second concern is that hometown favoritism of CEOs might be correlated with other observable CEO characteristics. Lastly, there is also a concern about potential measurement errors related to the hometown favoritism variable.

3.5.1 Addressing CEO-Bank Matching

The main endogeneity concern is that there exists the possibility that unobserved bank heterogeneity (both time-invariant and time-variant) simultaneously explains the matching between CEOs and banks. For instance, banks that have traditionally lent more to counties proximate to the CEO's hometown (or have time-invariant unobservable bank characteristics such as the mission ethos of the bank that targets these areas) could attract a CEO whose preference is to operate in these counties. If so, my findings that banks lend more in counties proximate to the hometown of CEOs could simply reflect the matching between CEOs and these banks, and not CEO hometown favoritism.

In all models in this chapter, I follow the literature (e.g., Malmendier, Tate, and Yan, 2011; Graham, Harvey, and Puri, 2013; Custodio and Metzger, 2014; Dittmar and Duchin, 2016; Schoar and Zuo, 2017) and rely on within-bank variation to deal with this time-invariant CEO-bank matching. The inclusion of bank FE means that the hometown variable is partially identified by *changes* in distance to the counties that the bank operates in as a result of *change*s in bank CEOs at the same bank (because

CEOs at the same bank can have different hometowns).[64] Thus, any time-invariant bank characteristics that would be correlated with a CEO's preference to join any particular bank would be differenced out, and my results can be interpreted as causal.

The inclusion of bank fixed effects to mitigate CEO-bank matching due to time-invariant omitted bank heterogeneity is only valid if these omitted variables remain constant through time. However, if changes in bank strategies or characteristics are the primary determinants behind CEO turnovers, the concern is that the hometown favoritism effect of incoming CEOs is irrelevant, as the board of directors would select *any* CEO willing to implement the bank's new business strategy of expanding its business to areas proximate to the hometown of the CEO (Fee et al., 2013). As an example, banks with a new business plan to expand to California would be more likely to appoint a California-born CEO and, at the same time, implement strategies to open more branches and increase lending in California.

If so, the hometown favoritism effect I document would in fact be attributed to the bank (led by the board of directors), and not the CEO. This implies that some CEO changes could be driven by endogenous policy considerations set by the board and that using these turnovers for identification would cause me to *over*-attribute the hometown favoritism effect to the CEO (Fee et al., 2013).[65] To mitigate this time-varying CEO-bank matching concern, I use two identification strategies: (1) a subsample of

[64] To illustrate, in 2013, William Demchak (born in Pittsburgh, Pennsylvania) replaced James E Rohr (born in Cleveland, Ohio) as CEO of PNC Financial Services Group Inc. This generates a change to the distance between the CEO's hometown and a given county. For example, Lake County is 50 km away from the outgoing CEO James E Rohr's birthplace but is 213 km away from the new CEO William Demchak's birthplace.

[65] See Fee et al. (2013) and Custodio and Metzger (2014) for a detailed discussion on CEO-firm matching.

exogenous CEO turnovers and; (2) exogenous changes in credit markets (boom-bust periods).

3.5.1.1 Exogenous Turnovers

The first strategy focuses on a subsample of banks that experience changes in their CEO for plausibly exogenous reasons. To illustrate the intuition behind this strategy, consider an unfortunate situation where the current CEO suddenly passes away due to natural reasons, thereby forcing the board to reappoint a new CEO. The timing of this turnover event is likely to be exogenous because the board is unable to anticipate the CEO's sudden death, and therefore, the appointment of the new CEO is unlikely to be driven by policy considerations concerning lending or branching decisions near the CEO's hometown. Thus, exogenous CEO turnovers produces a shock to the distance to a CEO's hometown while being exogenous to the business strategies of the bank.[66]

To classify exogenous CEO turnovers, articles from the company's press release and reputable news journals such as the *Wall Street Journal* or *The Financial Times* are read to obtain the reason behind changes in CEOs. A turnover is considered as exogenous if it meets at least one of the following three criteria's: (1) the outgoing CEO departs as a result of death or illness; (2) is above 60 years old, and; (3) the turnover occurs as part of the bank's succession plan. Using the above criteria, I find that 60% of CEO turnovers can be classified as exogenous, consistent with the frequency of 'exogenous' turnovers classified in the literature (e.g., Dittmar and Duchin, 2016).

[66] While the choice of the incoming CEO is not random, the timing of the turnover is likely to be exogenous (Custodio and Metzger, 2014; Bushman et al., Forthcoming).

Table 3-3: Exogenous CEO Turnover Events

This table reports estimates of an OLS regression which estimates the effect of CEO hometown favoritism on bank business policies around CEO turnover events. I report estimates of the following equation:

$$Y_{i,k,t} = \alpha_{i,k,t} + \beta_1 Ln(dist.\ hometown)_{i,k,t} + Loan\ Controls_{i,k,t} + Bank\ Controls_{i,t} + Bank\ FE + County\text{-}Year\ FE + \varepsilon_{i,k,t}$$

where subscripts *i*, *k* and *t* indicate bank, county and year respectively. *Y* is either: (1) *Approval rate*, defined as the number of approved mortgage loan applications divided by the total number of applications received; (2) *Δln($originated loan)*, defined as the logarithmic originated mortgage loans relative to the prior year divided by logarithmic originated loans in the prior year; or (3) *Δbranches*, defined as the number of branches minus the number of branches in the prior year scaled by number of branches in the prior year. *Ln(dist. hometown)* is the logarithmic distance between the bank CEO's hometown county and the county in which lending or branching decisions take place. The coefficient β_1 on *Ln(dist. hometown)* is the variable of interest. In Panel A, I only include banks which have experienced at least one exogenous CEO turnover event. Exogenous CEO turnovers are defined as one of the following reasons: CEO's death, CEO's long-term illness, the turnover is part of a long-planned retirement, or the turnover takes place when the CEO is at least 60 years of age. In Panel B, I only include banks which have experienced at least one internal CEO turnover event. Internal CEO turnovers are defined as when the new CEO is an existing employee of the bank. Control variables include: *Assets, Leverage, ROA, Lending, Deposit, %female applicants, %minor applicants* and *Loan/Income.* Standard errors are clustered at the bank-level. The sample covers the period 1999–2014 for which data on CEO birthplace is available. Refer to Appendix 3-A1 for the definition and construction of variables used in this chapter. The constant is suppressed. t-statistics are reported in parentheses. ***, **, and * indicate significance at the 1, 5 and 10% level, respectively.

Panel A: Exogenous Turnovers	(1) Approval rate	(2) Δln($originated loan)	(3) Δbranches
Ln(dist. hometown)	-0.012***	-0.007***	-0.004***
	(-9.035)	(-4.202)	(-2.621)
Ln(dist. hometown) x Ln(dist. HQ)	0.001***	-0.000	0.001***
	(3.269)	(-0.988)	(3.101)
Ln(dist. HQ)	-0.006***	-0.026***	-0.007***
	(-4.129)	(-14.364)	(-4.548)
Bank FE	Yes	Yes	Yes
County-Year FE	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes
Adj. R-squared	0.196	0.104	0.049
Observations	365,993	283,372	61,259

Panel B: Internal Turnovers	(1) Approval rate	(2) Δln($originated loan)	(3) Δbranches
Ln(dist. hometown)	-0.012***	-0.011***	-0.003**
	(-8.848)	(-6.157)	(-2.367)
Ln(dist. hometown) x Ln(dist. HQ)	0.001***	-0.000	0.001**
	(3.833)	(-1.051)	(2.008)
Ln(dist. HQ)	-0.005***	-0.025***	-0.006***
	(-3.942)	(-13.642)	(-3.240)
Bank FE	Yes	Yes	Yes
County-Year FE	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes
Adj. R-squared	0.199	0.106	0.039
Observations	360,624	274,379	60,078

In Panel A of Table 3-3, I re-run the baseline analysis with only banks that experience at least one exogenous turnover following Dittmar and Duchin (2016). As before, bank FE and county-year FE are included. As observed, the coefficient of interest *Ln(dist. hometown)* remains statistically significant at the 1% level for all the dependent variables.[67]

In Panel B of Table 3-3, I focus on an alternative CEO turnover event; internal CEO turnovers. Internal CEO turnovers are CEO appointment events where the incoming CEO was already an employee of the bank before taking over the role of CEO. In this turnover setting, the choice of an internal CEO successor is likely to reflect continuity in the business strategy of the bank and less likely to reflect major changes in bank policies (Dittmar and Duchin, 2016). When limiting the sample of banks to only those that experience at least one internal turnover event, I find that *Ln(dist. hometown)* is still negative and significant. Taken together, the results from this section suggests that CEO hometown favoritism is likely to be an idiosyncratic style and that endogenous CEO-bank matching is unlikely to explain my findings.

3.5.1.2 Boom-Bust Periods

In the second strategy, I follow Opler and Titman (1994) and Yonker (2017b) and exploit exogenous variations in macroeconomic conditions —periods of economic booms and busts— to further alleviate concerns relating to time-varying endogenous CEO-bank matching. This approach has several advantages. First, boom and bust periods are largely exogenous to the CEO-bank matching process as firms do not

[67] It is important to note that I exploit cross-sectional variation in proximity to CEO hometown following exogenous turnovers. This differs from findings in Fee et al. (2013) who show that on average, exogenous turnovers are not followed by changes in firm policies.

change their CEOs in anticipation of business cycles.[68] Therefore, any change in bank policies during these periods should be largely exogenous to the CEO-bank matching process and can be directly attributed to the CEO.

Second, this approach allows me to observe if a CEO's hometown favoritism persists through changes in the external business environment, where business decisions are likely to be more complex and unstructured, and thus, more likely to be influenced by the CEO's innate characteristics. Lastly, the use of both boom and bust periods allows me to *contrast* decisions made by the CEO in her hometown when credit conditions are tight and likely to be most crucial for borrowers (bust periods) as compared to when credit is loose and additional credit is unlikely to matter (boom periods). Should CEO hometown favoritism matter, I should expect to observe increases in bank lending during bust periods as compared to boom periods.

Boom (*Boom*) years are defined as years 2004 to 2006 while bust (*Bust*) periods are years 2007 and 2008 and take the values of 1 for these years and 0 otherwise. *Boom* and *Bust* years are defined following house price growth rates in the U.S.; house prices in the U.S. grew aggressively from 2004 to 2006 and started to decline in 2007, marking the start of the subprime financial crisis.[69] The coefficients of interest are the interaction terms *Bust x Ln(dist. hometown)* and *Boom x Ln(dist. hometown).* A similar set of control variables and fixed effects are included as before. Table 3-4 reports the results.

[68] Fahlenbrach and Stulz (2011) show evidence that CEOs of banks whose incentives were better aligned with shareholders suffered larger losses in their compensation during the crisis, suggesting the inability of bank CEOs to anticipate the crisis.
[69] House price index data is obtained from https://fred.stlouisfed.org/series/USSTHPI.

The coefficient on *Bust x Ln(dist. hometown)* is negative and statistically significant in two out of the three columns (less Column (3)). This means that, during periods of distress, bank CEOs continue to extend more mortgage credit to counties proximate to their hometown (Columns (1)-(2)). While the coefficient is insignificant when looking at branch growth rates in Column (3), it is still negative. One reason that can explain this is that while CEOs could still continue lending to counties that are proximate to her hometown in economic downturns, branching decisions might be more inelastic and sticky, and thus lose power in the regression tests.

The coefficient on *Boom x Ln(dist.hometown)* is statistically insignificant in all columns. This suggests that there is no hometown favoritism when credit is loose and when applicants are less likely to be declined credit. Taken together, the results clearly show that CEO hometown favoritism is particularly salient in times of economic downturns. Bank CEOs make a conscious choice during bust periods to continue extending mortgage credit to borrowers nearer to their hometown, when borrowers require it the most.

3.5.2 Omitted CEO Characteristics

Another endogeneity concern is that CEO hometown favoritism might be correlated with other observable CEO characteristics. For instance, CEOs who lend more to counties nearer to their hometowns could be overconfident, as they might falsely believe that they have superior information on economic conditions in their hometown (even if they do not). To rule out this concern, I include in Table 3-5 other observable CEO characteristics that have been shown to influence firm policies.

Table 3-4: CEO Hometown Favoritism: Boom and Bust Periods

This table reports estimates of an OLS regression which estimates the effect of CEO hometown favoritism on bank business policies during boom and bust periods. I report estimates of the following equation:

$$Y_{i,k,t} = \alpha_{i,k,t} + \beta_1 Boom_t \times Ln(dist.\ hometown)_{i,k,t} + \beta_2 Bust_t \times Ln(dist.\ hometown)_{i,k,t} + Loan\ Controls_{i,k,t} + Bank\ Controls_{i,t} + Bank\ FE + County\text{-}Year\ FE + \varepsilon_{i,k,t}$$

where subscripts *i*, *k* and *t* indicate bank, county and year respectively. *Y* is either: (1) *Approval rate*, defined as the number of approved mortgage loan applications divided by the total number of applications received; (2) *Δln($originated loan)*, defined as the logarithmic originated mortgage loans relative to the prior year divided by logarithmic originated loans in the prior year; or (3) *Δbranches*, defined as the number of branches minus the number of branches in the prior year scaled by number of branches in the prior year. *Ln(dist. hometown)* is the logarithmic distance between the bank CEO's hometown county and the county in which lending or branching decisions take place. *Bust* is a dummy that equals 1 for years of 2007-2008 and 0 otherwise. *Boom* is a dummy that equals 1 for years of 2002-2004 and 0 otherwise. The coefficient β_1 on *Boom x Ln(dist. hometown)* and β_2 on *Bust x Ln(dist. hometown)* are the variables of interest. Control variables include: *Bust x Ln(dist. HQ), Boom x Ln(dist. HQ), Ln(dist. HQ), Assets, Leverage, ROA, Lending, Deposit, %female applicants, %minor applicants* and *Loan/Income*. Standard errors are clustered at the bank-level. The sample covers the period 1999–2014 for which data on CEO birthplace is available. Refer to Appendix 3-A1 for the definition and construction of variables used in this chapter. The constant is suppressed. t-statistics are reported in parentheses. ***, **, and * indicate significance at the 1, 5 and 10% level, respectively.

	(1) Approval rate	(2) Δln($originated loan)	(3) Δbranches
Bust x Ln(dist. hometown)	-0.011***	-0.016***	-0.002
	(-4.615)	(-4.925)	(-1.195)
Bust x Ln(dist. hometown) x Ln(dist. HQ)	0.000	0.004***	0.000
	(0.935)	(7.196)	(1.205)
Boom x Ln(dist. hometown)	-0.003	0.001	-0.002
	(-1.512)	(0.514)	(-0.768)
Boom x Ln(dist. hometown) x Ln(dist. HQ)	0.001**	-0.002***	0.000
	(2.169)	(-4.312)	(0.884)
Ln(dist. hometown) x Ln(HQ)	-0.000	-0.002***	0.000**
	(-0.960)	(-7.864)	(2.159)
Ln(dist. hometown)	-0.006***	-0.004***	-0.003**
	(-5.421)	(-2.847)	(-2.318)
Bank FE	Yes	Yes	Yes
County-Year FE	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes
Adj. R-squared	0.178	0.097	0.058
Observations	558,932	408,184	85,086

Specifically, in Table 3-5, I control for: the education background of the CEO, if the CEO is from an Ivy League or has an MBA (Bertrand and Schoar, 2003; Custodio, Ferreira, and Matos, 2013); the age of the CEO (Yim, 2013); if the CEO was born during the Great Depression in years 1920-1929 (Malmendier et al., 2011); if the CEO began her career in a recession (Schoar and Zuo, 2017); if the CEO is overconfident (Malmendier et al., 2011); and if the CEO has military experience (Benmelech and Frydman, 2015).[70]

The variable of interest *Ln(dist. hometown)* remains negative and statistically significant at the 1% level across all three dependent variables. This gives me confidence that the hometown favoritism measure is not capturing other observable CEO characteristics.

3.5.3 Refining Definitions of CEO's Hometown Favoritism

The implicit assumption behind the measurement of CEO hometown favoritism is that it is constructed using the CEO's birth county and state. However, one could argue that there are measurement errors associated with the variable. For instance, the variable might not capture CEO hometown effects if the CEO's family relocates to a new place soon after the CEO's birth.

[70] I thank Abhishek Srivastav and Tim King for providing data on bank CEO overconfidence and military experience.

Table 3-5: Other CEO Characteristics

This table reports estimates of an OLS estimation regression which estimates the effect of CEO hometown favoritism on bank business policies while controlling for other observable CEO characteristics. I report estimates of the following equation:

$$Y_{i,k,t} = \alpha_{i,k,t} + \beta_1 Ln(dist.\ hometown)_{i,k,t} + CEO\ characteristics_{i,t} + Bank\ Controls_{i,t} + Bank\ FE + County\text{-}Year\ FE + \varepsilon_{i,k,t}$$

where subscripts *i*, *k* and *t* indicate bank, county and year respectively. *Y* is either: (1) *Approval rate*, defined as the number of approved mortgage loan applications divided by the total number of applications received; (2) *Δln($originated loan)*, defined as the logarithmic originated mortgage loans relative to the prior year divided by logarithmic originated loans in the prior year; or (3) *Δbranches*, defined as the number of branches minus the number of branches in the prior year scaled by number of branches in the prior year. *Ln(dist. hometown)* is the logarithmic distance between the bank CEO's hometown county and the county in which lending or branching decisions take place. The coefficient β_1 on *Ln(dist. hometown)* is the variable of interest. *CEO characteristics* includes observable CEO characteristics: *Ivy League*, a dummy that equals 1 if the CEO obtains a degree from an Ivy League institution; *MBA*, a dummy that equals 1 if the CEO has an MBA degree; *Age*, the age of CEO; *Depression baby* is a dummy that equals 1 if the CEO is born between 1920 and 1929; *Crisis career starter* is a dummy that equals 1 if the CEO starts her career (assuming at the age of 22) during a crisis period (defined in the NBER database); *Overconfidence* is a dummy variable that equals 1 if moneyness of the option holdings is 67% and above; *Military experience* is a dummy that equals 1 if the CEO has prior military experience. Control variables include: *Assets, Leverage, ROA, Lending, Deposit, %female applicants, %minor applicants* and *Loan/Income*. Standard errors are clustered at the bank-level. The sample covers the period 1999–2014 for which data on CEO birthplace is available. Refer to Appendix 3-A1 for the definition and construction of variables used in this chapter. The constant is suppressed. t-statistics are reported in parentheses. ***, **, and * indicate significance at the 1, 5 and 10% level, respectively.

	(1) Approval Rate	(2) Δln($originated loan)	(3) Δbranches
Ln(dist. hometown)	-0.004*	-0.028***	-0.006***
	(-1.879)	(-7.646)	(-3.002)
Ln(dist. hometown) x Ln(dist. HQ)	0.000	0.001**	0.001***
	(1.011)	(2.366)	(3.083)
Ln(dist. HQ)	-0.011***	-0.042***	-0.007***
	(-4.925)	(-11.726)	(-3.266)
MBA	-0.045*	-0.053*	-0.264***
	(-1.724)	(-1.855)	(-3.356)
Ivy League	-0.195***	-0.210***	-0.662***
	(-6.282)	(-3.048)	(-2.843)
Age	-0.006***	-0.008***	0.002***
	(-12.213)	(-14.325)	(2.804)
Depression baby	-0.073	-0.021	0.269
	(-1.444)	(-0.076)	(1.129)
Crisis career starter	0.045***	-0.015***	-0.015*
	(12.224)	(-2.924)	(-1.746)
Overconfidence	-0.032***	0.009	-0.014
	(-7.472)	(1.463)	(-1.460)
Military experience	0.159***	0.096**	0.326***
	(4.214)	(2.089)	(3.722)
Bank FE	Yes	Yes	Yes
County-Year FE	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes
Adj. R-squared	0.122	0.131	0.086
Observations	209,237	158,544	32,310

In this section, I refine the CEO hometown measure to sharpen inference. First, I condition the baseline results on the CEO's degree of attachment to her hometown by interacting *Ln(dist. hometown)* with *Hometown UG,* a dummy variable that equals 1 if the CEO undertakes an undergraduate degree in the same state as her birth state. The intuition behind this test is that CEOs who attended university in the state of her birth should be more attached to her hometown as she would have spent most of her formative years till after university in the same state (or area) and would have had the opportunity to establish deep relationships and emotional attachments (Mesch and Manor, 1998). In contrast, a CEO who attended university in a non-hometown state would imply that the CEO has likely moved away prior to university, or at the very least, moved away from the state of birth to attend university and subsequently, be less likely to be attached to it.

Second, since 58% of CEOs in the sample work for a bank headquartered in the same state as their birth state, the baseline findings may capture effects linked to a bank's HQ location.[71] To completely isolate the CEO's hometown favoritism effect from the bank's HQ effect, I interact *Ln(dist. hometown)* with *Out-of-state CEO*, a dummy variable that equals 1 if the CEO was born in a state different from the bank's HQ state. Table 3-6 displays the interactions results with *Hometown UG* in Panel A and *Out-of-state CEO* in Panel B.

[71] This possibility is remote since I already control for *Ln(dist. HQ).*

Table 3-6: Refining Measures of CEO's Hometown Favoritism

This table reports estimates of an OLS estimation regression which estimates the cross-sectional CEO hometown favoritism effects on bank business policies. I report estimates of the following equation:

$$Y_{ikt} = \alpha_{ikt} + \beta_1 CEO\ characteristics_{it} \times Ln(dist.\ hometown)_{ikt} + Loan\ Controls_{ikt} + Bank\ Controls_{it} + Bank\ FE + County\text{-}Year\ FE + \varepsilon_{ikt}$$

where subscripts *i*, *k* and *t* indicate bank, county and year respectively. *Y* is either: (1) *Approval rate*, defined as the number of approved mortgage loan applications divided by the total number of applications received; (2) *Δln($originated loan)*, defined as the logarithmic originated mortgage loans relative to the prior year divided by logarithmic originated loans in the prior year; or (3) *Δbranches*, defined as the number of branches minus the number of branches in the prior year scaled by number of branches in the prior year. *Ln(dist. hometown)* is the logarithmic distance between the bank CEO's hometown county and the county in which lending or branching decisions take place. *CEO characteristics* is either: *Hometown UG*, defined as a dummy that equals 1 if the CEO undertakes an undergraduate degree in the same state as her birth state in Panel A or *Out-of-state CEO*, defined as a dummy that equals 1 if the CEO was born in a state different from the bank's HQ state. The coefficient β_1 on *CEO characteristics x Ln(dist. hometown)* is the variable of interest. Control variables include: (*Hometown UG x Ln(dist. HQ), Hometown UG* in Panel A), (*Out-of-state CEOs x Ln(dist. HQ),Out-of-state CEOS* in Panel B*), Ln(dist. HQ), Assets, Leverage, ROA, Lending, Deposit, %female applicants, %minor applicants* and *Loan/Income*. Standard errors are clustered at the bank-level. The sample covers the period 1999–2014 for which data on CEO birthplace is available. Refer to Appendix 3-A1 for the definition and construction of variables used in this chapter. The constant is suppressed. t-statistics are reported in parentheses. ***, **, and * indicate significance at the 1, 5 and 10% level, respectively.

Panel A: Birth State UG Degree CEOs	(1) Approval rate	(2) Δln($originated loan)	(3) Δbranches
Hometown UG x Ln(dist. hometown)	-0.018***	-0.008***	-0.005**
	(-8.905)	(-2.904)	(-2.260)
Hometown UG x Ln(dist. hometown) x Ln(dist. HQ)	0.001***	-0.001***	0.000
	(2.837)	(-2.921)	(1.044)
Ln(dist. hometown) x Ln(dist. HQ)	-0.001***	-0.001***	0.000
	(-5.234)	(-4.546)	(1.195)
Ln(dist. hometown)	0.007***	-0.000	-0.000
	(4.262)	(-0.060)	(-0.233)
Bank FE	Yes	Yes	Yes
County-Year FE	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes
Adj. R-squared	0.186	0.098	0.058
Observations	550,376	402,306	83,361

Panel B: Out-of-State CEOs	(1) Approval rate	(2) Δln($originated loan)	(3) Δbranches
Out-of-state CEO x Ln(dist. hometown)	-0.017***	-0.033***	0.002
	(-4.689)	(-6.489)	(0.506)
Out-of-state CEO x Ln(dist. hometown) x Ln(dist. HQ)	0.002***	0.007***	-0.000
	(3.178)	(9.986)	(-0.334)
Ln(dist. hometown) x Ln(dist. HQ)	0.001***	-0.003***	0.001**
	(2.971)	(-14.502)	(2.344)
Ln(dist. hometown)	-0.007***	-0.006***	-0.004***
	(-5.440)	(-3.485)	(-2.581)
Bank FE	Yes	Yes	Yes
County-Year FE	Yes	Yes	Yes

Other Controls	Yes	Yes	Yes
Adj. R-squared	0.185	0.098	0.056
Observations	558,932	408,184	85,086

As shown in Panel A of Table 3-6, the coefficient on *Hometown UG x Ln(dist. hometown)* is negative and statistically significant across all dependent variables, indicating that the CEO hometown favoritism effect (i.e., higher mortgage approval, mortgage loan origination and branch growth rates) at counties proximate to the CEO's hometown, is stronger when the CEO obtains her undergraduate degree from her birth state. This is consistent with the idea that when CEOs spend more time in their state of birth, they become more deeply rooted to the local community and exhibit a stronger tendency to favor their hometown. As nearly two-thirds of CEOs in the sample undertook their undergraduate degree in the state that they were born, the hometown favoritism effect that I document in the baseline analysis is underestimated.

Columns (1)-(2) of Panel B show statistically negative coefficient estimates on the interaction term *Out-of-state CEO x Ln(dist. hometown)*, indicating that the CEO hometown favoritism effect is stronger for out-of-state CEOs. For instance, a CEO who was born in California but now works for a bank in Ohio show more hometown favoritism to counties in California as compared to a California born CEO lending in Californian counties.

Overall, the results in this section show that endogeneity concerns pertaining to CEO-bank matching, omitted CEO characteristics and measurement errors are unlikely to influence the interpretation of my findings. Subsequently, any alternate endogeneity driven interpretation would have to persist through all the identification strategies and fixed effects. This gives me confidence that I am indeed identifying a causal link between CEO hometown favoritism and bank business policies (mortgage lending and branching decisions). The results also suggest that CEOs favor their hometown because they are emotionally attached to it and care for their hometown communities. I investigate this more in the next section.

3.6 Why Do Bank CEOs Show Favoritism to their Hometown?

Results in the previous section show that CEOs favor their hometown with more mortgage lending and bank branching decisions. In this section, I put forth three reasons to explain this finding. The three main reasons are: (1) informational advantages; (2) private benefits due to agency conflicts and; (3) altruistic hometown attachments.

First, CEOs could favor their hometown due to informational advantages (Coval and Moskowitz, 1999; 2001; Ivkovic and Weisbenner, 2005; Malloy, 2005).[72] CEOs could have local contacts that still reside and work in their hometowns and provide them with information regarding local economic conditions and trends (Cohen et al., 2008). CEOs could also be better informed about the local culture which reduces information asymmetry in loan decisions (Fisman et al., 2017). Finally, informational advantages could also arise through accessibility to key politicians and regulators who could provide information on legislations or tax policies that could influence bank business decisions (Mian et al., 2010; Duchin and Sosyura, 2012).

A second reason that could explain hometown favoritism is the pursuit of private benefits due to the presence of agency conflicts (Jensen and Meckling, 1976;

[72] Coval and Moskowitz (1991; 2001) and Ivkovic and Weisbenner (2005) show that mutual fund managers and individual investors overweight their investments towards local firms and subsequently, outperform in these holdings. Malloy (2005) find that local analysts make more accurate forecasts.

Shleifer and Vishny, 1997). Potential private benefits to a manager could be numerous and range from monetary benefits to personal utility. By conducting business in her hometown, a CEO could obtain local awards, local directorship positions as well as speaking arrangements. Further, these hometown favored business strategies of additional credit could also increase the utility of the CEO by increasing her status or popularity. Importantly, hometown favoritism motivated by agency conflicts could be seen as a form of corporate philanthropy to increase the private utility of the CEO at the expense of firm shareholders (Masulis and Reza, 2014).

Finally, the last reason why CEOs favor their hometown in lending and business decisions is due to an altruistic hometown attachment. Place attachment theory suggests that people develop deep attachments to places that they are familiar with such as their hometown, and that these attachments forms a key portion of their personal identity (Low and Altman, 1992; Manzo, 2003; Gieryn, 2000; Hernandez et al., 2007; Lewicka, 2011). These attachments are stronger the longer one resides in the area and increases with the strength of social bonds established in the area (Mesch and Manor, 1998). Importantly, place attachment theory suggests that individuals are more likely to invest their time and money as well as care more about the welfare of people that reside in their place of attachment (Vaske and Kobrin, 2001; Manzo and Perkins, 2006). Subsequently, it suggests that CEOs may be driven by an altruistic purpose to favor residents in their hometown in lending policies.

3.6.1 Bank-Level Evidence

Crucially, the three interpretations lead to different empirical predictions. If informational advantages were the reason behind my findings, the hometown favoritism effect that I document in the previous section would arise as an optimal business strategy of the bank. Therefore, I should observe that increases in hometown mortgage lending should lead to positive outcomes for banks. If hometown favoritism were driven by agency motives, hometown favoritism should be related to agency costs and lead to less favorable bank outcomes. Importantly, while hometown attachment could indeed manifest as private benefits accrued to the CEO in the presence of agency conflicts, what distinguishes the altruistic motive from the agency argument is that shareholders of the bank are *not* harmed by this altruistic hometown attachment. In this interpretation, the resources of the bank are simply reallocated to serve the areas proximate to the hometown of the CEO.

Ideally, I would like to observe the ex-post performance of mortgage loans that were made by the bank in conjecture with hometown favoritism to disentangle between these reasons. Unfortunately, data unavailability prevents me from doing so. Instead, I rely on bank-level measures to infer and disentangle the possible explanations behind why CEOs favor their hometown. I estimate the following bank-level equation:

$$Y_{it} = \alpha_{it} + \beta_1 \%mortgage\ loan\ in\ home\ state + Bank\ Controls_{it} \\ + Bank\ FE + Year\ FE + \varepsilon_{kt} \quad (3\text{-}2)$$

where *i and t* indicate bank *i and* year *t* respectively. I create a new variable, *%mortgage loan in home state*, which is the proportion of mortgage loans a bank has in the state that the CEO was born in, and regress it against several bank-level outcomes (*Y*). *Y* is total loans scaled by total assets, bad loans and ROA. If hometown mortgage lending is optimal (suboptimal), I should observe that increases in hometown mortgage lending will lead to better (worst) bank outcomes. Instead, if hometown favoritism does not harm shareholder value, I should expect to see an insignificant sign on the coefficient of *%mortgage loan in home state*.

Results are shown in Table 3-7. In all specifications, I include bank and year fixed effects. I also conduct the analysis using a subsample of only *Out-of-state* CEOs as many CEOs work for banks headquartered in the state of their birth. As observed, the coefficient on *%mortgage loan in home state* is statistically insignificant at the conventional levels.[73] It is interesting to note that the proportion of total loans to total assets is not related to *%mortgage loan in home state* in Columns (1)-(2). This implies that CEOs do not change the proportion of their assets to increase mortgage lending to their hometowns, and that lending is simply reallocated from counties that are located further away to counties that are geographically proximate. Also, increases in mortgage lending to the CEO's hometown state is not related to aggregate loan performance of the loan portfolio (Columns (3)-(4)) or the profitability of the bank (Columns (5)-(6)). Taken together, the evidence seems to lend support to the altruistic hometown attachment interpretation of why CEOs favor their hometown in business policies.

3.6.2 CEO-Level Evidence

I next focus my attention on the individual traits of the CEO that would likely be related to altruistic behavior. If CEOs care more about fulfilling credit demands of residents nearer to her hometown for altruistic reasons, I should expect hometown effects to be more pronounced if CEOs display traits such as selflessness or patriotism. Put another way, I infer the *motive* of why CEOs lend more to their hometown, conditional on a number of traits that are related to altruism.

[73] The results are similar even I lag *%mortgage loan in home state* by one or two years.

As I am unable to directly observe a CEO's degree of altruism, I infer the value of these traits based on the CEO's cultural heritage. Nguyen et al. (Forthcoming) show that bank CEOs exhibit distinct behavior depending on the cultural values of the country from which their ancestors immigrated from. For instance, CEOs whose ancestors were from a country that emphasizes restraint make better use of a bank's economic resources.

Specifically, I infer the level of CEO altruism based on her inherited cultural values of: (1) *Collectivism,* which reflects an individual's integration to groups; (2)-(3) *Patriotism* and *Selflessness*, which measures how much a society values individual sacrifice for their country and other people; and (4) *Humane-oriented,* which measures the extent to which a society encourages an individual to be altruistic.[74]

CEO cultural scores based on her ancestor's country of origin is assigned to the CEO and interacted with *Ln(dist. hometown).*[75] If CEOs lend to their hometown for altruistic reasons, I should expect to observe that higher scores on the cultural variables (that is, a negative sign on the interaction) should lead to higher lending and branching decisions in counties proximate to the hometown of the CEO.

[74] Refer to Nguyen et al. (Forthcoming) for a detailed description of the data collection process. I thank the authors for providing the data on CEO cultural traits.
[75] If, for instance, the father of the CEO was from Italy, the score for Italy for these four traits would be assigned to the CEO.

Table 3-7: CEO Hometown Favoritism and Bank Performance

This table reports estimates of an OLS estimation regression which estimates the proportion of lending by the bank in the home state of the CEO to various measures of bank performance I report estimates of the following equation:

$$Y_{it} = \alpha_{it} + \beta_1 \%mortgage\ loan\ in\ home\ state_{it} + Bank\ Controls_{it} + Bank\ FE + Year\ FE + \varepsilon_{it}$$

where subscripts *i* and *t* indicate bank and year respectively. *Y* is either: (1) *Total Loans/Total Assets,* a bank's total loans divided by its total assets (Columns (1)-(2)); (2) *Bad Loans/Total Assets*, total non-performing loans divided by total assets (Columns (3)-(4)); or (3) *ROA*, net income divided by total assets (Columns (5)-(6)). *%mortgage loan in home state* is a bank's portion of mortgage lending made in the CEO's birth state. The coefficient β_1 on *%mortgage loan in home state* is the variable of interest. Standard errors are clustered at the bank-level. The sample covers the period 1999–2014 for which data on CEO birthplace is available. Refer to Appendix 3-A1 for the definition and construction of variables used in this chapter. The constant is suppressed. t-statistics are reported in parentheses. ***, **, and * indicate significance at the 1, 5 and 10% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	All CEOs	Out-state CEOs	All CEOs	Out-state CEOs	All CEOs	Out-state CEOs
	Total Loans/Total Assets		Non-performing loans/Total Assets		ROA	
%mortgage loan in home state	0.009	0.023	-0.001	-0.010	-0.015	0.792
	(0.960)	(0.332)	(-1.180)	(-1.013)	(-0.158)	(0.895)
Assets	0.008	-0.006	0.003	0.003	-0.192**	0.042
	(0.966)	(-0.396)	(1.328)	(0.475)	(-2.119)	(0.190)
Leverage	-0.139	-0.057	0.038	-0.008	-21.655***	-23.674***
	(-1.224)	(-0.208)	(0.932)	(-0.068)	(-12.771)	(-6.173)
ROA	0.001	-0.002	-0.007***	-0.006**		
	(0.632)	(-0.670)	(-8.974)	(-2.063)		
Lending			-0.019**	-0.052*	0.209	-0.515
			(-2.577)	(-1.704)	(0.636)	(-0.638)
Deposit	0.187***	0.111	0.022**	0.001	-2.101***	-1.853
	(2.969)	(1.070)	(1.995)	(0.045)	(-4.451)	(-1.445)
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-squared	0.145	0.167	0.515	0.338	0.411	0.383
Observations	5,357	922	5,357	922	5,357	922

Results of interactions of *Ln(dist. hometown)* with *Collectivism, Patriotism, Selflessness* and *Humane-oriented* are shown in Panels A, B, C and D of Table 3-8 respectively. The coefficient on the interaction terms are generally negative and statistically significant across all panels. This indicates that CEOs who inherit cultural values that place a greater emphasis on collectivism, patriotism, selflessness and humane-orientation make more lending near their hometown as compared to other CEOs. These results are in-line with CEOs favoring their hometown for non-self-serving reasons.

It is worth noting that these findings also rule out the agency explanation behind hometown favoritism. If agency motives prevailed, I should observe the *opposite* results in the interaction analysis; e.g., the hometown favoritism effects should be stronger when the CEO is *less collectivistic* (more individualistic).

3.6.3 Recipient Level Evidence

Should the altruistic hometown attachment interpretation explain why CEOs favor their hometown, I should expect the hometown favoritism effect to be more prevalent in counties and amongst loan applicants that are performing less well and require additional help (Vaske and Kobrin, 2001; Manzo and Perkins, 2006). This is consistent with earlier evidence that the hometown favoritism effect is stronger during periods of economic downturns, when credit conditions are tight and additional credit is likely to matter most. If hometown favoritism were driven by altruism, bank mortgage lending would be targeted specifically to groups of people that would require it the most.

Table 3-8: CEO Hometown Favoritism and Cultural Traits

This table reports estimates of an OLS estimation regression which estimates the CEO hometown favoritism effects on bank business policies conditional on the cultural characteristics of the CEO. I report estimates of the following equation:

$$Y_{ikt} = \alpha_{ikt} + \beta_1 CEO\ characteristics_{it} \times Ln(dist.\ hometown)_{ikt} + Loan\ Controls_{ikt} + Bank\ Controls_{it} + Bank\ FE + County\text{-}Year\ FE + \varepsilon_{ikt}$$

where subscripts *i*, *k* and *t* indicate bank, county and year respectively. *Y* is either: (1) *Approval rate*, defined as the number of approved mortgage loan applications divided by the total number of applications received; (2) *Δln($originated loan)*, defined as the logarithmic originated mortgage loans relative to the prior year divided by logarithmic originated loans in the prior year; or (3) *Δbranches*, defined as the number of branches minus the number of branches in the prior year scaled by number of branches in the prior year. *Ln(dist. hometown)* is the logarithmic distance between the bank CEO's hometown county and the county in which lending or branching decisions take place. *CEO characteristics* are the CEO's inherited cultural values of *Collectivism*, which reflects an individual's integration in groups (Panel A); *Patriotism* and *Selflessness*, which capture how much a society values individual sacrifice for their own country and other people (Panels B and C); and *Humane-oriented*, which measures the extent to which a society encourages an individual to be altruistic (Panel D). The coefficient β_1 on *CEO characteristics x Ln(dist. hometown)* is the variable of interest. Control variables include: (*Collectivism x Ln(dist. HQ), Collectivism* in Panel A), (*Patriotism x Ln(dist. HQ), Patriotism* in Panel B), (*Selflessness x Ln(dist. HQ), Selflessness* in Panel C), (*Humane-oriented x Ln(dist. HQ), Human-oriented* in Panel D), *Ln(dist. HQ), Assets, Leverage, ROA, Lending, Deposit, %female applicants, %minor applicants* and *Loan/Income*. Standard errors are clustered at the bank-level. The sample covers the period 1999–2014 for which data on CEO birthplace is available. Refer to Appendix 3-A1 for the definition and construction of variables used in this chapter. The constant is suppressed. t-statistics are reported in parentheses. ***, **, and * indicate significance at the 1, 5 and 10% level, respectively.

Panel A: CEO's Collectivism Culture	(1) Approval rate	(2) Δln($originated loan)	(3) Δbranches
Collectivism x Ln(dist. hometown)	-0.017***	-0.034***	-0.004
	(-4.381)	(-6.269)	(-0.946)
Collectivism x Ln(dist. hometown) x Ln(dist. HQ)	0.001*	0.004***	0.001
	(1.897)	(4.818)	(1.064)
Ln(dist. hometown) x Ln(dist. HQ)	-0.004*	-0.020***	-0.004
	(-1.747)	(-5.910)	(-1.043)
Ln(dist. hometown)	0.064***	0.143***	0.014
	(3.942)	(6.233)	(0.751)
Bank FE	Yes	Yes	Yes
County-Year FE	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes
Adj. R-squared	0.226	0.105	0.083
Observations	298,238	197,651	41,435

Panel B: CEO's Patriotism Culture	(1) Approval rate	(2) Δln($originated loan)	(3) Δbranches
Patriotism x Ln(dist. hometown)	-0.009**	-0.050***	-0.005
	(-2.089)	(-8.121)	(-1.019)
Patriotism x Ln(dist. hometown) x Ln(dist. HQ)	0.000	0.007***	0.001
	(0.753)	(7.342)	(1.108)
Ln(dist. hometown) x Ln(dist. HQ)	-0.002	-0.028***	-0.005
	(-0.736)	(-8.979)	(-1.323)
Ln(dist. hometown)	0.027*	0.178***	0.015

	(1.791)	(8.319)	(0.943)
Bank FE	Yes	Yes	Yes
County-Year FE	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes
Adj. R-squared	0.239	0.108	0.091
Observations	298,238	197,651	41,435

Panel C: CEO's Selflessness Culture	(1) Approval rate	(2) Δln($originated loan)	(3) Δbranches
Selflessness x Ln(dist. hometown)	-0.011	-0.134***	-0.022*
	(-1.102)	(-9.538)	(-1.886)
Selflessness x Ln(dist. hometown) x Ln(dist. HQ)	0.002	0.021***	0.003
	(1.311)	(10.189)	(1.110)
Ln(dist. hometown) x Ln(dist. HQ)	-0.001	-0.012***	-0.002*
	(-1.143)	(-16.975)	(-1.767)
Ln(dist. hometown)	-0.001	0.048***	0.006
	(-0.159)	(9.565)	(1.479)
Bank FE	Yes	Yes	Yes
County-Year FE	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes
Adj. R-squared	0.239	0.108	0.091
Observations	298,238	197,651	41,435

Panel D: CEO's Humane-oriented Culture	(1) Approval rate	(2) Δln($originated loan)	(3) Δbranches
Humane-oriented x Ln(dist. hometown)	-0.024***	-0.041***	-0.004
	(-7.667)	(-8.905)	(-1.178)
Humane-oriented x Ln(dist. hometown) x Ln(dist. HQ)	0.002***	0.005***	0.001
	(4.565)	(7.051)	(1.106)
Ln(dist. hometown) x Ln(dist. HQ)	-0.008***	-0.023***	-0.003
	(-4.201)	(-8.438)	(-1.084)
Ln(dist. hometown)	0.091***	0.165***	0.014
	(7.129)	(8.947)	(0.950)
Bank FE	Yes	Yes	Yes
County-Year FE	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes
Adj. R-squared	0.226	0.105	0.083
Observations	298,238	197,651	41,435

In Panel A of Table 3-9, I condition the baseline results on county-level characteristics. I define *Struggle county* in Columns (1)-(2) as the % unemployment rate of the county and as the % of non-home owners in the county in Columns (3)-(4) respectively and interact it with *Ln(dist. hometown)*.[76] The negative coefficient on the interaction term *Struggle county x Ln(dist. hometown)* in all columns indicate that CEOs lend more to proximate counties with weaker economic conditions, i.e., those with high unemployment rates and in counties with a higher proportion of residents staying in rented homes.

In Panel B of Table 3-9, I directly condition the results on the characteristics of the mortgage applicants received by the bank in a county-year. *Marginal applicant* is defined in Columns (1)-(2) using the applicant's reverse income decile where a higher index indicates poorer applicants; in (3)-(4) using the applicant's loan-to-income where a higher ratio indicates riskier applicants; and in (5)-(6) as the ratio of non-white applicants reviewed by the bank in the county. I then interact *Marginal applicant* with *Ln(dist. hometown).*

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[76] A high proportion of non-home ownership indicates that a large proportion of local residents live houses that they rent, instead of own.

Table 3-9: CEO Hometown Favoritism and Mortgage Loan Recepients

This table reports estimates of an OLS estimation regression which estimates CEO hometown favoritism on bank business policies conditional on county and applicant characteristics I report estimates of the following equation:

$$Y_{ikt} = \alpha_{ikt} + \beta_1 \textit{Struggle county}_{kt} \textit{ or Marginal applicant}_{ikt} \textit{ x Ln(dist. hometown)}_{ikt} + \textit{Loan Controls}_{ikt} + \textit{Bank Controls}_{it} + \textit{Bank FE} + \textit{County-Year FE} + \varepsilon_{ikt}$$

where subscripts *i*, *k* and *t* indicate bank, county and year respectively. *Y* is either: (1) *Approval rate*, defined as the number of approved mortgage loan applications divided by the total number of applications received; (2) *Δln($originated loan)*, defined as the logarithmic originated mortgage loans relative to the prior year divided by logarithmic originated loans in the prior year; or (3) *Δbranches*, defined as the number of branches minus the number of branches in the prior year scaled by number of branches in the prior year. *Ln(dist. hometown)* is the logarithmic distance between the bank CEO's hometown county and the county in which lending or branching decisions take place. In Panel A, *Struggle county* is defined using the county's unemployment rate (Columns (1)-(2)) or the county's proportion of houses not occupied by its owner (Columns (3)-(4)). In Panel B, *Marginal applicant* is defined using the mortgage applicant's reverse income decile (Columns (1)-(2)), loan-to-income ratio (Columns (3)-(4)), or race (Columns (5)-(6)). The coefficient β_1 on *Struggle county or Marginal applicant x Ln(dist. hometown)* are the variables of interest. Control variables include: (*Struggle county x Ln(dist. HQ), Struggle county* in Panel A), (*Marginal applicant x Ln(dist. HQ), Marginal applicant* in Panel B), *Ln(dist. HQ), Assets, Leverage, ROA, Lending, Deposit, %female applicants, %minor applicants* and *Loan/Income*. Standard errors are clustered at the bank-level. The sample covers the period 1999–2014 for which data on CEO birthplace is available. Refer to Appendix 3-A1 for the definition and construction of variables used in this chapter. The constant is suppressed. t-statistics are reported in parentheses. ***, **, and * indicate significance at the 1, 5 and 10% level, respectively.

Panel A: Struggling Counties	(1)	(2)	(3)	(4)
Struggle county defined as:	Unemployment rate		%non-home owner	
	Approval rate	Δln($originated loan)	Approval rate	Δln($originated loan)
Struggle county x Ln(dist. hometown)	-0.001***	-0.003***	-0.019**	-0.035***
	(-2.873)	(-5.072)	(-2.481)	(-3.272)
Struggle county x Ln(dist. hometown) x Ln(dist. HQ)	0.000***	0.001***	0.007***	0.006***
	(3.306)	(6.140)	(4.877)	(3.454)
Ln(dist. hometown) x Ln(dist. HQ)	-0.001***	-0.005***	-0.002***	-0.003***
	(-2.968)	(-8.884)	(-4.222)	(-5.375)
Ln(dist. hometown)	-0.001	0.011***	-0.002	0.004
	(-0.371)	(3.071)	(-0.681)	(0.949)
Bank FE	Yes	Yes	Yes	Yes
County-Year FE	Yes	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes	Yes
Adj. R-squared	0.184	0.101	0.178	0.098
Observations	558,051	407,556	558,051	407,556

Panel B: Marginal Applicants	(1)	(2)	(3)	(4)	(5)	(6)
Marginal applicant defined as:	Reverse Income Deciles		Loan/Income		%Minor applicants	
	Approval rate	Δln($originated loan)	Approval rate	Δln($originated loan)	Approval rate	Δln($originated loan)
Marginal applicant x Ln(dist. hometown)	0.001***	-0.001**	-0.001*	-0.004***	0.003	-0.050***
	(-3.304)	(-2.124)	(-1.646)	(-3.785)	(0.619)	(-7.265)
Marginal applicant x Ln(dist. hometown) x Ln(dist. HQ)	0.000	0.000	-0.003***	-0.003***	0.000	0.006***
	(1.171)	(0.320)	(-81.865)	(-57.466)	(0.019)	(5.595)
Ln(dist. hometown) x Ln(dist. HQ)	-0.004**	-0.002	0.001***	-0.003***	0.000	-0.003***
	(-2.309)	(-1.038)	(13.474)	(-24.035)	(0.150)	(-9.785)
Ln(dist. hometown)	-0.001	-0.018***	-0.014***	-0.000	-0.004**	-0.017***
	(-0.759)	(-7.918)	(-14.734)	(-0.002)	(-2.249)	(-7.680)
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
County-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-squared	0.197	0.113	0.197	0.113	0.178	0.097
Observations	558,932	408,184	558,932	408,184	558,932	408,184

The coefficient on the interaction terms are generally statistically negative, indicating that CEO hometown favoritism effects are stronger in counties where applicants face a higher barrier in securing mortgage loans; i.e., those that are poorer, riskier, and belonging to a minority group.

Given that home ownership has been a hallmark of the "American dream" (Laeven and Popov, 2017), my findings that CEOs increase mortgage lending more when proximate counties are struggling and among marginal applicants supports the idea that CEOs aim to help their hometown residents achieve their aspirations. It is worth pointing out that these cross-sectional results do not reflect aggregate changes in the loan portfolio of the bank. The performance of the loan portfolio of the bank does not change conditional on hometown mortgage lending. Increases in mortgage loans to struggling counties and marginal applicants located nearer to the hometown of the CEO are met with decreases in mortgage lending from applicants that are located further away.

3.6.4 Small Business Lending Evidence

So far, the analysis focuses on mortgage lending. However, given also the importance of credit supply to small business outcomes and local economic effects (e.g., Rice and Strahan, 2010; Chodorow-Reich, 2013; Krishnan, Nandy, and Puri 2014), I conduct an out-of-sample test to examine whether counties located nearer to the CEO's hometown also enjoy more small business lending. I obtain small business lending data from the Community Reinvestment Act (CRA) database collected by the Federal Financial Institutions Examination Council (FFIEC). As before, the data are aggregated at the bank-county-year level.

I use two dependent variables, *Δln(#loans)* and *Δln($loans)*, which measure the change in small business loan originations (in number and nominal amount) by a bank in a given county, relative to the prior year.[77, 78] As before, I include bank and county-year fixed effects. Results are shown in Panel A of Table 3-10. As small business loans vary substantially in size, I further categorize them into three size brackets: Columns (1)-(2) consider loans whose amount is below $100,000 while Columns (3)-(4) consider loans between $100,000 and $250,000. Columns (5)-(6) consider loans between $250,000 and $1,000,000.

The coefficient on *Ln(dist. hometown*) is negative and statistically significant at the 1% level in Columns (1)-(4); meaning that CEOs increase their lending to small businesses whose business loans do not exceed $250,000 in counties proximate to the CEO's hometown. This reflects favoritism to only the small and medium sized businesses, but not when loans are large (more than $250,000). This aligns well with the earlier interpretation of a hometown altruistic driven motive. Should CEOs be motivated by other reasons (such as agency conflicts, e.g., the pursuit of fame), it is more likely that CEOs would favor lending to larger firms (that would borrow larger amounts) which would grant them more visibility and repute in the local community.

In Table 3-10 Panel B, I repeat the analysis of bank-level outcomes (following Section 3.6.1) conditional on the proportion of small business lending the bank makes in the home state of the CEO. The coefficient on *%small business loan in home state*

[77] A disadvantage of the CRA data is that CRA only reports data that is originated while HMDA data allows me to observe the entire pool of loan level applications (including loans that are rejected). Thus, I am unable to construct the *Approval rate* variable for small business lending.

[78] *Δln(#loans)* is defined as the logarithmic number of small business loans made relative to the prior year divided by the logarithmic number of loans in the prior year. *Δln($loans)* is the logarithmic total loans made relative to the prior year divided by the logarithmic total loans in the prior year.

Table 3-10: CEO Hometown Favoritism and Small Business Lending

This table (Panel A) reports estimates of an OLS regression which estimates the effect of CEO hometown favoritism on small business lending and Panel B reports estimates of an OLS regression which estimates the effect of CEO hometown favoritism on aggregate bank performance. I report estimates of the following equation in Panel A:

$$Y_{i,k,t} = \alpha_{i,k,t} + \beta_1 Ln(dist.\ hometown)_{i,k,t} + Bank\ Controls_{i,t} + Bank\ FE + County\text{-}Year\ FE + \varepsilon_{i,k,t}$$

where subscripts *i*, *k* and *t* indicate bank, county and year respectively. *Y* is *Δln(#loans)* in odd-numbered columns, defined as logarithm of the number of loans originated relative to the prior year divided by logarithm number of loans in the prior year. In even-numbered columns, *Y* is *Δln($loans),* defined as logarithm $ amount of loans originated relative to the prior year divided by logarithm $ amount of loans in the prior year. Columns (1)-(2) include loans whose amount at origination is less than or equal to $100,000. Columns (3)-(4) include loans whose amount at origination is more than $100,000 but less than or equal to $250,000. Columns (5)-(6) include loans whose amount at origination is more than $250,000 but less than or equal to $1,000,000. *Ln(dist. hometown)* is the logarithmic distance between the bank CEO's hometown county and the county in which lending or branching decisions take place. The coefficient β_1 on *Ln(dist. hometown)* is the variable of interest in Panel A. I report estimates of the following equation in Panel B:

$$Y_{i,t} = \alpha_{i,t} + \beta_1 \%small\ business\ loan\ in\ home\ state_{i,t} + Bank\ Controls_{i,t} + Bank\ FE + Year\ FE + \varepsilon_{i,t}$$

where subscripts *i* and *t* indicate bank and year respectively. *Y* is either: (1) *Total Loans/Total Assets* defined as the number total loans divided by total assets (Columns (1)-(2)); (2) *Bad Loans/Total Assets*, defined as total non-performing loans divided by total assets (Columns (3)-(4)); or (3) *ROA*, defined as total income divided by total assets (Columns (5)-(6)). *%small business loan in home state* is the total small business loans that the bank makes in the state that the CEO was born divided by total small business loans. The coefficient β_1 on *%small business loan in home state* is the variable of interest in Panel B. Standard errors are clustered at the bank-level. The sample covers the period 1999–2014 for which data on CEO birthplace is available. Refer to Appendix 3-A1 for the definition and construction of variables used in this chapter. The constant is suppressed. t-statistics are reported in parentheses. ***, **, and * indicate significance at the 1, 5 and 10% level, respectively.

Panel A: Small Business Lending	(1)	(2)	(3)	(4)	(5)	(6)
Loan size	Amount <=$100k		100k<Amount <=$250k		250k<Amount <=$1000k	
	Δln(#loans)	Δln($loans)	Δln(#loans)	Δln($loans)	Δln(#loans)	Δln($loans)
Ln(dist. hometown)	-0.007***	-0.012***	-0.008***	-0.009***	0.003	0.002
	(-3.535)	(-8.837)	(-2.735)	(-4.235)	(0.958)	(1.152)
Ln(dist. hometown) x Ln(dist. HQ)	0.001***	0.002***	-0.001	-0.001*	-0.004***	-0.003***
	(3.499)	(8.943)	(-1.201)	(-1.790)	(-6.341)	(-8.479)
Ln(dist. HQ)	0.001	-0.012***	-0.027***	-0.034***	-0.011***	-0.019***
	(0.486)	(-8.186)	(-8.040)	(-13.652)	(-3.312)	(-7.731)
Assets	0.052***	0.034***	0.032***	0.025***	0.052***	0.015**
	(10.116)	(9.316)	(3.490)	(3.568)	(5.140)	(2.074)
Leverage	0.337***	0.164**	0.150	0.307**	0.768***	0.525***
	(3.342)	(2.548)	(0.832)	(2.280)	(4.012)	(3.859)

ROA	-0.041***	-0.015***	-0.005*	-0.000	-0.000	0.006**
	(-21.866)	(-11.951)	(-1.695)	(-0.177)	(-0.005)	(2.478)
Lending	0.439***	0.261***	0.015	-0.041	-0.054	-0.072**
	(18.339)	(16.347)	(0.363)	(-1.313)	(-1.252)	(-2.262)
Deposit	0.555***	0.353***	-0.267***	-0.173***	-0.231***	-0.140***
	(19.860)	(17.348)	(-5.067)	(-4.285)	(-4.306)	(-3.531)
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
County-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-squared	0.043	0.033	0.036	0.055	0.031	0.051
Observations	277,496	277,483	117,654	117,654	113,175	113,175

Panel B: Small Business Lending & Bank Outcomes	(1) All CEOs	(2) Out-state CEOs	(3) All CEOs	(4) Out-state CEOs	(5) All CEOs	(6) Out-state CEOs
	Total Loans/Total Assets		Non-performing loans/Total Assets		ROA	
%small business loan in home state	0.050	-0.014	-0.002	0.003	-0.014	0.340
	(1.146)	(-0.148)	(-1.267)	(0.256)	(-0.148)	(0.484)
Assets	-0.014	-0.292***	0.004	0.004	-0.292***	-0.276
	(-0.800)	(-2.847)	(1.130)	(0.462)	(-2.847)	(-1.298)
Leverage	-0.094	-22.183***	0.036	-0.012	-22.183***	-22.500***
	(-0.307)	(-10.401)	(0.606)	(-0.077)	(-10.401)	(-5.030)
ROA	-0.001	0.267	-0.007***	-0.006*		
	(-0.252)	(0.697)	(-7.065)	(-1.772)		
Lending			-0.021**	-0.066	0.267	-0.177
			(-2.199)	(-1.631)	(0.697)	(-0.252)
Deposit	0.198**	-2.494***	0.040***	0.013	-2.494***	-2.098*
	(2.038)	(-5.043)	(2.722)	(0.499)	(-5.043)	(-1.691)
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-squared	0.163	0.183	0.418	0.402	0.497	0.340
Observations	3,913	775	3,913	775	3,913	775

is insignificant in all columns and is not related to the proportion of *Total Loans/Total Assets* (Columns (1)-(2)), *Bad Loans/Total Assets* (Columns (3)-(4)), and *ROA* (Columns (5)-(6)), consistent with results and explanations laid out in Section 3.6.1. This, again, supports the hometown altruistic explanation and does not lend support to the information and agency motives for hometown favoritism.

Taken together, while I do not have one single test to powerfully rule out alternative interpretations such as the information or agency explanations, the body of evidence strongly points to the altruistic hometown attachment motive as the main explanation of the effects I document. That is, CEOs make more mortgage and small business lending as well as open more branches nearer to their hometown because they want to help their hometown communities. This does not harm the bank's performance and benefits residents proximate to the CEO's hometown at the expenses of those located further away.

3.7 The Effects of CEO's Hometown Favoritism on County Development

The findings so far show that banks make more lending and open more branches in areas closer to a CEO's hometown. In this section, I explore if counties that have larger exposure to hometown favoritism enjoy greater economic benefits.

I aggregate data at the county-year level and exploit variation in a county's exposure to CEOs hometown favoritism in the following equation:

$$Y_{kt} = \alpha_{kt} + \beta_1 \textit{Hometown Favoritism Exposure}_{kt} + \beta_2 \textit{HQ Favoritism Exposure}_{kt} + \textit{County Controls}_{kt} + \textit{County FE} + \textit{Year FE} + \varepsilon_{kt} \qquad (3\text{-}3)$$

where subscripts k and t indicate county and year, respectively. The dependent variable is one of the following two county-level measures of economic development: (1)

Ln(Personal Income), the natural logarithm of individual income from wages, investment enterprises and other ventures and; (2) *Unemployment rate*. *Hometown Favoritism Exposure* is the fraction of branches in the county that is exposed to bank's CEO hometown favoritism. A branch is considered to be exposed to a CEO's hometown favoritism if it is located within a 645 kilometer radius (25th percentile) to the bank CEO's birthplace.[79, 80] It measures the proportion of branches in the county which belong to CEOs that considers the county "home". I also include *HQ Favoritism Exposure* to control for possible bank HQ effects. All models include county and year fixed effects as well as time-varying county-level variables for population size and the HHI of county-level deposit concentration (Cetorelli and Strahan, 2006).

The results in Table 3-11 Panel A suggest that counties with a higher exposure to CEO hometown favoritism are associated with significantly higher personal incomes (Column (1)) and lower unemployment rates (Column (2)). These findings indicate that exposure to hometown favoritism lead to positive local economic developments. It should be pointed out that although I include county and year fixed effects (and therefore hold constant any time-invariant county characteristics that could bias my findings), the results I document in this section should be interpreted with caution as it lacks the strong identification of my main tests. Nonetheless, at the minimum, I show that there is a positive correlation between hometown favoritism and county-level benefits to residents that are proximate to the CEO's hometown.

[79] I obtain consistent results when using other thresholds.

[80] In addition to using the fraction of exposed branches, I also use the fraction of mortgage lending (Panel B) and small business lending (Panel C) that is exposed to hometown favoritism and obtain consistent results.

Table 3-11: CEO Hometown Favoritism and County Outcomes

This table reports estimates of an OLS estimation regression which estimates if CEO hometown favoritism affects county economic development. I report estimates of the following equation:

$$Y_{kt} = \alpha_{kt} + \beta_1 \textit{Hometown Favoritism Exposure}_{kt} + \textit{County Controls}_{kt} + \textit{County FE} + \textit{Year FE} + \varepsilon_{kt}$$

where subscripts *k* and *t* indicate county and year respectively. *Y* is either: (1) *Ln(Personal Income),* the natural logarithm of the individual's income from wages, investment enterprises and other ventures, or (2) *Unemployment rate*. *Hometown Favoritism Exposure* is the fraction of branches (Panel A) in the county that is exposed to CEO's hometown favoritism. A branch is considered to be exposed to hometown favoritism if it is located within 625 km (25^{th} percentile) from the bank CEO's birthplace. *Hometown Favoritism Exposure* is defined as the fraction of mortgage lending and the fraction of small business lending that are exposed to CEO's hometown favoritism in Panels B and C respectively. The coefficient β_1 on *Hometown Favoritism Exposure* is the variable of interest. Standard errors are clustered at the county-level. The sample covers the period 1999–2014 for which data on CEO birthplace is available. Refer to Appendix 3-A1 for the definition and construction of variables used in this chapter. The constant is suppressed. t-statistics are reported in parentheses. ***, **, and * indicate significance at the 1, 5 and 10% level, respectively.

Panel A: Exposure measured using #branches	(1) Ln(Personal Income)	(2) Unemployment rate
Hometown Favoritism Exposure$_{t-1}$	0.016***	-0.268***
	(3.542)	(-4.224)
HQ Favoritism Exposure$_{t-1}$	0.016***	-0.193**
	(3.039)	(-2.539)
Ln(HHI)$_{t-1}$	0.000	0.006
	(0.042)	(0.259)
Ln(Population)$_{t-1}$	-0.002	0.020
	(-1.166)	(1.228)
County FE	Yes	Yes
Year FE	Yes	Yes
Adj. R-squared	0.356	0.209
Observations	22,741	22,741

Panel B: Exposure measured using Mortgage Loan Originations	(1) Ln(Personal Income)	(2) Unemployment rate
Hometown Favoritism Exposure$_{t-1}$	0.041***	-0.726***
	(7.499)	(-9.841)
HQ Favoritism Exposure$_{t-1}$	0.017***	-0.234***
	(3.025)	(-2.948)
Ln(HHI)$_{t-1}$	-0.002	0.004
	(-0.737)	(0.176)
Ln(Population)$_{t-1}$	-0.002	0.016
	(-1.210)	(1.121)
County FE	Yes	Yes
Year FE	Yes	Yes
Adj. R-squared	0.356	0.209
Observations	22,741	22,741

Panel C: Exposure measured using Small Business Loan Originations	(1) Ln(Personal Income)	(2) Unemployment rate
Hometown Favoritism Exposure$_{t-1}$	0.019***	-0.416***
	(4.565)	(-7.268)
HQ Favoritism Exposure$_{t-1}$	0.009**	-0.263***
	(2.077)	(-4.334)
Ln(HHI)$_{t-1}$	-0.002	0.007
	(-0.718)	(0.318)
Ln(Population)$_{t-1}$	-0.002	0.017
	(-1.071)	(1.151)
County FE	Yes	Yes
Year FE	Yes	Yes
Adj. R-squared	0.356	0.209
Observations	22,741	22,741

An alternate way to interpret the results is that hometown favoritism in one area implies bias against residents in another. Since residents in a given county are not easily able to select their level of exposure to bank CEO's hometown favoritism, this implies that some counties have lower levels of economic outcomes as a result of their lower exposure to favoritism. This suggests that hometown favoritism, while arising out of the goodwill of bank CEOs, could inadvertently contribute to economic inequality.

3.8 Conclusion

This chapter provides evidence on the effects of CEO hometown favoritism on a firm's production outputs, i.e., bank credit allocation decisions, and uses it to quantify the effects of hometown favoritism on the real economy. I find that banks increase mortgage and small business origination and open more branches in counties that are closer to the CEO's birthplace and that this effect reflects the CEO's altruistic hometown attachment rather than information advantages or agency costs. Specifically, the hometown favoritism effect is stronger during economic downturns, among altruistic CEOs, in poorer counties, and among marginal applicants. I interpret this as evidence of CEOs helping their hometown residents to achieve their aspirations of home ownership.

Furthermore, hometown favoritism does not affect the bank's profitability. I also find suggestive evidence that hometown favoritism is associated with positive county-level economic outcomes. The findings imply that hometown favoritism is beneficial to residents proximate to the CEO's hometown with no additional cost to the bank. However, since residents in a given county cannot easily control their exposure to favoritism, this indicates that some "unlucky" counties with lower exposure to favoritism may have to experience lower economic developments. Therefore, hometown favoritism, while arising out of the altruistic goodwill of the CEO, might inadvertently contribute to economic inequality.

Appendix 3-A1: Definition of Variables

Variables	Definition	Source
Main Variables		
Ln(dist. hometown)	The natural logarithm of the physical distance between the bank CEO's hometown county and the county in which lending or branching decisions take place	Various sources
Ln(dist. HQ)	The natural logarithm of the physical distance between the bank HQ county and the county in which lending or branching decisions take place	SOD
Hometown state	A dummy that equals 1 if the CEO's birth state and the state in which the lending or branching decisions take place is the same	Various sources
HQ state	A dummy that equals 1 if the bank's HQ state and the state in which the lending or branching decisions take place is the same	SOD
Bank Characteristics		
Assets	Natural logarithm of total assets	FRY-9C
Leverage	Total liabilities divided by total assets	FRY-9C
ROA (%)	Earnings before interest and taxes divided by total assets	FRY-9C
Lending	Total loans divided by total assets	FRY-9C
Deposit	Total deposits divided by total assets	FRY-9C
Non-performing loans	Non-performing loans divided by total assets	FRY-9C
%mortgage loan in home state	The fraction of mortgage lending made in the CEO's birth state	HMDA
%small business loan in home state	The fraction of small business lending made in the CEO's birth state	CRA
Mortgage Loan Characteristics		
Approval rate	The number of mortgage loan applications approved divided by the total number of applications received by a bank in a county-year	HMDA
Δln($originated loan)	The logarithmic originated mortgage loans relative to the prior year divided by logarithmic originated loans in the prior year by a bank in a county-year	HMDA
Δbranches	The number of branches minus the number of branches in the prior year scaled by number of branches in the prior year for a bank in a county-year	HMDA
%female applicants	The ratio of the number of applications from female applicants to the total number of applications reviewed for each bank-county-year	HMDA
%minor applicants	The ratio of the number of applications from minority applicants to the total number of applications reviewed for each bank-county-year. Minority applicants include all applicants whose reported race is non-white	HMDA
Loan/Income	The average ratio of the loan amount in a mortgage application to the applicant's income for applications reviewed in each bank-county-year	HMDA
Reverse Income Decile	10 – Applicant's Income Decile	HMDA
Small Business Loan Characteristics		
Δln(#loan)	The logarithm of the number of loans originated relative to the prior year divided by logarithm number of loans in the prior year	CRA
Δln($loan)	The logarithm $ amount of loans originated relative to the prior year divided by logarithm $ amount of loans in the prior year	CRA
County Characteristics		
Unemployment rate	Unemployment rate of the county	Bureau of Labor Statistics
%non-home owner	The fraction of houses not occupied by the owner in the county	Bureau of Labor Statistics
Ln(Personal Income)	The natural logarithm of the average individual's income from wages, investment enterprises and other ventures in the county	Bureau of Labor Statistics
Ln(HHI)	The natural logarithm of the HHI of deposits (calculated as the summation of the deposit^2 of branches) in the county	SOD

Ln(Population)	The natural logarithm of the population in the county	Bureau of Labor Statistics
Home Favoritism Exposure	The proportion of branches in a county that is considered exposed to CEO hometown favoritisim. A branch is considered to be exposed to hometown favoritism if it is located within 625 km (25th percentile) from the bank CEO's birthplace	Various
HQ Favoritism Exposure	The proportion of branches in a county that is considered exposed to the HQ. A branch is considered to be exposed to HQ favoritism if it is located within 625 km (25th percentile) from the bank's HQ	Various
CEO Characteristics		
MBA	Dummy equals 1 if the CEO has an MBA degree	BoardEx
Ivy League	Dummy equals 1 if the CEO obtains a degree from an Ivy League institution	BoardEx
Age	The age of the CEO	BoardEx
Depression baby	Dummy equals 1 if the CEO is born between 1920 and 1929	BoardEx
Crisis career starter	Dummy equals 1 if the CEO starts her career (assuming at the age of 22) during a crisis	BoardEx, NBER crisis database
Overconfidence	Equals 1 if the CEO holds exercisable stock options that are at least 67% in the money.	BoardEx
Military experience	Dummy equals 1 if the CEO has prior military experience	BoardEx
Hometown UG	Dummy equals 1 if the CEO undertakes an undergraduate degree in her birth state	BoardEx
Out-of-state CEOs	Dummy equals 1 if the CEO was born in a state different from the bank's HQ state	BoardEx
Collectivism	Measures the individual integration to groups based on the cultural ancestry of the CEO	Hofstede
Patriotism	Measures how much a society values individual sacrifice for their own country based on the cultural ancestry of the CEO	European Value Survey (EVS)
Selflessness	Measures how much a society values individual sacrifice for other people based on the cultural ancestry of the CEO	European Value Survey (EVS)
Humane-oriented	Measures how much a society encourages individuals to be altruistic based on the cultural ancestry of the CEO	GLOBE

Appendix 3-A2: CEO's Birth State

This table reports descriptive statistics of states in which bank CEOS were born in. The sample covers the period 1999–2014 for which data on CEO birthplace is available.

Birth State	#CEOs	Percentage (%)
AL	13	2.68
AR	2	0.41
AZ	3	0.62
CA	27	5.57
CT	10	2.06
DC	2	0.41
FL	10	2.06
GA	13	2.68
HI	3	0.62
IA	6	1.24
IL	20	4.12
IN	19	3.92
KS	4	0.82
KY	7	1.44
LA	3	0.62
MA	17	3.51
MD	9	1.86
ME	8	1.65
MI	11	2.27
MN	7	1.44
MO	8	1.65
MS	19	3.92
MT	2	0.41
NC	31	6.39
ND	1	0.21
NE	2	0.41
NJ	16	3.3
NY	48	9.9
OH	25	5.15
OK	3	0.62
OR	2	0.41
PA	48	9.9
RI	4	0.82
SC	13	2.68
SD	2	0.41
TN	2	0.41
TX	18	3.71
UT	3	0.62
VA	24	4.95
VT	3	0.62
WA	8	1.65
WI	3	0.62
WV	6	1.24

Appendix 3-A3: Alternate Definition of CEO Hometown Favoritism

This table reports estimates of an OLS regression which estimates the effect of CEO hometown favoritism on bank business policies. I report estimates of the following equation:

$$Y_{i,k,t} = \alpha_{i,k,t} + \beta_1 Hometown\ state_{i,k,t} + Loan\ Controls_{i,k,t} + Bank\ Controls_{i,t} + Bank\ FE + County\text{-}Year\ FE + \varepsilon_{i,k,t}$$

where subscripts *i*, *k* and *t* indicate bank, county and year respectively. *Y* is either: (1) *Approval rate*, defined as the number of approved mortgage loan applications divided by the total number of applications received; (2) *Δln($originated loan)*, defined as the logarithmic originated mortgage loans relative to the prior year divided by logarithmic originated loans in the prior year; or (3) *Δbranches*, defined as the number of branches minus the number of branches in the prior year scaled by number of branches in the prior year. *Hometown state* is a dummy variable that equals 1 if the county that bank decisions take place in is in the state where the CEO was born and 0 otherwise. The coefficient β_1 on *Hometown state* is the variable of interest. Control variables include: *Hometown state*HQ state, Assets, Leverage, ROA, Lending, Deposit, %female applicants, %minor applicants* and *Loan/Income.* Standard errors are clustered at the bank-level. The sample covers the period 1999–2014 for which data on CEO birthplace is available. Refer to Appendix 3-A1 for the definition and construction of variables used in this chapter. The constant is suppressed. t-statistics are reported in parentheses. ***, **, and * indicate significance at the 1, 5 and 10% level, respectively.

	(1) Approval rate	(2) Δln($originated loan)	(3) Δbranches
Hometown state	0.011***	0.021***	0.006**
	(6.831)	(9.414)	(2.450)
HQ state	0.020***	0.096***	0.012***
	(13.883)	(48.762)	(5.551)
Bank FE	Yes	Yes	Yes
County-Year FE	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes
Adj. R-squared	0.178	0.095	0.058
Observations	559,263	408,377	85,138

Conclusion

This thesis contains three independent chapters that study banking in the U.S. The first two chapters study how supervisors and regulators influence bank behavior. The third chapter explores how bank CEOs allocate credit.

The findings in Chapter 1 are of broad interest to regulators and help inform policy debates regarding regulations and supervision. It studies the effects of supervision on bank behavior. I use a novel setting, the closure of regulatory offices, as negative shocks to the efficacy of bank supervision. I find that following regulatory office closures, banks under the supervision of the closed office become riskier and expand their loan portfolios more aggressively as compared to banks located in the same counties but are not under the supervision of the closed office. Further, I show that supervisors were not too strict prior to office closures and that closures led to worst bank outcomes. Banks affected by regulatory office closures prior to the 2007-2009 financial crisis exhibit lower risk-adjusted returns, larger loan losses and a higher probability of failure during the crisis, leading to higher bank resolution costs. Finally, I show evidence that information asymmetry issues between supervisors and banks is one mechanism that impedes supervision.

Chapter 1 paints a positive picture of the effectiveness of a decentralized structure of bank supervision where supervisory offices are located close to the banks which they examine due to informational advantages. Supervisors should carefully weigh the cost savings of maintaining a more centralized organizational structure against the possibility of less effective bank supervision, keeping in mind that bank fragility might not manifest immediately but only during economic downturns.

Chapter 2 draws attention to the darker side of interactions between regulators and bank directors and suggests that connections between regulators and bankers warrant more scrutiny. The chapter shows that connections established while bank directors hold public service positions in regulatory agencies undermines supervisory effectiveness. I demonstrate that banks with public service connections hold less capital for a given increase in risk than non-connected banks. As a result, connected banks are able to shift risk to the financial safety-net and extract larger public subsidies. The analysis also shows that preferential treatment by regulators is one reason why connected banks are able to shift risk to the safety-net. Further, risk-shifting at well-performing connected banks lead to wealth transfers from the taxpayer to shareholders of these banks.

Despite public service positions carrying no formal authority on matters relating to supervision and enforcement, connected banks are still afforded preferential treatment. Supervisors should consider the costs of maintaining supervisory consistency against the potential benefits of industry insights that bankers bring during their involvement with regulatory agencies.

Chapter 3 provides evidence on the effects of CEO hometown favoritism on credit allocation and bank policies. I find that banks increase mortgage and small business lending as well as open more branches in counties that are closer to the CEO’s birthplace as compared to counties that are located further away. CEOs show favoritism to their hometown due to altruistic attachments; hometown favoritism effects are stronger during economic downturns, among altruistic CEOs, in poorer counties, and among marginal applicants. Finally, I show that CEO hometown favoritism does not affect the bank's profitability and is associated with positive economic outcomes in counties exposed to greater favoritism.

The findings in Chapter 3 implies that since residents of a given county cannot easily control their exposure to favoritism, hometown favoritism, while arising out of the altruistic goodwill of the CEO, might inadvertently contribute to economic inequality.

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