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ESSAYS IN BANKING:
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MICHAEL F. PRICE COLLEGE OF BUSINESS

BY

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I dedicate this dissertation to both of my grandfathers. Though I never met him, Leonid Khodes's insatiable intellectual curiosity fuels me. In completing this dissertation, I reclaim what antisemitism deprived him of. My other grandfather, Mark Pugachev, gave me the opportunity to do so. His relentless courage and sacrifice delivered our family from the ashes of the Soviet Union to the Land of Opportunity. My dissertation would also have been impossible without the people who distract me from it. I gratefully acknowledge my wife, Sarah, for the patience and encouragement that she showers me with, daily; my mother, Lucy, and father, Vlad, for being incomparable role-models of work-ethic and perseverance; and the rest of my family for unconditional love and support.

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Abstract

This dissertation comprises three essays. The first documents the equilibrating effect of bank M&As on deposit and loan markets; the second explores board interlocks as a means of bank-shock transmission across sectors; the last investigates the value of payout to bank investors.

Chapter 1 examines deposit and loan volume and price changes around all ownership-changing U.S. bank M&As between 1998 and 2016. I find that M&A impacts target markets differently based on their deposit-loan imbalances. In markets where loans are scarce relative to deposits, lending (deposit-gathering) increases by more (less); where deposits are relatively scarce, the effects reverse. Thus, M&A reduces deposit-loan imbalances, equilibrating markets. Deposit-loan rebalancing correlates with better economic outcomes. Deposit and loan price analysis, however, is inconclusive. Overall, my findings support a welfare-enhancing view of bank M&A.

Chapter 2 uses 1,245 U.S. bank enforcement actions (EAs) issued between 1990 and 2017 to show that board interlocks transmit bank shocks into the real economy. When a non-financial firm (NFF) and bank share a common director, NFF stock prices fall around EAs issued to the bank. The effect is stronger for more severe EAs. During enforcement years, common directors participate less on NFF boards and more on bank boards. These results are unlikely to reflect an impaired credit relationship, director reputational damage, or endogenous director selection. They imply that board interlocks could transmit larger bank shocks into the real economy.

Chapter 3 reexamines whether investors value payout and why. I study abnormal stock returns around regulatory EAs that restrict bank dividends and repurchases. Market reactions are significantly worse for enforced banks that pay out than for those that do not. Withstanding alternative explanations and parallel trend concerns, these results present rare, causal evidence of a value to corporate distribution. The cross-section of abnormal returns suggests that risk-shifting,

not agency cost reduction, drives payout. In my sample of distressed banks, especially around financial crises, the ability to shift risk through payout has value.

Chapter 1: A Double-Edged Wand:

How Bank M&A Equilibrates Deposit and Loan Markets

1. Introduction

At the core of a strong economy lies a healthy financial sector, allocating capital toward its highest and best use (King and Ross, 1993). When this sector fundamentally changes, people notice. A topic of intense academic and regulatory debate has been the extensive rate of bank consolidation through merger and acquisition (M&A).[1] M&A transformed the U.S. banking landscape from over 15,000 institutions in 1980 to under 6,000 today. One well-documented effect is that bank M&A *benefits* depositors and borrowers (Focarelli and Panetta, 2003; Park, Pennacchi and Sopranzetti, 2005). Another is that it *harms* these same stakeholders (Prager and Hannan, 1998; Garmaise and Moskowitz, 2006). The latest published review on the subject concludes that "*extant literature provides no consistent evidence whether the participating financial firms benefit from M&As, whether the customers of these firms benefit, or whether societal risks have increased or decreased as a result of this activity*" (DeYoung, Evanoff and Molyneux, 2009, pg. 88). The present article aims to reconcile conflicting evidence by reexamining the volume and price impact of all U.S. bank M&As between 1998 and 2016.

I argue that an important, overlooked dimension of M&A heterogeneity – target market characteristics – may partially explain discord in prior work. M&A is not random; acquirers choose to acquire into certain markets and not others. Market characteristics are bound to influence that decision. Bank operations are not uniform; policies in certain markets may differ from those in

[1] Throughout, I use the term bank informally to denote any depository institution or its holding company.

other ones. Market characteristics are also likely to influence those policies. Thus, if researchers model bank decision-making post-M&A but do not control for market conditions, omitted variable concerns may bias or at least destabilize coefficients.

One characteristic particularly important in explaining a bank's supply of loans or deposit accounts is a market's demand for these products. Consider a bank that operates in three different markets: one where incumbents gather just enough deposits to satisfy local loan demand; a second where local deposit volumes exceed loan demand; and a third where loan demand exceeds deposits. The first market can be characterized as 'well-funded' whereas the latter two exhibit 'deposit-loan imbalances' (DLI). That is, certain frictions induce an imbalance between the supply and demand of funds.[2] The second market is 'deposit-heavy' whereas the third is 'loan-heavy'. Now consider an acquirer that purchases this bank. If the well-funded market is at equilibrium, the acquirer may have fewer inefficiencies to exploit. There, deposit-gathering and lending might persist at the target's pre-acquisition levels. However, in the deposit-heavy market, a profit-maximizing acquirer might improve on target policies by supplying more loans or letting some deposits run off. Conversely, the acquirer might curtail lending or compete more aggressively for deposits in the loan-heavy market. Previous research tests M&As' net effect over all target markets which masks this important heterogeneity. Thus, the estimated impact of M&A can change, sample to sample, based on whether deposit- or loan-heavy markets are more prevalent.

A key obstacle in testing the above framework is measuring a market's DLI. Although such a measure would have many useful applications, I am aware of no previous attempts to create one. One reason is the difficultly in doing so accurately. To precisely measure DLI, one would need to

[2] The exact nature of these frictions does not affect my argument, but examples could include regulatory intervention, bank herding incentives, strategic considerations, collusion, regional credit bubbles, and others. Section 5.3, below, discusses these forces in more detail.

simultaneously model deposit and loan levels and prices, invoking data and econometric challenges. In this paper, I make a preliminary effort to model DLI. I estimate within-year regressions of log market deposit volumes on log small business loan volumes. This method compares a market's deposit level against the levels of other markets that have similar loan volumes, that year. Positive residuals denote a deposit-heavy (or loan-light) market whereas negative residuals reflect a loan-heavy (or deposit-light) one.[3]

Endogeneity presents another challenge to testing the effects of M&A. Immeasurable forces that jointly determine customer welfare, an acquirer's M&A decision, and target markets' DLI can bias estimates. My bank-market-year panel allows me to partially allay these concerns using two rich sets of fixed effects. The first, bank-year fixed effects, holds constant all differences between acquirers and other banks, comparing outcomes across markets concurrently served by the same bank. The second, bank-market plus year fixed effects, is even more powerful. It mutes all time-invariant bank, market, and bank-market differences. Thus, it compares the same bank's operations in the same market across various years. These specifications allow me to isolate variation into cross-sectional and time-series components.

Both specifications show that DLI significantly moderates the effect of M&A on target bank customers. As predicted, acquirers raise fewer deposits and issue more loans in deposit-heavier markets and do the opposite in loan-heavier ones. They increase (decrease) deposit-gathering by about 2 percentage points in the loan-heaviest (deposit-heaviest) subset of recently acquired markets; lending is relatively unchanged. In the deposit-heaviest markets, lending increases by an annualized 7 percent for at least three years while deposit-gathering remains constant. The stronger effect for loans than for deposits is reassuring because a bank can adjust

[3] Although this simplified approach likely correlates with a market's true DLI, I explore alternative measures, as well.

lending (investment policy) with greater ease than deposit acceptance (fund-raising). Results hold when using different measures of market deposit-heaviness and are not driven by the crisis period or by large banks. My analysis fails to identify a clear impact of M&A on deposit and loan pricing.

In line with Berger, Saunders, Scalise, and Udell (1998), target rivals partly absorb these changes. At the market-level, however, M&A still helps dissolve persistent DLI. Lending expands (contracts) by about 2 percent a year in the deposit-heaviest (loan-heaviest) markets over the three post-acquisition years. Market-level deposit changes are more muted but still support the above framework. Finally, I present evidence that declines in DLI correlate with positive economic welfare changes, measuring market welfare as median income, unemployment rate, GDP, poverty rate, or new housing.

In sum, I show that bank M&A equilibrates target markets. Acquirers redistribute deposit-gathering and lending from areas in which these services abound to those in which they are scarce. In this sense, my evidence is consistent with bank M&A enhancing social welfare. Besides showing that DLI moderates bank M&A's impact on customers, my paper makes several methodological contributions. To my knowledge, it is the first to test both volume and price effects in both deposit and loan markets through a unified framework. Most research focuses on only one dimension which can yield conflicting evidence and inconsistent implications. For instance, Karceski, Ongena, and Smith (2005) show that 48 Norwegian bank M&As between 1983 and 2000 damage firm value for large commercial target borrowers. Focarelli and Panetta (2003) show that 43 Italian transactions between 1990 and 1998 increase household deposit rates in the long run. In isolation, these findings feed opposing views of bank M&A. However, discord may reflect different methodologies, countries, time-periods, and, especially, customer classes. I test volume and price effects in multiple classes of depositors and borrowers through a unified framework to obtain more robust and comparable conclusions. I also bypass sample selection issues in nearly all bank M&A articles by studying the entire universe of ownership-changing U.S. bank M&A. For example, Kahn, Pennacchi, and Sopranzetti's (2005) sample includes only 30 mergers in the 10 largest banking markets. Erel (2011) studies 350 mergers involving the largest acquirers. Liebersohn (2017) looks at 348 mergers whose impact on 200 markets falls around a regulatory threshold. In contrast, my sample reflects millions of deposit and loan accounts affected by 2,673 M&A transactions between 3,649 targets and 2,598 acquirers in 1,887 markets.[4] Ample bank, market, and time-series variation provide representative conclusions. Additionally, my empirical tests utilize a careful fixed effects framework that disentangles cross-sectional from time-series variation. I believe this approach has many relevant applications to empirical analysis in banking and corporate finance. Finally, the variable I propose to measure DLI promises fruitful extensions into other research questions.

2. Related research

Although empirical conclusions about bank M&A vary, they share a common theoretical foundation. Drawing on Williamson's (1968) seminal work, bank mergers are understood to benefit customers if they create operating efficiencies for the surviving institution and those efficiencies are passed down as lower costs or better services. Efficiency gains, however, can come at the price of higher market power. By acquiring rivals, banks reduce competition, allowing them to extract rents. Whether the mean effect in an M&A sample is positive or negative is believed to reflect the net impact of these forces.

[4] If target or acquirer is a multibank holding company, each subsidiary bank is considered to participate in the transaction. This induces a many-to-many relationship between target and acquirer bank.

Two articles lucidly illustrate the tradeoff between operating efficiencies and market power. Using Italian data, Sapienza (2002) shows that observed loan prices decrease modestly when the merger involves two banks with no market overlap, decrease substantially when institutions overlap but have small market-shares, and actually increase as the banks' combined market share rises. Erel (2011) presents corroborating evidence using 350 large U.S. mergers. 'In-market mergers', those where target and acquirer overlap in at least one geographic market, generate significant loan spread declines whereas out-of-market mergers do not. When market overlap between target and acquirer is largest, however, customer gains dissipate. Both articles conclude that mergers reduce loan prices, presumably reflecting cost efficiencies at the consolidated bank, but these effects vanish in cases of dramatic market power increase.

Earlier empirical papers investigate the effect of bank M&A on small business loan supply. These papers typically measure small business lending at the bank-year level using call report data. In one study, Strahan and Weston (1998) find that mergers between small banks increase small business lending but other mergers have no effect. Conversely, Avery and Samolyk (2004) and Craig and Hardee (2007) find that large bank acquirers negatively impact small business credit availability. Peek and Rosengren (1998) demonstrate that whether acquirers increase or decrease small business lending depends on the role this business line plays in their preexisting operation. Berger et al. (1998) decompose the effect and show that competing banks likely compensate for reduced acquirer loan supply.

Other research takes the borrower's perspective. Using Norwegian data, Karceski, Ongena and Smith (2005) show that M&A reduces target bank borrowers' stock prices, on average, and forces them to terminate their banking relationships more frequently. Garmaise and Moskowitz (2006) study commercial real estate loan prices around U.S. bank M&A and find that competition-reducing mergers induce not only worse loan pricing but even higher crime rates. In a panel of Italian borrowers, Bonaccorsi di Patti and Gobbi (2007) find that customers' credit supply temporarily decreases if their bank is acquired but then recovers. Degryse, Masschelein and Mitchell (2011) find small Belgian borrowers with fewer banking relationships are more likely to be 'dropped' by the acquiring bank. Kahn, Pennacchi and Sopranzetti (2005) are among the few to examine consumer – not commercial – loans. They find that U.S. mortgage rates fall in merger markets *before the merger*, consistent with increased competition driving down prices. On the other hand auto loan rates do not. Likewise, Nguyen (2014) finds that the impact differs by borrower class: M&A decreases small business lending but its negative effect on mortgage lending rebounds.

Less research exists on depositor impact. Exceptions include Prager and Hannon (1998) who study deposit rates in U.S. markets around substantial horizontal mergers.[5] In these markets, deposit rates fall; yet, for less substantial mergers, rates increase relative to a control group. Studying Italian M&A, Focarelli and Panetta (2003) find that the negative impact on deposit rates reverses in the long term, which the authors attribute to a lengthy gestation period for efficiency gains to be realized. In a recent working paper, Bord (2017) shows bank M&A also harms depositors through higher fees.

To summarize, the average M&A harms (Avery and Samolyk, 2004; Craig and Hardee, 2007; Garmaise and Moskowtiz, 2006; Degryse, Masschelein, and Mitchell, 2011), benefits (Strahan and Weston, 1998; Sapienza, 2002; Erel, 2011) and does not affect (Berger et al. 1998; Bonaccorsi di Patti and Gobbi, 2007) small business borrowers. Some consumer classes lose but others are unaffected (Kahn, Pennacchi, and Sopranzetti, 2005; Nguyen, 2014). Depositors are

[5] They define substantial mergers as ones that increase market Hirschman-Herfindahl Index (HHI) at least 200 points to a pro forma level of at least 1800.

harmed (Prager and Hannon, 1998; Bord, 2017) but only in the short-run (Focarelli and Panetta, 2003). Few studies overlap in country and nearly none in methodology or time period. All attribute negative effects to market powers and positive ones to efficiency gains. The collective body of prior work provides only one, unambiguous conclusion: more, more consistent, and more comprehensive evidence is needed.

The present paper offers two key insights to this conflicted literature: (i) bank M&A should affect different markets differently and (ii) this heterogeneity can be gleaned, *a priori*, by understanding market conditions. M&A allows firms to expand or solidify access to certain markets (Napier, 1989, and Anderson, Havila, and Holstrom, 2003), but why should they pursue the same strategies in each one? By modeling whether greater opportunities exist on the loan or deposit side in a given area, we can better predict what policies profit-maximizing agents would adopt. Doing so addresses the important question, "How does M&A impact target bank customers," in manner more consistent with strategic business considerations.

In a sense, my paper extends Park and Pennacchi's (2009) theoretical framework. These authors theoretically demonstrate that mergers can disparately affect borrowers and depositors. In their model, large acquirers rely heavily on non-deposit funding sources and utilize more efficient lending technologies. Consequently, when a large acquirer gains or increases market share, competition for that market's deposits drops while competition for loans increases. Under their assumptions, bank M&A can simultaneously hurt depositors and help borrowers by adjusting competition in each market. Moving beyond the dimension of bank size, I hypothesize that *any* acquisition benefits customers to the extent that it increases competition in that market. Consistent with this framework, my results imply that acquirers compete less in target markets that are oversaturated and more where lending or deposit-gathering is scarce.

3. Data and variables

My sample period starts in 1998, the first year deposit rate information becomes available, and ends in 2016. The units of observation are merger-market, market-year or bank-market-year combinations. Following prior work (e.g. Berger et al., 2004; Liebersohn, 2017), I define markets as metropolitan statistical areas (MSAs) or as counties when the county falls outside an MSA.[6] A bank is considered to operate in a given market-year if the FDIC's Summary of Deposits (SOD) dataset reports at least one bank branch in that market-year. All variables measured in dollars are inflated up to 2016 values using the FRED consumer price index for urban customers.

3.1. Mergers and acquisitions

M&A data come from the National Information Center's Transformations file which details bank ownership changes. To retain true M&As, I exclude splits, asset sales, and mergers induced by bank failure (transformation codes 5, 7, and 50, respectively). I also exclude 'in-family' mergers, following Francis, Hasan, and Wang (2008) and Erel (2011). These transactions, common after the 1997 Riegle-Neal Act, consolidate multiple institutions within the same holding company but do not impact ultimate ownership. If the target (acquirer) is a holding company, not a bank, I classify all subsidiary banks owned by that holding company as targets (acquirers) which allows me to maintain a bank-market-year level of analysis.

3.2. Deposit volumes

From SOD, I obtain June 30th branch-year deposit volumes for all branches of every FDIC insured depository institution. Deposit volumes are aggregated into market-year and bank-market-year levels. I then compute each bank's deposit market share, dividing its deposit volume in a

[6] Because MSA definitions and delineations can change from year to year but one county can belong to at most one MSA, I use the 2016 relationships between counties and MSAs throughout my sample. That is, MSA *m* is defined to contain county *c* in year *y* if and only if it contains county *c* in 2016. Doing so avoids mismeasuring several dramatic swings in deposit and loan volumes, year-over-year, when, in fact, only the geographies are redefined.

market by the market's total deposit volume. Each deposit market's Hirschman-Herfindahl Index (HHI) is calculated as the sum of squared market shares for all banks in that market-year.

3.3. Loan volumes

From the Community Reinvestment Act (CRA) website, I obtain small business and farm (SBF) loan origination volumes. Each year-end, federally regulated depository institutions report SBF data if they exceed a size threshold and meet other criteria.[7] The 2016 threshold was $1.216 billion in total assets. Using bank-market-year and market-year SBF volumes, I compute market share and market HHI, as above. Missing data for smaller lenders introduces measurement error. Concerns are partially allayed by Berger et al. (1998) and Berger, Goulding and Rice (2014) who find that larger banks actually fund more small business loans. Further, Greenstone, Mas, and Nguyen (2014) estimate that this database includes 86 percent of all business loans under $1 million in 2007. I use home mortgage loan volumes as a robustness measure, obtaining loan-level residential mortgage origination data from Home Mortgage Disclosure Act (HMDA) Loan Application Registers. In 2016, the reporting threshold was $44 million for depository institutions.[8] From these data, I measure home mortgage loan origination volume in dollars for market-years and bank-market-years, computing market share and HHI as above. Prior to 2004, HMDA respondents did not report the primary identifier that can be traced back to financial data. I follow Xie (2016) in populating earlier values by relying on 2004 relationships between RSSD and the HMDA identifier.

3.4. Deposit and loan rates

Deposit and loan rate data come from RateWatch, a company that surveys financial institutions. By disclosing rates, a financial institution learns about its competitors' rates. Although

[7] For a full description of which institutions must report, refer to https://www.ffiec.gov/cra/reporter.htm.
[8] For a full description of which institutions must report, refer to www.ffiec.gov/hmda/reporter.htm.

survey response is voluntary, current coverage is high. For my 2016 sample, 85 percent of banks by number and 95 percent by asset size report a rate for at least one of the 4 products I sample in at least one branch. Although the data include hundreds of variants on a few dozen deposit and loan products, I select two loan and two deposit products based on data availability and prior work.[9] Deposit rates include the $10,000 minimum, 12-month CD rate following Cortez and Strahan (2017), and the $0 minimum interest checking account per Azar, Raina, and Schmalz (2016). Like Dlugosz, Kyu Gam, Gopalan, and Skrastins (2017) and Mora (2017), respectively, I select the 15-year fixed home mortgage rate and 5-year new auto loan rate. Each is measured by bank-market-year. The median within a bank-market-year provides a market-year measure.

RateWatch collects data by surveying ‘rate-setting’ branches which establish rates for other branches of the same institution. A bank can have multiple rate-setting branches at a given point in time that determine prices for different regions or products. RateWatch provides files to link rate-setters with rate-following branches. These files include the FDIC’s unique Branch Numbers (UNINUMBR) and branch latitude-longitude coordinates. I use the UNINUMBR to tie branch rates to regulators primary identifier, RSSD IDs, from the SOD database. Because RateWatch does not provide head office UNINUMBRs, I match head offices to RSSD IDs using their geographic coordinates.

For most of my sample, institutions are surveyed at a monthly frequency but in 2011, RateWatch begins collecting weekly deposit rate information. Because my study uses annual observations, I include rates from June surveys (to be consistent with the SOD timing). From 2011 onward, deposit rates come from the first survey collected in June. Surveys cover more institutions for deposits than for loans although coverage increases monotonically for both over time.

[9] For example, 12-month CDs with $10,000 and $100,000 minimum balances are listed as two separate products.

3.5. *Deposit-loan imbalance*

The key independent variable for my empirical analysis is a measure of market-year DLI. I estimate within-year regressions of a market's log deposits on log SBF loans and the square of log SBF loans. The quadratic term is discussed below. This approach compares a market's deposit volume with the deposit volume of other markets that have similar loan volumes in the same year. Signed residuals proxy for the market's deposit-heaviness, MDH. Positive residuals denote a market with greater-than-expected deposit levels or, equivalently, lower-than-expected loans; negative residuals imply that the market has too few deposits or too many loans.

Figure A1 plots the relationship between log deposits and log loans for all market-years in my sample.[10] It also plots linear and quadratic fit curves between these variables. SBF loan volumes, alone, can explain nearly 80 percent of the variation in deposit levels. Thus, a market-year's deviation from its predicted value of deposits should reasonably estimate whether that market is deposit-heavy or loan-heavy. About 8,800 observations, roughly 20 percent of market-years, appear along the y axis, meaning these areas include no reported SBF loans that year. Half are in Texas, Kansas, Nebraska, Georgia, Iowa and North Dakota; 85 percent comprise 443 counties with no reported SBF loans for at least 10 years of my 19 year sample period. The median population in these markets is only 6,700. Because most are repeated observations of rural areas, they represent only 7 percent of my bank-market-year panel. Although some may be actual cases where no new SBF loans were issued that year, most cases likely reflect the reporting threshold which rural banks may not meet. Including these observations exposes my study to measurement error but dropping them introduce selection bias that may be worse. My baseline analysis retains

[10] Although I measure DLI via within-year, Figure A1 reports results from a pooled regression for ease of presentation.

these market-years but I implement several robustness tests, including omitting these observations, to ensure they do not drive my results.

The figure also shows that that zero-loan market-years unduly flatten the best-fit line, rendering residuals from the quadratic specification more accurate. Even without these observations, the quadratic specification fits the data better. Moreover, a quadratic relationship is conceptually appealing. If lending exhibits diminishing returns to scale, a bank's propensity to lend out its first dollar in deposits should be much higher than its billionth. This logic extends to market volumes of loans and deposits.

3.6. Control variables

From June 30th Call Reports, I obtain bank-year data on total assets, nonperforming assets, and total equity; the latter two are scaled by total assets. Bank-year controls mirror Sapienza (2002). From the Bureau of Economic Analysis, I obtain log market-year population and income per capita and from the Bureau of Labor Statistics (BLS), I obtain a market's unemployment rate.

3.7. Final sample and summary statistics

My final sample includes 393,413 bank-market-years comprising 12,562 banks that operate in 2,349 markets between 1998 and 2016. It covers the universe of 2,673 ownership-changing M&As between 5,504 distinct banks. Over 40 percent of all U.S. banks participates in M&A activity over my sample period and over 75 percent of markets experience at least one transaction during that time.

Table A1, Panel A, reports sample means and medians for key bank-year variables. Statistics are presented for the full sample, and subsamples of target- and acquirer-year observations. Acquirer-years are typically much larger than target-years although both are larger than M&A non-participant bank-years. Note the severe right skew from the largest banks conducting many acquisitions. Consistent with Park and Pennacchi's (2009) model, acquirer-years rely less deposit-based funding. In line with Berger et al. (1998) and many others, small business and farm loans comprise a lower fraction of their asset portfolios than target-years'. Capital ratios for target-years and acquirer-years are similar to each other and to M&A non-participant bank-years. Acquirer-years exhibit fewer nonperforming assets than target-years or M&A non-participant bank-years. On average, acquirer-years offer higher deposit rates and pricier loans; this finding likely reflects the different markets in which targets and acquirers operate.

Panel B reports market-year summary statistics. A target or acquirer market-year is any market-year in which a target or acquirer has a branch; the two can overlap. Acquirers operate in about 3 times as many markets as targets. Interestingly, target markets are larger, on average which could reflect dilution from acquirers' wider branch networks. Target market-years are also wealthier and have lower unemployment rates. They tend to be slightly loan-heavy but less so than acquirer market-years. Target market-years are less concentrated and their deposit and loan rates exceed acquirer market-year rates.

In Panel C, market-years are split on MDH. I report statistics for the lowest (loan-heaviest), middle two, and highest (deposit-heaviest) MDH quartiles. The loan-heaviest quartile is much smaller than the other three in terms of population with a slightly lower median income and unemployment rate. Higher concentration measures likely reflect fewer market participants. On most dimensions, the deposit-heaviest quartile resembles the middle two, although, by definition, it has more deposits and fewer loans. Notably, the deposit-heaviest quartile has higher loan and deposit rates which may drive higher (lower) deposit (loan) volumes, although pricing differences could also reflect an urban versus rural divide.

4. Bank-market-year analysis of customer outcomes

This section examines M&A's impact on customer outcomes. I examine deposit and SBF loan volumes as well as prices for the four deposit and loan products described in Section 3.4. I refer to these six variables, collectively, as 'customer welfare measures'.

4.1. Customer welfare changes around M&A

I begin with univariate analysis of changes in customer welfare measures around M&A. For deposit and loan volumes, I compute log changes between the pro-forma bank's *t-1* level in market *m* and the consolidated bank's *t+2* level in the same market, where year *t* is the acquisition year. The pro-forma bank's level is defined as the sum of target and acquirer levels in market *m*. For the four rate variables, I measure the raw difference between the target's *t-1* rate and the consolidated bank's *t+2* rate. Thus, volume outcomes denote log changes whereas rate outcomes are expressed as percentage point differences.

Table A2 reports the mean 3-year change in each customer welfare measure over my sample. The number of observations with non-missing data are listed below sample averages. I also rank observations by their *t-1* MDH levels and report means for the highest and lowest quartiles. The last row presents t-statistics from mean difference tests between these quartiles. On average, acquirers raise more deposits and issue more loans in target markets, consistent with depositor and borrower welfare gains. Decreased deposit and loan rates imply that the average depositor may be worse off, pricing-wise, but the average borrower better off, in line with Park and Pennacchi's (2009) predictions. Lower loan prices are consistent with Sapienza (2002) and Erel (2011) and lower deposit rates echo Prager and Hannan (1998). Although deposit rates and volumes appear to change in opposite directions, the two are not necessarily at odds. Acquirers could raise deposits while reducing rates if they account holders other benefits like lower fees, sign-up bonuses, or favorable pricing on loans linked to deposit accounts. Significant sample composition differences between the deposit level and rate tests may also affect the results. Because larger banks are more likely to subscribe to RateWatch, observations for which rate data is available are skewed toward large banks acquiring large targets.

Quartile differences present the first piece of evidence that DLI moderates M&A outcomes. For the loan-heaviest quartile, deposit-gathering increases by 9 percent over three years but for the deposit-heaviest quartile, it remains around pro-forma bank levels. Conversely, in the loan-heaviest markets lending stays flat, but in the deposit-heaviest ones, acquirers grow loans by nearly 30 percent in 3 years.[11] Quartile differences are statistically significant which suggests that acquirers grow deposits (loans) by more in loan-heavy (deposit-heavy) markets, than in deposit-heavy (loan-heavy) ones. Deposit rate changes present weaker evidence. CD rates fall only in the deposit-heavy sample but the mean difference from the loan-heavy market change is only marginally significant. For the other products, I detect no difference between quartiles as rates fall all around. Overall, this table offers preliminary evidence that (i) the impact of M&A on target markets varies by the markets' DLI and (ii) M&A could help alleviate DLI by increasing deposits (loans) in loan-heavy (deposit-heavy) markets.

To test if these conclusions survive multi-variate analysis, I estimate the following regression:

[11] A 30 percent change appear suspiciously large but, annualized, 9 percent growth could be reasonable. First, this measures bank-market-level impact, not overall growth at the market level. It is plausible that acquirers invest heavily in markets that have the most under-served lending opportunities. Because acquirers are much larger, they have access to more funding, easing credit constraints. Further, Table A7, below, suggests the disparate impact of M&A on loan-heavy and deposit-heavy markets can be detected at the market level. Combined with Table A6 evidence that rivals may offset acquirer policies, the acquirer's loan growth should be large. Still, the figure must be cautiously interpreted for two reasons. First, these are univariate statistics. Table A3, below, shows a somewhat tempered effect in multivariate analysis. Second, targets, which are smaller than their acquirers, on average, are less likely to report SBF lending in *t-1* which inflates percent changes. Notwithstanding, these issues affect both quartiles so it is not obvious that they would bias mean difference tests.

$$\Delta_3 Y_{z,m} = \alpha + \beta_1 MDH_{z,m} + \boldsymbol{\beta X}_{z,m} + \epsilon_{z,m} \tag{1}$$

where z and m respectively index the merger and market. As before, the dependent variable alternates between 3-year changes in customer welfare measures. The coefficient of interest, β_1, relates market DLI to the outcome variable.[12] Note that this specification imposes a stronger assumption than the quartile difference tests above: a linear relationship between DLI and customer welfare outcomes. Controls, measured as of *t-1*, come from previous bank M&A studies. Prager and Hannon (1998) and Sapienza (2002) argue that in-market M&As, those in which the acquirer already operates in the target's market, affect customers differently than out-of-market ones. To model this, I include an indicator, *In-market M&A*, equal to one if the acquirer operates in the given target market in *t-1* and zero, otherwise. Other controls include market concentration, *Concentration*, measured by the Hirschman-Herfindahl Index (HHI); natural logarithms of market population and income as well as local unemployment rate; natural logarithms of target and acquirer total assets; and ratios of nonperforming assets to total assets and equity capital to total assets for both banks. *Concentration* is measured as the deposit market HHI for tests of deposit volumes and rates and SBF loan market HHI for tests of loan volumes and rates.

Table A3, Panel A, reports estimated coefficients. For deposit and loan volumes, univariate results from Table A2 continue to obtain in this linear specification. In deposit-heavier target markets, acquirers increase deposit-gathering by less and lending by more; in loan-heavier markets, the opposite holds. However, tests of deposit rates fail to support Table A2's tentative

[12] A potential concern with these and subsequent tests is reverse causality. One could argue that MDH results from higher or lower deposit and loan levels as much as it causes them. However, it is important to consider that MDH is measured at the market-level whereas changes in customer welfare outcomes, at the bank-market level. Even if a single bank's lending, for example, could materially impact an entire market's MDH, deposit-gathering must not increase proportionately or MDH would not change. Finally, MDH is measured with a lag relative to customer welfare measures. It is chronologically impossible for the *ex post* outcomes to affect *ex ante* MDH.

conclusions. The negative β_1 is statistically indistinguishable from zero for both deposit products and mortgage loans but marginally significant for car loans.

Although Equation 1 controls for several important determinants of customer welfare changes, results may still be driven by unobservable differences that correlate with certain banks' pursuit of certain targets and not others. For that reason, Panel B estimates the same regressions using merger fixed effects. These specifications compare outcome variables across different markets acquired through the same merger, holding constant factors such as acquirers' strategic objectives or the targets' core strengths. These tests echo a significantly negative (positive) β_1 in Column 1 (2) and magnitudes hardly change from Panel A. Again, MDH predicts lower deposit and loan pricing although the difference is only significant for interest checking accounts. Overall, Table A3 reinforces key findings from Table A2: acquirers increases deposit-gathering (lending) by less (more) in deposit-heavy markets, whereas in loan-heavy ones, the opposite holds. These conclusions obtain even when comparing markets acquired through the same transaction, mitigating target-, acquirer-, deal-, or time-driven endogeneity concerns. Rate change results are mostly insignificant but when significant, support the framework above.

To gauge these effects' economic magnitude, consider the hypothetical acquisition in the introduction. Suppose an acquirer gains access to three markets: a 'well-funded' one with an average level of deposits for its loan volume that year, a deposit-heavy one in which deposit levels exceed their predicted value by one standard deviation (0.705), and a loan-heavy one in which deposits fall one standard deviation below their predicted value. Table A3, Panel B suggest that over three years, the acquirer will raise 3.45 percent less (more) deposits from and issue 13.04 percent more (less) loans to the deposit-heavy (loan-heavy) market than the well-funded one. Annualized, this amounts to a 1.13 (4.17) percent change in deposit-gathering (lending).

Figure A2A illustrates these results. First, I rank all merger-markets dyads by average MDH over the three years before the merger. Observations are then sorted into 20 equal-sized bins. Within each bin, I compute the average 3-year log difference in deposit and SBF volumes. These differences are annualized, plotted by bin, and connected by a line of best fit. The clear linear trends support Tables A2 and A3.

4.2. Customer welfare levels around M&A

The previous two tables measure acquirer changes from target/pro-forma levels in target markets. This approach lucidly captures an acquirer's operational shift in these markets but the cost is an omitted counterfactual. It could be that deposit-heavy markets naturally shed deposits and gain loans as they move toward equilibrium whereas loan-heavy markets organically do the opposite. If so, one might expect similar coefficients for β_1 absent any M&A impact. To address this issue, I employ the full panel of bank-market-year observations, allowing me to test whether M&A, itself, matters. Framed differently, this approach tests whether MDH moderates the impact of M&A. I estimate the following regression:

$$Y_{b,m,t} = \alpha + \beta_1 M\&A_{b,m,(t-1:t-3)} + \beta_2 \overline{MDH}_{m,(t-1:t-3)} + \beta_3 (M\&A\ x\ \overline{MDH})_{b,m,(t-1:t-3)} + \boldsymbol{\beta X}_{b,m,t} + [(\gamma_{b,t} | (\gamma_{b,m} + \delta_t)] + \epsilon_{b,m,t} \quad (2)$$

where b, m, and t index bank, market, and year, respectively. The dependent variable alternates between customer welfare measures. Whereas the preceding analysis focused on changes, Equation 2 switches to levels. Not only are levels in line with prior work but they dramatically simplify inference of this fixed effects model. Independent variables include an indicator, *M&A*, equal to one if bank *b* acquired another bank in market *m* over the last three years and zero, otherwise. I also include the market's average MDH over the past three years, $\overline{MDH}$. The coefficient of interest, β_3, models the interaction between these two. It shows how M&A outcomes vary by $\overline{MDH}$. Controls mirror those in Table A3 with one exception. Because Equation 1 was estimated at the merger-market level, I controlled for target and acquirer characteristics pertaining to each merger. Given the bank-market-year panel structure in Equation 2, I cannot include target and acquirer attributes; most observations are not M&A-related. Instead, I control for the size, asset nonperformance, and capitalization of each bank-year.

Omitted factors that codetermine dependent and independent variables impede β_3's causal estimation. Variables like a market's time-varying investment opportunities affect M&A decisions, MDH, and lending/deposit-gathering outcomes. Regional economic factors may induce merger-waves or deposit-heaviness at certain times and not at other times. Although instruments such as the Riegle-Neal Act's staggered implementation have been used as exogenous shocks to bank M&A (e.g. Black and Strahan, 2002; Rice and Strahan, 2010), I am aware of no instrument exogenous to deposit-gathering and lending yet relevant to MDH.[13]

Fortunately, the three-dimensional panel structure allows me to suppress potentially contaminating heterogeneity using two-way fixed effects. Bank-year fixed effects allow me to compare a bank's operations in one market to the same bank's concurrent operations in another market. This approach mitigates endogeneity concerns from time-varying bank-level omitted variables like corporate culture or risk-appetite. For example, bank-year fixed effects allow me to compare Bank of America's (BOA's) lending in New York City (NYC) with BOA's lending in the Boston in the same year. β_3 estimates how differences in MDH between NYC and Boston relate to BOA's lending policy in those cities if it acquires into them around the same time. In this

[13] Regarding the Riegle-Neal Act, specifically, I cannot use it as an instrument because my sample starts after its 1994-1997 implementation, nor could I adjust my sample period, since the first year RateWatch data become available is 1998.

specification, cross-market differences still threaten identification. Toward that end, I exploit bank-market fixed effects which account for time-constant (or to some degree sticky) bank, market, or bank-market conditions. Doing so eliminates cross-sectional variation by comparing a bank's operations in a given market at one point in time to that bank's operations in the same market at other points in time. For example, bank-market fixed effects allow me to compare BOA's lending in NYC across different years. β_3 estimates how the impact of BOA's acquisitions in NYC differs across periods in which NYC is more and less deposit-heavy. This approach suppresses sticky forces like demographics or cultural savings preferences that codetermine bank operations and MDH. With bank-market fixed effect specifications, I also include year dummies to account for national time series trends like the prime rate or quantitative easing programs. Thus, my methodology decomposes variation into purely cross-sectional and purely time-series components.

Table A4 summarizes these regressions; Panel A (B) reports bank-year (bank-market plus year) fixed effect models. Controls that do not vary within bank-year are dropped in Panel A. On average, a bank sources fewer deposits from and issues fewer loans to markets recently acquired through M&A than it does from/to other markets (β_1, Columns 1 and 2). However, because *In-market M&A* is included below as a separate variable, *M&A* represents the effect for out-of-market acquisitions. Thus, β_1 reflects the obvious results that a bank's operations are generally smaller in markets recently expanded into than in their core markets. The very positive coefficient on *In-market M&A* denotes much larger loan and deposit operations when the bank acquires more market share in an existing market through M&A. Interestingly, unlike what Tables A2 and A3 revealed for acquirers, the average bank gathers *more* deposits from deposit-heavy markets and lends *more* to loan-heavy ones. In other words, β_2 in Columns 1 and 2 implies that a market's DLI is sticky and only equilibrates gradually.

M&A alters the relationship between DLI and bank policies. The negative (positive) β_3 in Column 1 (2) implies that M&A offsets a market's DLI. A bank merging into a deposit-heavy market gathers fewer deposits and issues more loans in that market than it does in a loan-heavy one, acquired at the same time. Thus, M&A counteracts market DLI.

Columns 3 through 6, again, provide ambiguous estimates of M&A's impact on deposit and loan pricing. β_1 is generally insignificant but significantly positive for checking accounts. β_2 suggests that a bank sets marginally lower checking account rates (Column 2) in deposit-heavy-markets than in loan-heavy ones but the other products' pricing is unaffected. β_3 is consistently insignificant. Even the statistically significant coefficients are economically negligible (half a basis point). One explanation for non-results is that prices could be set by efficient markets and the bank's choice is only how much funding to demand or supply at market rates. Inconsistencies could also reflect uniform bank pricing strategies across markets (Calem and Nakamura, 1998; Hannan and Prager, 2004) or rate stickiness (Kahn, Pennacchi, and Sopranzetti, 1999). Finally, another explanation is a selection bias toward larger institutions when sampling deposit and loan products. Larger banks may be less likely to adjust rates in individual markets.

Whereas Panel A reflects within bank-year, cross market variation, Panel B is estimated using only time-series variation. Similar trends emerge. A bank acquiring into a given market when that market is deposit-heavy grows deposits slower and loans faster than the same bank acquiring into the same market when that market is loan-heavy (β_3, Columns 1 and 2). β_1 suggests that banks raise more deposits but reduce lending slightly after acquiring a new market although the coefficient on *In-market M&A* shows both increase after an in-market acquisition. Finally, β_2 again suggests DLI persistence.

Columns 3 through 6 continue to provide ambiguous results. When significant, *M&A* and *In Market M&A* dummies suggest worse pricing for bank customers; however, CD rates are only worse for in-market mergers and these transactions appear not to affect loan rates. Column 4, β_2, shows that a bank's checking account rate in a given market decreases (increases) when the market becomes more deposit-heavy (loan-heavy), but Columns 3, 5, and 6 detect no relationship. Heterogeneous effects could reflect different pricing strategies for different products. Meanwhile, β_3 appears positive in Column 4 but insignificant in other columns and economically small for all products. Because the rate analysis fails to produce a discernable trend, I exclude it in subsequent tables. Untabulated work affirms that M&A does not affect rates in a consistent, interpretable way, in my sample.

Volume estimates obtained from cross-market and time-series tests point in the same direction and are consistent with Tables A2 and A3 and Figure A2A: the effect of bank M&A on markets differs by the market's DLI. Acquirers gather more (fewer) deposits from and lend less (more) to recently acquired loan-heavy (deposit-heavy) markets than to other markets in which they concurrently operate. They gather more (fewer) deposits from and lend less (more) to recently acquired markets when these markets are loan-heavy (deposit-heavy) than when they are not. By doing so, acquirers counteract DLI persistence. These results also suggest that prior tests of a 'mean' M&A impact on deposit or loan outcomes mask this important determinant, which could explain conflicting findings. According to Panel B, a one standard deviation swing in MDH is enough to flip the sign on M&A's impact on deposits or loans.

4.3. Robustness Tests

I measure DLI as residuals from within-year regressions of log deposit volumes on log SBF loan issuance and its square. One concern is that comparing a market to all others within the same year masks important regional differences in deposit-loan ratios. It could be that an equilibrium deposit-to-loan ratio in one region is very impractical for another. I choose a nation-wide counterfactual because, over my sample period, the acquisition market for many banks is nation-wide. Thus, when deciding which markets to acquire into, banks might compare across all potential target-markets. However, in a robustness test, I replicate my main results re-estimating MDH using within-state-year regressions instead of within-year ones. These findings are presented in Table A5, Columns 1 (deposits) and 2 (loans). As before, Panel A (B) reports bank-year (bank-market plus year) fixed effect estimates. This, and subsequent robustness tests, include all controls from Table A4 although they are not presented for brevity. Magnitudes are nearly unchanged from Table A4 and the same conclusions obtain.

Another concern is that markets with no recorded SBF loans might drive my results. Recall that 20 percent of market-years in my sample contained no reported SBF loans. For these observations, my methodology still estimates MDH although any estimate is very noisy. Because these are very rural areas served by few banks, they represent only 7 percent of bank-market-year observations. Still, to ensure that they do not drive my results, I exclude these market-years when measuring MDH and drop all related bank-market-years. Coefficients from the re-estimated Equation 2 are reported in Columns 3 (deposits) and 4 (loans) of Table A5. Highly significant results continue to obtain.

I estimate MDH using OLS which is sensitive to outliers. Logging deposit and loan volumes partly allays these concerns. I do so further by winsorizing my MDH estimates at their 1 percent tails. Results in Columns 5 and 6 show very similar, statistically significant estimates.

Another issue is my reliance on one loan type, SBF, to infer DLI. SBF loans factor into this study as the dependent variable in Column 2 and also as part of the HHI measures in Columns

2, 5, and 6. I focus on SBF loans for several reasons. First, small business lending positively affects economic development (Craig, Jackson, and Thomson, 2007; Hakenes, Hasan, Molyneux, and Xie, 2014) so it is an important phenomenon to understand. Because of this, many studies in the banking literature focus on SBF lending (e.g. Berger and Udell, 1995; Petersen and Rajan, 2002). A second reason is to maintain consistency with prior work. Third, data-availability is a factor; banks are required to report SBF loans but not most other loan types. Proxying for overall lending with SBF lending has two shortcomings. One is that many smaller banks are exempt from reporting, which induces missing data issues. Greenstone, Mas, and Nguyen (2014) partially allay this concern; they estimate that 86 percent of SBF loans by volume are included in the CRA database I use. Further, nearly all studies before me explicitly focus on the effects of the largest M&As so this issue actually makes my results more, not less, comparable with theirs. The second, more serious, concern is that my results could be ungeneralizable to other loan types. Unfortunately, bank-market-year level data do not exist for other important loan categories such as commercial or construction and industrial. They do exist for home mortgage lending and some papers, including Loutskina and Strahan (2009) and Gilje, Loutskina, and Strahan (2016) use mortgage lending to proxy for overall credit market outcomes. Home mortgage data is available for a wider scope of banks. Whereas the 2016 SBF reporting threshold was $1.216 billion, banks over $44 million had to report mortgage lending. The main drawback to using HMDA loans is that most of these are immediately sold. Thus, banks have incentives to grant as many loans as possible in all markets. Also, the causal link between mortgage lending and economic development is more tenuous. Columns 7 and 8 of Table A5, replicate the main results from Table A4 measuring deposit-heaviness with home mortgage loan volumes. Specifically, I recreate MDH using a market's log home mortgage loans and its square instead of log SBF loans and its square. I also swap bank-market-year home mortgage loans for SBF loans as the dependent variable in loan volume regressions and control for a home mortgage-based HHI. In Panel A, the results are qualitatively similar to my baseline findings. In Panel B, Column 7, similar results obtain for deposits when measuring MDH and market concentration with home mortgage volumes. In Column 8, however, two differences emerge. First, whereas Table 4A showed that banks issue fewer SBF loans in markets after they were recently acquired than at other times, M&A relates positively to mortgage lending. Possibly related to this difference, MDH no longer moderates the impact of M&A on lending (β_3). One explanation is that because lenders rarely keep home mortgage loans on the books, they have little incentive to toggle home mortgage lending based on market characteristics. Still, similar results for 3 of 4 tests using home mortgage data support the notion that my results are not SBF loan specific or driven by data reporting problems.

One issue common to most finance papers over my sample period is the 2007-2009 financial crisis. Borisova, Fotak, Holland, and Megginson (2015), Agarwal, Arisoy, and Naik (2017), and many others detect different results in and outside of the financial crisis years. Because merger activity increased around that period, one may wonder how these years affect my findings. This issue is addressed by re-estimating Equation 2 excluding observations from the crisis years, 2007-2009. Columns 9 and 10 show that my baseline results are unaffected.

Another concern is that large banks can drive the results. For example, Berger and Bouwman (2009) find that recently merged banks contribute much of the banking industry's liquidity. However, their result stems mostly from large institutions as the authors show that small and medium banks create more liquidity when they do not merge than when they do. Another example is Minton, Taillard and Williamson (2014) who show that bank board financial expertise relates positively to risk-taking at the onset of the recent financial crisis. Their result is also driven by large banks; small and medium ones exhibit no relationship. In light of these and similar papers, one may wonder whether my findings reflect just the largest institutions, which actively merge and acquire. I re-estimate Equation 2 discarding all observations for banks over $1 billion dollars in total assets. Although the Federal Reserve considers banks over $10 billion large, I choose a stricter threshold because it allows me to test another, related concern: missing SBF data. The mandatory reporting threshold for SBF loans is around $1 billion, although some smaller banks choose to report SBF volumes. If SBF lending is mismeasured for smaller banks, then by throwing out all banks over $1 billion, I severely bias myself away from finding results. Still, Columns 9 and 10 of both panels find statistically significant differences between M&A's effect on deposit-heavy and loan-heavy markets. In unreported analysis, identical inferences obtain when excluding banks over $10 billion.

4.4. Deposit and loan volumes of target rivals around M&A

Next, I examine M&A's impact on other market participants. Existing evidence is mixed. For example, Berger et al. (1998) show that the target's rivals offset acquirer-driven changes to SBF lending but Prager et al. (1998) find that they follow acquirers in reducing deposit rates after market-power enhancing mergers. I test M&A's impact on rival operations by modifying Equation 2. I replace the indicator, *M&A*, with *Rival*, an indicator that captures whether the bank's competitor in a given market was acquired in the past three years. The interaction term changes accordingly and all else remains constant about the specifications.

Results presented in Table A6 provide mixed support. Columns 1 and 2 of Table A6 respectively summarize bank-year fixed effects regressions of log deposit and loan volumes; Columns 3 and 4 report bank-market plus year fixed effect versions. According to Column 1, the impact of M&A on rivals' deposit-gathering activity does not depend on MDH (β_3). However, Column 2 suggests that rivals refocus lending from deposit-heavy markets, which acquirers pursue more, toward loan-heavy ones, which acquirers pursue less. Rather than alleviate DLI, like acquirers, target rivals exacerbate it. Note the bank-year fixed effects preclude the possibility that these result reflect rivals and acquirers exchanging market share; coefficients are estimated within bank-year so they more plausibly indicate rivals' strategic refocusing. Time series estimates in Columns 3 and 4 imply that rivals gather more deposits from recently acquired markets when they are deposit-heavy than when they are loan-heavy, apparently amplifying DLI. However, β_3 is insignificant for the loan regression. Overall, this table suggests that acquirers and rivals behave differently around acquisitions and that rivals may actually exacerbate DLI.

5. Market-year analysis of customer outcomes

In this section, I adopt a broader vantage point, the market-year level, from which to assess bank M&As' overall impact. Doing so allows for preliminary welfare analysis. The results should be interpreted cautiously because aggregating up to the market-year level presents omitted variable concerns that are more difficult to mitigate with fixed effects.

5.1. Deposit and loan volumes of markets around M&A

Opposing effects for rivals and acquirers raises the question of whether customers experience any impact. If the aggregate demand for and supply of funds of all market participants is stable, there is little room to argue bank M&A is good or bad for society I test for the net effect on markets with the following market-year level regression:

$$Y_{m,s,t} = \alpha + \beta_1 M\&A_{m,s,(t-1:t-3)} + \beta_2 \overline{MDH}_{m,s,(t-1:t-3)} + \beta_3 (M\&A\ x\ \overline{MDH})_{m,s,(t-1:t-3)} + \boldsymbol{\beta X}_{m,s,t} + [(\gamma_{s,t}|(\gamma_m + \delta_t)] + \epsilon_{m,t} \quad (3)$$

where m, s, and t index market state and year. Independent variables mirror those in Table A4, but dependent variables are measured as market-year sums of bank-market-year values. This model tests whether a market's deposit or loan volumes change in the three years after an acquisition and whether the direction of this change depends on the preexisting DLI.

As in the previous regressions, endogeneity challenges the causal inference of β_3. I expect the most important threat to be temporally and regionally varying investment opportunities. I cannot use market-year fixed effects to eliminate these because no variation would remain. However, state-year fixed effects can partially offset endogeneity concerns. These specifications, reported in Table A7, Panel A, compare markets against their in-state peers at a given point in time to test how acquisitions affect deposit and loan levels in the cross-section. Another source of potentially confounding heterogeneity comes from unobservable, time-stable market-level differences. For example, the difference between deposit levels in Los Angeles and a Louisiana bayou town should be largely time-constant and may correlate with M&A and/or MDH in those areas. I eliminate such differences via market fixed effects and report results in Panel B. Market fixed effect regressions also include year dummies to control for national trends. Thus, I obtain cross-sectional and time-series estimates analogous to Sections 4.2 through 4.4.

However, I caveat that the following analysis, unlike bank-market-year regressions, is more susceptible to omitted variable concerns. One bank is unlikely to determine an entire market's MDH but, at the market-year level, the fact that some market are acquired into while others are not can very likely correlate with future deposit, loan, and, resultantly, MDH measures. Fixed effects can only alleviate these concerns imperfectly. Still, consistent results should reassure that the bank-market-year analysis is valid and relevant.

Columns 1 and 2 report results for market loan and deposit levels, respectively. Column 1 shows that, on average, deposits and loans are higher in recent M&A markets than their in-state peers (β_1). Again, β_2 indicates DLI persistence. β_3 shows that, on average, M&A's effect on market deposits varies little with MDH although its effect on loans varies in the predictable directions.

Returning to Column 1's insignificant β_3, I test whether insignificance comes from either or both ends of the MDH distribution. That is, does M&A not reduce deposit-gathering in deposit-heavy markets, not increase it in loan-heavy markets, or both? To test this, I replace the continuous MDH with a discrete indicator. In Column 3 (4), the indicator equals one if the market was in the highest (lowest) quartile of MDH within any of the past three years and zero, otherwise. These columns actually show that at both tails, an effect exists. This exercise is repeated for loans and results also support a more extreme impact of M&A on the deposit-heaviest and loan-heaviest markets. Thus, even at the market level, M&As lead market equilibration. Figure A2B illustrates these results through a similar procedure to Figure A2A. In loan-heavy (deposit-heavy) markets, M&A reduces (increase) lending by about two percent, annually, for three years whereas the impact for deposits is more muted.

5.2. M&A as an equilibrating mechanism

Having shown that M&A affects acquirer deposit-gathering and lending and that these effects can be detected at the market-year level, I proceed by directly measuring the association between M&A and MDH. Again, I present these results as associations since market-year analysis entails more identification challenges. I estimate the following regressions:

$$Y_{m,t}|\Delta_1 Y_{m,t} = \alpha + \beta_1 M\&A_{m,(t-1:t-3)} + \beta_2 FFF_{m,t} + \beta_3 (M\&A\ x\ FFF)_{m,t} + \boldsymbol{\beta X}_{m,t}$$
$$+ \left[\left(\gamma_{s,t}\middle|(\gamma_m + \delta_t)\right] + \epsilon_{m,t} \quad (4)$$

Variables are defined as in Equation 3 except for the dependent variable and DLI, which are discussed below. Table A8 presents results. The first (second) four columns are estimated using state-year (market plus year) fixed effects. In Columns 1 and 5, the dependent variable is the change in MDH from *t-1* to *t* and DLI is measured as the *t-1* MDH level. In the remaining columns, the dependent variable is the MDH level at year *t*. In Columns 2 and 5, DLI is measured as $\overline{MDH}$, defined above; in Columns 3 and 7 (4 and 8), it is an indicator equal to 1 if MDH over any of the past three years falls in the highest (lowest) MDH quartile.

β_1 alternates between negative, positive, and insignificant, depending on the specification, revealing inconsistencies in the mean impact of M&A on markets. However, β_2 portrays a steadier relationship. In Columns 1 and 5, the negative coefficient indicates that MDH naturally reverts toward zero. Meanwhile, β_3 shows that recently targeted markets exhibit a faster speed of adjustment. This is consistent with M&A equilibrating markets, although causality is more difficult to claim in this table. According to Column 1 estimates, recently acquired markets exhibit a 44 (=0.036/0.081) percent faster speed of adjustment. Columns 2 and 5 show that, in aggregate, recent MDH does not significantly moderate M&A's impact on current MDH. However, Columns 3, 4, 7, and 8 documents an association between M&A and equilibration in both extreme quartiles. Figure A3 graphically depicts the equilibration differential between market-years in which M&A does and does not occur. I rank market-years into 20 bins by lagged MDH then compute the average change in MDH by bin. Averages are plotted separately by M&A market-years (Xs; dashed line) and non-M&A market-years (Os; solid line). As in Table A8, M&A market-years appear to equilibrate faster than non-M&A ones. Outliers in the deposit-heaviest bin strongly affect these results but the lines of best fit have similar slopes when excluding these observations.

5.3. Potential sources of disequilibrium

The preceding analysis assumes that excessive DLI is inefficient, raising the question, "How can an inefficient disequilibrium be sustained?" In other words, why must incumbent banks wait for acquirers to step in instead of correcting the inefficiency, themselves? First, Table A8 and

Figure A3 suggest that market participants *do* correct imbalances in the long run but the process hastens in recently acquired markets. This is consistent with acquirers equilibrating markets, although causality cannot be established from this these tests. Examining markets in which incumbents and acquirers operate provides further insight. For each M&A in my sample, I identify the acquirer, target, and target rivals as well as all markets in which these banks operate in *t-1*. Then, for every bank, I compute a *bank-year* measure of MDH as the weighted average market-year MDH for each market in which that bank competes. Weights are assigned as bank-market-year deposits divided by bank-year deposits, which captures the market's importance to a given bank. For each M&A, I treat target rivals as if they were one large bank, aggregating MDH for all rival markets into a single data point. Next, I rank M&A transactions by target MDH and sort into 20 equally sized bins. Finally, within each bin, I compute average acquirer, target, and rival MDH and plot these points in Figure A4. The solid (dotted and dashed) line connects acquirer (target and rival) averages by bin.

This figure shows that across the entire distribution, MDH for targets and their rivals is nearly identical whereas acquirer MDH is significantly attenuated. In other words, target and rival compete in markets with similar DLI but acquirers access different, complementary markets. Whatever forces drive the target to over- or underinvest in a given market likely drive its rivals, but not acquirers, to do the same. Of course, part of this result mechanically stems from targets and rivals competing in the same market (as do acquirers, commonly). More importantly, it illustrates that acquirers are more diversified than incumbents with respect to the MDH of their geographic presence.

As for the source of DLI, there exist at least two well studied frictions that can preclude a competitive equilibrium in banking and invite rent-seeking acquisitions. Both rely on the empirical fact that acquirers are more geographically diversified than incumbents. Regulatory intervention is one obvious rationale. Consider a market in which participant banks are small and/or geographically constrained. Suppose the optimum solution for this market is to collect $40 million in local deposits, acquire $10 million in brokered deposits, and satisfy the market's $25 million SBF loan demand. This market's equilibrium deposit-to-loan ratio would be $40:$25. However, regulators dis-incentivize brokered deposits (Goldberg and Hudgins, 2002; Shaffer, 2012) through higher insurance premiums and less favorable liquidity ratings.[14] Therefore, the marginal cost of these deposits might outweigh the marginal benefit in satisfying, say, an incremental $5 million in SBF loan demand. As a result, the market's actual deposit-to-loan ratio would be $40:$20, flagging it as a deposit-heavy market. An external bank with access to liquidity (perhaps, in loan-heavy markets) can acquire an incumbent and reap rents by transferring liquidity toward this deposit-heavy market. The acquirer, often larger, might also have a larger base over which to allocate the costs of brokered deposits. In this case, acquirers could alleviate DLI that incumbents could not by lending more. Indeed, Gilje, Loutskina, and Strahan (2016) find evidence of liquidity redistribution around exogenous deposit windfalls from the shale oil boom.

Bank herding incentives (Scharfstein and Stein, 1990; Rajan, 1994; Acharya and Yorulmazer, 2008) provide another potential explanation. Suppose two banks are in a market with $40 million in deposits and $25 million in profitable loan demand. Each collects $20 million in deposits but one issues $15 million in SBF loans. If the other bank responds optimally and lends $10 million, it may be labeled a credit rationer, inviting deposit outflows or foregone future business. Thus, a sustainable short-term equilibrium might be to mimic other market participants and lend $15 million. This would generate the suboptimal market deposit-to-loan ratio of $40:$30,

[14] https://www.fdic.gov/deposit/insurance/assessments/proposed.html and https://www.fdic.gov/regulations/safety/manual/section6-1.pdf

rather than the optimal $40:$25, flagging a loan-heavy market. An external bank that is less reliant on that market for funding can acquire either incumbent and withstand depositor discipline.

There are also strategic considerations: if loan demand dries up, it may be more practical to reinvest deposits into suboptimally risky loans than to let core deposits run off or pay deposit rates without offsetting income. There are game-theoretical considerations: incumbents trying may over-compete in a market in the short-term yielding suboptimally high loan or deposit levels. There are other stories as well but wherever the disequilibrium comes from, regulators have long been concerned with it.

It could be operational: banks may inordinately lend to a given market for strategic reasons. It could be competitive: overinvestment may occur if banks are fighting to control a market. It could have demographic, behavioral, or other causes. Whatever the cause, regulators have long been aware of and concerned with these imbalances. The CRA, passed in 1977, mandates that banks reinvest in markets from which they gather deposits, preventing them from unduly exporting funds from communities. [15] Evidence in Bhutta (2011), Munoz and Butcher (2013), Avery and Bevoort (2015) support the positive effect of this law on development, which partly validates a negative view of DLI.

5.4. Suggestive evidence on welfare effects

So far I have shown that acquirers redistribute lending and deposit-gathering from relatively abundant to relatively scarce markets and that markets move toward equilibrium after acquisitions. I have also argued that access to more geographically diverse markets allows acquirers to alleviate DLI-inducing frictions faster than incumbents could. However, whether these changes aid or harm markets remains to be seen.

[15] Among many sources, Consumer Compliance Outlook concisely summarizes this law: https://consumercomplianceoutlook.org/2014/first-quarter/understanding-cras-assessment-area-requirements/

A full-scale analysis is beyond the scope of this paper but Figure A5 provides strong and consistent evidence from five different sources. To construct the figure, I compute year-over-year changes in each of six measures of consumer welfare: median income and unemployment rate as described above, payroll from the BLS County Business Patterns database, poverty rate from the Census Small Area Income and Poverty Estimates, and the number and dollar value of new housing unit permits from the Census Building Permits Survey. Changes in poverty and unemployment rates are computed as percentage point differences whereas the other variables are log differenced. I also compute year-over-year MDH differences. Finally, I sort market-years into 20 MDH bins and, within each bin, I calculate the correlation between market welfare changes and MDH changes. Correlations are plotted in Figure A4 as well as a line of best fit and its 90 percent confidence interval.

The results suggest that reducing DLI enhances market welfare. In loan-heavy markets, MDH increases associate with median income increases, but the effect dissipates as deposit-heaviness rises. For payroll, number of new housing units, and value of new housing units, correlations are even negative for high MDH markets. In contrast, measures of low market welfare, unemployment rate and poverty rate, exhibit the opposite trend. In loan-heavy markets, MDH increases correspond to less unemployment and poverty. This result weakens then reverses at higher for deposit-heavy markets. This analysis, albeit rudimentary, provides evidence from six different sources that when DLI falls, economic welfare rises. To the extent that bank M&A can facilitate equilibration, the phenomenon can be welfare-enhancing.

6. Conclusion

This paper argues that previous research relating bank M&A to customer welfare overlooks a key determinant: deposit-loan imbalances (DLI) in the target's markets. DLI are likely to affect the acquisition decision and the lending and deposit-gathering policies acquirers pursue. Rent-seeking motives can guide larger banks serving a more diverse set of markets to acquire into imbalanced markets, and alleviate DLI via profit-maximizing behavior.

Consistent with this intuition, I show that a market's deposit heaviness (MDH) moderates acquirer policies in recently acquired markets. The effect is strong enough to change the predicted sign: in markets that are one standard deviation above (below) the mean MDH, acquirers decrease (increase) deposit-gathering and increase (decrease) lending. These results are robust to different measures of MDH and to excluding potentially confounding observations. MDH does not appear to moderate acquirers' deposit and loan pricing decisions in recently acquired markets. M&A's impact on target rival behavior is less conclusive but partially consistent with rivals refocusing operations away from targeted markets. Overall, I show that acquisitions attenuate DLI. I present evidence that lower DLI correlates positively with six measures of economic welfare. These results support a social welfare enhancing view of bank M&A as a means of redistributing deposit-gathering and lending services from abundant to scarce markets.

This study also makes several methodological contributions. First, instead of looking at a single dimension of customer welfare, price or volume for depositors or borrowers, I examine all. To my knowledge, my paper is the first to consider the four through a unified lens. Doing so provides a more comprehensive view of bank M&A impact and illustrates that M&A affects different customer groups differently. Second, my M&A sample exceeds that of most previous work. I use the entire universe of U.S. bank M&A over a 19 year span and measure its effects on customers in every U.S. market. Third, my results emphasize how important it is to understand which heterogeneity which drives empirical estimates. I highlight examples where one fixed effect specification yields certain conclusions qualified by another specification. I carefully discern between time-series and cross-market heterogeneity although my main findings are supported by both. Fourth, I contribute by proposing a measure to gauge a market's DLI. This variable can help answer questions like "why do banks lend more to some markets than others" and "should banks that operate in deposit-heavy markets have more or less capital than banks that operate in others?" These questions and others spurred by my results, I leave for future research.

Chapter 2: Neglecting Peter to Fix Paul:

How Director Interlocks Transmit Bank Shocks across Sectors

1. Introduction

On October 23th, 2009, Cascade Bancorp (CACB) signed a Written Agreement with the Federal Reserve Board. Written Agreements are enforcement actions (EAs) through which regulators require or prohibit certain activities by banks or their employees. Over two thousand EAs were issued during the recent U.S. banking crisis. This particular one identified capital deficiencies and restricted shareholder distributions. It also required the directors to monitor EA compliance and report to regulators, quarterly. Consistent with adverse wealth implications, CACB experienced cumulative abnormal stock returns of negative 6.4% from the trading day before enforcement to the day after. Over the same timeframe, the stock prices of Idaho Power Co. (IDA) and Schnitzer Steel Industries, Inc. (SCHN) fell by 0.50 and 0.75%, respectively, on a risk-adjusted basis. These firms are not in the same industry as CACB so information spillover should not explain their returns. Nor do they have a credit relationship with CACB. What they do share is a common director, Judith Johansen. When the EA was issued, Ms. Johansen served on each firm's board. Not only was she a member, but she served on the audit and compensation committees at all three, and chaired SCHN's compensation committee.[16] In this paper, we investigate whether directors like Ms. Johansen, who link banks with non-banks, transmit financial sector shocks into other industries.

[16] For more information, refer to Ms. Johansen's LinkedIn profile: https://www.linkedin.com/in/judi-johansen-63103612b/.

Many papers before us study how bank shocks propagate through the economy (e.g. Bernanke and Blinder, 1988; Bernanke and Gertler, 1995; Chava and Purnanandam, 2011; Puri, Rocholl, and Steffen, 2011; Iyer, Peydró, da-Rocha-Lopes, and Schoar, 2014; and Amiti and Weinstein, 2018). Their focus, however, has been on one specific mechanism, bank lending. Other transmission channels have received relatively little attention. Our paper complements this literature by highlighting another mechanism: directors who concurrently serve on boards of banks and non-financial firms (NFFs). CACB's case illustrates the potential for a "*bank-linked director channel*" of financial sector shock transmission which operates as follows. Bank shocks impose additional responsibilities on bank directors. With limited resources to monitor and advise multiple firms, directors who also serve NFFs redistribute effort toward the bank. Doing so weakens NFF governance. To the extent that governance is value-relevant (Chhaochharia and Grinstein, 2007; Nini, Smith, and Sufi, 2012; Burt, Hrdlicka, and Harford, 2018), such shocks reduce NFF stock prices. If bank-linked directors are especially valuable to the NFF, which we show to be the case, their inattention can destroy shareholder wealth. Given how frequently and severely economic and regulatory events shock bank governance demands, bank-linked directors can induce substantial valuation spillover.

The shocks this paper examines are EAs issued between 1990 and 2017 by federal regulators against publicly held U.S. banks and bank holding companies. Detailed in Section 2, EAs target bank conduct deemed 'unsafe or unsound'. This setting offers three key advantages for empirical analysis. First, these orders impact banks in meaningful ways (Curry, O'Keefe, Coburn, and Montgomery, 1999). Some force managers to alter lending practices (Deli, Delis, Hasan, and Liu, 2016; Delis, Staikouras, and Tsoumas, 2017) and risk taking (Fissel, Jacewitz, Kwast, and Stahel, 2018). Others highlight internal control weaknesses by barring employees from future banking sector employment (Roman, 2017). EAs affect bank reputation (Delis, Iosifidi, Kokasa, Ongena, and Xefteris, 2017), borrowers (Deli, Delis, Hasan, and Liu, 2016; Delis, Staikouras, and Tsoumas, 2017), competitors (Slovin, Sushka, and Polonchek, 1999) and local markets (Danisewicz, McGowan, Onali, and Schaeck, 2018) so it no surprise that they impact shareholders (Brous and Leggett, 1996). Second, EAs offer a rare laboratory to directly observe corporate governance shocks. They signal poor governance at the bank (Nguyen, Hagendorff, and Eshraghi, 2016) but, more importantly, the issues they expose can require substantial management and director resources to address. Curry et al. (1999) writes that EAs "*requiring remedial measures generally remain in effect for approximately two years. However, in the more serious cases, actions can last up to three or four years and during this period are subject to amendments mandating further actions by the institution*" (pg. 5). Following enforcement, regulatory scrutiny increases sharply, providing more work for the directorate and greater incentive for diligence (Rezende and Wu, 2014; Danisewicz et al., 2018). Thus, EAs specifically shock director responsibilities. Third, these interventions are plausibly exogenous to the stock price performance of our observational units: NFFs whose boards interlock with enforced bank boards. It is very unlikely that NFFs connected to banks through common directors, alone, can steer bank behavior enough to warrant regulatory intervention. Exogeneity supports our interpretation that the bank-linked director channel drives bank shocks' wealth effects on interlocked NFFs.

Indeed, these wealth effects are substantial. The average NFF in our sample experiences a negative 17 basis point, or $24 million, cumulative abnormal return over the three days centered on its interlocked bank's EA. This result withstands alternative measurement windows and sample selection methodologies; placebo tests confirm significant stock price declines *only* around EA-issuance dates and *only* for EAs issued to interlocked banks. Cross-sectional analysis supports internal validity: more value-destructive bank EAs induce more negative interlocked NFF returns.

After documenting cross-sector EA spillover, we test whether common director resource reallocation can explain it. Three pieces of evidence support this view. First, we show that bank-linked directors are especially valuable to the NFF because they disproportionately serve on and chair important committees and are more likely to be designated financial experts. Because they serve important roles, their monitoring and advising resources cannot be easily substituted by other board members. Second, outside NFF directors who concurrently serve on a bank board expend fewer resources on the NFF, relative to other board members, during bank enforcement years. We propose a novel method to measure outside director resource expenditure, building off of Farrell, Friesen, and Hersch (2008). These authors write "*any differences that may exist in compensation across individual outside directors for a given firm in a given year typically result from serving on different committees, serving as chair of a committee, serving as lead director or differences in meeting attendance*" (pg. 153). Because each dimension reflects effort spent on the firm, within-firm-year differences in compensation capture differential resource expenditure. Finally, we follow these withheld resources by examining director compensation on bank boards. Outside bank directors expend more resources on the bank board during enforcement years than they do on the same board in other years. These three observations form a coherent story: EAs cause valuable directors to reallocate resources from NFF to bank.

Although extant literature motivates our focus on resource reallocation, we consider three alternative explanations. Distinguishing between them is important for our paper's conclusion and policy implications. EA-impaired lending between enforced bank and interlocked NFF could also induce negative NFF returns. If an EA restricts bank lending, it could increase the NFF's borrowing cost, leading shareholders to adjust prices downward. In Section 5, we argue that this possibility is very remote in our sample. To explicitly test it, we hand-collect lending information from SEC filings for all NFFs in our sample. Significantly negative returns obtain when we exclude observations in which the enforced bank has lent to the interlocked NFF, in which the EA explicitly mentions lending, and in which the linking director is a bank insider, more capable of influencing credit decisions. Second, EAs could also affect NFF stock prices by providing new information about the linking director's quality. Enforcement could signal that the director, who failed to keep her bank out of trouble, is less qualified than NFF shareholders previously thought. If director aptitude is value-relevant, NFF stock prices should fall. We find that EAs have no impact on a director's future appointments, so rational shareholders should not perceive worse director quality. Finally, one could argue that our results exhibit a selection bias: directors serving poorly performing (hence, enforced) banks also serve poorly performing NFFs (those whose stock prices are falling). Our event study methodology precludes this explanation. The return estimation model captures persistently negative returns in the intercept. After accounting for firm and market performance, NFF returns should not consistently underperform their predicted values precisely around interlocked bank EA dates. Thus, resource reallocation most plausibly explains EA shock spillover.

Our main contribution is to explore a new mechanism, the bank-linked director channel, through which financial sector shocks spill over into the real economy. Our research integrates two extant literature streams: financial sector shock transmission and corporate governance. Bank shocks have been shown to affect real output and income growth (Jayaratne and Strahan, 1996), income levels (Ashcraft, 2005), and even local crime rates (Garmaise and Moskowitz, 2006). There exists an especially rich literature on the transmission of financial crises (Chava and

Purnanandam, 2011; Puri, Rocholl, and Steffen, 2011; Iyer, Peydró, da-Rocha-Lopes, and Schoar, 2014) into non-financial sectors. But in each of these papers, the underlying mechanism through which bank events affect the real economy is presumed to be lending. Content with this explanation, the field does not ask if and how bank shocks can *otherwise* affect the economy. To our knowledge, we are the first to identify a non-lending mechanism. The bank-linked director channel is substantial; in Section 4.2, we estimate that bank EAs removed at least $166 billion in NFF market capitalization between 1990 and 2017. This figure excludes their impact on interlocked private firms or, more significantly, larger banking shocks' impact on all firms. Consider the Basel III Accords and the Dodd-Frank Act, both of which increase bank director responsibilities. If NFFs and banks compete for limited director resources, higher governance requirements in the financial sector impose negative externalities on interlocked NFFs. More generally, our paper illustrates resource reallocation, even across sectors, as a previously overlooked cost to regulation.

Meanwhile, the corporate governance literature has recently shown that when a director serves multiple firms, tightening or loosening resource requirements at one company impacts the stock price of another (Falato, Kadyrzhanova, and Lel, 2014; Hauser, 2018). These studies leave two crucial questions unanswered: how can a single director be important enough to impact firm value and by what means does the shock spill over? We provide initial evidence on both. Directors with multiple appointments are more likely to serve on or chair important committees and to be designated financial experts. When one of the appointments is at a bank, the likelihood increases further. These findings bolster Falato, Kadyrzhanova, and Lel's (2014) and Hauser's (2018) surprising result that shocks to one director's attention are of sufficient consequence to impact stock prices. Like these papers, ours measures stock returns at one firm around plausibly unrelated events at another, when the two are linked only by a common director. Unlike these papers, however, we offer direct evidence that resource reallocation drives these value effects. Our paper supplements theirs by providing the missing link to their causal chain. We measure resource expenditure through outside director compensation regressions that include firm-year fixed effects. This straightforward, methodological innovation promises useful applications to future governance research.

Our paper intersects two other literature streams. One establishes that banker-directors, bank insiders who also serve on NFF boards, harm NFFs and benefit banks (Dittmann, Maug and Schneider, 2010; Kang and Kim, 2017). These directors reduce shareholder gains around acquisitions (Hilscher and Şişli-Ciamarra, 2013), dampen R&D activity (Ghosh, 2016), and are associated with lower shareholder value (Güner, Malmendier, and Tate, 2008). We highlight another NFF cost of employing banker-directors: these individuals' resources can be diverted into the volatile and heavily regulated banking industry, weakening governance. We also point out that bank shock exposure extends beyond banker-directors. Interlocks exist because NFF executives serve on bank boards and because non-executive directors often serve multiple boards. Our paper suggests that studying these connections, which outnumber banker-director connections four to one, may prove fruitful. Finally, we advance a budding literature on bank EAs. Extant work studies EA's impact on borrowers, depositors, competitors, and shareholders; no paper we are aware of investigates interlocked NFFs.

2. Enforcement actions

An institution's primary federal regulator conducts on-site examinations and off-site monitoring.[17] If either reveals bank operations to be 'unsafe or unsound', regulators can issue an EA to address deficiencies. Different EA types address different issues. Brous and Leggett (1996), Srinivas, Byler, Wadhwani, Ranjan, and Krishna (2014), and Nguyen, Hagendorff, and Eshraghi (2016) focus on three EA types considered severe: Prompt Corrective Action Directives require immediate recapitalization; Cease and Desist Orders and Formal Agreements require managerial action to remedy unsafe or unsound activities. All three are legally enforceable and the latter two differ only in that the Cease and Desist Orders are issued with or without the bank's consent but Formal Agreements represent voluntary managerial commitment. The latter are also referred to as Written Agreements or Consent Orders. Appendix A details the distribution of EA type in our study. As discussed below, our sample closely mirrors the universe of publicly available bank EAs. The most common orders are Sanctions against Personnel, which ban named individuals from further employment at any financial institution. Figure B1 portrays the time-series distribution of all publicly disclosed EAs over our sample period. The solid (shaded) bar depicts non-severe (severe) orders.

The EA setting allows us to trace shock spillover through the bank-linked director channel. It also offers several practical advantages. First is an abundance of and heterogeneity in events. Our final sample contains 1,245 orders issued to banks within 159 publicly held bank holding company structures that share 763 directors with 792 NFFs between 1990 and 2017. Ample

[17] Four U.S. regulators supervise banks and bank holding companies over our sample period. The Federal Reserve Board supervises holding companies and Federal Reserve member state-chartered banks; the Federal Deposit Insurance Corporation supervises Federal Reserve non-member, state-chartered banks; the Office of the Comptroller of the Currency supervises nationally chartered banks and, as of July 21, 2011, savings and loan associations; and the Office of Thrift Supervision supervised savings and loan associations before its July 21, 2011 dissolution.

variation across time, director, bank, and NFF ensures our results reflect general economic relationships; it also allows for a rich set of fixed effects to address confounding factors. Second, unlike most bank shocks, EAs exhibit well defined boundaries. Identification is more straightforward because data demarcate exactly which institution experiences which type of shock on which date. Finally, although EAs do affect bank performance, their impact is moderate relative to other bank shocks discussed in the press and academic literature. The fact that less pronounced bank events affect interlocked NFFs suggests more dramatic ones, like sweeping regulatory reform, bailouts, or financial crises should have even stronger implications. Although investigating the bank-linked director channel around these more prominent events would be interesting, uncertain event dates, counterfactual concerns, and omitted variable issues render the EA setting better identified.

3. Data and sample

Beginning with the BoardEx universe of publicly listed firms, we compare start and end dates of board appointments to identify directors who simultaneously hold bank and NFF board appointments. Director-company-years are merged with the Center for Research and Security Prices (CRSP) stock price data and Compustat financial data. From SNL Financial, we obtain issue date, EA type, and the Federal Reserve's primary identifier, RSSD, for the universe of EAs issued between 1990 and 2017. When the EA recipient is a bank, its RSSD is matched to its holding company RSSD using the National Information Center's 'Relationships' file; otherwise we retain the holding company recipient's RSSD. We then use the Federal Reserve Bank of New York's list of publicly traded financial firms to match recipient RSSDs with Permcos, yielding 3,242 EAs issued to publicly held holding companies and their subsidiary banks between 1990 and 2017.

In many cases, EAs are issued to the same financial institution in close succession. For example, the Federal Deposit Insurance Corporation can issue an EA against a bank and, days later, the Federal Reserve could issue one against that bank's holding company for the same infraction. The latter event is unlikely to provide news to the market. To limit dilution from subsequent EAs, we discard 892 orders issued within 30 days of a previous order to the same entity.[18] Of the remaining orders, 1,245 are issued to 159 banks that collectively share 763 directors with 792 NFFs. Because multiple EAs can be issued on the same day to the same entity, our sample includes 990 distinct bank-event dates. Banks can receive multiple EAs over our sample period and have multiple directors who serve on one or many NFF boards. Thus, 990 bank events generate 6,847 distinct NFF-event-date combinations with sufficient stock price data to compute returns. Table B1, Panel A, summarizes the time-series distribution of EAs, dates, bank-event dates, banks, directors, bank-linked NFFs, and NFF-event dates.

Our study relates to work on 'banker-directors', bank insiders who serve as independent directors on NFF boards (Kroszner and Strahan, 2001). Panel B of Table B1 frames our sample within this research stream. We split our sample of enforced bank-linked directors into three categories based on employment and do the same for all bank-linked directors in BoardEx. To distinguish bank insiders from outside directors, we identify all persons whose primary employment can be traced to a bank listed in the BoardEx-CRSP intersection. Likewise, we differentiate directors whose NFF employment appears in the BoardEx database of public firms from those primarily employed at private firms or outside the BoardEx dataset.[19] Only 17.3% of enforced bank-linked directors are bank insiders; most work outside of the banking sector.

[18] Our baseline results obtain when dropping this restriction or excluding EAs within 3 months of a previous order.
[19] Employment information for all but 39 directors can be found in the BoardEx database. In Table 1, Panel B, these 39 individuals are categorized as employed at a private firm.

Panel B also frames our sample within two other research streams: work on certified inside directors, insiders who hold outside appointments on different boards (Masulis and Mobbs, 2011), and 'busy directors', those concurrently serving three or more unrelated boards (Ferris, Jagannathan, and Pritchard, 2003). For both sub-fields, employment status (currently employed or retired) is relevant so Panel B separately tabulates director-years in which the director is currently employed by the bank or NFF and those after employment. We present the percentage of busy directors, average director age, and average firm size (measured in total assets) from the director-NFF-year sample. Of these director-NFF-years, 26.7% meet Masulis and Mobbs' (2011) definition of certified inside director and 38.9% meet the 'busy' definition. Unsurprisingly, retired directors are more likely to be considered busy. Overall, bank-linked directors represent a significant portion of the director pool: 11% of all director-NFF-year observations.

4. EA spillover through director resource reallocation

This section describes our main empirical tests and presents our findings. First, we confirm results from prior literature, that EAs negatively impact recipient banks. We then show EAs' negative effect on interlocked NFF share prices. Finally, we investigate director resource reallocation as an explanation for this bank shock spillover.

4.1. Bank shareholder reactions to EAs

Researchers from Brous and Leggett (1996) onward consistently find negative abnormal bank stock returns around severe EAs. We confirm these results for all EAs issued to publicly held banks over our sample period in the event study framework of Campbell, Lo, and MacKinlay (1997). Expected returns are estimated using the Fama and French (1993) 3-factor model:

$$r_{b,t} = \alpha_b + \beta_{MKT,b} * r_{MKT,t} + \beta_{SMB,b} * r_{SMB,t} + \beta_{HML,b} * r_{HML,t} + \epsilon_{b,t} \qquad (5)$$

where $r_{b,t}$ denotes day *t* stock return for bank *b*, $r_{MKT,t}$ denotes day *t* excess return on the market portfolio and $r_{SMB,t}$ and $r_{HML,t}$ are day *t* returns on the size and value factors, respectively. Excess market returns are computed as returns on the value-weighted portfolio of all CRSP stocks less the short-term treasury yield. Data on each factor come from Kenneth French's website. We use a traditional event window of 41 trading days centered on the EA issuance date and estimate parameters over the 252 trading days ending 21 days before issuance.

Figure B2 echoes results in prior literature by plotting daily abnormal returns around EA issuance, smoothed using a 3-day rolling average. The solid line (left axis) represents all EAs and the dashed one (right axis) represents only severe orders, which extant work commonly focuses on. Both lines portray substantial, negative shocks to a bank's value around the EA date, but the effect is stronger for severe orders. For the full (severe) subsample, mean stock prices fall by about half a percentage point (2.5 percentage points) around the event. The decline starts a few days before the EA and continues for several days after. A prolonged reaction could reflect delayed public access to EA issuance as banks can but do not necessarily disclose EA receipt the day the order is signed. Before enforcement and after the EA's effects are fully realized, stock returns deviate randomly around zero, which suggests that poor bank performance unrelated to the EA is unlikely to drive stock devaluation. Overall, these confirm that the negative EA effects documented by Brous and Leggett (1996), Slovin, Sushka, and Polonchek (1999), and Roman (2017) hold in our sample.

4.2. NFF shareholder reactions to EAs

Having replicated prior evidence on EAs' bank stock price impact, we turn to our units of interest, interlocked NFFs. We hypothesize that NFFs exhibit negative abnormal stock returns around dates on which their director-linked banks receive EAs. To test this, we employ a similar procedure as above and graph results in Figure B3. NFF returns hover around zero or fall slightly before the interlocked bank EA, fall sharply around the EA, and continue to fluctuate randomly afterwards. Whereas the mean EA takes several days to fully impound in the bank's stock price, its smaller impact on the NFF CAR is realized sooner. As in Figure B2, orders categorized as more severe elicit more negative returns.

To quantify the effects, we focus on abnormal returns cumulated over the (-1, +1) window (CARs) although we use alternative windows for robustness. Table B2 reports results of our NFF event study. Beside CARs, we present the test statistic introduced by Kolari and Pynnönen (2010). It accounts for cross-sectional dependence of abnormal returns, which can inflate unadjusted statistics. Return dependence is likely in our sample as multiple, related EAs can be issued on a single day. For example, on April 13, 2011, the Federal Reserve Board issued orders to ten banking organizations for deficient residential mortgage loan servicing and foreclosure practices.[20] If each bank recipient shares a distinct director with a distinct NFF, these would appear in our sample as ten separate events but information from the ten orders likely overlaps. We also present the z-statistic from the Cowan's (1992) rank test, as a non-parametric alternative which is less susceptible to outliers.

In our sample, the mean NFF linked to an EA recipient bank experiences a statistically significant 17 basis point stock price decline around the EA (Row 1). Similar results obtain if we extend the event window to 11 days centered on enforcement (Row 2) or the two week period (-1,+8) roughly corresponding to the full bank stock price decline in Figure B2 (Row 3). Row 4, which re-estimates our event study after winsorizing returns at the 1% tails, shows that CARs are not driven by extreme outliers. Our tests so far exclude EAs issued within 30 days of a previous

[20] https://www.federalreserve.gov/newsevents/pressreleases/enforcement20110413a.htm

order to the same bank; we modify this restriction in Rows 5 and 6. The former confirms negative returns absent this restriction; the latter presents similar results when extending it to three months. Because Row 5 is diluted by non-news orders, the mean CAR's magnitude shrinks but is still statistically significant.

One concern with any event study is that results reflect a spurious correlation or a negative performance trend that the estimation model could not properly capture. To address this possibility, the next several rows of Table B2 report placebo tests. In the seventh (eighth) row, we move the event date back (forward) one month; in the ninth (tenth) row, we move it back (forward) three months; and in the final row, we randomly assign all event dates in our sample to firms in our sample. The KP test rejects significant returns in every placebo test. The rank test rejects significant returns in every case except Row 8, where returns are marginally negative. Even in this row, the negative returns' average magnitude is less than one third of the baseline. Because placebo tests should suffer the same misspecification problems as the original test, Rows 7 through 11 suggest our results do not capture spurious correlation or negative trend.

These findings present our first empirical contribution: bank-linked directors transmit bank shocks to NFFs. We estimate the effect's economic magnitude as the product of the mean CAR (-0.17%), the number of events (6,848), and the NFFs' mean market capitalization two days before its director-linked bank's EA ($14.3 billion). By this estimate, EAs removed $166.5 billion of market capitalization over our 28 year sample period, *excluding their much larger direct effect on recipient banks*.

Our broader conclusion, however, extends beyond the dollar figure. We employ the EA setting because it provides a clean framework to trace the bank-linked director channel. EAs are far from the most severe shocks banks have faced over the last 30 years. Research often focuses on more substantial ones like housing price bubbles (Gan, 2007), Basel Accord negotiations (Kang and Stulz, 2000), the Russian debt default (Chava and Purnanandam, 2011), bank bailouts (Diamond and Rajan, 2002; Dam and Koetter, 2012), and, of course, the recent financial crisis (Ivashina and Scharfstein, 2010; Afonso, Kovner, and Schoar, 2011; and Berger and Bouwman, 2013). These papers explore the effects of larger bank shocks on the economy through the bank lending channel. Table B2, however, suggests that the overall economic impact could be greater than previously considered because such shocks can also spill over through the bank-linked director channel. Unfortunately, testing most of these is difficult because they affect many banks simultaneously and often coincide with wider economic shocks that impact NFFs independently.

We test our findings' internal validity by regressing NFF CARs on bank CARs. If our results truly capture a shock spillover around the bank event, we should observe more negative NFF shareholder reactions around events that elicit worse bank returns. Director-NFF specific factors, such as how important the director is to a given firm, are likely to moderate this effect. Thus, we include director-NFF fixed effects in our baseline regressions as well as year dummies to capture time effects in abnormal returns. Unlike the sample for Table B2, multiple directors connecting the same bank and NFF appear as multiple observations to retain director-level heterogeneity. Because directors can serve on multiple boards, we cluster standard errors at the director level. Our baseline model is as follows:

$$CAR_{c,d,b,t} = \alpha + \beta * BankCAR_{b,t} + \gamma' Controls + \mu_c \times \mu_d + \lambda_t + \epsilon_{c,d,b,t} \quad (6)$$

where *c*, *d*, *b*, and *t* respectively index NFF, director, bank, and enforcement year. Appendix B defines control variables in detail.

Table B3 reports results. Column 1 shows that the unconditional within-director-NFF correlation is positive, suggesting that more severe bank CARs yield more severe NFF ones.

Column 2 confirms these results after controlling for board and financial characteristics of the NFF; Columns 3 and 4 also include time-varying director and interlocked bank controls, respectively, and conclusions remain unaffected. Our baseline tests investigate the conventional 3-day event window for both bank and NFF CARs. Column 5 replaces 3-day bank and NFF CARs with CARs from the (-1,+8) window because Figure B2 suggests that the average bank CAR is fully realized over a longer window. A significantly positive coefficient continues to obtain. Although bank CARs provide one measure of the EA's severity, extant literature commonly uses another. Among others, Srinivas et al. (2014) focus on three EA types deemed severe, a priori. These categories are described in Section 2. When replacing the bank CAR measure with an indicator equal to one if the EA type is among the three categories, we obtain statistically insignificant results (Column 6). In Column 7, however, we extend the event window to the (-1,+8) period which could more fully capture shareholder sentiment. In this specification, severe EAs do elicit more negative NFF returns, confirming the larger magnitudes seen in Figure B3. Whereas our baseline tests include all EAs in our sample, individually, Columns 8 and 9 explore two aggregation schemes to avoid generating multiple observations from related events. The former averages bank CARs to the director-NFF-year level and the latter averages to the NFF-bank-year level. Results continue to hold. Finally, to demonstrate that the fixed effects do not drive our results, Column 10 shows the same inference and similar magnitude absent fixed effects.

4.3. Director resource reallocation

Next, we investigate whether director resource reallocation can explain negative NFF CARs around interlocked bank EAs. These orders identify governance deficiencies which can require substantial corporate governance resources to remedy (Curry et al., 1999). If individuals serving on multiple boards have limited resources, then additional bank board requirements can shift advising or monitoring away from NFF boards. Thus, negative NFF stock returns could reflect shareholder expectations that valued directors will reallocate time or effort away from NFF boards. Consistent with this framework, Falato, Kadyrzhanova, and Lel (2014) show that when a director serves on two boards, a CEO death at one company decreases the other company's stock price. The effect is stronger when the linking director holds positions of greater responsibility. Hauser (2018) shows the converse: when mergers dissolve entire boards of directors, director resource constraints are lifted, and other companies that these directors serve experience stock price appreciation. Both papers argue that director resource reallocation drives valuation spillovers.

This explanation could only hold in our setting if bank-linked directors provide value to NFFs that cannot be easily replaced. Following the literature on board activities (e.g. Brick and Chidambaran, 2010; Masulis and Mobbs, 2014; Fedaseyeu, Linck and Wagner, 2018), we focus specifically on audit and compensation committee service. The American Bar Association's Corporate Director's Guidebook (2007) states that "*the time required of directors of public companies is significant, particularly for members of the audit committee and the compensation committee*" (p. 1513). BoardEx also provides information on individuals' board functions. We only retain observations after the company begins reporting committee membership to ensure that directors are not mis-classified as serving no committees when, in fact, the firm's committee service data is unavailable. We estimate the probability that a bank-linked director serves on or chairs committees through the following probit regression:

$$\Pr(SERVE_{c,d,t} = 1 | \mathrm{X} = \mathrm{x}) = \Phi(\alpha + \beta_1 * BLD_{c,d,t} + \beta_2 * NLD_{c,d,t} + \gamma_1' Director\ Controls + \gamma_2' Firm\ Controls + \mu_k + \lambda_t) \tag{7}$$

where *c, d*, and *t* respectively index, NFF, the outside director, and year. *SERVE* alternates between several measures of director responsibility detailed in the following paragraph. *BLD* is an indicator equal to one for bank-linked directors. Thus, β_1 measures whether bank-linked directors are more likely to serve in positions of higher responsibility. Because many bank-linked directors serve on several boards, one concern is that β_1 actually captures the effect of multiple board service (often interpreted as a proxy for director reputation) rather than the effect of *bank* board service. To alleviate this concern, we include an indicator, *NLD*, equal to one if the director is linked to another NFF. Thus, our reference category comprises outside directors who only serve on one NFF board in a given year and no bank boards. We adopt director controls from extant literature: sex (Adams and Funk, 2012), independence, tenure and age (Fedaseyeu, Linck, and Wagner, 2018), whether the director is co-opted (Coles, Daniel, and Naveen, 2014), and the firm's importance to the director (Masulis and Mobbs, 2014). The latter is proxied for by size ranking of a given firm within all public firms that director concurrently serves. A rank of one denotes the largest, presumably most important, appointment. To control for board governance, we include several common covariates from the corporate governance literature: CEO duality indicator, CEO tenure, board size, and proportion of outside directors (e.g., Adams and Ferreira, 2008; Ryan and Wiggins, 2004); other firm characteristics and industry fixed effects, μ_k, are adapted from Masulis and Mobbs (2014).

Table B4 reports marginal effect estimates. The dependent variable assumes a value of one if the director serves on any board committee (Column 1), the audit *or* compensation committees (Column 2), the audit *and* compensation committees (Column 3), or chairs any committee (Column 4), and zero, otherwise. A positive β_1 suggests that bank-linked directors are more likely to serve each important role. A chi^2 test reported at the bottom of the table assesses the difference between bank-linked and NFF-linked directors. Differences are significant except for Column 2. These results suggest that *bank*-board service, specifically, not just outside board service, correlates with positions of higher responsibility.

Another relevant dimension is director expertise, specifically financial expertise. Agrawal and Chadha (2005) and others argue that financial expertise impacts a company's governance, a notion enshrined by the Sarbanes-Oxley Act. We collect directors' financial expertise status (available from 2002 onwards) from BoardEx and use it as dependent variable in Column 5. Results show that bank-linked directors are also more likely to be designated financial experts than individuals with exactly one outside appointment and than individuals linked to other NFFs. Intuitively, exposure to the banking industry enhances one's financial expertise. In sum, negative NFF stock returns around interlocked bank EAs appear plausible because these directors serve important, presumably value relevant, board positions.

We proceed to test whether EAs displace these directors' resources. Because director resource expenditure cannot be precisely measured, we rely on an implicit measure: director compensation. The underlying idea is that cash pay for outside directors varies little within firm-year, other than for resource expenditure reasons (Farrell, Friesen, and Hersch, 2008). In a given year, a firm will compensate outside directors at the same rate but in proportion to how many meetings they attend, which committees they serve on, and which they chair. Meeting attendance has been interpreted as one measure of director output and commitment to the firm (e.g. Adams and Ferreira, 2008; Cai, Garner, and Walkling, 2009; and Masulis, Wang, and Xie, 2012). Thus, a director's annual cash compensation, controlling for committee membership and chairmanship, captures her meeting attendance and thus, her resource expenditure.[21] As other measures of

[21] For this test, a direct measure of meeting attendance would be ideal but is not available. Masulis and Mobbs (2014) and others employ an indicator from the IRR/Risk Metrics dataset equal to one if a director attends fewer than 75%

participation, we consider total compensation (cash, bonuses, equity, and retirement/pension contributions) as well as the Board Function Index (BFI) introduced by Fedaseyeu, Linck, and Wagner (2018). BFI adds all board positions the director holds, as member or chair, to the chairman/chairwomen position he/she potentially holds. To test EAs' impact on director resource expenditure, we estimate the following regression:

$$Y_{c,d,t} = \alpha + \beta_1 * EBLD_{c,d,t} + \beta_2 * NEBLD_{c,d,t} + \beta_3 * NLD_{c,d,t}$$
$$+\gamma' Director\ Controls + \mu_c \times \lambda_t + \epsilon_{c,d,t} \quad (8)$$

where *c*, *d*, and *t* index the NFF, director, and fiscal year, respectively. The dependent variable, *Y*, is either log cash compensation, log total compensation, or BFI. *EBLD* (*NEBLD*) is an indicator equal to one if the director is linked to a bank and her bank receives (does not receive) an EA that fiscal year, and zero otherwise. When the EA is issued in the last five months of the NFF's fiscal year, *EBLD* flags one for the EA year and also for the following fiscal year. This allows for a sufficient window to measure changes in resource expenditure. We also include the indicator, *NLD*, equal to one if the director is linked to another NFF. Thus, our reference category comprises outside directors who serve on only one NFF board in a given year. Firm-level controls are subsumed by the firm-year fixed effects, $\mu_c \times \lambda_t$.

In these tests, our sample begins in 2006, the year in which director-specific compensation data become available. We exclude appointment years because compensation and committee participation of newly appointed directors might be lower due to mid-year appointments. For the same reason, we exclude director-years in which the director does not serve the full fiscal year. We follow Adams and Ferreira (2008) in dropping observations in which the director receives no

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of meetings in a given year. In unreported analysis, we find this indicator varies too little within our sample for meaningful tests. For example, in the post-SOX period, less than 1% of directors failed to attend 75% of the meetings.

cash compensation. Finally, we exclude firm-years with fewer than three outside directors to ensure sufficient within-firm-year variation to estimate our coefficients of interest.

Table B5 presents results. Columns 1, 2, and 3 test whether EAs affect outside directors' cash compensation. Column 1 shows that, on average, non-enforced bank-linked directors are paid more than their coworkers with exactly one outside board seat; enforced bank-linked directors are not. We compare β_1 and β_2 through an F-test and report results at the bottom of the table. According to the highly significant F-statistic, non-enforced bank-linked directors are paid more than enforced ones on the same board in the same year, implying that they participate more. In Column 2, we add control variables and similar results obtain.[22] Moreover, the pay gap is robust to excluding board chairpersons (Column 3), a position that comes with significantly more responsibility and, thus, more pay. Column 4 shows that EBLDs are not substituting cash for non-cash pay because total compensation for these directors is still lower than NEBLD compensation. Column 5 suggests that EBLDs serve on fewer committees than NEBLDs but this result dissipates after controlling for director characteristics and meeting attendance via compensation (Column 6). Overall, lower meeting attendance appears a more plausible manifestation of EA-induced resource reallocation than reduced committee membership.

We also test the resource reallocation channel by measuring outside director resource expenditure on *bank* boards during enforcement years. If EAs consume director resources and the NFF stock devaluation reflects shareholder anticipation of director resource outflow, we should observe higher resource expenditure on bank boards during EA years. Thus, we estimate the following regression:

[22] In an unreported test, we exclude 86 director-NFF-year observations representing EAs in the 'severe' categories defined by Srinivas et al. (2014). Results resembling Column 2 indicate that these EAs do not drive our conclusions.

$$Y_{b,d,t} = \alpha + \beta * EA_{b,d,t} + \gamma_1' Director\ Controls + \gamma_2' Bank\ Controls$$

$$+\mu_b \times \mu_d + \lambda_t + \epsilon_{b,d,t} \quad (9)$$

where *b*, *d*, and *t* index the bank, director, and fiscal year, respectively. EA is an indicator equal to one if the bank receives an EA in a given fiscal year, and zero otherwise. To retain director-level heterogeneity, we estimate this regression at the bank-director-year level. Because enforcement occurs at the bank-year level, we do not include firm-year fixed effects as before. Instead, we employ director-bank fixed effects and include year dummies to capture common pay trends over time. This specification absorbs persistent director characteristics considered in Table B5 such as sex and persistent bank characteristics such as size. We consider age but not tenure as the two are perfectly collinear within director-bank fixed effects. We include the same director controls as in Equation (8), board controls as in Equation (7) and Tier 1 capital and non-performing assets as bank-specific financial controls. In this test, we favor salary over committee appointments as our measure of resource expenditure because it is implausible that *all* directors would increase committee membership during enforcement years and chairmanship of each committee is mechanically limited to one or two directors per year.

Table B6 presents results. Column 1 shows that in a univariate setting, bank directors make about 10% more during years in which their banks become enforced. Multivariate estimates in Column 2 are qualitatively similar although the magnitude increases substantially. Column 3 presents the intuitive result that severe EAs command more director resource commitment than non-severe ones. Both severe and non-severe orders, however, associate with more director pay from the bank during the EA year. Finally, Column 4 shows using total compensation as the dependent variable presents an identical conclusion. Existing directors could scarcely justify higher salaries immediately after regulators reveal a corporate governance deficiency. Therefore, the fact that banks pay their outside directors more during enforcement years supports the view that EAs consume director resources.

Overall, this section suggests that resource reallocation can explain EAs' spillover to director-linked NFF stock prices. During years in which their banks are enforced, bank-linked directors attend fewer NFF board meetings and more bank board ones. Because these directors are disproportionately more likely to hold important NFF appointments, a negative impact on NFF shareholder value is plausible.

5. Alternative explanations

In this section, we prod our results' validity by exploring three alternative explanations. We test whether the shock spillover identified above can be attributed to EA-induced credit frictions, signals about director quality, or a director selection mechanism whereby poorly performing banks and NFFs appoint the same directors. At best, these hypotheses could explain our shock spillover results in Section 4.2 or the resource reallocation results in Section 4.3; any one story has a difficult time accounting for both. For example, why should a damaged credit relationship between enforced bank and director-linked NFF correlate with that director attending fewer NFF board meetings and more bank board meetings? Although we cannot rule out these alternative explanations entirely, we conclude that resource reallocation likely accounts for the largest portion of the shock spillover.

5.1. EA-induced credit frictions between bank and NFF

Substantial research explores lending as a channel of financial sector shock transmission. Seminal papers, including Bernanke and Blinder (1988) and Bernanke and Gertler (1995), argue that bank shocks affect credit availability which, in turn, affects companies reliant on bank credit. Amiti and Weinstein (2018) empirically trace this transmission channel, and many others find support (Jayaratne and Strahan, 1996; Ashcraft, 2005; Garmaise and Moskowitz, 2006; Chava and Purnanandam, 2011; Puri, Rocholl, and Steffen, 2011; Iyer, Peydró, da-Rocha-Lopes, and Schoar, 2014). Consequently, our next step is to test whether the lending channel can explain our results. NFFs can hire banker-directors or send their executives to serve on bank boards specifically to obtain credit at favorable terms. If the EA shocks a bank's ability to lend, negative returns for the director-linked NFF could reflect tighter credit constraints, not director resource reallocation. Indeed, Roman (2017) finds significantly negative abnormal returns for corporate borrowers around their lenders' EAs. On the other hand, her paper and Deli et al. (2017) both show that several borrower welfare measures actually improve after EAs so the overall impact is ambiguous.[23]

A priori, there are four reasons to doubt that the EA-constrained credit drives our results. First, only a very small portion of NFFs in our sample have lending relationships with their director-linked banks. We identify lending relationships from SEC filings and find that only 6.3% of EAs in our sample are issued to banks that simultaneously lend to and share a director with the same NFF. Second, only Cease and Desist Orders, Formal Agreements, and Prompt Corrective Actions Directives can directly affect lending because regulators can only require operational changes through these orders. Collectively, 105 out of 1,245 EAs in our sample fall into these categories. We read through the 98 of these with detailed documents provided by SNL Financial and find that only 39 even mention lending. The remainder highlight procedural issues, mostly related to compliance with the bank-secrecy act or anti-money laundering regulation. In the

[23] These papers study how bank EAs impact corporate borrowers, but their samples differ appreciably from ours along two dimensions. First, both papers are constrained by lending relationships provided by DealScan, a database that reports syndicated loan activity for large public corporations, whereas our lending relationships are hand-collected from SEC filings. Because DealScan includes only syndicated loans, our methodology includes a wider set of relationships. Conversely, our sample is restricted to banks that share a common director with their borrowers, yielding a narrower set. Therefore, conclusions about how EAs impact borrowers can differ between our paper and theirs.

subsample that does mention lending, restrictions are somewhat mild and vague. For example, Sterling Financial Corporation's October 9, 2009 Cease and Desist Order requires that "*the bank… cease and desist from… operating with a large volume of poor quality loans.*" Others, similarly, restrict only risky or poor quality lending. It is less likely the bank's poor quality loans are issued to the large, public NFFs in our sample. Third, many researchers including Booth and Deli (1999), Kroszner and Strahan (2001), and Byrd and Mizruchi (2005) point out that when a banker serves on a borrowing firm's board, lending still occurs at arms' length. Therefore, even if EAs were to damage the credit relationship, the firm should still be able to obtain credit on comparable terms from the open market. Finally, Table B1, Panel B shows that less than 20% of directors in our sample are bank insiders. Independent bank directors who also serve on NFF boards have even less ability to secure bank credit for the NFF at favorable terms.

Nevertheless, we directly dampen the lending channel as an explanation for our spillover findings. Since August 2004, publicly traded corporations are required to file Form 8-K, Section 2.03, with the SEC to announce the "*creation of a direct financial obligation or an obligation under an off-balance sheet arrangement.*" We hand collect lending data from the EDGAR database to determine whether the lending channel interacts with our sample in a significant way.[24] Because we need several years of regulatory filings to check for the presence of a lending relationship, we exclude EAs issued before 2009. This allows us to check at least four years of regulatory filings. For each link in our sample, we search through the NFF's 8-K filings from August, 2004, the month when firms began reporting Section 2.03, through the year each NFF's final bank-linked EA was issued. For each loan, we record the lender's identity. For syndicated loans, those with

[24] Another way to observe lending relationships is through the DealScan syndicated loan database. Smaller firms, however, obtain credit directly from a bank, bypassing the syndicated loan market. To avoid the selection issue that would bias our results toward reflecting larger firms, we collect loan information directly from SEC filings, DealScan's source data.

multiple lenders, we follow the protocol in Ivashina (2009) to identify the lead arranger, who maintains the lending relationship. Although other syndicate members also supply credit, it is more difficult to argue that they have a true ‘lending relationship’ with the NFF. To be conservative, we assume that the NFF has a lending relationship with the identified bank if it borrowed from said bank at any point before the EA, not only if it has a loan outstanding when the EA is issued. With this information, we replicate our Section 4.2 NFF event studies, focusing on the lending channel.

Table B7 reports results. Because we only track lending relationships for NFFs linked to EAs issued after 2008, we first replicate our baseline event study results in the post-2008 period. Row 1 shows that the mean CAR mirrors its full sample period value in Table B2, Row 1. Row 2 isolates events in which the bank also lends to its director-linked NFF. The coefficient is nearly twice as large but marginally insignificant (unreported p-value of 0.1232). Statistical insignificance could reflect a dramatically smaller sample size. That returns are more negative, on average, when the bank and NFF are connected through both channels is consistent with Roman’s (2017) findings. In Row 3, we exclude these lender-linked events. Returns are still significantly negative, though slightly smaller in magnitude. These findings suggest that the lending channel is unable to fully explain our results.

We also read through each Cease and Desist Order, Formal Agreement, and Prompt Corrective Action Directive in our sample to determine which orders mention lending. In Row 4, we focus only on EAs that explicitly mention lending restrictions.[25] The mean CAR for this subsample is similar to the full sample but statistically insignificant, which could again reflect the

[25] The subsample of lending-restricting EAs includes two extreme outliers with positive returns of 130% and 38%. Given the small sample size in this test, including these outliers yields a mean CAR of positive 1.4%. Table 7 results exclude these observations.

small number of observations. When discarding lending-restricting EAs in Row 5, the mean CAR changes little from the full sample value.

As a final test, we exploit director heterogeneity. The bankers-on-board literature focuses on banker-directors as the ones most able to secure credit for the NFF. Thus, the lending channel should be more pronounced in this subset. Rows 6 and 7 respectively isolate events that are and are not linked by banker-directors, to demonstrate that the results vary little between these two subgroups. Overall, Table B7 shows that spillover effects hold even after dropping observations in which the lending channel can plausibly operate.

5.2. EA-induced shocks to director reputation

Bank EAs can also provide new, value-relevant information to director-linked NFF shareholders about the common director's quality. If the director failed to keep her bank out of trouble, she may not be as capable as NFF shareholders previously thought. In that case, NFF stock devaluation could reflect news about the director, not only expected resource reallocation.

We test whether EAs reveal news about director quality by studying future outside directorships. If EAs cause shareholders to reappraise the linking director's quality downward and shareholders are generally correct in their assessment, bank-linked directors should hold fewer board seats after enforcement. We check whether serving on an enforced bank in year t reduces the director's outside board seats in year t+2. Our sample is obtained by culling the BoardEx data to one observation per director-year. For directors who serve on multiple boards in one year, we retain data from the company with the highest market capitalization. We consider all directors who serve on NFF boards. Because an individual's number of board appointments is highly persistent, we follow Wintoki, Linck, and Netter (2012) and sample every third year of data.[26] We adopt

[26] The correlation between our dependent variable and its 1-year (2-year) lag is 0.92 (0.72).

Harford and Schonlau's (2013) ordered logit specification in which the dependent variable is the 2-year lead number of outside board seats:

$$SEAT_{d,t+2} = \alpha + \beta_1 * EBLD_{d,t} + \beta_2 * NEBLD_{d,t} + \beta_3 * NLD_{d,t} + \gamma' Controls + \mu_k + \lambda_t + \epsilon_{d,t+2} \quad (10)$$

Variables are defined as in Table B5 and standard errors are clustered by director.

Table B8 reports coefficients from estimating Equation (10). In Columns 1 and 2, coefficients on both enforced and non-enforced bank-linked director dummies show that bank board service corresponds with more future board seats. These results reinforce Table B4 findings that bank-linked directors are valued. Difference tests at the bottom of the table show that enforcement does not affect future board appointments.

In Column 3, we estimate a probit model to determine whether enforced bank-linked directors are more likely to be dismissed. If so, we cannot observe board seats in t+2 and might mistakenly conclude that enforcement has no effect. Our dependent variable is an indicator equal to one if the director remains in our dataset for the three next years and zero, otherwise. The difference test at the bottom of the table confirms that enforced bank-linked directors are no more likely to be dismissed than their non-enforced counterparts. This table suggests that rational markets are unlikely to interpret EAs as negative signal about director quality.

5.3. Director selection to enforced bank and NFF

Another concern stems from non-random assignment of NFF-linked directors to enforced and non-enforced banks. It could be the case that poorly governed, worse performing banks appoint worse directors. Poorly governed, worse performing NFFs might appoint the same directors. If so, firms that employ enforced bank-linked directors would exhibit negative returns; however, the empirical design of our NFF event studies precludes this explanation. Abnormal returns represent deviations from *within-firm* expected returns, whereas the selection bias outlined above rests on a cross-sectional comparison. Thus, it could not explain why *excess* returns computed from within-firm regressions should be negative precisely around the event date. Persistent negative performance is captured in our estimation model's intercept. Moreover, if NFFs that select enforced bank-linked directors perform worse, in general, this performance differential should but does not persist into the placebo tests reported in Table B2, Rows 7 through 11.

In contrast, our tests of resource reallocation are more susceptible to selection concerns such as those raised by Hermalin and Weisbach (1988). Güner, Malmendier, and Tate (2008) discuss in detail the endogeneity of board composition in the case of banker-directors. These directors often choose to serve on large, stable NFFs with many collateralizeable assets (Kroszner and Strahan, 2001). This induces a correlation between banker-directors and better NFFs; meanwhile, other bank-linked directors serve on worse performing NFF's boards. More successful firms are likely to pay their directors more, especially to banker-directors. If this is the case, banker-directors may cause the pay differential between enforced and non-enforced bank-linked directors as they are underrepresented among worse firms and overrepresented among better ones. Table B1, Panel B confirms this is the case as banker-directors compose 15% of director-years linked to enforced banks and 21% of director-years linked to all banks. Masulis and Mobbs (2011), excluding banks from their sample, argue that NFF insiders' independent director appointments are less likely to suffer from endogeneity. We follow their intuition by dropping banker-directors and replicating Tables B4 and B5. Unreported tests confirm that our results hold which reduces the likelihood that endogenous director selection explains our results.

6. Summary and discussion

We investigate the effects of bank EAs on director-linked NFF stock prices. NFFs experience significantly negative returns around 1,245 orders issued between 1990 and 2017 to interlocked banks. Those that destroy more *bank* shareholder value destroy more *NFF* value, as well. EA induced reallocation of bank-linked director resources is most likely to explain these findings for three reasons. First, these directors disproportionately serve important and hard-to-substitute NFF roles like audit committee chair or financial expert. Second, they expend fewer resources on NFF boards during years in which their bank is enforced. Building of off Farrell, Friesen, and Hersch (2008), we measure resource expenditure through within-firm-year regressions of outside director compensation. Third, they expend more resources on bank boards in enforcement years. Our results are unlikely reflect EA-induced credit constraints between bank and interlocked NFF, EA-induced shocks to linking director reputation, and simultaneous selection of worse directors to poorly performing NFFs and enforced banks.

This study advances the banking literature by highlighting a previously unexplored channel through which financial sector shocks can affect non-banks. Extant research focuses on lending as the mechanism through which bank shocks propagate into the real economy. We show, however, that director interlocks also facilitate spillover by redistributing monitoring and advising resources away from NFFs in response to bank shocks. We focus on EAs as a straightforward, clean laboratory to explore the bank-link director channel of financial sector shock transmission although they are moderate relative to other bank events. For example, asset price crashes or regulatory overhauls should have far stronger effects on interlocked NFFs. Given that banking sector's volatility and heavy regulatory framework, such shocks are not uncommon. One direct implication of our study is that stronger bank governance requirements, like those accompanying

Dodd-Frank and Basel III, may divert director resources away from the non-financial sector. Thus, we posit a previously unexplored cost to financial sector regulation.

Additionally, our work helps clarify a puzzling result in corporate governance research. Several recent studies imply that a single director's attention can have significant enough implications to affect overall firm value. Our work supports this view but offers an important caveat about sample selection. In prior studies and ours, the directors whose attention matters are those with multiple board appointments. We show that these individuals disproportionately serve prominent board positions. Consequently, their expertise cannot be easily replaced, which helps justify value implications. To our knowledge, ours is the first paper to actually measure director resource expenditure. We develop intuition from prior literature into a straightforward econometric specification. The result is a measure of director resource expenditure with many useful applications in future work. How do corporate events like mergers or CEO changes affect director workload? Do reputational penalties follow director shirking? What explains the cross-section of director resource expenditure? These questions we leave for future research.

Chapter 3: The Risk-Shifting Value of Payout: Evidence from Bank Enforcement Actions

1. Introduction

More than half a century after Miller and Modigliani's (1961) Dividend Irrelevance Proposition, finance researchers still debate whether and why investors value dividends. Empirical evidence supports a value to payout: Fama and French (1998), Pinkowitz, Stulz, and Williamson (2006), and Kim, Park, and Suh (2016) document a robust, generally positive, relationship between the cross-section of firm value and payout level. Other studies beginning with Pettit (1972) find a positive time series relationship between unexpected payout changes and market value changes.

One issue with this literature is that observed payout levels reflect endogenous managerial decisions. Unobservable factors like expected future profitability simultaneously impact payout decisions and investor valuation. Baker and Wurgler (2004) and Li and Lie (2006) suggest that causality goes both ways: not only do investors price stocks to reflect payout expectations but managers adjust payout in response to market demand. Turning to within-firm payout *changes* instead of cross-sectional *levels* introduces different identification concerns. Dividend changes may come at times when market values are changing for other reasons. Expected return models like the Market Model can, at best, imperfectly mitigate this concern (Campbell, Lo, and MacKinlay, 1997). In short, endogeneity undermines a causal interpretation of the positive link between firm value and dividends. The question can only be addressed using shocks to payout policy that are otherwise exogenous to market valuation; such shocks are rarely observed.

In the present paper, I argue that enforcement actions (EAs) in the banking industry, a phenomenon gaining recent academic attention, present exactly the shocks needed to understand payout's causal impact on firm value. U.S. regulators issue EAs in response to 'unsafe or unsound' bank operations. These orders restrict activities, sometimes including payout. I collect data on 341 EAs issued to publicly held bank holding companies between 1991 and 2015 and measure abnormal stock returns around the subset that include a payout-restricting clause (POREAs). Although POREA receipt is nonrandom, it appears orthogonal to whether recipients actually pay dividends or repurchase shares; 26% of POREA recipients distribute no cash in the three years before enforcement. This heterogeneity facilitates difference-in-differences estimation. POREA recipient banks that do not pay dividends or repurchase shares, *those for which the payout constraint does not bind*, serve as counterfactuals for recipients that do pay out, *those for which the constraint does bind*. This framework holds constant the EA's possible signal effect, unrelated to but contemporaneous with the payout restriction. Because such a signal should not vary between payout and non-payout firms in expectation, any difference between the two groups' stock returns should reflect value lost from reduced access to payout. Consistent with a value to payout, banks that distribute cash to shareholders before enforcement experience a 4.5 percentage point greater decline in market value over the three days surrounding POREA receipt.

Though my findings corroborate decades of associative evidence, they are an important step toward identification. Previous papers relate firm value to a single dimension of payout variation – firm *or* time. The present paper exploits both through difference-in-differences estimation. One concern is that payout and non-payout firms differ along other dimensions that disparately affect stock market reactions. I show that in my sample, the two groups are statistically indistinguishable on risk and performance characteristics such as profitability, capitalization, and asset quality. Moreover, both sets are selected into the same, very distinct sample of U.S. publicly held bank holding companies whose unsafe or unsound operations threaten banking sector capital enough to warrant regulatory intervention. Most importantly, abnormal stock returns preceding enforcement appear nearly identical for both sets. These similarities suggest the parallel trend assumption, crucial for difference-in-differences estimation, likely holds. Although payout firms are typically larger and their stock trades more frequently, results only strengthen when controlling for these attributes in multivariate analysis. Withstanding robustness and placebo tests, results maintain that restricting access to payout destroys value.

Next, I explore why investors in this sample value payout. Prior research attributes the prevalence of dividends to their role in reducing agency costs of equity (Farre-Mensa, Michaely, and Schmalz, 2014). My results, however, invoke a less common theory of dividend relevance. As noted by Black (1976) and explored at length by Smith and Warner (1979), firms can use payout to shift risk from equity-holders to creditors by moving cash, the safest asset, out of the firm and into shareholders' pockets. The value of risk-shifting rises in financial distress (Eisdorfer, 2008) of which POREAs are a common symptom.

I test the risk-shifting theory using two key variables. The first is distance to default, which I measure as a bank's z-score.[27] As a firm approaches default, its z-score becomes smaller and the option to shift risk via cash distribution becomes more valuable. Thus, POREAs restricting that option should yield more negative returns for dividend payers or share repurchasers with lower z-scores. This intuition predicts a positive relationship between z-score and abnormal return. All firms, however, whether they pay out or not, are subject to a second, countervailing force. Shareholders should be less surprised by POREAs if their firm is closer to default. Thus, investors of more troubled firms, those with lower z-scores, should react less negatively to regulatory

[27] Not to be confused with Altman's Z, the z-score was formalized by Boyd and Graham (1986) and has been used extensively since, including by Laeven and Levine (2009) and Delis, Hasan, and Tsionas (2014). Conceptually, it estimates the number of standard deviations of return on assets a bank is from its default threshold.

intervention. This intuition predicts a negative relationship between z-score and abnormal return. The cross-section of abnormal returns confirms both effects: returns relate negatively to z-scores for banks that do not pay out; for banks that do, there exists an offsetting, positive relationship.

The second variable is inside stock ownership. Mirroring the intuition above, a high fraction of inside ownership implies less of an information shock from regulatory intervention because more shareholders can anticipate it. Accordingly, inside ownership should positively predict abnormal POREA returns. For firms that pay out, however, there exists an incremental impact, just as before. The direction of this second effect depends on whether investors value payout more as a means to reduce agency costs or to shift risk. Firms with high inside ownership face fewer agency problems because insiders are forced to internalize the costs of their private benefits (Jensen and Meckling, 1976).[28] Thus, payout is *less valuable in reducing agency costs of equity* for highly insider-owned firms. On the other hand, the more of the firm insiders own, the more incentive and ability management has to shift risk (John and John, 1993; John, Saunders, and Senbet, 2000). Thus, payout is *more valuable in shifting risk* for highly insider-owned firms. If shareholders value payout primarily as a means to reduce agency costs, inside ownership will relate positively to abnormal POREA returns; if risk-shifting is more important, the relationship will be negative. In this sense, inside ownership can simultaneously evaluate both theories of payout relevance. As with the tests of z-score, I find evidence of the anticipation effect: for firms that do

[28] Morck, Schleifer and Vishny (1988) and McConnell and Servaes (1990) find a u-shaped relationship between inside stock ownership and firm value, concluding that agency costs initially fall then rise with inside ownership. Himmelberg, Hubbard, and Palia (1999) build upon Demsetz and Lehn's (1985) work, however, to show the u-shaped relationship reflects model misspecification. Their corrections support a negative, monotonic relationship. More recently, Coles, Lemmon and Meschke (2012) design a structural model to obtain the u-shape absent agency costs. Their results derive from endogenous codetermination of firm value, scale, performance, and managerial ownership. Empirical work including Rozeff (1982), Jensen, Solberg, and Zorn (1992), Ang, Cole, and Lin (2000), Singh and Davidson (2003) and Fahlenbrach and Stulz (2009) also supports a monotonic relationship in line with Jensen and Meckling's (1976) initial argument.

not pay out, inside ownership positively predicts abnormal returns. For firms that do pay out, there exists an incremental negative relationship that more than offsets the positive one. This finding implies that investors of payout banks in my sample rely on payout more as a risk-shifting vehicle than as a means to reduce agency costs.

Previous research on risk-shifting through dividends has been inconclusive. One reason is likely the bond-market event study methodology employed. Bessembinder et al. (2009) and Ederington, Guan, and Yang (2015) show that illiquidity and heteroskedasticity plague bond market event studies. By examining relatively liquid stock markets, the present paper tests provides a novel, potentially more informative test of risk-shifting through payout. My results support recent work by Acharya, Le and Shin (2017) who show ten large U.S. banks used dividends to shift risk during the recent financial crisis. I extend their findings to midsized banks using a panel over 20 times larger and nearly 10 times longer. More importantly, I document that investors *value* the ability to shift risk through payout in times of financial distress.

Finally, I contribute to a small yet burgeoning literature on bank EAs. From Brous and Legget (1996) to Roman (2017), authors consistently find negative abnormal returns around EA issuance. My study qualifies these results: EAs that do not restrict payout yield no negative returns. These findings suggest the payout restriction, not regulatory intervention, is value-relevant.

2. Related literature

This section briefly introduces the extensive theoretical and empirical literatures on payout, emphasizing bank payout, and the much younger literature on enforcement actions.

2.1. Theoretical predictions

Miller and Modigliani (1961) propose dividend irrelevance under three conditions: perfect information, perfect capital markets, and rational investors. As a result, theories of dividend relevance typically relax these conditions.

Theories departing from perfect information are among the oldest. If managers know more than outside shareholders, payout can credibly signal expected cash flows (Bhattacharya, 1979), investment prospects (Miller and Rock, 1985), or, broadly, firm quality (Allen, Bernardo and Welsh, 2000). These papers focus on signals from firm-initiated dividend policy changes. The payout policy changes I examine are regulator-initiated. Whether POREAs offer a stronger or weaker signal than firm-initiated payout reductions is not clear, a priori. POREA signals could be stronger if regulatory intervention reveals the recipient's operations are dire enough to threaten banking sector capital. On the other hand, Pettway (1980), Flannery (1998), and Berger, Davies, and Flannery (2000) question regulators' timeliness in identifying problems. They suggest markets discover and discipline problem banks before regulators do. If so, news from a regulatory intervention may already be impounded in stock prices before POREA issuance.

Another market imperfection is incomplete contracting. If management effort is unobservable, Jensen and Meckling (1976) argue that executives act to maximize their own, not shareholders', wealth. Such actions present agency costs between inside and outside shareholders. Excess free cash flow can exacerbate these costs by affording managers higher spending flexibility. Jensen (1986) argues that corporate payout alleviates agency problems by reducing free cash flow. Easterbrook (1984) suggests that by committing to a steady dividend stream, managers agree to continuously access capital markets for funding, which lowers agency costs through capital market discipline. In sum, payout can add value by reducing agency costs between managers and outside shareholders.

A different agency cost that Jenson and Meckling (1976) discuss is risk-shifting or asset substitution between equity and debt holders. Managers, acting to maximize shareholder wealth, can issue fairly priced debt then devalue it by assuming more risk. The upside potential of risk accrues to equity-holders and the downside potential beyond the default threshold, to debt-holders. Thus, risk taking exploits debtholders to increase shareholder value. Shareholders can also shift risk by pocketing their firms' safest asset – cash. Black (1976) cynically posits "there is no easier way for a company to escape the burden of debt than to pay out all of its assets in the form of a dividend, and leave the creditors holding an empty shell." Smith and Warner (1979) examine this possibility more rigorously. In the banking sector, priority claimants include depositors. Although deposit insurance protects small depositors, large ones still face the risk of ruin. Moreover, deposit insurance has long been understood to exacerbate risk-shifting incentives through moral hazard. In the John, John, and Senbet (1991) model, risk shifts toward the deposit insurer posing an externality to the banking industry. In Acharya, He, and Shin (2017), dividends shift shareholder risk toward the banking industry because banks are codependent. In cases of bank and deposit insurance fund bailouts, a staple of the last two U.S. banking crises, shareholder risk ultimately lands in taxpayer pocketbooks.

Other theories abstract from perfect rationality to explain payout. Shefrin and Statman (1984) argue investors may be psychologically compelled to prefer dividends. Citing this argument, Baker and Wurgler (2004) show that firms initiate or omit dividends based on time-variant investor demand. Li and Lie (2006) extend the model and show firms increase or decrease dividends to cater to investor preferences. These authors raise important reverse causality concerns that challenge research on market reactions to firm-initiated payout policy adjustments. Do market values increase because managers raise payout or do managers raise payout because doing so can boost market values? Unlike nearly all other papers in this literature which study *firm-initiated* payout changes, mine avoids such concerns by studying externally imposed payout changes; only a radical cynic could believe regulators restrict payout specifically to reduce banks' stock prices.

2.2. Empirical evidence

At least as early as Elton and Gruber (1970), empiricists have tried justifying payout through dividend and capital gains tax rate differentials. The United States taxes capital gains at a lower rate than dividends. Lewellen, Stanley, Lease and Schlarbaum (1978) study individual investors' portfolios and reveal tax rates to be a negligible determinant of individual portfolio dividend yields. Allen and Michaely (2003) reaffirm this result for subsequent years. Could institutional investors, many of whom do not pay taxes, compose the dividend seeking clientele? Brav and Heaton (1988) suggest "prudent man" regulations in the 1974 ERISA implementation could push institutional investors toward high payout stocks, even for non-tax reasons. However, Michaely, Thaler, and Womack (1995) and Hoberg and Prabhala (2008) discredit the idea that institutional investors seek dividends by showing that dividend omissions, if anything, increase institutional ownership. Further, Brav, Graham, Harvey, and Michaely's (2005) survey of corporate managers reveals that investor tax considerations are, at best, a second-order payout policy determinant. In sum, DeAngelo, DeAngelo, and Skinner (2009) write: "little or no empirical basis exists for viewing [personal taxes, transaction costs, or the "prudent man" regulation] as a major determinant of firms' payout decisions."

A convincing group of studies investigates payout preference using dual class stocks: one class pays cash dividends, the other, equal value stock dividends. By studying the same firm, this setting attempts to hold all but the distribution method constant. Long (1978) and Ang, Blackwell, and Megginson (1991) find a premium on the cash dividend paying shares *even when there is no tax advantage to capital gains*. However, Poterba (1986) and Bailey (1988) suggest that transaction costs of liquidating the stock dividend could explain value differences. All four papers compare one dividend paying stock to a near-ideal counterfactual yet conclusions are mixed, highlighting how complicated inference may be even in the cleanest of settings. To follow these papers' focus on clean identification, I utilize a difference-in-differences framework but lever evidence from over 200 firms to add statistical and economic credibility.

Signaling theories of dividends enjoy limited empirical support. Many authors document price movement in the same direction as dividend policy adjustments (Pettit, 1972; Aharony and Swary, 1980; Asquith and Mullins, 1983; Healy and Palepu, 1988; and Michaely, Thaler, and Womack, 1995). Bessler and Nohel (1996) and Forti and Schiozer (2015) extend these findings specifically to the banking sector. Researchers almost ubiquitously interpret this relationship as evidence of managerial signaling. Yet, such a reading masks other potential benefits of payout. Woolridge (1983) points out that dividend signaling models are consistent with wealth/risk transfer models, although his empirics reaffirm the former interpretation. The present paper makes a rare attempt to disentangle the signal effect from potential value to payout, per se.

Besides short term event studies around unexpected payout policy adjustments, empirical tests of signaling offer conflicting conclusions. For example, Charest (1978) finds a positive (negative) abnormal return over the two years following a dividend increase (decrease). However, DeAngelo, DeAngelo and Skinner (1996) find no evidence that unexpected dividend changes predict future earnings. Grullon, Michaely, and Swaminathan (2002) show that return on assets actually falls (rises) after large dividend increases (decreases). Li and Zhao (2008) find firms with the most incentive to signal through dividends do so less often. The present paper informs this debate by revisiting the idea that market reactions to payout policy changes reflect other valued aspects of payout – not just a performance signal.

Agency cost theories of payout have enjoyed consistent empirical support. Pinkowitz, Stulz and Williamson (2006) show that in countries with weak shareholder protection, investors place a premium on dividends and discount firm cash holdings. Low investor protection allows managers to exploit outside shareholders. Thus, outside investors value dividends as a means of reducing agency cost. At the firm level, Rozeff (1982) and Jensen, Solberg, and Zorn (1992) find a negative relationship between dividend payout and inside ownership. Higher inside ownership implies lower incentive misalignment between manager-owners and outside shareholders (Jensen and Meckling, 1976). These authors argue that lower incentive misalignment reduces investor demand for dividends. Ang, Cole, and Lin (2000) and Singh and Davidson (2003) support this empirically. My study acknowledges and builds on this framework.

Tests of payout as an agency cost between stock and bondholders are more limited and again offer conflicting results. All have been bond market event studies around unexpected dividend announcements. Woolridge (1983) and Handjinicolaou and Kalay (1984) do not find evidence that payout increases agency costs of debt, although Dhillon and Johnson (1994) and Maxwell and Stephens (2003) do. Tsai and Wu (2015) use the more comprehensive TRACE database to reexamine the question and fail to identify risk-shifting. However, these studies suffer at least two concerns. Each tests for agency costs of debt by studying bond market reactions around unexpected payout policy changes. As Bessembinder et al. (2009) report, the average bond does not trade on the average day. Ederington, Guan and Yang (2015) find that significant heteroskedasticity in bond trading renders conventional significance tests misspecified. Both issues reduce bond-market event study power. Second, the above empirical tests do not consider dividend restricting covenants, common in bonds. Such clauses limit stockholders' ability to shift risk. Thus, any voluntary dividend adjustment is likely to satisfy dividend restricting bond covenants which further biases against observing negative bond returns. My paper avoids issues with bond market event studies by focusing only on stock returns. It closely relates to Kanas (2013) and Acharya, He, and Lin (2017) who find evidence of bank risk-shifting through dividends. One major difference is that these papers conclude by documenting evidence for bank risk-shifting whereas I offer an important, incremental observation: *investors value a bank's risk-shifting through payout*, especially around times of distress.

As for my setting, academic research on bank enforcement actions is limited but has blossomed rapidly in recent years. Delis, Staikouras, and Tsoumas (2017) evaluate EA's efficacy and conclude that the orders are generally effective in reducing nonperforming assets but ineffective in boosting capital. Danisewitcz, McGowan, Onali, and Schaeck (2018) show an unintended consequence is contracted lending, especially for single-market banks, although Roman (2017) and Deli, Delis, Hasan, and Liu (2016) find that existing bank borrowers actually benefit. The two studies whose setting most closely resembles mine are Brous and Leggett (1996), which documents negative stock market reactions to 62 EAs issued between 1989 and 1991, and Nguyen, Hagendorff, and Eshraghi (2016) which uses a sample of EAs between 2000 and 2013 to show that corporate governance can mitigate the likelihood of an EA and its impact on stock holders. I contextualize these papers' findings by showing that market reactions to non-payout restricting EAs are indistinguishable from zero. In fact, market reactions to POREAs are also insignificant *unless the firm distributes cash over the last three years*. These results suggest that the performance signal from EA issuance is either value irrelevant or has already been impounded before enforcement. News of restricted payout access appears to be the only value-relevant component.

3. Enforcement actions

Four regulators supervise U.S. financial institutions over my sample period.[29] They conduct periodic safety and soundness examinations. If bank operations are deemed unsafe or unsound, the regulator can issue an EA requiring or restricting management action. Once issued, EAs persist indefinitely until the recipient has addressed regulatory concerns. Among EA types, Written Agreements[30], Cease and Desist Orders, and Prompt Corrective Action Directives are considered the most severe. My sample includes only the first two as the third are rare in public institutions and signal critical undercapitalization. The content of Written Agreements and Cease and Desist Orders is similar; they differ only in that the former denotes management's voluntary consent to address regulatory concerns whereas the latter is issued without recipient consent.

Regulators derive authority to restrict payout under Title 12, Part 1818(b) of the U.S. Codified Federal Regulations. To summarize, a regulator may issue an EA to an institution which has been "*engaging in unsafe or unsound practice in conducting business of such depository institution or has violated… a law, rule, or regulation…*" To remedy risky behavior, regulators can "*take such other action as the [regulator] determines to be appropriate.*" This catch-all clause is invoked to require approval before payout. Appendix D presents an example of a POREA issued jointly to a bank and its holding company. It prescribes a list of policy, internal control, accounting and operational improvements to be attained. Finally, the document requires

[29] The Federal Reserve Board (FRB) examines holding companies and Federal Reserve member state-chartered banks; Federal Deposit Insurance Corporation (FDIC) supervises Federal Reserve non-member state-chartered banks; the Office of the Comptroller of the Currency (OCC) supervises nationally chartered banks and savings and loan associations after July 21, 2011; and the Office of Thrift Supervision (OTS) supervised savings and loan associations before July 21, 2011 when it was disbanded.
[30] Written Agreements are also referred to as Formal Agreements or Consent Orders.

both bank and holding company to obtain regulatory approval prior to declaring a dividend and the holding company to do so before redeeming stock.

Some EAs are issued to holding companies while others to bank subsidiaries. For the latter, the bank has no publicly traded shares outstanding so these POREAs can only restrict upstreaming dividends to the holding company, not to ultimate shareholders. Doing so still reduces shareholder access to payout because many bank holding companies in my sample own one bank and have very little income generated from nonbank subsidiaries. For example, Community National Bancorp (the Company) writes on its 1991, first quarter 10-Q filing:

> "*As all of the operations of the Company are conducted through the Bank, the Company's liquidity is largely determined by, and dependent upon, dividends received from the Bank. The Bank's ability to pay dividends is subject to applicable governmental policies and regulations and, pursuant to the consent order issued by the OCC, the Bank may not pay dividends without the approval of the OCC. As a result, there can be no assurance that the Company will have sufficient cash flow to meet its current and future obligations, including making the interest payments on the Notes due on October 30, 1991*."

If Community National Bancorp is concerned with meeting interest payments on debt, payment to shareholders, residual claimants, is at least as improbable.

Not all EAs restrict payout. In my sample, 229 do and 110 do not. When do regulators issue POREAs rather than EAs without payout restrictions (nonPOREA)? Appendix E presents a nonPOREA issued jointly to a different bank and holding company. The chief distinction between the two appendixes is that the former cites violations affecting the bank's capital position. Examiners criticize management for accruing interest on nonperforming assets, not recognizing losses in a timely manner, and inadequately managing nonperforming loans. The payout restriction seems to respond to the capital threat. In contrast, the violations cited in Appendix E are more procedural and pose less risk to the bank's going concern. Although it is anecdotal, this distinction typifies EAs in my sample.

4. Data and sample

To obtain a sample of publicly held POREA recipients, I begin with the Federal Reserve Bank of New York's list of publicly traded banks and bank holding companies. Each entity is linked to its subsidiaries using the National Information Center's Relationships file. Hand collecting EAs from four separate regulators, I determine whether any entity within the public bank holding company structure received an EA between 1991 and 2015. I retain only Cease and Desist Orders and Written Agreements as two of three order types which can potentially contain payout restrictions. The third, Prompt Corrective Action Directives, signal severe undercapitalization. Because of their unique implications, including them would distort my analysis. I read through each order for a clause restricting dividends or share repurchases. When the regulator does not provide a copy of the order, I inspect SEC filings and annual reports for mention of payout restriction and retain only orders for which I can verify a restriction.

I reduce my sample to orders most likely to contain actual news and not a restatement of information. Redundant POREAs can arise if, for example, one regulator restricts a bank's ability to upstream dividends while another restricts the holding company's shareholder distribution. Other times, EAs may be issued to the same entity for violations discovered sequentially. If EAs issued to the same banking organization contain payout restrictions, subsequent ones issued while the first is still active will not affect access to payout. To avoid diluting information content, I retain only the first POREA issued to a banking entity over my sample period.

The final step is to determine whether payout restrictions bind. Dividend restrictions are deemed binding if their recipient paid dividends over the three preceding years and nonbinding, otherwise. Because dividend streams persist (Lintner, 1956), investors receiving dividends are likely to expect them going forward. Thus, POREAs should downward revise investor dividend expectations for firms that recently paid dividends. Dividends are measured as CRSP distribution codes of 1000-1999. For orders that also restrict repurchases, I sum dividend and repurchase activity over the past three years and apply the same classification scheme. Share repurchases constitute events in which outstanding shares decrease with no change to CRSP's cumulative factor to adjust shares. I impose a $0.05/share materiality threshold on average annual distribution to classify the order as binding. Henceforth, I refer to binding (nonbinding) POREAs as treated (control) ones and their recipients as treated (control) firms or banks. *Note, all firms in this sample receive POREAs; 'treatment' is not POREA issuance but rather investor payout expectation.*[31]

My final sample includes 169 treated and 60 control POREAs, issued between 1991 and 2015. Figure C1 plots the time-series distribution of these. Each is the first that a banking organization receives over my sample period. Using the CRSP daily file, I compute buy-and-hold returns and share turnover over the 252 trading days ending 21 trading days before issuance. Buy-and-hold returns measure market performance and share turnover reflects stock liquidity. I obtain measures of capitalization (tier 1 capital to risk weighted assets, *T1C*), cash reserves (cash to total assets, *Cash)*, asset portfolio risk (nonperforming assets to total assets, *NPA;* allowance for loan and lease losses to total assets, *ALLL*); profitability (quarterly net income to total assets, Q*NI*), and overall risk (the sum of the equity to asset ratio and *QNI* divided by the rolling 12-quarter standard

[31] POREAs do not expressly prohibit payout but, rather, mandate prior regulatory approval. Of the 169 POREA recipients in my sample that distributed profits in the three years before issuance, only 28 continue to do so while the POREA is active; for 13 of these, quarterly payout does not exceed 2.5 cents per share. The restriction appears to bind for most firms. Unreported analysis reveals that those who obtain permission to pay out are safer and perform better.

deviation of *QNI*, *Z-score*) from the CRSP-Compustat merged file as of the quarter before POREA receipt.[32] When CRSP-Compustat data are missing, I hand collect from regulatory filings. Finally, I collect the fraction of shares owned by insiders (*Inside*) for treated or control banks from the DEF14 SEC filing immediately before issuance. Variables are described further in Appendix C.

Table C1 reexamines the differences between POREA and nonPOREA recipients by comparing market and financial characteristics of the two groups. I present summary statistics for each group across *TA*, *T1C*, *Cash*, *NPA*, *ALLL*, *QNI*, *ShrTO*, *MTB*, and *BHR*.[33] I also report two tailed p-values from t-tests of mean differences and Kolmogorov-Smirnov (KS) test for distributional differences. Because most variables are non-normally distributed in my sample, I rely more on the latter test. KS tests shows the two groups differ significantly across every dimension. The median nonPOREA recipient is four times as large as the median POREA recipient and its shares turn over nearly twice as fast. The median nonPOREA recipient experienced slightly positive buy-and-hold stock returns over the last year and positive return on assets in the last quarter whereas the median POREA recipient lost 40% of its market value in the last year and posts a 20 basis point quarterly net loss. Across capitalization, nonperforming assets, and allowance for loan and lease losses, the median nonPOREA recipient is significantly safer and appears to collect less cash. Finally, market-to-book ratios imply overall value creation (destruction) for nonPOREA (POREA) recipients. In sum, POREA recipients are riskier than and underperform nonPOREA recipients.

[32] For 58 cases in which z-score cannot be computed for publicly traded bank holding companies, I compute it using bank data obtained from call reports. In each case, the bank is the lone bank in the bank-holding company structure.
[33] To conserve resources, I omit comparisons of *Inside* and *Z-score*. These variables require hand collection and my empirical analysis does not consider nonPOREA recipients.

Table C2 disaggregates POREA recipients into treated and control groups. The two are statistically indistinguishable along most characteristics. Significant differences in size, liquidity, and inside ownership are explored in further in Section 5.3.

5. Do investors value payout?

In this section, I present my main empirical tests on whether payout is value-relevant to investors in my sample. I also defend assumptions necessary for difference-in-differences estimation and present robustness tests.

5.1. Event study around POREA issuance

My first empirical tests are event studies around POREA issuance for treated and control groups. I estimate expected returns using the Fama and French 3-Factor model:

$$R_{i,t} = \alpha_i + \beta_{1,i} R_{m,t} + \beta_{2,i} R_{SMB,t} + \beta_{3,i} R_{HML,t} + \varepsilon_{i,t} \tag{11}$$

where $R_{i,t}$ is firm i's stock return on day *t*, $R_{m,t}$ is the market's return on the same day, and $R_{SMB,t}$ and $R_{HML,t}$ are returns on the Small-Minus-Big (SMB) and High-Minus-Low (HML) indexes from Kenneth French's website. Excess returns on the value weighted index of all CRSP stocks, obtained from the same source, proxy for market returns. The model is estimated over 126 days ending 21 days before POREA issuance. Residuals proxy for abnormal returns, which I cumulate from the day before issuance to the day after to obtain the cumulative abnormal return (CAR).

I test for CAR significance using Kolari and Pynnönen's (2010) z-statistic. Figure C1 shows that POREA issuance clusters around the Savings and Loans crisis in the early 1990s and the recent banking crisis (2009 through 2011) so event-time clustering may affect my sample. Investor uncertainty about their company's viability likely increases with POREA issuance so event-induced volatility may also affect inference. The Kolari and Pynnönen (KP) test is robust to these two concerns as well as heteroskedasticity. As a nonparametric alternative, I present p-values from the Wilcoxon Ranked Sum (WRS) test to verify that extreme outliers do not drive the results.

Table C3 presents evidence on EAs' wealth implications for investors. Panel A displays mean, precision-weighted, and median returns from four event studies. Beside returns are two-tailed p-values from the KP and WRS tests. For precision-weighed returns, CARs are weighted by the inverse of their estimation period return standard deviations. The first and second rows test for significant returns around POREA and nonPOREA issuances, respectively. The mean POREA produces significantly negative abnormal returns of 2.35% whereas nonPOREAs actually induce a marginally positive market reaction, on average. This surprising result is consistent with market participants receiving nonPOREAs as good news for their firms. Investors might believe it makes their bank safer. However, the WSR test's failure to reject zero abnormal returns suggest the positive reaction could stem from several outlying observations. Regardless, CARs for POREAs and nonPOREAs should not be compared directly because they likely contain very different clauses and Table C1 identifies dramatic recipient differences. Rows 3 and 4 divide POREAs into treated and control subsamples. The mean treated CAR is -3.52% and significantly negative with 99% confidence. In contrast the mean CAR for control firms is statistically indistinguishable from zero and even positive.

5.2. Treated versus control bank return differences

In expectation, POREA issuance sends a similar signal to investors of treated and control firms about their bank's financial condition and performance. If this signal is news, investors should adjust stock prices. However, treated firm investors are incrementally impacted because the order also reduces their payout expectations. Because control firm investors are unlikely to expect payout after three years without it, they should be unaffected by the payout-restricting clause. Thus, differences between treated and control firms' market reactions likely stem from treated investors' restricted payout access. If payout is valued, treated bank stock prices should decrease more than control bank stock prices. This consideration produces my first hypothesis:

H1: Market reactions to POREAs are significantly worse for treated firms than for control firms.

Table C3, Panel B examines mean and median differences between these two groups' CARs. Group means (medians) are compared via t-test (WSR test). When directly comparing treated and control returns, one-tailed p-values are more appropriate because *H1* is based on a signed difference, not just a difference. Both tests point to more negative CARs for treated POREA recipients with 95% confidence or better.

These results offer a preliminary estimate of the value of payout or, more exactly, the value destroyed when payout is restricted. Mean differences suggest that payout could constitute 4.5% of a stock's worth. However, since control POREA returns are statistically indistinguishable from zero, a more conservative estimate would subtract zero from the treated mean. Still, restricted payout access destroys a non-negligible 3.5% of the mean firm's value over three days.

5.3. Identification challenges

If treated and control firm POREAs deliver identical information about identical firms to identical investors, mean CAR differences well approximate the value of payout in my sample. The failure of any of these idealized assumptions can alternatively explain the treated and control CAR difference. If so, it would be wrong to attribute his difference to the value of payout.

First, consider differences in POREA content. All POREAs restrict payout but they also identify risky behavior and prescribe corrective measures. If, for some reason, treated POREAs contain worse news than control ones, content, not payout implications, could drive CAR differences. Unfortunately, the orders' content is inherently qualitative so I cannot easily compare differences. Reading through each, however, I do not observe systematic content differences between treated and control POREAs and no economic reason exists to suspect such differences.

Next, consider potentially different shareholder bases. If investors form clienteles around payout preference, treated POREAs could produce more negative market reactions for other reasons. Payout-seeking investors are naturally more sensitive to payout restrictions than non-payout-seeking investors. This explanation would refine my results' interpretation: POREAs destroy more value for payout-seeking investors than for non-payout-seeking investors. However, this interpretation offers the same conclusion I present – payout has value – *by assumption* and thus does not challenge my findings. Allowing some investors to 'seek' payout in a theoretical framework *assumes* they value it. On the other hand, if payout-seeking investors are more sensitive to POREA content *other than the payout restriction*, that dissimilarity could explain treated POREA's more negative CAR. I know of no rationale, however, to presuppose this. Moreover, evidence that a clientele even exists around payout status is weak, at best. Literature reviews by Farre-Mensa, Michaely, and Schmalz (2014) and DeAngelo, DeAngelo, and Skinner (2009) emphasize researchers' consistent failure to detect static investor payout clienteles.

Finally, firm differences can challenge the causal interpretation of my results. If firms that do and do not distribute cash over the last three years differ systematically and those differences affect a POREA's impact on firm value, then more negative treated CARs could reflect firm characteristics, not lost value from restricted payout. Table C2 shows that for eight (nine) of eleven variables, equivalence in the two groups' distributions (means) cannot be rejected at conventional confidence levels. Exceptions include asset size (the mean treated firm is larger) and inside ownership (the mean treated firm is less insider-owned). The t-test also suggests the mean treated firm's stock is more liquid, although the more appropriate KS test does not. Differences along these dimensions question whether market reactions can be compared between the groups. Plausibly, investors of large, liquid firms may discover the POREA sooner and thus react instantly, whereas smaller and less frequently traded control firms may take longer to adjust stock prices. Because less of the average treated firm's shareholder base has inside information, enforcement may be more of a shock. Although I fail to detect a difference in returns over the year ending 21 trading days before issuance, control shares' prices could fall just before the POREA, muting the reaction around the event date. These possibilities push control CARs down and treated CARs up, biasing my results toward significant differences.

I address these concern in four ways. First, rather than testing group mean and median CAR differences, I test pairwise mean and median differences where observations are matched on the variables that could obscure inference. Second, in a robustness test, I extend the measurement window to (-1,+9) to allow for slower market reactions for control firms. Third, I account for these differences by including size, share turnover, and inside ownership as covariates in multivariate analysis in sections 5.5 and 6. Finally, I show that in the 15 days preceding issuance the two groups' mean and median returns exhibited nearly identical trends. These tests support my results' robustness and causal interpretation.

Table C4 reports matched sample mean and median CAR differences. Treated and control firms are matched on size (row 1), share turnover (row 2), inside ownership (row 3), and buy-and-hold returns (row 4). In row 5, I use propensity-score matching where the propensity for treatment – payout over the last three years – is estimated from a logistic regression of these four variables. For all rows, matches must fall within a 5% caliper. Panel A matches without replacement while Panel B allows a single control unit to match up to three treated ones. One-sided p-values from t-tests of mean differences and WSR tests of median differences are also reported. Out of 20 tests, 19 are statistically significant at traditional levels. The WSR test for the size-matched sample in Panel A is marginally insignificant (p-value of 0.1137). In general, results in Panel A are less significant because of the limited number of observations (58 or 59). Panel B is characterized by higher statistical power from a larger sample. Overwhelmingly, these tests support that my baseline results reflect a value to payout, not differences between treated and control firms.

5.4. Robustness tests

To ensure that the CAR differences are not artifacts of a particular sample or model specification, I report a series of robustness tests in Table C5. The first row reproduces results from Table C3, Panel B, which I refer to as the baseline specification.

In the baseline specification, I estimate expected returns using the Fama and French 3-Factor model. I test four other expected return models to ensure that poorly estimated expected returns do not drive my results. Row 2 of Table C4 uses CARs computed from the Fama and French 3-Factor model augmented with a custom factor to capture banking industry performance. Returns for NASDAQ's ^BANK index proxy for banking industry performance. Row 3 uses the market model to estimate returns, dropping the SMB and HML factors. Rows 4 and 5 use market adjusted and raw returns, respectively, to shed noise from model estimation. Control firm CARs continue to exceed those of treated firms with high confidence.

The baseline specification uses a 126-day estimation period ending 21 days before EA issuance. Given firms' very negative pre-EA performance, sensitivity to the three factors on the issuance date may be better captured using an estimation window before and after enforcement. Row 6 tests for return differences when factor sensitivities are estimated using returns 63 days before and 63 days after the 41 days centered on POREA issuance. Significant differences persist.

Financial distress severely drives down stock prices in my sample so that trades that move prices a small dollar amount induce large percentage returns. Row 7 of Table C5 reports return differences after winsorizing the return universe at 1% tails prior to estimation. The return difference shrinks but retains high statistical significance.

Because control firms are smaller and have potentially less liquid stock (Table C2), it may take longer for their stock prices to impound information. Row 8 tests differences in abnormal returns cumulated over the (-1,+9) window. In this specification, the difference doubles. The larger estimate may be more reasonable. Section 5.3 presents evidence that stock prices likely take longer than three days to impound the POREA's full impact.

An important assumption in any event study is that value-relevant information released around the event but unrelated to it is noise, not bias. Although I cannot control for all concurrent value-relevant news for all companies, Compustat does report the release dates of quarterly earnings figures. Row 9, retains only observations with quarterly earnings announcement dates outside the 5 trading day window centered on POREA issuance. Results continue to hold.

Rows 10 and 11 look within treated firms. The former compares CARs between firms that paid out in the last year, reported in the treated column, and those that paid out in the last three years but not in the last year, reported in the control column. Although the former set has more negative returns, the difference is not significant which could reflect payout expectations lasting longer than one year, an elevated value to payout when firms recently missed dividends, or statistical power concerns from a smaller sample. Row 11 compares firms that discontinued payout after POREA receipt, reported in the treated column, against those that obtained permission to continue paying while the POREA was active, reported in the control column. Statistically different CARs suggest the market can distinguish firms that will obtain permission to pay out.

Because holding companies are usually the publicly traded entities, POREAs issued to holding companies could be more visible or more pertinent to investors. My baseline sample uses the first POREA issued to either bank or holding company. To test whether this heterogeneity matters, rows 12 and 13 focus on holding company and bank POREAs, separately. Holding company POREAs induce more stock price devaluation but in both subsets, the difference persists.

The next two rows split the baseline event study into two windows. Row 14 compares 115 treated and 40 control POREAs issued during crisis year. I define crisis years to include the savings and loan crisis from the 1980s through 1992 and the recent banking crisis from 2009 through 2011. Row 15 replicates the analysis for 54 treated and 20 control POREAs issued during non-crisis years. Results show that the mean and median difference between the treated and control CARs is dramatically higher for POREAs issued during crisis years. It is statistically insignificant during non-crisis years, consistent with greater value-relevance of payout during a crisis. However, low statistical power in the non-crisis years could also affect this result.

Finally, to show that the event itself, and not random chance, drives my results, I report two placebo tests in rows 16 and 17. Respectively, they test pseudo-event dates 30 days before and after POREA issuance. Means and median differences dramatically attenuate for treated pseudo-events while control ones persist around zero. Group differences dissipate.

5.5. Multivariate analysis

Next, I test whether the differences above persist when controlling for other potential determinants of returns. Using ordinary least squares, I estimate the following model:

$$CAR_i = \alpha + \beta_1 * Treated_i + \gamma' \boldsymbol{X} + \varepsilon_i \tag{12}$$

where *CAR* is firm *i*'s 3-day abnormal return and *Treated* equals one if the firm distributes profits over the last three years and zero otherwise. I control for bank size, capitalization, cash holdings, asset portfolio risk, profitability, stock liquidity, stock performance, and market-to-book ratio. Covariates are winsorized at 5% tails. I use White standard errors to account for heteroskedasticity.

Column 1 of Table C6 restates univariate results from Table C5. In the next column, I control for size and liquidity because my treated and control samples differ significantly across these characteristics. Although they also differ across inside ownership, I focus on that separately in Table C7. Even after accounting for significant differences in size and share turnover, my initial results hold. The third column affirms these results after including other bank-year covariates. The fourth column also includes year-fixed effects. Because the sample period covers two crises in which investors were particularly bearish about bank stocks, within-year CARs may more meaningful. Therefore, Column 4 becomes my baseline multivariable specification for subsequent analysis. Overall, this table suggests payout represents at least 4.5 to 5.3% of shareholder value for firms in my sample.

5.6. Parallel trend and effect duration

Figure C2 cumulates mean abnormal returns over the 31 day window centered on POREA issuance for treated and control firms. It illustrates several key results. First, the parallel trend assumption, crucial for difference-in-differences estimation, appears likely to hold in my sample. For both groups, cumulative abnormal returns hover around zero in the pre-event period. This also reaffirms the unbiasedness of the Fama and French 3-Factor model for my sample. Slightly before POREA issuance, returns for both groups begin to decrease. However, for the control group, returns rebound and fluctuate within the -4% to +2% range over the next three trading weeks. In contrast, treated POREA recipients continue their downward stock price drift until at least 5 days after POREA receipt. At that point, returns level off around -8%. The more important point is that restricted access to payout appears to *permanently* devalue the mean payout firm. Rather than CARs reflecting stockholder overreaction and correction, the lower stock price persists for at least 15 trading days after investors lose access to payout.

To summarize Section 5, firms that pay dividends or repurchase shares in the three years before POREA receipt experience significantly negative abnormal returns around these orders; firms that do not pay out experience insignificant returns. Stock prices may take at least five days to fully impound the orders' impact. EAs appear to cause permanent stock price devaluation. This setting holds constant any potentially negative performance signal associated with POREAs. Insignificant control group returns, however, imply that the performance signal is value irrelevant. This finding suggests that POREAs do not offer the market new information even when they deem bank operations unsafe or unsound. Most importantly, differential reactions between treated and control imply that investors value the ability to distribute profits. For the mean firm in my sample, 3.5% to 8% of its market value derives from payout access.

6. Why do bank investors value payout?

In this section, I explore why investors in my sample value payout. I focus on two theories discussed in Section 2.1: (1) distributions shift risk from shareholders to other corporate claimants; and (2) distributions reduce the agency costs of equity.

6.1. Evidence from default likelihood

A first order determinant of risk-shifting is default likelihood. The closer a firm is to default, the more likely it is to shift risk (Eisdorfer, 2008; Danielova, Sarka, and Hong, 2013; Chen and Duchin, 2014; Li, Lockwood, and Miao, 2017; and Denes, 2017). If distributions allow firms to do so, payout should add more value closer to default.[34] Thus, restricted access to payout should destroy more market capitalization closer to default. This reasoning yields my second hypothesis:

H2: Distance to default positively predicts abnormal POREA returns for treated firms.

I measure distance to default as the bank's z-score, following Boyd and Graham (1986), Laeven and Levine (2009), Hakenes, Hasan, Molyneux, and Xie (2015) and others. Z-score is the sum of equity capital to assets and net income to assets divided by the standard deviation of net income to assets. Standard deviations are computed over a rolling 12-quarter window. Conceptually, z-score measures how many standard deviations of earnings a bank is from its default threshold. Following Laeven and Levine (2009), I log the z-score to mitigate the effect of distributional non-normality. Larger values denote safer banks. I test *H2* by adding z-score, individually and interacted with *Treated*, to my baseline specification in Table C6, Column 4:

$$CAR_i = \beta_1 * Treated_i + \beta_2 * Z\text{-}score_i + \beta_3 * Treated * Z\text{-}score_i + \gamma' \boldsymbol{X}_i + \delta_t + \varepsilon_i \quad (13)$$

Default risk can affect returns a second way. The closer a firm is to default, the less surprised shareholders should be about regulatory intervention. This intuition holds for payout and non-payout banks. Therefore, a negative β_2 would be consistent with this 'anticipation effect' while a positive β_3 supports *H2*.

The first four columns of Table C7 present results from estimating Equation 13. Column 1 omits the interaction term. The mean difference between treated and control CARs persists at a level similar to Table C6; z-score appears not to moderate returns. When the interaction term is

[34] One concern is that firms too close to financial distress cannot distribute cash because doing so would violate debt covenants. Aretz, Banerjee, and Pryshchepa (2018) show that firms moderately close to default shift risk while those very close do not, consistent with this intuition. This concern, however, pertains to non-financial firms, not the banks in my sample. Banks rely primarily on deposits, not loans or bonds, for debt-financing. Deposit contracts place no restriction on bank operations, which is why regulators step in when banks become too risky.

included in Column 2, support for H2 emerges: POREA recipients who do pay out experience worse abnormal returns the closer they are to default. These findings suggest that distribution is more valuable in financial distress and are thus consistent with a risk-shifting value to payout. A negative β_2 supports the anticipation effect for all firms. Notably, the mean difference between treated and control CARs more than doubles. This suggests that after accounting for risk, the value to dividends could be greater than estimated in the prior section. Columns 3 and 4 replicate Columns 1 and 2, respectively, but include other covariates. Results become slightly stronger.

6.2. Evidence from inside ownership

Z-score allows me to test the risk-shifting theory of payout relevance; however, it does not rule out a value to payout in reducing agency costs. Another variable, insider stock ownership, can estimate the net effect of risk-shifting and agency cost reduction theories for my sample. Jensen, Solberg, and Zorn (1992) and others show that dividends are less important to firms with high inside ownership. Referencing Jensen and Meckling's (1976) seminal work, they claim manager shareholdings mitigate agency costs between inside and outside shareholders. Managers with large stakes are forced to internalize more of their value-destroying behavior. Rationally, they avoid such behavior. Morck, Schleifer and Vishny (1988) and McConnell and Servaes (1990) have challenged the notion that agency costs strictly decrease in inside ownership but empirical work does support a monotonic relationship (Rozeff (1982), Ang, Cole, and Lin (2000), Singh and Davidson (2003), and Fahlenbrach and Stulz (2000)).[35] If payout also reduces agency problems, it can substitute for inside ownership as a corporate governance mechanism (Pinkowitz, Stulz, and Williamson's, 2006). Based on this intuition, POREAs that reduce access to payout should

[35] Refer to Footnote 2 for a discussion on this conflicted literature.

command *less negative* returns for highly insider-owned firms because shareholders of such firms rely less on payout to mitigate agency costs.

The risk-shifting theory of payout relevance implies the opposite. John and John (1993) and John, Saunders, and Senbet (2000) argue that when manager interests align with outside shareholder interests, risk-shifting is more likely. Moreover, the average POREA recipient in my sample loses 34% of its market value the year before enforcement. Financial distress further incentivizes risk-shifting (Section 6.1). Because managers control the firm, shareholder incentive and *ability* to shift risk increase with inside ownership (Saunders, Strock, and Travlos, 1990; Eisdorfer, 2008). Based on this intuition, POREAs that reduce access to payout should command *more negative* returns for highly insider-owned firms because shareholders of such firms rely more on payout to shift risk. These countervailing effects motivate my third hypothesis[36]:

H3a: If investors value payout primarily as a means to reduce agency costs of equity, then higher inside ownership will associate with less negative treated firm CARs.

H3b: If investors value payout primarily as a means to shift risk, then higher inside ownership will associate with more negative treated firm CARs.

Echoing Section 6.1, inside ownership should also have an anticipation effect on treated and control firms' returns. POREAs offer less value-relevant news if a larger fraction of shareholders possesses inside information. Therefore, as inside ownership increases, stock returns for all recipients, regardless of payout expectations, should be less negative. To test these predictions, I replace *Z-score* in Equation 13 with *Inside*, the fraction of shares held by insiders:

[36] A closely related variable, ownership concentration, offers similar predictions. Shleifer and Vishny (1986) and Admati, Pfeiderer, and Zechner (1994), among others, argue that ownership concentration reduces agency costs of equity. Zhang (1998) shows that when ownership is more concentrated, risk-shifting is more likely. Thus, the value of payout to reduce agency costs (shift risk) should decrease (increase) in ownership concentration just like it does in inside ownership. I prefer inside ownership as my independent variable of interest as it more directly connects managerial decisions like payout and shareholder incentives.

$$CAR_i = \beta_1 * Treated_i + \beta_2 * Inside_i + \beta_3 * Treated * Inside_i + \gamma' \boldsymbol{X}_i + \delta_t + \varepsilon_i \qquad (14)$$

A positive (negative) β_3 would support *H3a* (*H3b*) while a positive β_2 would be consistent with an anticipation effect. Columns 5 through 8 of Table C7 report results from estimating this regression. Column 5 omits the interaction term. The mean difference between treated and control CARs persists at levels similar to Table C6. Inside ownership relates marginally to returns, in a direction consistent with the anticipation effect: the more of the firm insiders own, the less negative are POREA returns. When including the interaction term in Column 6, several important results emerge. First, a negative β_3 supports *H3b*: the more valuable payout is in shifting risk and the less valuable it is in reducing agency costs, the more harm a POREA causes shareholders. A positive β_2, again, suggests POREAs present less value-relevant news to market participants when more of those participants are insiders. Also of note, β_1, loses significance. That is, after accounting for inside ownership, a proxy for agency costs and risk-shifting expectations, payout status can no longer explain returns. The next two columns show that results strengthen when including controls. The final column includes both *Z-score* and *Inside*, individually and interacted with *Treated*. It suggests that the effects in Section 6.1 and 6.2 mutually hold.

To summarize, Section 6 finds evidence consistent with a risk-shifting value of payout and inconsistent with an agency-cost reducing value. As firms approach default, restricted payout access destroys more shareholders wealth. More wealth is also destroyed when shareholders have more incentive and ability to shift risk, as proxied for by inside ownership. In fact, inside ownership can explain away the difference between treated and control group returns. Neither effect holds for POREA recipients that do not distribute cash over the last three years. For these non-payout firms, default likelihood and inside ownership positively predict returns, consistent with shareholders better anticipating regulatory intervention.

7. Conclusion

I show that restricted access to payout reduces firm value. Whereas extant literature emphasizes the role of payout in mitigating agency costs of equity, my results highlight a largely under-explored attribute: payout deflects shareholder risk toward other corporate claimants. For my sample, this advantage dominates agency cost reduction. My work complements Acharya, Le, and Shin (2017) who show that large U.S. banks shifted risk through dividends in the latest crisis.

Difference-in-differences estimation supports a causal interpretation of my results. The first difference is time and the second is whether or not investors expect payout. POREAs issued to non-payout firms serve as counterfactuals for those issued to payout firms. These groups are indistinguishable across risk characteristics and share a common performance trend. This setting offers two main advantages over prior research. First, it holds constant signal effects of payout policy changes. Netting out the signal, a difference in the two groups' market reactions should represent the value of payout. Second, whereas previous research examines a firm's choice to adjust payout, the adjustment in my setting is imposed onto the firm, mitigating reverse causality concerns (Baker and Wurgler (2004)). Both facets help causally link payout and firm value.

To my knowledge, this paper is the first to investigate EAs through a difference-in-differences framework and the only one to focus on POREAs. Although over 20,000 EAs have been issued over the last quarter century, Brous and Leggett (1996) and Nguyen, Hagendorff, and Eshraghi (2016) are the only two published papers I am aware of that study how EAs affect shareholder wealth. My work expands their samples and refines their results. The only value-relevant component of an EA appears to be a binding payout restriction. NonPOREAs produce insignificant CARs as do POREAs issued to recipients that do not distribute profits.

My work is a preliminary step toward understanding the value in payout. The sample of POREA recipients is certainly non-random. This setting, flush with risk, is one in which shareholders most appreciate the ability to shift risk. If payout is valuable in financial distress, however, and all firms have some ex ante probability of reaching financial distress, then even investors of healthy firms should value dividends and repurchases. Whether the risk-shifting value of payout extends to other industries or even to non-troubled banks or whether the *risk* of risk-shifting is priced into creditor claims, are interesting topics for future research.

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Appendixes

Appendix A: Enforcement actions by type

This appendix tabulates and describes the different enforcement action (EA) types in our sample.

EA type	N	Description
Cease and desist orders	73	Order directing bank management to take specific measures to remedy 'unsafe or unsound' activities.
Cease and desist orders against a person	24	Order directing bank employees to take specific measures to remedy 'unsafe or unsound' activities.
Formal agreements/written agreements/consent orders	32	Binding agreement between bank and regulators to take specific measures to remedy 'unsafe or unsound' activities.
Fines levied against a person	62	Fines against persons for unsafe or unsound actions, legal violations, and/or compliance failures.
Other fines	99	Fines against bank for unsafe or unsound actions, legal violations, and/or compliance failures.
Restitution by a person	25	Orders requiring individuals to reimburse bank or regulator for wrongdoing
Sanctions against personnel	887	Order terminating individual's employment and/or prohibiting him/her from future bank employment without prior regulatory approval.
Other	43	All orders which do not fit these categories: call report infractions, deposit insurance threats, hearing notice or other actions, orders requiring restitution, prompt corrective actions, home mortgage disclosure act violations

Appendix B: Chapter 2 variable definitions

This appendix describes the computation and intuition behind variables used in our study. Variable names appear in the text or in tables can be modified by 'NFF' or 'bank' prefixes to specify which entity they refer to.

__**EA characteristics**__

CAR(-1,+1) – 3-day cumulative abnormal returns from the Fama-French (1993) 3-Factor model estimated in the 252 trading days ending one month before enforcement. The 3-day window is centered on the EA issue date.

CAR(-1,+8) – 10-day cumulative abnormal returns from the Fama-French (1993) 3-Factor model estimated in the 252 trading days ending one month before enforcement. The 10-day window begins the day before the EA issued date and ends 8 days after.

Mean CAR(-1,+1) – The average of Bank CAR(-1,+1) computed within a director-NFF-year (bank-NFF-year) in Column 8 (9) of Table 3.

Severe EA – an indicator equal to one if the EA is one of three types defined as severe by Srinivas et al. (2014); zero, otherwise. Those categories are (1) Cease and Desist Orders, (2) Written Agreements, Formal Agreements, or Consent Orders, and (3) Prompt Corrective Action Directives.

__**Linked director flags**__

BLD – an indicator equal to one if the NFF director also serves on a bank board in year *t*; zero, otherwise.

EBLD – an indicator equal to one if the NFF director also serves on the board of an enforced bank in year *t*; zero, otherwise.

NEBLD – an indicator equal to one if the NFF director serves on the board of a non-enforced bank in year *t*; zero, otherwise. It holds that BLD=EBLD+NEBLD.

NLD – an indicator equal to one if the NFF director serves on another NFF board but not a bank board in year *t*; zero, otherwise.

__**Director board activity**__

Audit committee chair (member) – an indicator equal to one if the director chairs (is a member of) the audit committee in year *t*; zero, otherwise. Data start in 1999.

Corporate governance committee chair (member) – an indicator equal to one if the director chairs (is a member of) the corporate governance committee, which also subsumes the nomination committee, in year *t*; zero, otherwise. Data start in 1999.

BFI – the sum of all board positions the director holds, as member or chair, and the chairman/chair-women position he/she holds as in Fedaseyeu, Linck, and Wagner (2018).

__**Director Characteristics**__

Board chair – an indicator equal to one if the director chairs the board in year *t*; zero, otherwise. Data start in 1999.

Cash compensation – the natural logarithm of an outside director's cash compensation from a given board in thousands of US Dollars.

Cash compensation only – an indicator equal to one if the firm pays only cash compensation; zero, otherwise.

Compensation committee chair (member) – an indicator equal to one if the director chairs (is a member of) the compensation committee in year *t*; zero, otherwise. Data start in 1999.

Co-opted director – an indicator equal to one if the director was appointed to the board after the CEO was hired; zero, otherwise.

Director age – the director's age.

Director tenure – the number of years the director has served the company.

Female director – an indicator equal to one if the director is female; zero, otherwise.

Financial expert – an indicator equal to one if the director is classified as financial expert in year *t*; zero, otherwise. Data starts in 2002.

Independent director – an indicator equal to one if the director is classified as an independent director in year *t*; zero if the director is not independent or not employed by the company

Inside director – an indicator equal to one if the director is employed by the company in year *t*; zero, otherwise.
Rank – the rank of a given firm's market capitalization to the market capitalizations of all other firms a given director serves in year *t*. Largest firms are assigned a rank of one.
SEAT – a director's number of independent outside board appointments
Total compensation – the natural logarithm of the sum of cash, bonus, equity, and retirement/pension contribution compensation.

Board Characteristics

Board size – the number of board members.
CEO duality – an indicator equal to one if the CEO also chairs the board of directors in year t; zero, otherwise.
CEO tenure – the length the current CEO has served the company.
% **board outsiders** – the share of independent directors on the board of directors.

Firm Characteristics

Leverage – the ratio of book debt to total assets.
Market capitalization – the natural logarithm of the firm's market capitalization.
Return on assets – the ratio of net income to total assets.
Size – the natural logarithm of total assets.
Tobin's Q – the ratio of the sum of market value of equity and book value of debt to total assets.

Bank-specific characteristics

Non-performing asset ratio – the ratio of nonperforming assets to total assets. Nonperforming assets consist of foreclosed real estate, loans on non-accrual status, and loans over 90 days past due but still accruing interest.
Risk-adjusted capital ratio – the ratio of Tier 1 capital to risk-weighted assets. It is only available for banks, not non-financial firms or non-bank financials like insurance companies or credit card companies.

Appendix C: Chapter 3 variable definitions

This appendix lists variables used in my empirical analysis, how each is calculated, the date on which they are measured, and the databases from which they come. Regression analysis uses natural log of the three scale-dependent variables, *TA*, *ShrTO*, and *Z-score*.

Symbol	Variable	Calculation	As of	Data Sources
Treated	Treated indicator variable	1 if firm paid dividends or repurchased shares over the last three years; 0 otherwise	POREA Issue date	CRSP daily file
TA	Total Assets	Total assets in millions	Quarter-end immediately preceding POREA issue	CRSP-Compustat merged file
T1C	Tier 1 capital	Tier 1 Capital over risk weighted assets	Quarter-end immediately preceding POREA issue	CRSP-Compustat merged file
Cash	Cash holdings	Cash over total assets	Quarter-end immediately preceding POREA issue	CRSP-Compustat merged file
NPA	Nonperforming assets	Nonperforming assets over total assets	Quarter-end immediately preceding POREA issue	CRSP-Compustat merged file
ALLL	Allowance for loan and lease losses	Allowance for loan and lease losses over total assets	Quarter-end immediately preceding POREA issue	CRSP-Compustat merged file
QNI	Quarterly net income	Quarterly net income over total assets	Quarter-end immediately preceding POREA issue	CRSP-Compustat merged file
BHR	Buy-and-hold returns	Buy-and-hold returns over last year	POREA issue date -21	CRSP daily file
ShrTO	Share turnover	Trading volume over last year scaled by shares outstanding	POREA issue date -21	CRSP daily file
MTB	Market to book assets	Book value of liabilities plus market value of equity over book value of assets	Book values as quarter-end immediately preceding POREA issuance; market value as of POREA issuance date -21	CRSP daily file; CRSP-Compustat merged file
Inside	Fraction of Inside Ownership	Shares held by corporate insiders divided by total shares outstanding	DEF14 filing immediately preceding POREA issuance	SEC EDGAR filings
Z-score	Distance to default	The sum of equity-to-assets and QNI scaled by rolling 12 month standard deviation of QNI	Quarter-end immediately preceding POREA issue	CRSP-Compustat merged file; Call reports when holding company data unavailable

Tables

Table A1: Chapter 1 summary statistics

This table reports summary statistics for key variables. Panel A summarizes bank-years in my sample whereas Panels B and C summarize market-years. In Panels A and B, means and medians are presented for the full sample and subsamples of target and acquirer observations. In Panel C, the sample is split by market deposit-heaviness (MDH) quartiles, which is computed as the residual from within-year, quadratic, market-level regressions of log deposit volume on log loan volume and its square. Higher (lower) values denote deposit-heavier (loan-heavier) markets. Total deposits, small business and farm (SBF) loans, total capital and nonperforming assets are scaled by total assets. Concentration measures the Hirschman-Herfindahl Index, derived by summing squared market shares over all market participants; this is done separately for loan and deposit markets. CD rate is the median rate offered on a 1-year, \$10,000 minimum certificate of deposit; interest checking rate is the median rate offered on a \$0 minimum interest checking account; Home mortgage rate is the median rate offered on a 15-year, \$175,000 maximum fixed rate mortgage; Car loan rate is the median rate offered on a 5 year new car loan. Medians are taken within bank-year (market-year) in Panel A (Panels B and C).

Panel A: bank-years

	Full Sample		Target		Acquirer	
	Mean	Median	Mean	Median	Mean	Median
Bank-years	189,428		5,174		6,505	
Total assets (\$000)	1,097,611	130,678	1,549,554	150,783	6,677,507	434,550
Total deposits	0.830	0.855	0.830	0.861	0.788	0.823
SBF loans	0.154	0.139	0.161	0.149	0.153	0.142
Total Capital	0.112	0.099	0.107	0.093	0.106	0.092
Nonperforming Assets	0.012	0.006	0.011	0.006	0.008	0.005
CD rate (%)	2.084	1.600	2.376	1.900	2.470	2.230
Interest checking rate (%)	0.474	0.250	0.475	0.175	0.459	0.200
Home mortgage rate (%)	5.088	5.375	5.019	5.375	5.242	5.604
Auto loan rate (%)	6.191	0.000	5.897	6.240	6.195	6.500

Panel B: market-years, target vs. acquirer

	Full Sample		Target		Acquirer	
	Mean	Median	Mean	Median	Mean	Median
Market-years	44,410		6,978		20,439	
MDH	-0.005	0.000	-0.060	-0.070	-0.097	-0.110
Population	129,456	21,347	517,877	66,907	247,984	40,950
Median income	35,669	34,098	37,328	35,612	36,256	34,755
Unemployment rate	6.310	5.700	5.919	5.400	6.095	5.600
Total deposits (000s)	3,357,074	359,924	14,956,251	1,012,568	6,607,940	680,146
SBF loans (000s)	121,381	15,712	500,324	65,724	238,619	38,507
HHI (deposit)	0.333	0.267	0.216	0.178	0.246	0.206
HHI (SBF)	0.270	0.227	0.210	0.172	0.236	0.200
CD rate (%)	2.005	1.500	2.552	2.190	2.324	2.075
Interest checking rate (%)	0.395	0.150	0.470	0.200	0.403	0.150
Home mortgage rate (%)	4.765	4.625	5.215	5.500	5.050	5.438
Auto loan rate (%)	5.466	5.625	5.740	6.000	5.637	5.990

Panel C: market-years, loan-heavy vs. deposit-heavy

	MDH Q1		MDH Q2&Q3		MDH Q4	
	Mean	Median	Mean	Median	Mean	Median
Market-years	10,353		23,366		10,691	
MDH	-0.217	-0.390	-0.031	-0.070	0.257	0.230
Population	88,056	8,248	162,902	28,891	96,260	25,670
Median income	35,488	33,758	35,718	34,364	35,739	33,776
Unemployment rate	6.126	5.400	6.348	5.800	6.406	5.800
Total deposits (000s)	2,061,436	113,804	3,798,308	452,698	3,647,400	484,455
SBF loans (000s)	109,219	12,897	152,649	23,891	64,821	8,309
HHI (deposit)	0.502	0.424	0.283	0.241	0.276	0.245
HHI (SBF)	0.374	0.329	0.255	0.218	0.202	0.172
CD rate (%)	1.744	1.190	2.044	1.538	2.117	1.640
Interest checking rate (%)	0.291	0.100	0.393	0.150	0.479	0.200
Home mortgage rate (%)	4.519	4.000	4.824	4.840	4.800	4.663
Auto loan rate (%)	5.251	5.140	5.492	5.740	5.567	5.750

Table A2: Deposit and loan volume and rate changes around M&As (univariate)

This table reports univariate statistics about how acquirers change operations in target markets around acquisitions. It summarizes merger-market level tests of changes in six customer welfare measures over the universe of ownership-changing U.S. bank M&As between 1998 and 2016. The first two columns respectively report log changes in deposit volumes and SBF loan issuance from the pro-forma bank's *t-1* level in a given market to the consolidated bank's *t+2* level in the same market, where year *t* is the acquisition year. The pro-forma bank level is defined as the sum of target and acquirer levels in a given market-year. Column 3 (4, 5, and 6) reports the mean difference in the 1-year, $10,000 minimum CD rate ($0 minimum interest checking account rate, $175,000 maximum 15-year fixed mortgage rate, 5 year fixed auto loan rate) from the target bank's *t-1* level in a given market to the consolidated bank's t+2 level in the same market. Changes are reported for my entire sample and for observations where the market's pre-acquisition market deposit-heaviness ($\overline{MDH}$) falls into the highest and lowest quartiles. $\overline{MDH}$ is computed by averaging the last three years' residuals from within-year, quadratic, market-level regressions of log deposit volume on log loan volume and its square. Higher (lower) values denote deposit-heavier (loan-heavier) markets. The number of observations with non-missing data is reported below mean changes. The final row reports t-statistics from a mean difference test between the highest and lowest quartiles.

	Volumes		Rates			
Sample	Deposits	Loans	CD	Checking	Mortgage	Auto
	(1)	(2)	(3)	(4)	(5)	(6)
Full	0.049***	0.143***	-0.581***	-0.251***	-0.824***	-1.551***
	7,446	4,373	1,334	1,265	1,689	1,377
Loan-heavy (Q1 $\overline{MDH}$)	0.090***	0.033	-0.193	-0.209***	-0.766***	-1.415***
	1,870	1,102	334	324	371	318
Deposit-heavy (Q4 $\overline{MDH}$)	-0.008	0.293***	-0.493***	-0.253***	-0.897***	-1.543***
	1,861	1,043	295	270	415	341
Q4-Q1 difference T-Statistic	4.855	-5.498	1.666	1.068	1.274	0.992

Table A3: Deposit and loan volume and rate changes around M&As

This table reports multivariate regression estimates of how acquirers change operations in target markets around acquisitions. It summarizes merger-market level tests of changes in six consumer welfare measures over the universe of ownership-changing U.S. bank M&As between 1998 and 2016. Dependent variables in the first two columns are log changes in deposit volumes and small business and farm (SBF) loan issuance, respectively. Changes are measured from the pro-forma bank's *t-1* level in a given market to the consolidated bank's *t+2* level in the same market, where year *t* is the acquisition year. The pro-forma bank level is defined as the sum of target and acquirer levels. The dependent variable in Column 3 (4, 5, and 6) is the difference in the 1-year, $10,000 minimum CD rate ($0 minimum interest checking account rate, $175,000 maximum 15-year fixed mortgage rate, 5 year fixed auto loan rate) from the target bank's *t-1* level in a given market to the consolidated bank's *t+2* level in the same market. The independent variable of interest, $\overline{MDH}$, measures a market-year's deposit heaviness. $\overline{MDH}$ is computed by averaging the last three years' residuals from within-year, quadratic, market-level regressions of log deposit volume on log loan volume and its square. Higher (lower) values denote deposit-heavier (loan-heavier) markets. Controls include an indicator, *In-market M&A*, equal to one if the acquirer already operated in the given market prior to the M&A and zero otherwise; the market's relevant Hirschman-Herfindahl Index (HHI); natural logarithms of the market's population and income, and its unemployment rate; natural logarithms of acquirer and target size and ratios of nonperforming assets to total assets and total capital to total assets for acquirer and target. Relevant HHI is defined as the deposit (SBF loan) market-year HHI for Columns 1, 3, and 4 (Columns 2, 5, and 6). Panel A tabulates pooled OLS regressions whereas Panel B tabulates merger fixed effect regressions. Although a constant is included in all specification, it is unreported for brevity. Standard errors are clustered by merger. P-values are reported below coefficient estimates. *, **, and *** denote statistical significance at the 10, 5 and 1 percent levels.

Panel A: Pooled OLS

Dependent Variable	Volumes		Rates			
	Deposits	Loans	CD	Checking	Mortgage	Auto
	(1)	(2)	(3)	(4)	(5)	(6)
$\overline{MDH}$	-0.049***	0.210***	-0.006	-0.029	-0.106	-0.380*
	-0.007	(0.006)	(0.899)	(0.405)	(0.190)	(0.068)
In-market M&A	-0.095***	-0.156*	0.007	0.070	0.139*	-0.101
	(0.000)	(0.062)	(0.908)	(0.131)	(0.093)	(0.414)
HHI	0.144*	-0.072	0.175	0.019	-0.483	0.428
	(0.054)	(0.860)	(0.297)	(0.910)	(0.267)	(0.410)
Population	0.061***	0.057***	-0.004	0.008	-0.032	-0.009
	(0.000)	(0.009)	(0.821)	(0.559)	(0.231)	(0.872)
Income	0.140**	-0.089	-0.026	0.102	-0.036	-0.281
	(0.011)	(0.613)	(0.858)	(0.317)	(0.878)	(0.500)
Unemployment	0.001	-0.009	-0.001	0.004	-0.021	-0.036
	(0.843)	(0.699)	(0.931)	(0.686)	(0.460)	(0.692)
Acquirer total assets	0.022*	0.022	-0.032	0.004	-0.037	-0.036
	(0.056)	(0.517)	(0.255)	(0.824)	(0.603)	(0.690)
Acquirer nonperforming assets	-1.071	-8.159***	1.588	1.973**	-2.040	3.540
	(0.361)	(0.002)	(0.305)	(0.028)	(0.344)	(0.508)
Acquirer total capital	0.055	-1.349	-1.975**	0.773	-0.140	5.078**
	(0.942)	(0.286)	(0.030)	(0.283)	(0.929)	(0.040)
Target total assets	-0.033**	-0.097***	0.014	0.019	0.158**	-0.103
	(0.010)	(0.005)	(0.585)	(0.312)	(0.029)	(0.222)
Target nonperforming assets	-1.191**	-0.346	-1.994**	-0.979	1.567	1.006
	(0.032)	(0.893)	(0.029)	(0.130)	(0.228)	(0.817)
Target total capital	1.330	0.804	-3.095***	-1.847***	6.095***	-1.439
	(0.162)	0	(0.003)	(0.010)	(0.009)	(0.554)
Observations	4,380	2,359	684	632	844	704
R^2	0.103	0.105	0.921	0.544	0.738	0.497
Fixed effects	None	None	None	None	None	None

Panel B: Merger fixed effects						
	Volumes		Rates			
Dependent Variable	Deposits	Loans	CD	Checking	Mortgage	Car
	(1)	(2)	(3)	(4)	(5)	(6)
$\overline{MDH}$	-0.049***	0.185***	-0.028	-0.055**	-0.046	-0.158
	(0.002)	(0.000)	(0.359)	(0.011)	(0.441)	(0.400)
In-market merger	-0.087***	-0.372***	-0.117*	-0.014	-0.069	-0.025
	(0.004)	(0.000)	(0.092)	(0.550)	(0.304)	(0.767)
Concentration	0.006	-0.026	-0.257*	-0.234**	-0.051	0.231
	(0.925)	(0.938)	(0.058)	(0.040)	(0.708)	(0.396)
Population	0.064***	0.066***	-0.022*	-0.003	-0.006	0.019
	(0.000)	(0.000)	(0.059)	(0.488)	(0.698)	(0.601)
Income	0.080	0.073	0.068	0.072	-0.018	-0.176
	(0.245)	(0.545)	(0.302)	(0.166)	(0.893)	(0.374)
Unemployment	-0.003	0.011	-0.003	0.004	0.000	0.044
	(0.482)	(0.196)	(0.592)	(0.310)	(0.998)	(0.146)
Observations	7,226	4,214	1,303	1,234	1,650	1,345
R^2	0.042	0.030	0.017	0.016	0.006	0.023
Fixed Effects	Merger	Merger	Merger	Merger	Merger	Merger
Mergers	2,588	1,089	475	436	345	405

Table A4: Acquirer deposit and loan volumes and rates around M&As

This table reports multivariate regression estimates of how acquirers operate in target markets around acquisitions. It summarizes bank-market-year level tests of six consumer welfare measures the universe of ownership-changing U.S. bank M&As between 1998 and 2016. Dependent variables in the first two columns are natural logarithms of a bank-market-year's deposit volumes and small business and farm (SBF) loan issuances, respectively. The dependent variable in Column 3 (4, 5, and 6) is a bank-market-year's advertised rate on a 1-year, $10,000 minimum CD rate ($0 minimum interest checking account, $175,000 maximum 15-year fixed-rate mortgage, 5 year fixed-rate auto loan). The first independent variable of interest, M&A, is an indicator equal to one if the given bank acquired another bank which operated in the given market over the past three years and zero, otherwise. The second independent variable of interest, $\overline{MDH}$, measures a market-year's deposit heaviness. $\overline{MDH}$ is computed by averaging the last three years' residuals from within-year, quadratic, market-level regressions of log deposit volume on log loan volume and its square. Higher (lower) values denote deposit-heavier (loan-heavier) markets. The third independent variable of interest interacts these two. Controls include an indicator, *In-market M&A*, equal to one if the acquirer already operated in the given market prior to the M&A and zero otherwise; the market-year's relevant Hirschman-Herfindahl Index (HHI); natural logarithms of the market-year's population and income; the market-year's unemployment rate; the natural logarithm of the bank-year's size, and ratios of the nonperforming assets to total assets and total equity to total assets. Relevant HHI is defined as the deposit (SBF loan) market-year HHI for Columns 1, 3, and 4 (Columns 2, 5, and 6). Panel A tabulates bank-year fixed effect regressions whereas Panel B tabulates bank-market plus year fixed effect regressions. Standard errors are clustered by bank-year and bank-market, respectively. Although a constant is included in all specifications, it is unreported for brevity. P-values are reported below coefficient estimates. *, **, and *** denote statistical significance at the 10, 5 and 1 percent levels.

Panel A: Bank-Year Fixed Effects

	Volumes		Rates			
Dependent Variable	Deposits	Loans	CD	Checking	Mortgage	Auto
	(1)	(2)	(3)	(4)	(5)	(6)
M&A	-0.108***	-0.157***	0.001	0.004**	-0.002	0.014
	(0.000)	(0.000)	(0.693)	(0.015)	(0.713)	(0.474)
$\overline{MDH}$	0.237***	-0.157***	-0.002	0.005***	-0.002	0.009
	(0.000)	(0.000)	(0.129)	(0.000)	(0.457)	(0.104)
M&A x $\overline{MDH}$	-0.179***	0.076***	0.000	0.001	0.008	-0.007
	(0.000)	(0.000)	(0.900)	(0.704)	(0.119)	(0.585)
In-market merger	1.244***	0.992***	0.011*	-0.010**	-0.020**	-0.006
	(0.000)	(0.000)	(0.056)	(0.018)	(0.028)	(0.777)
HHI	0.413***	0.730***	-0.018***	-0.002	-0.008	0.004
	(0.000)	(0.000)	(0.000)	(0.496)	(0.433)	(0.819)
Population	0.349***	0.447***	-0.002**	-0.002***	-0.003***	-0.004*
	(0.000)	(0.000)	(0.030)	(0.000)	(0.006)	(0.051)
Income	0.426***	0.457***	0.008*	-0.007***	-0.001	-0.001
	(0.000)	(0.000)	(0.054)	(0.003)	(0.864)	(0.959)
Unemployment	0.010***	-0.010***	0.000	-0.000	0.000	0.001
	(0.000)	(0.000)	(0.935)	(0.454)	(0.710)	(0.703)
Observations	383,024	383,024	197,263	190,036	98,939	119,234
R^2	0.175	0.207	0.001	0.002	0.001	0.000
Fixed Effects	BY	BY	BY	BY	BY	BY
Bank-Years	155,063	155,063	81,571	77,149	24,461	38,672

Panel B: Bank-Market Fixed Effects

	Volumes		Rates			
Dependent Variable	Deposits	Loans	CD	Checking	Mortgage	Car
	(1)	(2)	(3)	(4)	(5)	(6)
M&A	0.012***	-0.048**	-0.002	-0.008**	0.021**	0.029**
	(0.004)	(0.011)	(0.557)	(0.029)	(0.043)	(0.048)
$\overline{MDH}$	0.011**	-0.270***	-0.005	-0.015***	-0.003	0.014
	(0.015)	(0.000)	(0.199)	(0.003)	(0.846)	(0.541)
M&A x $\overline{MDH}$	-0.035***	0.166***	0.004	0.013**	-0.023	-0.014
	(0.000)	(0.000)	(0.328)	(0.029)	(0.143)	(0.559)
In-market M&A	0.271***	0.302***	-0.025***	-0.019**	0.021	0.043
	(0.000)	(0.000)	(0.007)	(0.022)	(0.229)	(0.152)
HHI	-0.247***	0.317***	-0.018	0.061	-0.016	-0.080
	(0.000)	(0.006)	(0.606)	(0.119)	(0.771)	(0.211)
Population	1.447***	-0.479***	-0.128***	0.484***	0.726***	-0.229
	(0.000)	(0.007)	(0.000)	(0.000)	(0.000)	(0.250)
Income	0.143***	0.003	0.063***	-0.029	0.242***	0.179*
	(0.000)	(0.974)	(0.003)	(0.207)	(0.004)	(0.077)
Unemployment	-0.004***	-0.002	-0.004***	-0.002	0.009***	0.040***
	(0.001)	(0.648)	(0.000)	(0.112)	(0.008)	(0.000)
Bank total assets	0.444***	0.727***	0.024***	0.038***	-0.010	0.025*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.190)	(0.075)
Nonperforming assets	1.418***	0.759**	-0.725***	0.003	-1.259***	2.373***
	(0.000)	(0.012)	(0.000)	(0.966)	(0.000)	(0.000)
Total capital	-2.560***	2.252***	0.417***	0.486***	-0.276**	0.106
	(0.000)	(0.000)	(0.000)	(0.000)	(0.032)	(0.660)
Observations	296,045	296,045	157,165	151,054	66,031	90,473
R^2	0.372	0.055	0.962	0.774	0.811	0.620
Fixed Effects	BM & Y	BM & Y	BM & Y	BM & Y	BM & Y	BM & Y
Bank-Markets	33,271	33,271	21,113	20,685	14,674	18,064

Table A5: Acquirer deposit and loan volumes around M&As (robustness)

This table modifies the baseline specification in Table 4 for various robustness concerns and reports the coefficients of interest. Although all other variables from Table 4 are included, they are unreported here for brevity. The first two columns recalculate $\overline{MDH}$ using within-state-year regressions instead of within-year regressions. The second two columns recalculate $\overline{MDH}$ using home mortgage loan volume instead of small business sand farm (SBF) loan volume and use the home mortgage market-year's Hirschman-Herfindahl Index (HHI) instead of the SBF market-year's HHI. The fourth column also swaps home mortgage loans for SBF loans as dependent variable. The fifth and sixth columns exclude observations related to markets with no SBF loans. The seventh and eighth columns winsorize $\overline{MDH}$ at the 1 percent tails. The ninth and tenth columns drop all observations within the crisis years (2008-2010). The final two columns drop all observations of banks greater than $1 billion (in 2016 dollars). Although all variables from Table 4, including the constant, are included in Table 5 specifications, they are unreported for brevity. Panel A tabulates bank-year fixed effect regressions whereas Panel B tabulates bank-market plus year fixed effect regressions. Standard errors are clustered by bank-year and bank-market, respectively. P-values are reported below coefficient estimates. *, **, and *** denote statistical significance at the 10, 5 and 1 percent level.

Panel A: Bank-year fixed effects

Dependent Variable	State-year MDH		Drop 0-Loan markets		Winsorize 1% tails		Mortgage loans		Drop crisis years		Drop banks>$1 bn	
	Deposits	Loans	Deposits	Loans	Deposits	Loans	Deposits	Loans	Deposits	Loans	Deposits	Loans
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
M&A	-0.123***	-0.166***	-0.131***	-0.169***	-0.112***	-0.144***	-0.122***	-0.193***	-0.113***	-0.152***	-0.072***	-0.060***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
$\overline{MDH}$	0.194***	-0.226***	0.210***	-0.216***	0.244***	-0.169***	0.350***	-0.132***	0.225***	-0.152***	0.378***	0.001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.803)
M&A x $\overline{MDH}$	-0.158***	0.111***	-0.168***	0.042**	-0.174***	0.036*	-0.189***	0.039**	-0.162***	0.063***	-0.263***	0.037**
	(0.000)	(0.000)	(0.000)	(0.023)	(0.000)	(0.064)	(0.000)	(0.031)	(0.000)	(0.001)	(0.000)	(0.016)
Observations	383,024	383,024	359,017	359,017	383,024	383,024	383,024	383,024	320,192	320,192	215,340	215,340
R^2	0.172	0.209	0.185	0.219	0.175	0.208	0.180	0.286	0.176	0.213	0.033	0.018
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	BY	BY	BY	BY	BY	BY	BY	BY	BY	BY	BY	BY
Bank-Years	155,063	155,063	145,360	145,360	155,063	155,063	155,063	155,063	131,376	131,376	131,677	131,677

Panel B: Bank-market plus year fixed effects

Dependent Variable	State-year MDH		Drop 0-Loan markets		Winsorize 1% tails		Mortgage loans		Drop crisis years		Drop banks>$1 bn	
	Deposits	Loans	Deposits	Loans	Deposits	Loans	Deposits	Loans	Deposits	Loans	Deposits	Loans
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
M&A	0.010**	-0.053***	0.010**	-0.014	0.018***	-0.022	0.009*	0.060**	0.011**	-0.044**	0.003	-0.014
	(0.017)	(0.005)	(0.025)	(0.479)	(0.000)	(0.265)	(0.072)	(0.014)	(0.025)	(0.034)	(0.512)	(0.515)
$\overline{MDH}$	0.011**	-0.296***	0.023***	-0.397***	0.008*	-0.336***	0.061***	-0.602***	0.012**	-0.246***	0.004	-0.237***
	(0.025)	(0.000)	(0.001)	(0.000)	(0.071)	(0.000)	(0.000)	(0.000)	(0.013)	(0.000)	(0.391)	(0.000)
M&A x $\overline{MDH}$	-0.033***	0.197***	-0.034***	0.101***	-0.053***	0.108***	-0.028***	-0.027	-0.037***	0.144***	-0.026***	0.182***
	(0.000)	(0.000)	(0.000)	(0.002)	(0.000)	(0.000)	(0.000)	(0.456)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	296,045	296,045	273,025	273,025	296,045	296,045	296,045	296,045	251,407	251,407	214,539	214,539
R^2	0.372	0.055	0.376	0.058	0.372	0.056	0.372	0.075	0.367	0.057	0.518	0.074
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	BM & Y	BM & Y	BM & Y	BM & Y	BM & Y	BM & Y	BM & Y	BM & Y	BM & Y	BM & Y	BM & Y	BM & Y
Bank-Markets	33,271	33,271	32,028	32,028	33,271	33,271	33,271	33,271	33,093	33,093	21,122	21,122

Table A6: Target rival deposit and loan volumes around M&As

This table reports multivariate regression estimates of how targets' competitors operate in target markets around acquisitions. It summarizes bank-market-year level tests of six consumer welfare measures the universe of ownership-changing U.S. bank M&As between 1998 and 2016. Dependent variables in the first two columns are natural logarithms of a bank-market-year's deposit volumes and small business and farm (SBF) loan issuances, respectively. The first independent variable of interest, rival, is an indicator equal to one if the given bank's competitor in the given market was acquired over the past three years and zero, otherwise. The second independent variable of interest, $\overline{MDH}$, measures a market-year's deposit heaviness. $\overline{MDH}$ is computed by averaging the last three years' residuals from within-year, quadratic, market-level regressions of log deposit volume on log loan volume and its square. Higher (lower) values denote deposit-heavier (loan-heavier) markets. The third independent variable of interest interacts these two. Controls include an indicator, *In-market competitor*, equal to one if the acquisition involved an acquirer that already operated in the given market prior to the M&A and zero otherwise; the market-year's relevant Hirschman-Herfindahl Index (HHI); natural logarithms of the market-year's population and income; the market-year's unemployment rate; the natural logarithm of the bank-year's size, and ratios of the nonperforming assets to total assets and total equity to total assets. Relevant HHI is defined as the deposit (SBF loan) market-year HHI for Columns 1, 3, and 4 (Columns 2, 5, and 6). Columns 1 and 2 tabulate bank-year fixed effect regressions whereas Columns 3 and 4 tabulate bank-market plus year fixed effect regressions. Standard errors are clustered by bank-year and bank-market, respectively. Although a constant is included in all specifications, it is unreported for brevity. P-values are reported below coefficient estimates. *, **, and *** denote statistical significance at the 10, 5 and 1 percent levels.

Dependent Variable	Deposits	Loans	Deposits	Loans
	(1)	(2)	(3)	(4)
Rival	-0.071***	0.033***	-0.005	-0.011
	(0.000)	(0.005)	(0.110)	(0.455)
$\overline{MDH}$	0.166***	-0.108***	-0.019***	-0.137***
	(0.000)	(0.000)	(0.000)	(0.000)
Rival x $\overline{MDH}$	0.017	-0.068***	0.013***	-0.026
	(0.178)	(0.000)	(0.002)	(0.143)
In-market competitor	-0.040***	0.020	0.002	0.009
	(0.001)	(0.145)	(0.585)	(0.639)
HHI	0.317***	0.773***	-0.227***	0.349***
	(0.000)	(0.000)	(0.000)	(0.003)
Population	0.348***	0.440***	1.456***	-0.539***
	(0.000)	(0.000)	(0.000)	(0.002)
Income	0.418***	0.455***	0.147***	-0.019
	(0.000)	(0.000)	(0.000)	(0.848)
Unemployment	0.011***	-0.010***	-0.004***	-0.003
	(0.000)	(0.002)	(0.001)	(0.494)
Bank Size			0.452***	0.733***
			(0.000)	(0.000)
Nonperformance Ratio			1.441***	0.789***
			(0.000)	(0.009)
Equity Ratio			-2.527***	2.287***
			(0.000)	(0.000)
Observations	383,024	383,024	296,045	296,045
R^2	0.157	0.198	0.368	0.054
Fixed effects	BY	BY	BM & Y	BM & Y
Bank-years	155,063	155,063	-	-
Bank-markets	-	-	33,271	33,271

Table A7: Market deposit and loan volumes around M&As

This table presents multivariate regression estimates of target market outcomes after an acquisition. It summarizes market-year level tests of deposit and loan volumes over the universe of ownership-changing U.S. bank M&As between 1998 and 2016. The dependent variables in Columns 1, 3, and 4 are natural logarithms of a market-year's deposit volumes; in Columns 2, 5, and 6, they are natural logarithms of a market-year's small business and farm (SBF) loan issuances. The first independent variable of interest, M&A, is an indicator equal to one if an acquisition occurred in the given market over the past three years and zero, otherwise. The second independent variable of interest, $\overline{MDH}$, measures a market-year's deposit heaviness. $\overline{MDH}$ is computed by averaging the last three years' residuals from within-year, quadratic, market-level regressions of log deposit volume on log loan volume and its square. Higher (lower) values denote deposit-heavier (loan-heavier) markets. The third independent variable of interest interacts these two. In Columns 3 and 5 (4 and 6), $\overline{MDH}$ is replaced with an indicator equal to one if the market-year is in the highest (lowest) quartile of $\overline{MDH}$, and zero otherwise; the interaction term changes accordingly. Controls include an indicator, *In-market M&A*, equal to one if the acquirer already operated in the given market prior to the M&A and zero otherwise; the market-year's relevant Hirschman-Herfindahl Index (HHI); natural logarithms of the market-year's population and income; and the market-year's unemployment rate. Relevant HHI is defined as the deposit (SBF loan) market-year HHI for Columns 1, 3, and 4 (Columns 2, 5, and 6). Panel A tabulates state-year fixed effect regressions whereas Panel B tabulates market fixed effect regressions. Standard errors are clustered by state-year and market, respectively. Although a constant is included in all specifications, it is unreported for brevity. P-values are reported below coefficient estimates. *, **, and *** denote statistical significance at the 10, 5 and 1 percent levels.

Panel A: State-year fixed effects

Dependent Variable	Deposits (1)	Loans (2)	Deposits (3)	Deposits (4)	Loans (5)	Loans (6)
	Volumes					
M&A	0.019***	0.659***	0.015***	-0.021***	0.402***	0.991***
	(0.000)	(0.000)	(0.002)	(0.000)	(0.000)	(0.000)
$\overline{MDH}$	0.324***	-2.160***				
	(0.000)	(0.000)				
M&A x $\overline{MDH}$	-0.000	0.532***				
	(0.982)	(0.000)				
Deposit-heavy quartile			0.224***		-1.790***	
			(0.000)		(0.000)	
M&A x Deposit-heavy quartile			-0.028***		1.017***	
			(0.001)		(0.000)	
Loan-heavy quartile				-0.288***		2.442***
				(0.000)		(0.000)
M&A x loan-heavy quartile				0.113***		-0.939***
				(0.000)		(0.000)
In-market M&A	0.125***	-1.381***	0.100***	0.103***	-1.097***	-1.191***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
HHI	-0.061***	-5.845***	-0.406***	-0.325***	-4.026***	-4.529***
	(0.006)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Population	0.911***	1.829***	0.927***	0.915***	1.663***	1.787***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Income	1.100***	0.937***	1.096***	1.090***	0.924***	0.975***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Unemployment	-0.027***	-0.016	-0.023***	-0.023***	-0.027**	-0.030***
	(0.000)	(0.125)	(0.000)	(0.000)	(0.012)	(0.005)
Observations	43,781	43,781	43,781	43,781	43,781	43,781
R^2	0.950	0.503	0.938	0.939	0.451	0.471
Fixed Effects	SY	SY	SY	SY	SY	SY
State-Years	950	950	950	950	950	950

Panel B: Market plus year fixed effects

	Volumes					
Dependent Variable	Deposits	Loans	Deposits	Deposits	Loans	Loans
	(1)	(2)	(3)	(4)	(5)	(6)
M&A	0.010***	0.094***	0.015***	0.010***	-0.096***	0.146***
	(0.000)	(0.001)	(0.000)	(0.000)	(0.005)	(0.000)
$\overline{MDH}$	0.219***	-3.040***				
	(0.000)	(0.000)				
M&A x $\overline{MDH}$	-0.002	0.302***				
	(0.780)	(0.000)				
Deposit-heavy quartile			0.072***		-1.244***	
			(0.000)		(0.000)	
M&A x Deposit-heavy quartile			-0.005		0.444***	
			(0.428)		(0.000)	
Loan-heavy quartile				-0.074***		1.529***
				(0.000)		(0.000)
M&A x loan-heavy quartile				0.018***		-0.424***
				(0.004)		(0.000)
In-market M&A	0.000	0.035	0.002	0.002	0.031	0.033
	(0.995)	(0.335)	(0.748)	(0.747)	(0.421)	(0.395)
HHI	0.354***	-2.381***	0.372***	0.395***	-1.127***	-1.166***
	(0.001)	(0.000)	(0.003)	(0.002)	(0.004)	(0.002)
Population	1.146***	1.976***	1.238***	1.235***	0.901**	1.042***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.012)	(0.003)
Income	0.489***	0.699***	0.532***	0.536***	0.137	0.081
	(0.000)	(0.004)	(0.000)	(0.000)	(0.599)	(0.751)
Unemployment	-0.005***	0.027***	-0.004***	-0.004***	0.014	0.014
	(0.000)	(0.008)	(0.000)	(0.000)	(0.224)	(0.200)
Observations	43,781	43,781	43,781	43,781	43,781	43,781
R^2	0.409	0.205	0.317	0.314	0.072	0.079
Fixed Effects	M & Y	M & Y	M & Y	M & Y	M & Y	M & Y
Markets	2,314	2,314	2,314	2,314	2,314	2,314

Table A8: Market equilibration around M&As

This table presents multivariate regression estimates of deposit-loan imbalances around an acquisition. It summarizes market-year level tests of market deposit heaviness, MDH, changes and levels over the universe of ownership-changing U.S. bank M&As between 1998 and 2016. MDH is computed as the residual of within-year, quadratic, market-year regressions of log deposits on log loans. Higher (lower) values denote deposit-heavier (loan-heavier) markets. The dependent variable in Columns 1 and 5 is the year-over-year difference in MDH; in the remaining columns, it is the MDH level. The first independent variable, M&A, is an indicator equal to one if an acquisition occurred in the given market over the past three years and zero, otherwise. The second independent variable of interest is one of four measures of MDH. The third intersects these two. Columns 1 and 5 measure the MDH independent variable as the one-year lagged MDH. In Columns 2 and 6, it is the average of the past three years' MDH, $\overline{MDH}$. In Columns 3 and 7 (4 and 8), it is an indicator equal to 1 if the market-year falls within the highest (lowest) quartile of MDH. Controls include an indicator, *In-market M&A*, equal to one if the acquirer already operated in the given market prior to the M&A and zero otherwise; the market-year's deposit market Hirschman-Herfindahl Index (HHI); natural logarithms of the market-year's population and income; and the market-year's unemployment rate. Columns 1 through 4 employ state-year fixed effects whereas Columns 5 through 8 employ market plus year fixed effects. Standard errors are clustered by state-year and market, respectively. Although a constant is included in all specifications, it is unreported for brevity. P-values are reported below coefficient estimates. *, **, and *** denote statistical significance at the 10, 5 and 1 percent levels.

	MDH Change (1)	MDH Level (2)	MDH Level (3)	MDH Level (4)	MDH Change (5)	MDH Level (6)	MDH Level (7)	MDH Level (8)
M&A	-0.018***	-0.016***	-0.010	-0.112***	-0.001	-0.011***	0.023***	-0.000
	(0.000)	(0.000)	(0.228)	(0.000)	(0.798)	(0.000)	(0.000)	(0.947)
Lag MDH	-0.081***				-0.360***			
	(0.000)				(0.000)			
M&A x Lag MDH	-0.036***				-0.022***			
	(0.000)				(0.000)			
$\overline{MDH}$		0.995***				0.951***		
		(0.000)				(0.000)		
M&A x $\overline{MDH}$		-0.007				-0.005		
		(0.116)				(0.209)		
Deposit-heavy quartile			0.848***				0.431***	
			(0.000)				(0.000)	
M&A x Deposit-heavy quartile			-0.135***				-0.052***	
			(0.000)				(0.000)	
Loan-heavy quartile				-0.978***				-0.468***
				(0.000)				(0.000)
M&A x loan-heavy quartile				0.241***				0.050***
				(0.000)				(0.000)
In-market M&A	-0.014***	-0.008**	-0.077***	-0.076***	-0.001	-0.006	0.002	-0.002
	(0.000)	(0.032)	(0.000)	(0.000)	(0.794)	(0.118)	(0.840)	(0.810)
HHI	-0.082***	0.022***	-0.983***	-0.729***	0.102	0.045	0.099	0.248*
	(0.000)	(0.003)	(0.000)	(0.000)	(0.117)	(0.212)	(0.416)	(0.051)
Population	0.008***	0.003***	0.049***	0.009***	0.222***	0.088***	0.465***	0.442***
	(0.000)	(0.003)	(0.000)	(0.007)	(0.000)	(0.000)	(0.000)	(0.000)
Income	0.023***	0.024***	0.011	-0.010	0.138***	0.063***	0.231***	0.254***
	(0.007)	(0.000)	(0.538)	(0.557)	(0.000)	(0.000)	(0.000)	(0.000)
Unemployment	0.002*	0.001	0.009***	0.011***	0.001	0.000	0.004**	0.004**
	(0.059)	(0.325)	(0.000)	(0.000)	(0.173)	(0.724)	(0.020)	(0.027)
Observations	43,772	43,781	43,781	43,781	43,772	43,781	43,781	43,781
R^2	0.049	0.934	0.510	0.517	0.192	0.672	0.173	0.160
Fixed Effects	SY	SY	SY	SY	M & Y	M & Y	M & Y	M & Y
State-Years	950	950	950	950				
Markets					2,314	2,314	2,314	2,314

Table B1: Sample composition

Panel A: Observation time series

This table summarizes observational counts of our sample along several key dimensions. Our sample contains enforcement actions (EAs) issued to banks that share at least one director with at least one non-financial firm (NFF) at the time. We retain only the first EA issued to a bank in a 30-day window. Because multiple EAs can be issued to the same bank on the same day, we refer to a bank's receipt of one or several EAs on a given day as an 'event'. We tabulate the number of EAs and events in our sample, by year, as well as how many banks, directors, and NFFs our sample includes each year. We also report the number of NFF-event observations. Below, we tally the total count of annual observations and present the number of distinct observations along each dimension.

(1) Year	(2) EAs	(3) Events	(4) EA recipient banks	(5) Linking directors	(6) Director-linked NFFs	(7) NFF-events
1990	1	1	1	4	2	2
1991	12	11	10	26	27	30
1992	15	10	10	30	34	34
1993	12	7	5	19	17	21
1994	0	0	0	0	0	0
1995	3	2	2	20	26	26
1996	12	6	5	27	31	39
1997	15	11	9	32	42	55
1998	24	18	16	92	112	130
1999	68	49	25	160	194	408
2000	55	50	23	165	202	525
2001	71	48	22	174	212	528
2002	70	38	26	158	197	378
2003	63	52	28	159	210	470
2004	57	45	25	142	194	419
2005	91	70	38	187	237	487
2006	55	45	25	132	194	385
2007	75	60	31	156	209	551
2008	89	77	43	183	236	511
2009	79	72	43	161	199	385
2010	61	51	35	107	127	245
2011	52	47	31	113	136	230
2012	50	43	25	93	114	225
2013	46	39	26	83	98	182
2014	58	45	26	76	88	194
2015	49	40	25	76	93	168
2016	29	27	17	52	64	122
2017	33	26	18	45	54	97
N	1,245	990	590	2,672	3,349	6,847
Distinct units	1,245	990	159	763	792	6,847

Panel B: Director sample

This table classifies and describes bank-linked directors in our sample and in the BoardEx-CRSP intersection. Our sample includes directors who concurrently serve an NFF and a bank that receives an enforcement action (EA). Directors are classified as persons who are or were bank executives at any point over our sample period, are or were NFF executives, or are or were executives of private firms in the 1990-2017 BoardEx universe. We further distinguish between director-NFF-years in which the director is currently an executive and those after the executive appointment ends. The fraction of director-NFF-years in which the director can be classified as 'busy' (i.e. simultaneously serves three or more boards) is reported along with the average of director's age, and the average size, in millions, of the firms they serve. Finally, we report the fraction of directors and director-NFF-years associated with current or former bank executive employment.

Directors linked to an EA recipient bank	Unique persons	Employment	Director-NFF-years	Percent 'busy'	Average age	Average NFF size
Bank executives	132	Current exec.	414	12.32%	58.27	12,348.92
		Former exec.	198	60.61%	64.53	19,951.89
NFF executives	296	Current exec.	1,090	19.82%	59.20	13,397.29
		Former exec.	791	58.53%	64.58	13,868.82
Private firm executives	335	Current exec.	1,236	48.87%	61.18	11,817.24
		Former exec.	355	38.31%	64.41	13,122.93
Total	763		4,084	38.93%	61.46	13,186.72
Current and former bankers	17.30%		14.99%			
BoardEx universe of bank-linked directors						
Bank executives	850	Current exec.	7,491	19.69%	59.10	10,206.13
		Former exec.	3,619	53.69%	62.44	12,874.45
NFF executives	1,558	Current exec.	8,972	22.49%	59.09	10,392.15
		Former exec.	9,942	56.73%	62.88	10,990.36
Private firm executives	2,198	Current exec.	17,033	43.59%	61.66	8,936.38
		Former exec.	5,867	44.64%	65.50	8,224.37
Total	4,606		52,924	39.90%	61.57	9,937.55
Current and former bankers	18.45%		20.99%			
All directors of public firms						
Bank-linked directors			11.07%			

Table B2: NFF stock returns around director-linked bank EAs

This table tests for evidence of shock spillover by measuring cumulative abnormal non-financial firm (NFF) stock returns around director-linked bank enforcement actions (EAs). An NFF is included in our sample if it shares a director with a bank on the day that bank receives an EA. To measure abnormal returns, we adopt the event study framework of Campbell, Lo, and MacKinlay (1997). We estimate the Fama-French three factor model over the 252 trading days ending 21 trading days before EA issuance. Estimated coefficients are applied to actual returns on each of the three factors around the event date. The residual term captures abnormal returns. Row 1 presents our baseline model in which abnormal returns cumulated over the (-1,+1) window, where day 0 is the EA issue date, measure stock price reaction. We also use this window in Rows 4 through 11. In Row 2, we adopt the (-5,+5) window to allow for a longer reaction window. In Row 3, we adopt an asymmetric (-1,+8) window as another possible measure of the full shareholder response. Row 4 winsorizes data at the 1% tails to mitigate against outlying returns driving our results. To reduce the effect of redundant EAs in our sample, the previous tests exclude EAs issued up to one month after another EA to the same bank. We toggle this criterion by dropping it (Row 4) and lengthening it to a 3-month window (Row 5). The following four rows present placebo tests in which the event date is assumed to occur one month earlier or later (Rows 7 and 8, respectively) and three months earlier or later (Rows 9 and 10, respectively). Finally, Row 11 randomly assigns event dates in our sample to NFFs in our sample. For each model, we present the number of observations and mean cumulative abnormal return, as well as statistics from Kolari-Pynnönen's (2010) parametric and Corrado's (1989) nonparametric tests for statistical significance. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Row	Sample	N	Mean CAR	KP-Z	Rank-Z
(1)	Baseline	6,847	-0.17%	-2.30**	-2.56***
(2)	CAR(-5,+5)	6,847	-0.22%	-1.75**	-1.629*
(3)	CAR(-1,+8)	6,847	-0.17%	-1.63*	-1.397*
(4)	Winsorized	6,847	-0.15%	-2.08**	-2.407***
(5)	Not excluding 'redundant' EAs	15,828	-0.12%	-2.90***	-3.427***
(6)	Exclude 3 months	4,590	-0.19%	-1.75**	-2.176**
(7)	PLACEBO: 1 month earlier	6,842	-0.01%	-0.24	-0.52
(8)	PLACEBO: 1 month later	6,832	-0.05%	-1.09	-1.426*
(9)	PLACEBO: 3 months earlier	6,824	-0.04%	-0.25	-0.22
(10)	PLACEBO: 3 months later	6,818	-0.02%	0.044	1.18
(11)	PLACEBO: dates & NFFs scrambled	6,188	0.00%	-0.58	1.12

Table B3: Cross-sectional CAR regressions

This table tests the internal validity of the shock spillover we measure in Table 2 by regressing cumulative abnormal non-financial firm (NFF) stock returns around director-linked bank enforcement actions (EAs) on measures of EA severity. To measure abnormal returns, we adopt the event study framework of Campbell, Lo, and MacKinlay (1997). We estimate the Fama-French three factor model over the 252 trading days ending 21 trading days before EA issuance. Estimated coefficients are applied to actual returns on each of the three factors around the event date. The residual term captures abnormal returns. In Columns 1 through 4, 6, and 8 through 10, the dependent variable is abnormal NFF return cumulated over the three days centered on EA issuance. In Columns 5 and 7, the dependent variable is the NFF return cumulated over the (-1,+8) window. In Columns 1 through 4 and 8 through 10, EA severity is measured as the 3-day cumulative abnormal bank stock return around the EA issuance date. In Column 5, it is measured as the (-1,+8) cumulative abnormal bank stock return. In Columns 6 and 7, it is measured as an indicator equal to one if the EA is a Cease and Desist Order, Written Agreement/Formal Agreement/Consent Order, or Prompt Corrective Actions Directives, defined as severe by Srinivas et al. (2014) and others; zero, otherwise. Columns 2 through 10 include the following NFF controls: CEO duality, % board outsiders, board size, return on assets, size, leverage, and Tobin's Q. Column 3 adds to these covariates indicators for whether the linking director is an NFF insider or a bank insider as well as measures of her tenure on the NFF and bank boards. Column 4 adds bank-level controls which mirror NFF-level ones. Appendix B defines each variable. Columns 1 through 7 and 10 consider the full combination of directors, NFF and EA events, while in Columns 8 and 9, abnormal NFF and bank returns are averaged at the director-NFF-year and bank-NFF-year levels, respectively. Columns 1 through 8 include director-NFF fixed effects while Column 9 uses NFF fixed effects and Column 10 includes no fixed effects. Columns 1 through 9 also employ year dummies. All specifications include a constant unreported for brevity. Standard errors are clustered at the director level in each column except Column 9, where they are clustered at the NFF level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	NFF CAR(-1,+1) (1)	NFF CAR(-1,+1) (2)	NFF CAR(-1,+1) (3)	NFF CAR(-1,+1) (4)	NFF CAR(-1,+8) (5)	NFF CAR(-1,+1) (6)	NFF CAR(-1,+8) (7)	Mean NFF CAR(-1,+1) (8)	Mean NFF CAR(-1,+1) (9)	NFF CAR(-1,+1) (10)
Bank CAR(-1,+1)	0.0654***	0.0662***	0.0663***	0.0754***						0.0520***
	(0.019)	(0.018)	(0.018)	(0.019)						(0.017)
Bank CAR(-1,+8)					0.0598***					
					(0.019)					
Severe EA type						-0.0002	-0.0085***			
						(0.002)	(0.003)			
Mean bank CAR(-1,+1)								0.0765**	0.0666**	
								(0.031)	(0.033)	
NFF CEO duality		0.0009	0.0010	0.0011	-0.0058**	0.0010	-0.0057**	0.0009	0.0016	-0.0009
		(0.001)	(0.001)	(0.001)	(0.003)	(0.001)	(0.003)	(0.002)	(0.002)	(0.001)
NFF % board outsiders		0.0005	-0.0002	0.0037	0.0228*	0.0013	0.0247**	-0.0008	0.0040	0.0098***
		(0.007)	(0.007)	(0.007)	(0.012)	(0.007)	(0.012)	(0.010)	(0.009)	(0.003)
NFF board size		-0.0003	-0.0003	-0.0003	-0.0000	-0.0003	0.0000	-0.0007	-0.0007	0.0001
		(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)	(0.000)
NFF return on assets		0.0001	0.0001	0.0001	-0.0003	0.0001	-0.0003	-0.0001	-0.0001	0.0002***
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
NFF size		0.0006	0.0006	0.0002	-0.0032	0.0005	-0.0033	0.0012	0.0009	0.0005
		(0.002)	(0.002)	(0.002)	(0.005)	(0.002)	(0.005)	(0.003)	(0.003)	(0.000)
NFF leverage		-0.0184**	-0.0185**	-0.0187**	-0.0185	-0.0184**	-0.0183	-0.0232*	-0.0175	-0.0057*
		(0.008)	(0.008)	(0.009)	(0.016)	(0.008)	(0.016)	(0.012)	(0.011)	(0.003)
NFF Tobin's Q		-0.0001	-0.0002	-0.0000	-0.0020	-0.0002	-0.0019	0.0005	0.0004	-0.0000
		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)
Bank CEO duality				0.0045**						
				(0.002)						
Bank % board outsiders				-0.0058						
				(0.005)						
Bank board size				-0.0005***						
				(0.000)						

Bank return on assets				0.0010						
				(0.001)						
Bank size				0.0048**						
				(0.002)						
Bank leverage				-0.0286*						
				(0.015)						
Bank Tobin's Q				-0.0058						
				(0.017)						
Inside NFF director			-0.0023							
			(0.003)							
NFF director tenure			-0.0008*							
			(0.000)							
Inside bank director			0.0046							
			(0.005)							
Bank director tenure			0.0001							
			(0.000)							
N	7,946	7,137	7,137	6,815	7,137	7,137	7,137	3,678	3,210	7,137
Director-NFFs	1,295	1,181	1,181	1,128	1,181	1,181	1,181	1,181		
NFFs									729	
F-statistic	2.44***	2.48***	2.4***	2.71***	2.8***	1.78***	2.37***	2.28***	1.26	2.47***
Aggregation	None	None	None	None	None	None	None	Dir.-NFF-yr.	Bank-NFF-yr.	None
Controls	None	NFF	NFF & dir.	NFF & bank	NFF	NFF	NFF	NFF	NFF	NFF
Fixed effects	Dir. x NFF & yr.	Dir. x NFF & yr.	Dir. x NFF & yr.	Dir. x NFF & yr.	Dir. x NFF & yr.	Dir. x NFF & yr.	Dir. x NFF & yr.	Dir. x NFF & yr.	NFF & yr.	None
Standard errors clusters	Director	Director	Director	Director	Director	Director	Director	Director	NFF	Director

Table B4: Bank-linked director role on NFF boards

This table assesses the linking directors' roles on NFF boards. Using a probit model of outside directors serving NFF boards, we measure whether a bank-linked director (BLD) is more likely than other outside directors to serve important roles on the NFF board. We also include an indicator, NLD, equal to one if the director links the NFF to another NFF but not to a bank; zero, otherwise. The reference category is a director who holds only one outside appointment. A director is said to serve an important role if she serves on any committee (Column 1), the audit or compensation committee (Column 2), the audit and compensation committees (Column 3), chairs any committee (Column 4), or is designated a financial expert (Column 5). All specifications include the following director-level controls which are defined in Appendix B: female director, director age, NFF co-opted director, NFF independent director, and NFF rank. They also include the following firm-level controls: NFF CEO duality, NFF CEO tenure, NFF % board outsiders, NFF board size, NFF market capitalization, NFF return on assets, and NFF Tobin's Q. All columns employ industry and year fixed effects. At the bottom of the table, we present the results of a Chi^2 test of equality between BLD and NLD. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	Serves any committee	Audit or compliance committee	Audit and compliance committee	Committee chair	Financial expert
	(1)	(2)	(3)	(4)	(5)
BLD	0.009***	0.026***	0.012*	0.092***	0.084***
	(0.003)	(0.005)	(0.006)	(0.007)	(0.007)
NLD	0.003	0.021***	-0.005	0.077***	0.062***
	(0.002)	(0.003)	(0.004)	(0.005)	(0.005)
Female director	0.014***	0.001	-0.018***	-0.040***	-0.053***
	(0.003)	(0.005)	(0.006)	(0.006)	(0.007)
Director age	0.001***	0.001***	0.002***	0.003***	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
NFF co-opted director	-0.000	0.000	-0.008**	-0.035***	0.003
	(0.002)	(0.003)	(0.004)	(0.005)	(0.005)
NFF independent director	0.231***	0.462***	0.464***	0.400***	0.552***
	(0.003)	(0.004)	(0.008)	(0.008)	(0.016)
NFF rank	0.006***	0.000	0.007***	0.004	0.009***
	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)
NFF CEO duality	-0.001***	-0.003***	0.001*	0.006***	-0.004***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
NFF CEO tenure	0.008***	0.004	0.005	0.006	-0.010**
	(0.002)	(0.003)	(0.003)	(0.004)	(0.004)
NFF % board outsiders	0.001***	0.001***	0.001***	-0.002***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
NFF board size	-0.003	-0.223***	-0.507***	-0.175***	-0.213***
	(0.007)	(0.011)	(0.013)	(0.016)	(0.016)
NFF market capitalization	-0.001**	-0.016***	-0.037***	-0.017***	-0.016***
	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)

NFF return on assets	0.010***	0.003**	-0.013***	0.001	0.011***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
NFF Tobin's Q	0.000	0.000***	0.000**	0.000	0.000*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
N	252,315	252,315	252,315	252,315	236,216
Directors	55,484	55,484	55,484	55,484	53,648
Fixed effects	Industry & yr.	Industry & yr.	Industry & yr.	Industry & yr.	Industry & yr.
Chi² statistics					
Full model	11,934.87***	13,108.12***	7,594.62***	4,441.27***	2,692.84***
BLD vs. NLD	4.43**	1.24	7.28***	4.88**	10.28***

Table B5: Resource reallocation on NFF boards following EAs

This table reports tests of outside directors' enforcement action (EA) induced resource reallocation on NFF boards. Director resource expenditure, measured with several proxies, is modeled as a function of whether the outside director concurrently serves on the board of an enforced bank (EBLD), a non-enforced bank (NEBLD), or another NFF (NBLD). The reference category is a director who holds only one outside appointment. A director-linked bank is considered enforced if it receives an EA in the current fiscal year or in the last five months of the previous fiscal year. Resource expenditure is measured as the log of cash compensation in Columns 1 through 3, total compensation in Column 4, and the Board Function Index (BFI) in Columns 5 and 6. The BFI, introduced by Fedaseyeu et al. (2018), adds the number of committee seats a director holds as chair or member to an indicator equal to one if she serves as board chairman; zero, otherwise. Columns 1 and 5 include no controls while the remaining columns control for the following director characteristics, defined in Appendix B: financial expert, female director, director age, NFF co-opted director, NFF independent director, NFF rank, NFF director tenure. Columns 2, 3, and 4 control for NFF committee memberships through indicators of whether the director serves on or chairs the audit, compensation, or corporate governance committees. Columns 2 and 4 also include an indicator for whether the director chairs the board of directors while Column 3 excludes chairpersons. Column 6 controls for a cash-only compensation indicator and the log of total compensation. Each column is estimated using NFF-year fixed effects. All specifications include a constant unreported for brevity. At the bottom of the table, statistics are presented for F-tests of the NEBLD coefficient's equality with the EBLD and the NBLD coefficients. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	Cash comp. (1)	Cash comp. (2)	Cash comp. (3)	Total comp. (4)	BFI (5)	BFI (6)
EBLD	0.017	-0.007	-0.007	0.005	0.118***	0.072***
	(0.012)	(0.012)	(0.011)	(0.013)	(0.026)	(0.027)
NEBLD	0.056***	0.029***	0.026***	0.029***	0.163***	0.086***
	(0.005)	(0.005)	(0.005)	(0.006)	(0.014)	(0.015)
NLD	0.033***	0.013***	0.010***	0.011***	0.136***	0.071***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.009)	(0.010)
NFF Board chair		0.581***		0.613***		
		(0.019)		(0.020)		
NFF audit comm. chair		0.129***	0.132***	0.152***		
		(0.005)	(0.005)	(0.005)		
NFF compensation comm. chair		0.076***	0.077***	0.099***		
		(0.005)	(0.005)	(0.005)		
NFF corp. gov. comm. chair		0.075***	0.074***	0.083***		
		(0.007)	(0.007)	(0.007)		
NFF audit comm. member		0.067***	0.069***	0.073***		
		(0.005)	(0.005)	(0.005)		
NFF comp. comm. member		0.040***	0.045***	0.042***		
		(0.004)	(0.004)	(0.004)		
NFF corp. gov. comm. member		0.030***	0.034***	0.025***		
		(0.005)	(0.005)	(0.005)		

NFF financial expert		0.016***	0.014***	0.014***		0.076***
		(0.005)	(0.005)	(0.005)		(0.009)
Female director		-0.006	-0.006	-0.004		-0.062***
		(0.004)	(0.004)	(0.004)		(0.009)
Director age		0.002***	0.002***	0.003***		0.003***
		(0.000)	(0.000)	(0.000)		(0.001)
NFF co-opted director		-0.001	0.000	-0.003		-0.027*
		(0.006)	(0.006)	(0.006)		(0.014)
NFF independent director		0.003	0.028*	-0.117***		1.204***
		(0.016)	(0.016)	(0.021)		(0.027)
NFF rank		-0.001	-0.001	-0.000		0.004
		(0.002)	(0.002)	(0.002)		(0.006)
NFF director tenure		0.003***	0.004***	0.005***		0.016***
		(0.000)	(0.000)	(0.000)		(0.001)
Cash-only compensation indicator						-0.007
						(0.014)
Total compensation						0.371***
						(0.020)
N	39,722	39,583	38,493	39,583	39,722	39,523
NFF-years	5,344	5,344	5,344	5,344	5,344	5,336
Fixed effects	NFF x yr.	NFF x yr.	NFF x yr.	NFF x yr.	NFF x yr.	NFF x yr.
F-statistics						
Full model	49.23***	163.00***	123.30***	208.70***	93.36***	291.60***
EBLD vs. NEBLD	9.33***	8.65***	8.84***	3.38**	2.48	0.25
NEBLD vs. NBLD	18.16***	9.39***	8.88***	10.73***	3.61*	1.21

Table B6: Resource reallocation on bank boards following EAs

This table reports tests of outside directors' enforcement action (EA) induced resource reallocation on bank boards. Director resource expenditure is modeled as a function of whether the bank receives an EA in the current fiscal year or in the last five months of the previous year. Resource expenditure is measured as the log of cash compensation in Columns 1 through 3 and total compensation in Column 4. In Column 3, the EA indicator is subdivided into indicators for whether or not the bank received a Cease and Desist Order, Written Agreement/Formal Agreement/Consent Order, or Prompt Corrective Actions Directives. These EAs are considered severe by Srinivas et al. (2014) and others. Columns 2 through 4 control for whether the director serves on or chairs the audit, compensation, or corporate governance committees or whether she chairs the board of directors and for the following director and bank characteristics: financial expert, female director, director age, bank co-opted director, bank independent director, bank rank, bank CEO duality, bank CEO tenure, bank % board outsiders, bank board size, bank return on assets, bank size, and bank Tobin's Q. All specifications include a constant unreported for brevity. Appendix B defines each variable. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	Cash comp. (1)	Cash comp. (2)	Cash comp. (3)	Total comp. (4)
EA	0.111**	0.176***		0.118**
	(0.056)	(0.059)		(0.052)
Non-severe EA			0.097*	
			(0.054)	
Severe EA			0.373***	
			(0.105)	
Bank board chair		-0.160	-0.148	0.047
		(0.318)	(0.316)	(0.215)
Bank audit committee chair		-0.029	-0.027	-0.059
		(0.105)	(0.105)	(0.102)
Bank compensation committee chair		0.130	0.130	0.114
		(0.125)	(0.124)	(0.117)
Bank corporate governance committee chair		0.064	0.037	0.067
		(0.140)	(0.140)	(0.140)
Bank audit committee member		-0.036	-0.030	-0.046
		(0.104)	(0.104)	(0.100)
Bank compensation committee member		0.064	0.064	0.066
		(0.088)	(0.087)	(0.086)
Bank corporate governance committee member		0.062	0.073	0.039
		(0.084)	(0.085)	(0.076)
Bank financial expert		0.203	0.204	0.223*
		(0.130)	(0.131)	(0.116)
Director age		0.025*	0.024*	0.032**
		(0.014)	(0.015)	(0.013)
Bank co-opted director		-0.106	-0.124	-0.032
		(0.120)	(0.119)	(0.106)

Bank independent director		0.571	0.570	0.637
		(0.649)	(0.628)	(0.629)
Bank rank		-0.099	-0.105	-0.045
		(0.073)	(0.072)	(0.067)
Bank CEO duality		-0.511***	-0.523***	-0.347***
		(0.094)	(0.095)	(0.088)
Bank CEO tenure		-0.004	-0.002	-0.007
		(0.007)	(0.007)	(0.006)
Bank % board outsiders		3.504***	3.414***	3.172***
		(0.848)	(0.819)	(0.766)
Bank board size		-0.017	-0.018	-0.006
		(0.014)	(0.014)	(0.013)
Bank return on assets		0.224***	0.226***	0.209***
		(0.065)	(0.065)	(0.061)
Bank size		-0.250	-0.266	-0.113
		(0.201)	(0.201)	(0.172)
Bank Tobin's Q		-3.410**	-3.409**	-3.010*
		(1.649)	(1.639)	(1.568)
Risk-adjusted capital ratio		0.286***	0.276***	0.288***
		(0.065)	(0.065)	(0.062)
Non-performing asset ratio		3.504***	3.414***	3.172***
		(0.848)	(0.819)	(0.766)
N	2,547	2,547	2,547	2,547
Director-banks	407	407	407	407
Fixed effects	Dir. x bank	Dir. x bank	Dir. x bank	Dir. x bank
F-statistic	5.68***	4.25***	4.14***	3.98***

Table B7: Lending subset event studies

This table reports tests of whether lending relationships between enforcement action (EA) recipient bank and director-linked non-financial firm (NFF) can explain the NFF's negative cumulative abnormal stock returns around bank EAs. An NFF is included in our sample if it shares a director with a bank on the day that bank receives an EA. To measure abnormal returns, we adopt the event study framework of Campbell, Lo, and MacKinlay (1997). We estimate the Fama-French three factor model over the 252 trading days ending 21 trading days before EA issuance. Estimated coefficients are applied to actual returns on each of the three factors around the event date. The residual term captures abnormal returns. Data limitations restrict these tests to the 2008-2017 period, over which lending relationships can be collected from SEC 8-K filings. Row 1 replicates our baseline results from Table 2 over the 2008-2017 sample period. Row 2 reruns the event study over the subset of observations in which a lending relationship between NFF and the director-linked EA-recipient bank has been reported. Row 3 reproduces the event study over the subset without a documented lending relationship. Row 4 (5) reproduces the event study over the subset in which the EA contains (does not contain) a clause which mentions lending. Row 6 (7) reproduces the event study over the subset in which the interlocking director is (is not) a bank executive. For each model, we present the number of observations and mean cumulative abnormal return, as well as statistics from Kolari-Pynnönen's (2010) parametric and Corrado's (1989) nonparametric tests for statistical significance. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Row	Sample	N	Mean CAR	KP-Z	Rank-Z
(1)	2008-2017 Baseline	2,066	-0.15%	-1.67**	-2.27**
(2)	Lender-linked	148	-0.33%	-1.15	0.962
(3)	Non-lender linked	1,918	-0.14%	-1.40*	-2.62***
(4)	Lending restriction	84	-0.17%	-0.27	-0.34
(5)	No lending restriction	6,761	-0.19%	-2.44***	-2.63***
(6)	Bank insider link	642	-0.15%	-0.89	0.029
(7)	Bank outsider link	6,205	-0.17%	-2.16**	-2.75***

Table B8: Director reputation effects of bank enforcement

This table reports tests of EAs' impact on future director appointments. In Columns 1 and 2, we present estimates from an ordered logit model wherein the number of outside appointments held by a director is a function of whether, three years ago, the NFF director concurrently served on the board of an enforced bank (EBLD), a non-enforced bank (NEBLD), on another NFF (NBLD). Because the number of board appointments is highly persistent, we sample every third year, per Wintoki, Linck, and Netter (2012). In Column 3, we present marginal effects from a probit model in which the dependent variable equals one if the director remains in our sample for the next three years; zero, otherwise. Columns 2 and 3 also include the following director and NFF controls: female director, director age, NFF director tenure, NFF size, NFF return on assets, NFF Tobin's Q, and NFF leverage. Appendix B defines each variable. All specifications include a constant unreported for brevity. At the bottom of the table, statistics are presented for Chi^2-tests of the EBLD coefficient's equality with the NEBLD coefficient. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	Future outside appointments		
	(1)	(2)	(3)
EBLD	4.569***	4.530***	0.005
	(0.162)	(0.160)	(0.010)
NEBLD	4.468***	4.395***	-0.013
	(0.165)	(0.173)	(0.008)
NLD	5.371***	5.252***	0.029***
	(0.036)	(0.040)	(0.002)
Female director		0.454***	0.008***
		(0.043)	(0.003)
Director age		0.047***	-0.001***
		(0.001)	(0.000)
NFF director tenure		-0.075***	-0.002***
		(0.002)	(0.000)
NFF size		0.091***	-0.002***
		(0.007)	(0.000)
NFF return on assets		0.001***	0.000***
		(0.001)	(0.000)
NFF Tobin's Q		-0.002	0.003***
		(0.005)	(0.001)
NFF leverage		-0.299***	-0.019***
		(0.055)	(0.004)
N	94,100	81,302	103,190
Directors	33,013	30,346	30,346
Chi^2 statistic			
Full model	27,492.19***	22,231.95***	3,992.31***
EBLD vs. NEBLD	0.22	0.39	1.78

Table C1: Descriptive statistics for POREA and nonPOREA subsamples

This table presents summary statistics for POREA and nonPOREA recipients in my sample. It reports sample size, mean, standard deviation, and 1st, 2nd, and 3rd quartile values for accounting and trading variables. *TA* are a firm's total assets; *T1C* is its ratio of Tier 1 capital to risk-weighted assets; *Cash* is its ratio of cash to total assets; *NPA* is its ratio of non-performing assets to total assets; *ALLL* is its ratio of allowance for loan and lease losses to total assets; *QNI* is its ratio of quarterly net income to total assets; *ShrTO* is share turnover in 252 trading days preceding EA receipt; *BHR* are buy-and-hold-returns over 252 trading days preceding EA receipt; and *MTB* is its ratio of market value of assets to book value of assets. Appendix I defines variables in detail. Mean and median differences are tested for significance using t-tests and Kolmogorov-Smirnov tests, respectively. Two sided p-values are presented. **, and *** denote statistical significance at the 5% and 1% levels, respectively.

	POREA						nonPOREA						Difference tests	
Variable	N	Mean	St.Dev.	Q1	Median	Q3	N	Mean	St.Dev.	Q1	Median	Q3	t test p-value	KS test p-value
TA	229	2,622	6,207	436	907	2,115	90	50,823	169,782	1,037	3,619	18,030	0.008***	0.000***
T1C	229	0.084	0.032	0.064	0.082	0.103	81	0.111	0.029	0.091	0.108	0.130	0.000***	0.000***
Cash	229	0.059	0.048	0.023	0.048	0.082	89	0.051	0.046	0.026	0.037	0.057	0.168	0.017**
NPA	219	0.061	0.039	0.031	0.057	0.085	79	0.023	0.053	0.003	0.009	0.020	0.000***	0.000***
ALLL	229	0.021	0.011	0.014	0.019	0.027	89	0.012	0.007	0.007	0.010	0.014	0.000***	0.000***
QNI	228	0.011	0.180	-0.009	-0.002	0.000	90	0.001	0.005	0.000	0.002	0.003	0.373	0.000***
ShrTO	229	907	1,398	211	416	1,030	109	1,062	1,182	245	721	1,406	0.290	0.002***
MTB	229	0.972	0.039	0.951	0.969	0.988	86	1.032	0.064	0.993	1.033	1.059	0.000***	0.000***
BHR	229	-0.329	0.530	-0.675	-0.400	-0.131	109	0.001	0.413	-0.189	0.042	0.187	0.000***	0.000***

Table C2: Descriptive statistics for treated and control subsamples

This table presents summary statistics for treated and control POREA recipients in my sample. It reports sample size, mean, standard deviation, and 1st, 2nd, and 3rd quartile values for accounting and trading variables. *TA* are a firm's total assets; *T1C* is its ratio of Tier 1 capital to risk-weighted assets; *Cash* is its ratio of cash to total assets; *NPA* is its ratio of non-performing assets to total assets; *ALLL* is its ratio of allowance for loan and lease losses to total assets; *QNI* is its ratio of quarterly net income to total assets; *ShrTO* is share turnover in 252 trading days preceding EA receipt; *BHR* are buy-and-hold-returns over 252 trading days preceding EA receipt; *MTB* is its ratio of market value of assets to book value of assets; *Inside* is the fraction common stock held by insiders, as defined by the SEC; *Z-score* is the number of standard deviations of *QNI* by which a firm's equity capital exceed its default threshold. Appendix I defines variables in detail. Mean and median differences are tested for significance using t-tests and Kolmogorov-Smirnov tests, respectively. Two sided p-values are presented. **, and *** denote statistical significance at the 5% and 1% levels, respectively.

	Treated						Control						Difference tests	
Variable	N	Mean	St.Dev.	Q1	Median	Q3	N	Mean	St.Dev.	Q1	Median	Q3	t test p-value	KS test p-value
TA	169	3,253	7,094	598	1,117	2,635	60	846	1,122	255	475	899	0.000***	0.000***
T1C	169	0.084	0.031	0.062	0.081	0.101	60	0.086	0.034	0.066	0.085	0.103	0.723	0.764
Cash	169	0.060	0.049	0.024	0.048	0.082	60	0.059	0.048	0.021	0.048	0.082	0.975	0.998
NPA	160	0.062	0.041	0.035	0.058	0.086	59	0.059	0.036	0.028	0.056	0.082	0.725	0.918
ALLL	169	0.021	0.012	0.014	0.020	0.027	60	0.021	0.010	0.016	0.019	0.024	0.849	0.652
QNI	169	-0.006	0.012	-0.008	-0.002	0.000	59	0.062	0.351	-0.010	-0.002	0.000	0.143	0.795
ShrTO	169	1,025	1,571	224	446	1,107	60	576	607	184	300	710	0.002***	0.318
MTB	169	0.974	0.038	0.951	0.969	0.986	60	0.967	0.041	0.951	0.968	0.990	0.295	0.914
BHR	169	-0.319	0.590	-0.678	-0.440	-0.153	60	-0.358	0.307	-0.628	-0.323	-0.113	0.514	0.281

Table C3: Event study results

This table displays results from four event studies. Rows 1 and 2 of Panel A measure cumulative abnormal 3-day returns (CARs) around POREA and nonPOREA issuances, respectively; rows 3 and 4 measure CARs around treated and control POREAs, respectively. Mean, precision-weighted (by the inverse of estimation model standard errors) and median CARs are displayed as are p-values from the Kolari and Pynnönen and Wilcoxon Rank Sum tests for statistical significance. Panel B displays treated and control group means and medians and one sided p-values from a t test (Wilcoxon Ranked Sum test) for significant differences in means (medians). *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Event study results

Row	Sample	N	CARs			Significance	
			Mean	PW	Median	KP z	WRS w
(1)	POREA	229	-2.35%	-1.08%	-0.89%	*0.019***	*0.001****
(2)	nonPOREA	109	0.63%	0.64%	0.22%	*0.082**	*0.118*
(3)	Treated	169	-3.52%	-1.65%	-1.38%	*0.006****	*0.001****
(4)	Control	60	0.93%	0.67%	-0.24%	*0.222*	*0.497*

Panel B: Statistical group-wise difference between treated and control groups

Treated mean	Control mean	t-test one sided p-value	Treated median	Control median	WRS one sided p-value
-3.52%	0.93%	4.45%	0.0063***	-1.38%	-0.24%

Table C4: Statistical pair-wise difference between treated and control units

This table displays mean and median CAR differences between matched samples of treated and control firms along with one sided p-values from t tests (Wilcoxon Signed Rank tests) of mean (median) differences. Panel A uses 1 to 1 matching whereas Panel B allows each control unit to match up to 3 treated units. Rows 1, 2, 3, and 4 match on size, share turnover, inside ownership, and buy-and-hold returns, respectively. Row 5 matches on propensity scores constructed from logistic regressions including all four as predictors. Refer to Appendix I for variable definitions. For Rows 1 through 4, units must fall within a 5% caliper of the matching variable. For Column 5, the propensity score must be within 5%. *, **, and *** denote statistical significance at the 10%, 5% and 1% levels, respectively.

Panel A: 1:1 matches

Row	Matching Variable	Match Criteria	Pairs	Mean Difference	t test one sided p-value	Median Difference	WSR one sided p-value
(1)	Size	5%	59	-4.73%	0.0296**	-1.24%	0.1137
(2)	Share turnover	5%	59	-4.04%	0.0296**	-1.32%	0.0975*
(3)	Inside ownership	5%	59	-2.91%	0.0774*	-1.52%	0.0505*
(4)	BHR over last year	5%	59	-4.34%	0.0116**	-1.96%	0.0310**
(5)	PSM on rows 1, 2, & 3	5%	58	-4.02%	0.0281**	-1.86%	0.0463**

Panel B: 1:3 matches

Row	Matching Variable	Match Criteria	Pairs	Mean Difference	t test one sided p-value	Median Difference	WSR one sided p-value
(1)	Size	5%	128	-3.61%	0.0080***	-2.33%	0.0180**
(2)	Share turnover	5%	149	-3.93%	0.0027***	-1.66%	0.0164**
(3)	Inside ownership	5%	128	-3.33%	0.0067***	-1.87%	0.0062***
(4)	BHR over last year	5%	146	-4.82%	0.0007***	-1.91%	0.0037***
(5)	PSM on rows 1, 2, & 3	5%	128	-2.83%	0.0209**	-0.55%	0.0561*

Table C5: Robustness tests

This table reports mean and median 3-day cumulative abnormal returns for treated and control groups along with one tail p-values from t tests (Wilcoxon Rank Sum tests) for mean (median) differences. Row 1 reports results from the baseline specification - an event study using the Fama and French 3-Factor model estimated 126 days ending 21 days before POREA issuance. Row 2 adds to that model a fourth factor, NASDAQ ^BANK index returns, to proxy for banking industry risk. Row 3 instead uses the market model and rows 4 and 5 test 3-day market adjusted and raw returns instead of abnormal returns. Row 6 uses a pooled estimation window, 63 days before and 63 days after the 41 days centered on POREA issuance. Row 7 winsorizes returns at the 1% tails before expected return model estimation and abnormal return accumulation. Row 8 cumulates returns from the day before to nine days after POREA issuance. Rows 9 includes only observations for which Compustat reports no earnings announcement within 2 days of enforcement. Row 10 compares POREAs issued to firms that paid out in the last year (treated column) against those issued to firms that paid out in the last 3 years but not in the last year (control column). Row 11 compares POREAs issued to banks that halted cash distribution after enforcement (treated column) against those issued to banks that continued to pay after enforcement (control column). Rows 12 and 13 separately examine POREAs issued to holding companies and banks, respectively. Rows 14 and 15 replicate the event studies using data only from crisis years and non-crisis years, respectively. Crisis years are defined as those of the savings and loan crisis ending in 1992 and the recent banking crisis in 2009 through 2011. Rows 16 and 17 test for abnormal return differences around placebo events 30 days before and after POREA issuance. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

			Means		t test one sided		Medians		WRS test one sided
Row	Sample	Treated	Control	C-T	p-value	Treated	Control	C-T	p-value
(1)	Baseline	-0.0352	0.0093	0.0445	0.0063***	-0.0138	-0.0024	0.0114	0.0138**
(2)	FF 3-factor + banking index factor	-0.0353	0.0077	0.0430	0.0078***	-0.0181	-0.0028	0.0154	0.0109**
(3)	Market model	-0.0366	0.0113	0.0479	0.004***	-0.0167	-0.0045	0.0121	0.0101**
(4)	Market adjusted returns	-0.0382	0.0127	0.0509	0.0018***	-0.0180	-0.0027	0.0153	0.0029***
(5)	Raw returns	-0.0350	0.0158	0.0508	0.0022***	-0.0150	0.0001	0.0150	0.0053***
(6)	Pooled estimation window	-0.0403	0.0084	0.0487	0.0035***	-0.0231	0.0010	0.0241	0.0033***
(7)	Daily returns winsorized at 1% tails	-0.0215	0.0093	0.0308	0.0037***	-0.0097	-0.0006	0.0090	0.0098***
(8)	(-1,+9) event window	-0.0750	0.0038	0.0788	0.0101**	-0.0331	0.0033	0.0365	0.0021***
(9)	No earnings announcements	-0.0333	0.0111	0.0444	0.0081***	-0.0094	0.0000	0.0094	0.0156**
(10)	Payout in *t-1* vs. *t-2* or *t-3* but not *t-1*	-0.0376	-0.0323	0.0053	0.3858	-0.0089	-0.0275	-0.0185	0.1718
(11)	Discontinued vs. continued payout	-0.0410	-0.0062	0.0348	0.0806*	-0.0224	-0.0022	0.0202	0.0335**
(12)	Only Holding Company POREAs	-0.0471	0.0130	0.0601	0.0258**	-0.0224	-0.0115	0.0109	0.0642*
(13)	Only Bank POREAs	-0.0282	0.0075	0.0357	0.0529*	-0.0092	0.0000	0.0092	0.0338**
(14)	Crisis years	-0.0463	0.0190	0.0653	0.0045***	-0.0279	0.0037	0.0316	0.0064***
(15)	Non-crisis years	-0.0117	-0.0101	0.0016	0.4575	-0.0013	-0.0041	-0.0028	0.1799
(16)	Placebo test: 30 days before event	0.0003	-0.0131	-0.0134	0.2447	-0.0069	-0.0036	0.0034	0.1796
(17)	Placebo test: 30 days after event	-0.0015	0.0045	0.0060	0.3450	-0.0044	-0.0038	0.0006	0.2456

Table C6: Multivariate regressions of cumulative abnormal returns on treatment status

This table reports results from regressing cumulative abnormal 3-day returns around POREAs (CARs) on *Treated*, an indicator equal to 1 if the POREA recipient paid dividends or repurchased shares over the three years preceding enforcement and zero, otherwise. Column 2 also includes covariates along which the treated and control subsamples statistically differ: *LTA* is the natural logarithm of a firm's total assets and *LShrTO* is the natural logarithm of share turnover over 252 trading days. Columns 3 through 6 also add *T1C*, the bank's ratio of Tier 1 Capital to total assets; *Cash,* its ratio of cash to total assets; *NPA,* its ratio of non-performing assets to total assets; *ALLL,* its ratio of allowance for loan and lease losses to total assets; *QNI,* its ratio of quarterly net income to total assets; *BHR,* its buy-and-hold-returns over 252 trading days; *MTB,* its ratio of market value of assets to book value of assets. Column 4 also includes year fixed effects. The constant is not reported for this specifications. Covariates are winsorized at the 5% tails. White standard errors are used to compute p-values, reported under coefficient estimates. *, **, and *** denote statistical significance at the 10%, 5% and 1% levels, respectively.

	(1) CAR	(2) CAR	(3) CAR	(4) CAR
Treated	-0.045**	-0.046**	-0.048**	-0.053**
	(0.010)	(0.010)	(0.011)	(0.029)
LTA		0.005	0.006	0.006
		(0.225)	(0.133)	(0.294)
T1C			0.120	0.046
			(0.673)	(0.901)
Cash			-0.077	-0.192
			(0.652)	(0.306)
NPA			0.026	-0.087
			(0.933)	(0.816)
ALLL			0.621	0.319
			(0.585)	(0.810)
QNI			1.192	1.051
			(0.349)	(0.483)
LShrTO		-0.013	-0.012	-0.006
		(0.139)	(0.198)	(0.585)
BHR			-0.007	-0.018
			(0.602)	(0.251)
MTB			0.328	0.504
			(0.191)	(0.180)
Constant	0.009	0.058	-0.291	
	(0.522)	(0.268)	(0.281)	
Observations	229	229	218	218
R-squared	0.027	0.039	0.050	0.087
Fixed Effects	None	None	None	Year
Standard Errors	White	White	White	White

Table C7: The effects of default risk and inside ownerships on abnormal returns

This table reports results from regressing cumulative abnormal 3-day returns (CARs) around POREAs on *Treated*, an indicator equal to 1 if the bank paid dividends or repurchased shares over the three years preceding enforcement and zero, otherwise. In Columns 1 through 4 the independent variable of interest are *Z-score*, which measures logged distance to default. In Columns 5 through 8, it is and *Inside*, which measures the fraction of shares held by corporate insiders. Column 9 includes both. Columns 2, 4, 6, 8 and 9 interacts the independent variable of interest interacted with the *Treated* dummy. Columns 3, 4, 7, 8 and 9 include the following controls (unreported): the natural logarithm of a firm's total assets; its ratio Tier 1 Capital to total assets; its ratio of cash to total assets; its ratio of non-performing assets to total assets; its ratio of allowance for loan and lease losses to total assets; its ratio of quarterly net income to total assets; the natural logarithm of share turnover over the 252 trading days preceding POREA receipt; buy-and-hold-returns over the 252 trading days preceding POREA receipt; and its ratio of market value of assets to book value of assets. All columns include year fixed effects. Continuous variables, including the unreported covariates, are winsorized at the 5% tails. White standard errors are used to compute p-values, reported under coefficient estimates. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1) CAR	(2) CAR	(3) CAR	(4) CAR	(5) CAR	(6) CAR	(7) CAR	(8) CAR	(9) CAR
Treated	-0.051**	-0.138***	-0.055**	-0.149***	-0.046**	0.025	-0.050**	0.036	-0.056
	(0.026)	(0.004)	(0.024)	(0.004)	(0.035)	(0.497)	(0.037)	(0.345)	(0.276)
Z-score	0.003	-0.025**	-0.005	-0.034**					-0.040***
	(0.735)	(0.025)	(0.711)	(0.022)					(0.004)
Treated *x* Z-score		0.036**		0.038**					0.039***
		(0.011)		(0.013)					(0.003)
Inside					0.088*	0.365***	0.099	0.418***	0.440***
					(0.094)	(0.007)	(0.139)	(0.004)	(0.001)
Treated *x* Inside						-0.335**		-0.399**	-0.430***
						(0.021)		(0.010)	(0.003)
Observations	228	228	217	217	229	229	218	218	217
R-squared	0.070	0.094	0.092	0.119	0.076	0.096	0.097	0.123	0.158
Controls	No	No	Yes	Yes	No	No	Yes	Yes	Yes
Fixed Effects	Year	Year	Year	Year	Year	Year	Year	Year	Year
Standard Errors	White	White	White	White	White	White	White	White	White

Figures

Figure A1: Log deposit and SBF loan levels between 1998 and 2016

This figure relates log deposits to log small business and farm (SBF) loans for every market-year in my sample. Also included are linear and quadratic best fit curves.

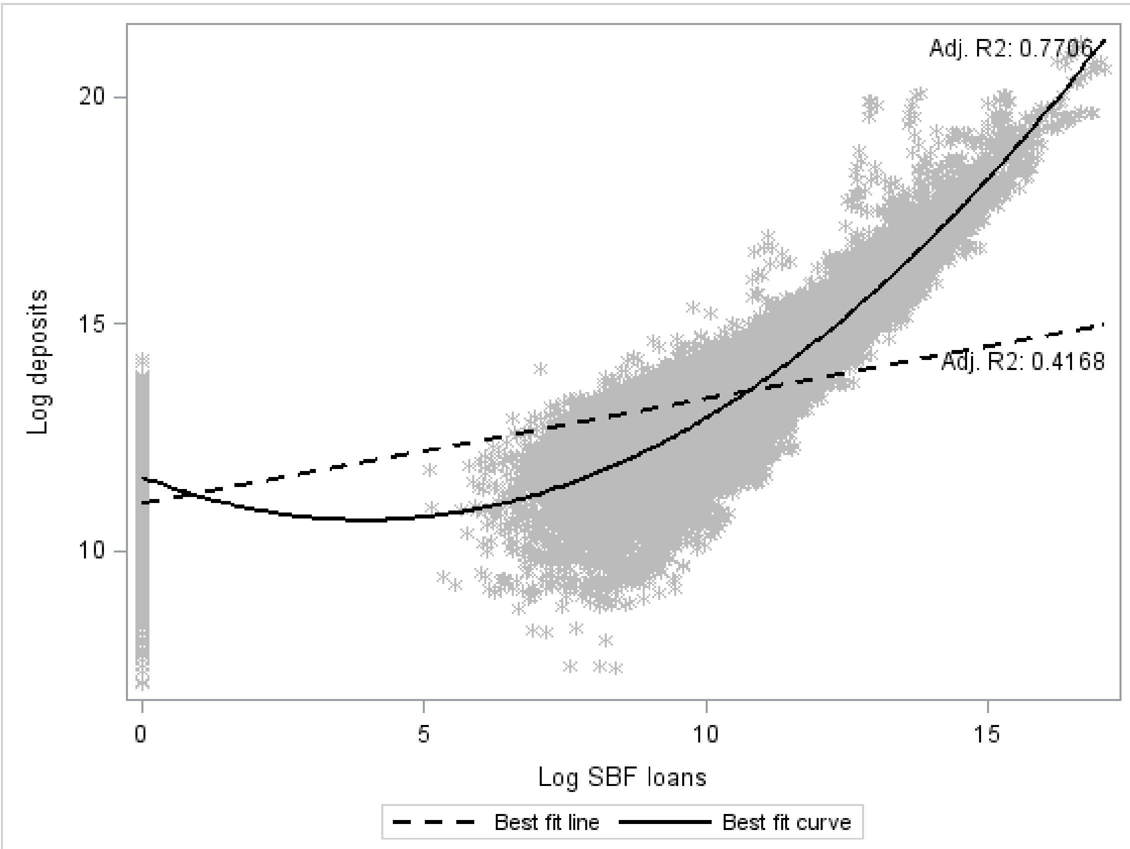
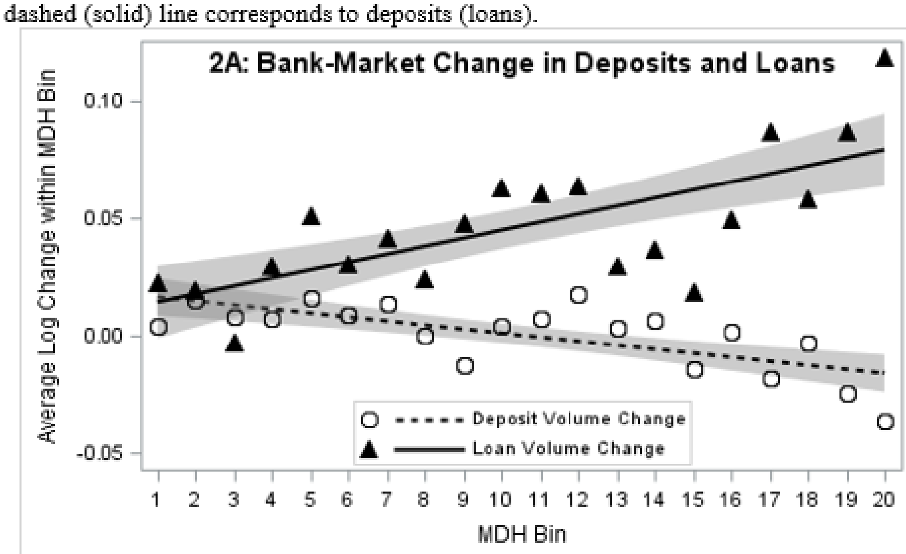


Figure A2: Deposit and loan volume changes around M&As

This figure relates market deposit-heaviness (MDH) to changes in deposit and small business and farm (SBF) loan levels around an acquisition. MDH measures a market-year's deposit heaviness and is computed by averaging the last three years' residuals from within-year, quadratic, market-level regressions of log deposit volume on log loan volume and its square. Higher (lower) values denote deposit-heavier (loan-heavier) markets. Panel A relates bank-market changes in lending and deposit-gathering to market MDH. I compute each acquirer-merger-market observation's average MDH over the three years immediately preceding the merger-year. Next, observations are sorted into 20 bins by average MDH. Finally, within each bin I compute and plot the annualized average log difference in deposit and SBF loan volumes from the pro-forma bank's *t-1* level to the consolidated bank's *t+2* level, where year *t* is the acquisition year. The pro-forma bank's level is defined as the sum of target and acquirer levels in a given market-year. Panel B relates market changes in lending and deposit-gathering to market MDH. I compute each merger-market observation's average MDH over the three years immediately preceding the merger-year. Observations are, again, sorted into 20 bins by average MDH. Log SBF loan and deposit differences are measured at the market level for each of the three years following an acquisition. Differences are averaged within each bin, annualized, and plotted. Circles (triangles) correspond with deposit (loan) volume changes. Also plotted are lines of best fit with 90 percent confidence intervals. The dashed (solid) line corresponds to deposits (loans).



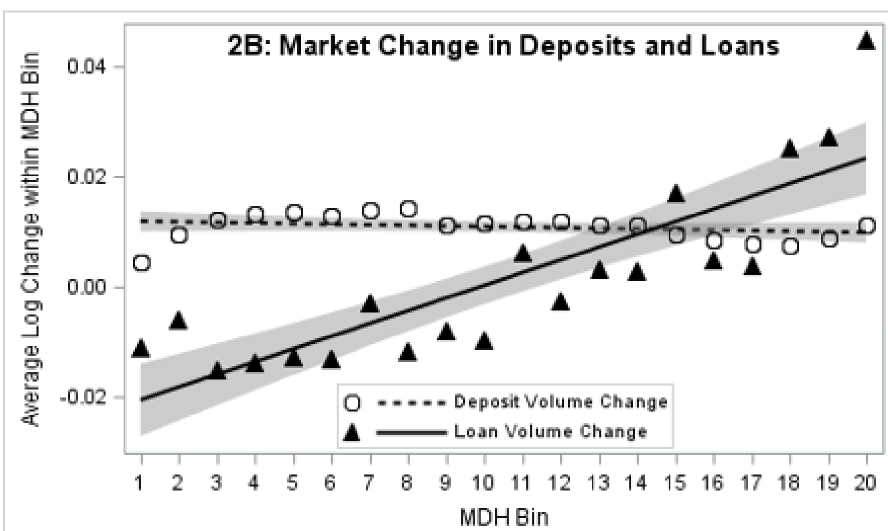


Figure A3: Acquirer, target, and target rival market MDH

This figure compares the weighted average market deposit-heaviness (MDH) of acquirers, targets, and target rivals. MDH measures a market-year's deposit heaviness and is computed by averaging the last three years' residuals from within-year, quadratic, market-level regressions of log deposit volume on log loan volume and its square. Higher (lower) values denote deposit-heavier (loan-heavier) markets. For each merger, I compute a weighted average target MDH as the sum of each target market's MDH, weighted by the fraction of its total deposits that the target holds in each market. The same is done for the acquirer in each merger. I treat all target competitors in a market as a single unit and compute their weighted average market MDH. Then, mergers are ranked by target's weighted average MDH and sorted into 20 bins. Mean MDH for acquirer, target, and rival is computed for each bin and plotted below. A solid (dotted and dashed) line connects the points for acquirers (targets and rivals, respectively).

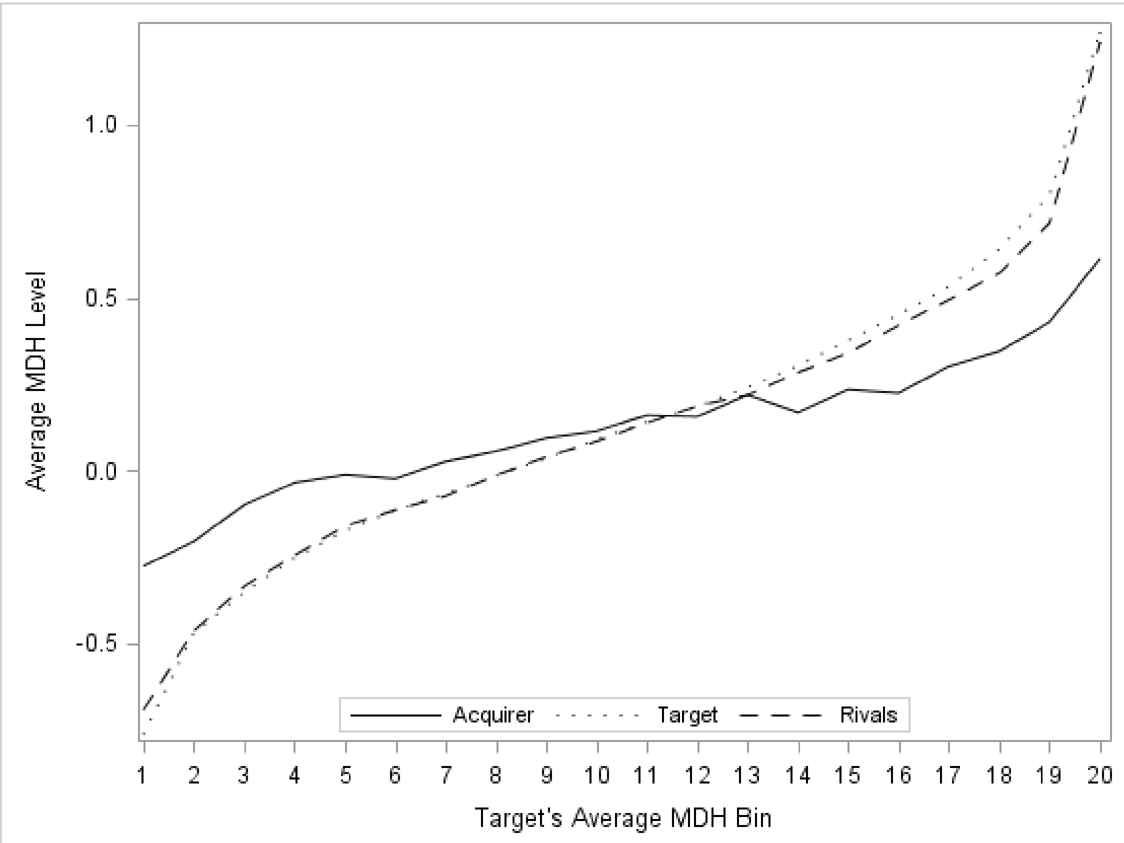


Figure A4: Equilibration in M&A and non-M&A market-years

This figure compares year-over-year changes in market deposit-heaviness (MDH) for different MDH levels. MDH measures a market-year's deposit heaviness and is computed by averaging the last three years' residuals from within-year, quadratic, market-level regressions of log deposit volume on log loan volume and its square. Higher (lower) values denote deposit-heavier (loan-heavier) markets. Market-years are ranked by MDH and sorted into 20 bins. Within each bin, the average year-over-year MDH change is computed separately for markets which experienced and M&A and those which did not. M&A market averages (non-M&A market averages) are plotted as X's (O's) and connected with a dashed (solid) line.

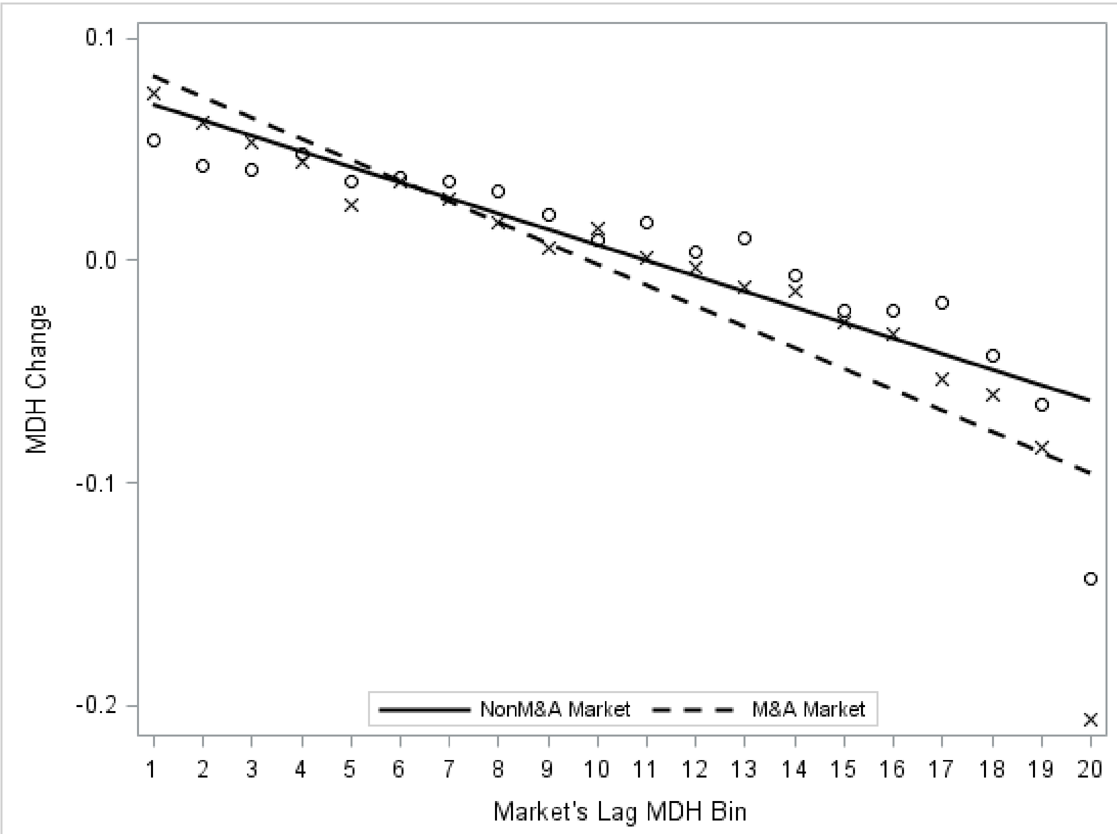


Figure A5: Changes in welfare measures around MDH changes

This figure relates market deposit-heaviness (MDH) levels to the correlation between MDH changes and changes in key economic indicators. MDH measures a market-year's deposit heaviness and is computed by averaging the last three years' residuals from within-year, quadratic, market-level regressions of log deposit volume on log loan volume and its square. Higher (lower) values denote deposit-heavier (loan-heavier) markets. First, I rank markets by MDH and sort them into 20 bins. Next, I compute Pearson correlations between year-over-year MDH differences and changes in economic indicators within each bin. Finally, these correlations are plotted as well as a line of best fit and 90 percent confidence intervals. The economic indicators include log median household income (5A), unemployment rate (5B), log payroll (5C), poverty rate (5D), log new housing units (5E), and log value of new housing units (5F).

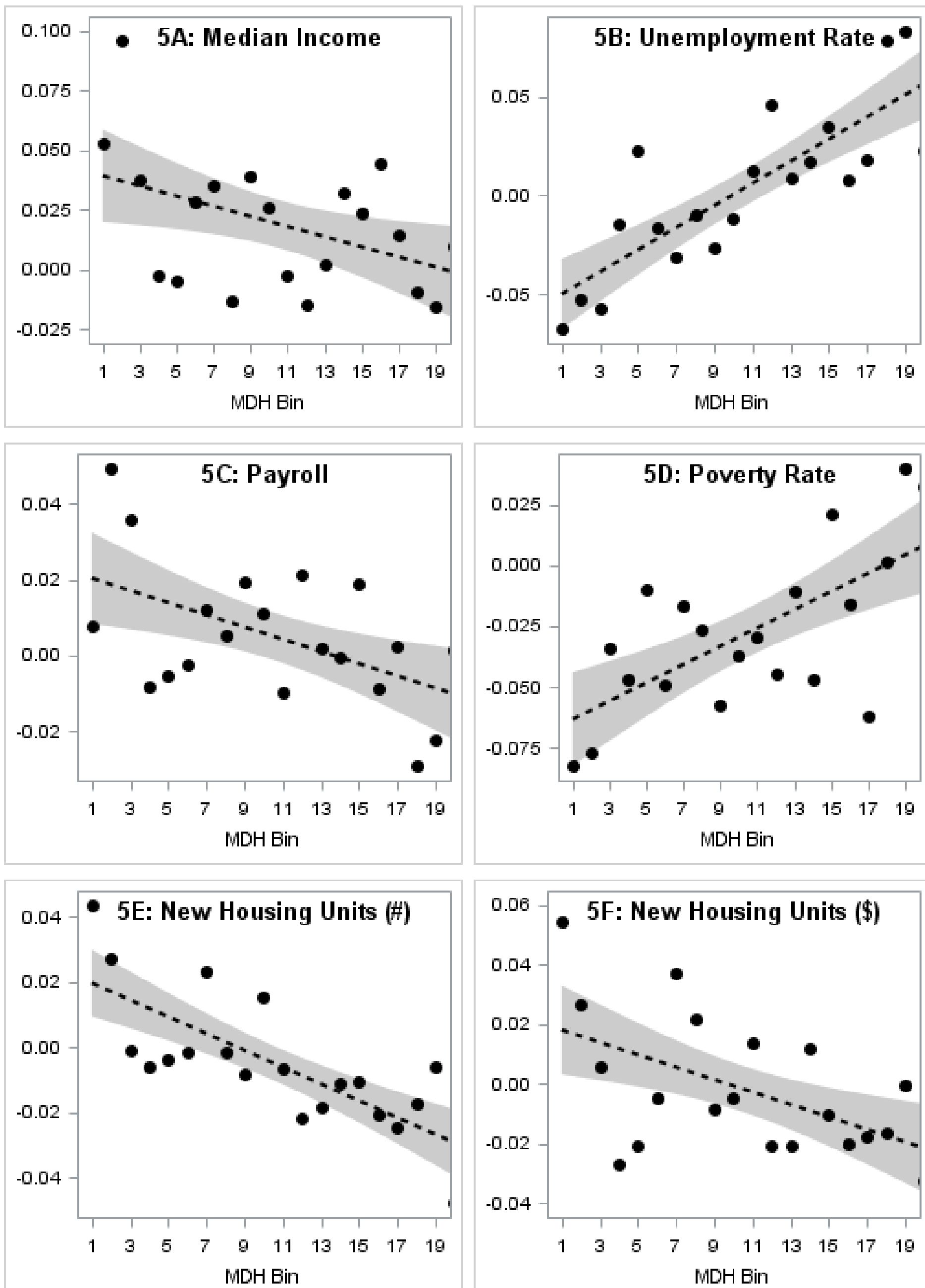


Figure B1: Enforcement actions issuance time series

This figure presents the time-series distribution of enforcement actions (EAs) issued to publicly held financial institutions between 1990 and 2017. Numbers beside each bar denotes total EAs. The striped portion of each bar represents the component that was one of three EA types considered severe by Srinivas et al. (2014) and others: Cease and Desist Orders, Written Agreements/Formal Agreements/Consent Orders, or Prompt Corrective Actions Directives. The solid portion represents the remainder.

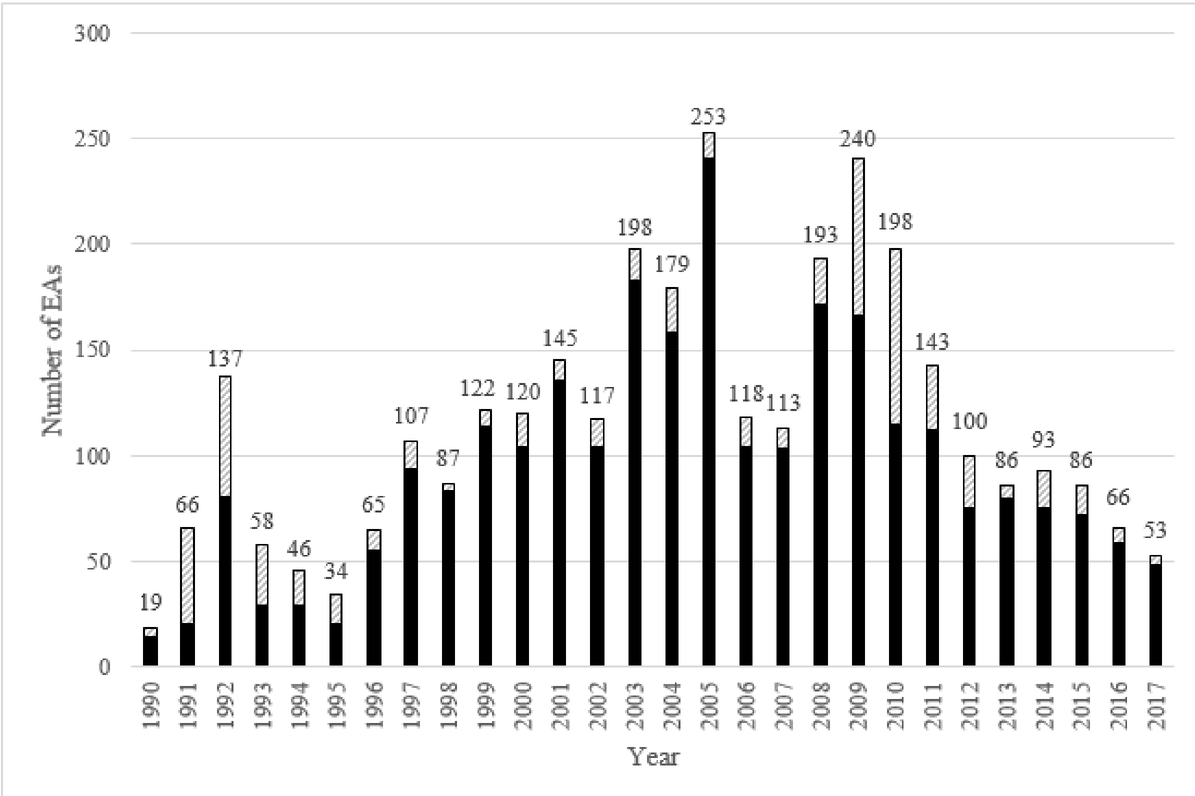


Figure B2: Bank stock returns around enforcement action receipt

This figure presents abnormal bank stock returns around enforcement action (EA) issue dates. To measure abnormal returns, we adopt the event study framework of Campbell, Lo, and MacKinlay (1997). We estimate the Fama-French three factor model over the 252 trading days ending 21 trading days before EA issuance. Estimated coefficients are applied to actual returns on each of the three factors around the event date. The residual term captures abnormal returns. Returns are cumulated from fifteen days before EA issuance to fifteen days after. The solid line (left axis) represent the mean cumulative abnormal return around all EAs while the dashed line (right axis) represents only EAs of three types considered severe by Srinivas et al. (2014) and others: Cease and Desist Orders, Written Agreements/Formal Agreements/ Consent Orders, or Prompt Corrective Actions Directives.

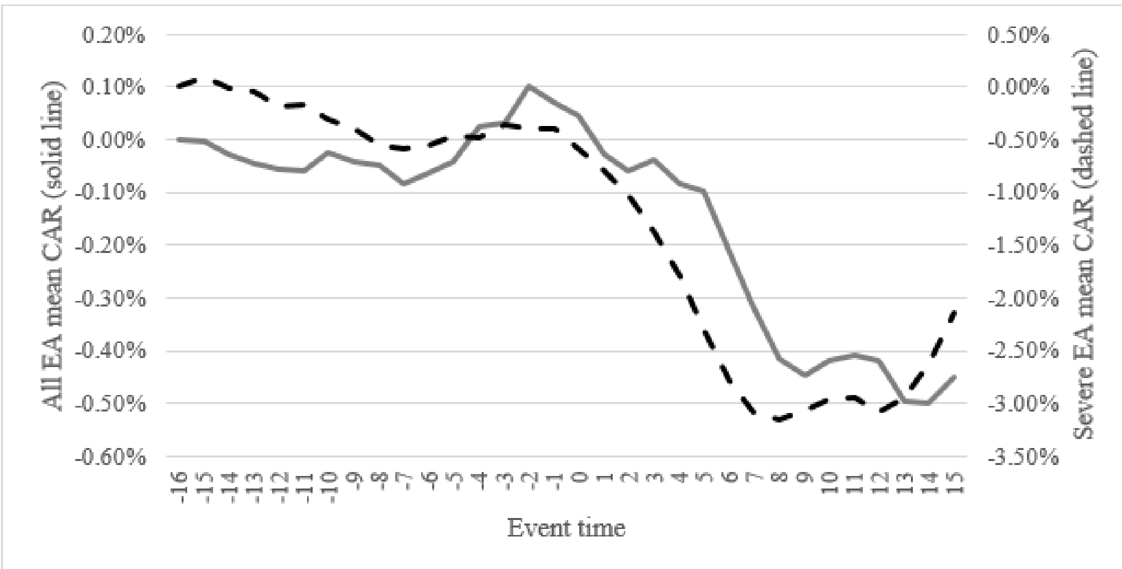


Figure B3: NFF stock returns around linked-bank enforcement action receipt

This figure presents abnormal non-financial firm (NFF) stock returns around enforcement actions (EA) issued to director-linked banks. An NFF is included in our sample if it shares a director with a bank on the day the bank receives an EA. To measure abnormal returns, we adopt the event study framework of Campbell, Lo, and MacKinlay (1997). We estimate the Fama-French three factor model over the 252 trading days ending 21 trading days before EA issuance. Estimated coefficients are applied to actual returns on each of the three factors around the event date. The residual term captures abnormal returns. Returns are cumulated from fifteen days before EA issuance to fifteen days after. The solid line (left axis) represent the mean cumulative abnormal return around all EAs while the dashed line (right axis) represents only EAs of three types considered severe by Srinivas et al. (2014) and others: Cease and Desist Orders, Written Agreements/Formal Agreements/Consent Orders, or Prompt Corrective Actions Directives.

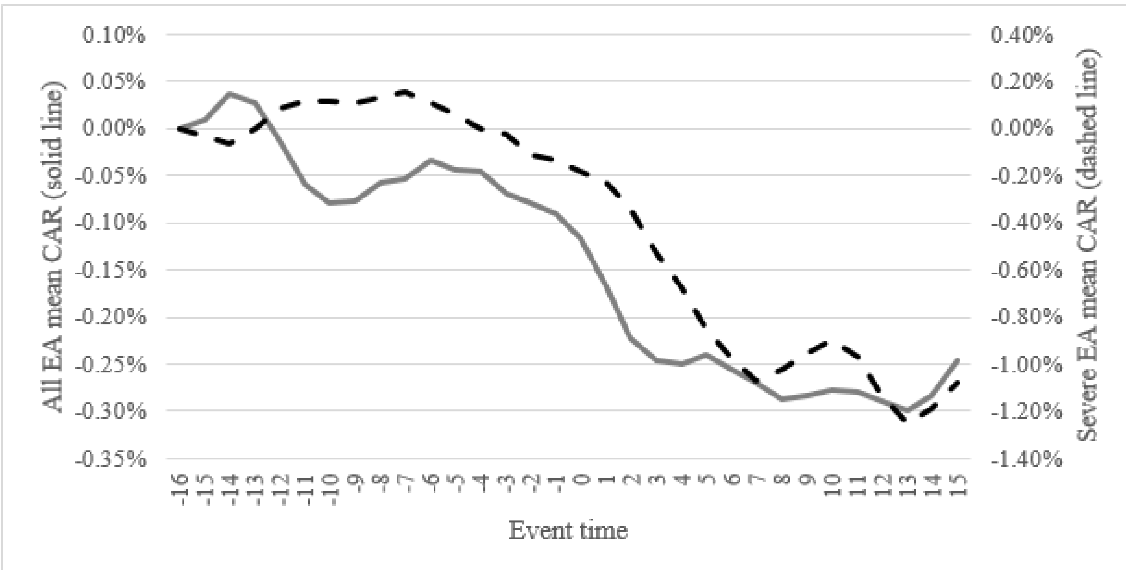


Figure C1: POREA distribution by year for treated and control samples
This figure plots POREAs issuances by year. Black (grey) bars represent treated (control) POREAs.

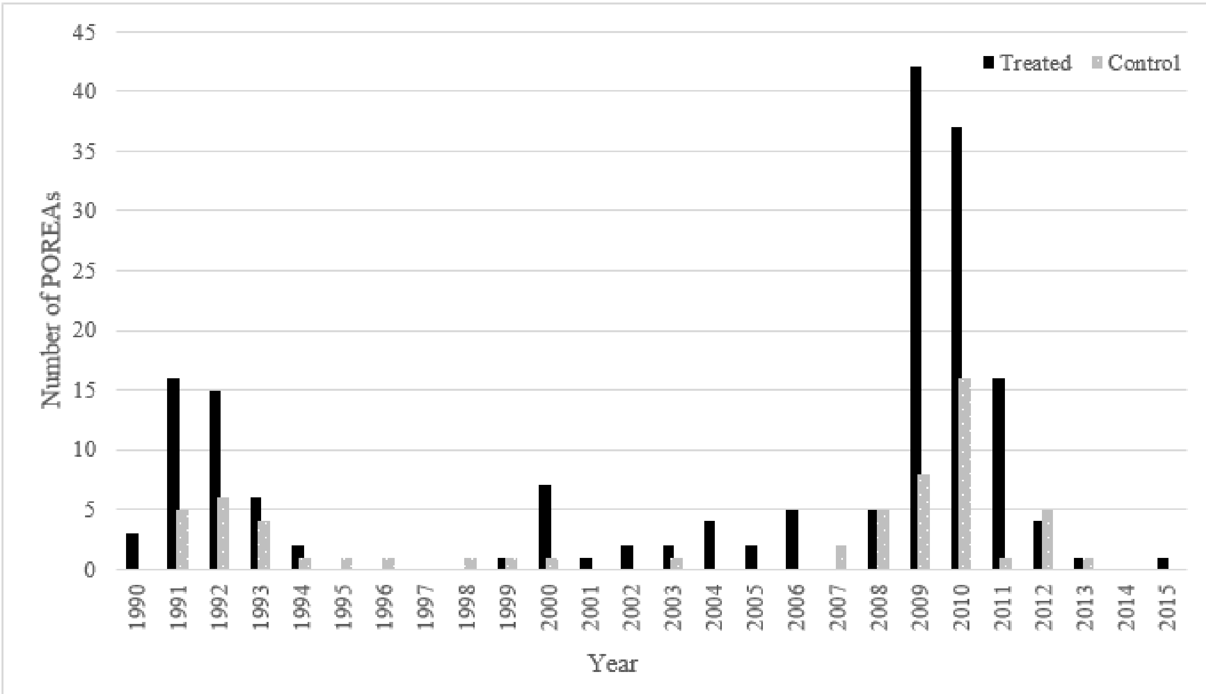


Figure C2: Cumulative abnormal returns before and after POREA issuance
This figure plots the cumulative mean abnormal returns from 15 days before to 15 days after POREA issue dates. Returns are computed using the Fama and French 3-Factor model calculated over the 126 day period ending 21 day period before POREA issuance. The solid (dotted) line represents treated (control) group returns.