Bank Consolidation and Financial Inclusion: The Adverse Effects of Bank Mergers on Depositors

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Abstract

I document that large banks have higher fees and higher minimum required balances on deposit accounts relative to small banks. As a result, bank consolidation makes it relatively more expensive for low-income households to maintain bank accounts. Using a difference-in-differences methodology to estimate a causal impact, I show that, following acquisitions of small banks by large banks, deposit account fees and minimum required balances increase, and deposit outflow is almost 2 percentage points per year higher, relative to acquisitions by other small banks. The effect of consolidation on deposit outflow is stronger in areas with a higher proportion of low-income households. Areas in which large banks acquire small banks subsequently experience faster growth in non-bank financial services such as check-cashing facilities, consistent with some of the outflow corresponding to depositors who leave the banking system altogether. Moreover, households in areas affected by bank consolidation are more likely to accrue unpaid debts and to experience evictions after personal financial shocks, in line with these households facing difficulty in accumulating emergency savings without bank accounts.

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1 Introduction

The U.S. banking industry has undergone dramatic consolidation over the past twenty-five years. In 1994, community banks with inflation-adjusted assets under \$10 billion comprised 57% of deposits and 70% of all bank branches; by 2018, these numbers had fallen to 20% and 44%, respectively.[1] As the role of large banks in the U.S. financial system has grown, it has become more important to understand the impact of bank size on the provision of financial services. While there is an extensive literature on both the positive and negative effects of bank consolidation on efficiency and lending, the impact on depositors and the distributional effects remain less understood.[2]

In this paper, I begin to fill this gap in the literature by providing evidence that the expansion of large banks has negative consequences for low-income depositors. Acquisitions of small banks by large banks cause some low-income depositors to exit the banking system, due to the high fees large banks charge on deposit accounts. Existing literature suggests that being "unbanked"—not owning a checking or savings account—has high long-term costs, including decreased ability to save for emergencies.[3] As I show, this lack of emergency savings lowers a household's ability to withstand personal financial shocks.

To explore how bank consolidation impacts low-income depositors, I first document that larger banks charge higher fees and higher minimum required balances on their deposit accounts using an extensive new dataset of account fees.[4] Figure 1 illustrates this relationship between bank size and average fee (left panel) and minimum account balance to avoid the fee (right panel) on checking accounts. These higher fees and minimum balances matter because survey evidence suggests that households respond to fees when making decisions on changing their bank providers or exiting the banking system altogether (Federal Deposit Insurance Corporation, 2015; Kiser, 2002). According to the FDIC Survey of Unbanked and Underbanked Households, almost 50% of households who do not currently have a bank account had one in the past; many cite high fees as one of the reasons for leaving the banking system.

To estimate the causal impact of consolidation on depositors, I use a difference-in-differences approach and compare, within the same county and year, the branch-level outcomes of acquisitions of small banks by large banks ("treatment" group) to outcomes of acquisitions by other small banks ("control" group). The main concern about a causal interpretation of this methodology is that whether a large or small bank is the acquirer may be correlated with factors that drive depositor behavior. For example, it is possible that large banks acquire worse-performing small banks or small banks with branches in neighborhoods that experience an increasing trend in the percentage of low-income households. However, based on observable characteristics, I find no evidence to support this type of selection. Zip codes where treatment and control branches are located are similar in both levels and trends of economic and demographic variables such as income, unemployment rate, and the percentage of low-income households. Similarly, I employ a propensity score matching procedure and show that my results are almost the same after restricting the analysis to a sample of treatment and control mergers matched on observable bank characteristics.

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[1] Sources: Summary of Deposits from the Federal Deposit Insurance Corporation and FFIEC Reports of Condition and Income (Call Reports).

[2] DeLong (2001), Avery and Samolyk (2004), Berger et al. (2005), and DeYoung et al. (2009), among others, examine the effects of consolidation on efficiency and lending. Prager and Hannan (1998) examine the effect of increased market power after mergers on deposit rates; Park and Pennacchi (2009) present evidence of deposit rate decreases after small bank acquisitions by large banks.

[3] See Barr and Blank (2008), Burgess and Pande (2005), and Celerier and Matray (2017).

[4] This finding has also been established by prior studies such as Board of Governors of the Federal Reserve System (2003), Hannan (2006), and Stavins (1999).

I further address concerns regarding causality in two other ways. First, I create an instrumental variable based on the finding that acquirers are more likely to buy target banks that are close to them geographically (Granja et al., 2017). Specifically, my main instrument is based on a target bank's geographic proximity to other large banks—calculated as the percentage of branches owned by large banks in 1994—in the zip codes where the target bank operates. This instrument plausibly satisfies the exclusion restriction; its effects on subsequent outcomes such as deposit growth come only through its effects on the acquisition decision. Second, my results are robust to limiting the analysis to a plausibly exogenous subset of peripheral branches, branches located in zip codes that contain less than 5% of the acquired bank's deposits. Since these branches are not central to the bank's operations, it is unlikely that any of their characteristics drive the acquisition decision; thus, for these branches, the acquisition is arguably exogenous.

This paper yields three primary sets of results. First, depositors leave small banks acquired by large banks, at least partially due to higher fees and required minimum balances after the acquisition. Branch-level deposit growth is lower at treatment branches than at control branches, corresponding to deposit outflow of about 1.8% per year after the merger. This effect is concentrated in the four years immediately after the merger and cumulatively, the difference in deposit growth between treatment and control branches is 12 percentage points over this period. Fees and minimum balances increase at treatment branches post-merger and, consistent with the hypothesis that depositors respond to the increased fees, deposit outflow is stronger in areas with more low-income households. Deposit outflow is also higher after a plausibly exogenous increase in large bank fees and minimum balances due to a regulatory change in 2011. Although other factors may also drive depositor outflow, neither preferences for small banks nor differences in customer service between small and large banks explain these results. In addition, increases in market power following acquisitions do not drive these findings. Thus, the effects of consolidation due to higher large bank fees are distinct from the effects due to increased market power (Garmaise and Moskowitz, 2006; Prager and Hannan, 1998).

Second, I find evidence consistent with some depositors, particularly those in low-income neighborhoods, exiting the banking system completely. My proxy for the presence of unbanked households is the number of check cashing facilities per capita in each zip code. This is an appropriate proxy because check cashing and formal banking services are substitutes; households who cannot, or choose not to, maintain a deposit account but receive checks turn to check cashing facilities. By five years after the merger, the number of check cashing facilities per capita in zip codes containing treatment branches increases by approximately one check cashing facility per seven zip codes. These results are stronger in zip codes with more than one branch involved in the acquisition, in zip codes with few other small bank branches, and in zip codes with more low-income households. These findings cannot be explained by differential trends in economic or demographic characteristics at treatment and control zip codes. Furthermore, post-merger branch closures do not drive the increase in check cashing facilities.

Third, there are long-term negative real consequences to becoming unbanked due to consolidation. Households in treated zip codes are less likely to withstand unemployment shocks during the Great Recession. Using household-level credit report data from TransUnion, I find that households in zip codes that had unemployment growth above the median in 2006-2010 are more likely to have debts sold to collection agencies if these zip codes contain a treated branch than if they contain a control branch. Similarly, treated zip codes with unemployment growth above the median experience higher rates of evictions than control zip codes with similar unemployment growth. Access to easy credit during the credit boom of 2002-2006 does not drive these results. These findings are consistent with more households in treated zip codes lacking the emergency savings needed to withstand shocks, due to not having bank accounts.

These findings have policy implications. Currently, when regulators decide whether to approve a bank merger or acquisition, they consider several channels through which the merger may impact firms and consumers. First, they examine the overall effect on competition for deposits by considering how measures of concentration might change after the merger. In addition, they often separately consider the possible impact of the merger on small business lending, since small and large banks engage in small business lending differently (Berger et al., 2005; Stein, 2002). The findings in this paper suggest that in addition to small business lending, policy makers should also consider the differential impact of mergers on depositors, especially lower-income ones who may be substantially impacted by a rise in bank fees.

For the branch-level findings in my paper, it is important to track the outcomes of each branch over time. As I discuss further in the Section 3 and in Appendix B, I combine data from the FDIC Summary of Deposits and SNL Financial, as well as my own address-based algorithm to create a consistent identifier for each branch over time that corrects data inconsistencies that sometimes arise at the time of branch ownership changes. For the purposes of tracking a single branch, irrespective of ownership changes or reorganization of deposits, my identifier improves on both the FDIC and SNL Financial identifiers.

In this paper, I take as exogenous the differences in fees and minimum required balances between small and large banks. The main underlying mechanism that drives these differences is large banks' access to wholesale funding, which reduces their reliance on retail depositors as a source of funding (Park and Pennacchi, 2009). Because they are able to access wholesale funding sources, which are cheaper than equity, large banks pay lower deposit rates on interest-bearing accounts and charge higher fees on transaction accounts. I show that the ability to access wholesale funding sources explains large banks' higher fees, even controlling for the greater services that large banks provide, such as more extensive branch and ATM networks. Importantly, this explanation suggests that intrinsic differences between small and large banks—unrelated to the costs of low-income depositors—drive the difference in fees. There is no evidence that lack of efficiency by large banks or absence of profit maximizing behavior by small banks explain large banks' higher fees (DeYoung and Rice, 2004; Kovner et al., 2014).

The closest paper to mine is Celerier and Matray (2017), which examines the effects of one aspect of the changes in the banking industry: competition. By contrast, I examine the impact of a related but opposing mechanism: consolidation and the emergence of large banks, irrespective of market concentration. Using variation in state branch banking deregulation laws, Celerier and Matray show that increased competition after deregulation led to higher branch density and caused previously unbanked households to open new bank accounts, especially in historically excluded areas. This paper, on the other hand, focuses on consolidation, which also partially resulted from the deregulation laws, and led to the predominance of large banks with their higher fees. While there are forces that pull people into the banking system (such as the branch density examined by Celerier and Matray), my findings suggest that there are also countervailing forces pushing them out.

More generally, this paper contributes to several strands of existing literature. First, there is an extensive literature on the effects of bank consolidation and mergers, which mainly finds positive effects on efficiency (DeLong, 2001; DeYoung et al., 2009; Hannan and Prager, 2006), and commercial loan rates (Erel, 2011), and neutral or positive results on small business lending (Berger et al., 1998; Peek and Rosengren, 1998). A smaller literature documents negative effects as well. For example, increased market power due to mergers increases crime (Garmaise and Moskowitz, 2006). In addition, Nguyen (2017) finds a negative effect on mortgage and small business lending from the branch closings associated with large mergers. Complementary to this literature on lending, I examine the effect on depositors, and focus on the pricing of retail bank accounts. To my knowledge, this is the first paper that considers the effect of acquisitions on deposit account fees and required minimum balances, and estimates the impact on financial inclusion.[5] Prager and Hannan (1998) and Park and Pennacchi (2009) also present evidence of the negative effects of mergers on depositors but they focus on deposit rates. Finally, related papers examine size-related financing frictions that drive differences in lending and funding between small and large banks (Kishan and Opiela, 2000; Stein and Kashyap, 2000; Williams, 2017).

Second, I contribute to the literature on the determinants and consequences of financial inclusion. A rich literature has found several factors that impact a household's banking status including household characteristics and preferences (Barr et al., 2011; Rhine et al., 2006), as well as bank branch density (Celerier and Matray, 2017). In addition, studies in both the US and developing countries have documented the positive effects of having a bank account on savings rates and asset accumulation (Ashraf et al., 2006; Celerier and Matray, 2017; Prina, 2015). I add to this literature by examining the consequences of an unbanked household's lack of savings after the household contends with a financial shock.

The rest of the paper is organized as follows. Section 2 outlines the existing research on the differences in fees between large and small banks and discusses the impact fees may have on depositors. Section 3 presents the data and methodology for the analysis of mergers, while Section 4 performs this analysis and examines what happens to deposit growth, fees, and the number of unbanked households. Section 5 examines the real and financial consequences for households pushed out of the banking system. Section 6 concludes.

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[5] Fees and minimums are more relevant than deposit rates for retail depositors, particularly lower-income ones. Amel and Hannan (1999) find that the supply of deposits in checking accounts does not seem to respond to the interest rates paid, while Stavins (1999) shows that deposits in checking accounts are sensitive to some fees.

2 Bank Consolidation and Bank Fees

In this section, I establish the mechanism by which bank consolidation may negatively impact low-income depositors. First, I document that large banks have higher fees and higher minimum required balances, relative to small banks. Second, I discuss existing survey evidence on the prevalence of financially fragile and unbanked households, who may find it difficult to pay high account fees and minimum required balances. These households' survey responses suggest that some of them respond to high account fees or minimum required balances by closing their deposit accounts and exiting the banking system.

2.1 Large Banks and Account Fees

Using an extensive new dataset, I document that large banks charge higher fees on their deposit accounts relative to smaller banks, as has been shown in several prior studies (Board of Governors of the Federal Reserve System, 2003; Hannan, 2006; Stavins, 1999). Although I take this documented difference as exogenous for my analysis of the effects of consolidation, I briefly discuss the main explanation: differences in access to wholesale funding between small and large banks. Importantly, there is no evidence that the difference in fees is driven by large banks discriminating against low-income depositors.

Data

I use a new dataset of bank account and product fees from RateWatch. RateWatch surveys commercial banks, thrifts, and credit unions, and provides fees and rates for a wide variety of deposit accounts, including checking, interest checking, savings, and money market deposit accounts.[6] RateWatch also collects data on the minimum required balances needed to avoid the monthly fee, as well as fees for other types of products and services such as loan applications, ATM usage, and overdraft protection.

The advantage of this dataset is that it contains a panel of posted fees and rates for more than 1000 banks, tracking each branch over time. I avoid problems of prior studies, which either inferred the level of bank fees from bank-level revenue data in the quarterly Reports of Condition and Income (Call Reports) or used small, repeated cross-section samples of several hundred banks. Like with other deposit account survey datasets, the disadvantage is that the dataset includes only posted fees and the minimum balance needed to avoid the fee. I do not observe whether the account fee can be avoided in other ways, such as using direct deposit or debit card transactions.

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[6] In many cases, RateWatch collects data on several different accounts for each bank. For each bank, I keep the account with the lowest fee, as this is the account most relevant for lower-income households.

Throughout the analysis, I follow the Federal Reserve's definitions and characterize as "small" those banks that have less than $10 billion in assets, in inflation-adjusted 2016 dollars.[7] Similarly, I define as "large" banks that have more than $10 billion in assets.[8] The Office of the Comptroller of the Currency (OCC) defines community banks as those with less than $1 billion in assets. My results are robust to using this definition instead.

Differences between Small and Large Banks

Next, I use data from RateWatch to show that transaction accounts at large banks have higher fees and higher minimum required balances needed to avoid the fees. Specifically, I run regressions of the form:

$$f_{b,c,t} = \alpha Large_{b,t} + \beta Large_{b,t} \times After2011_t + \lambda_{c,t} + \epsilon_{b,c,t} \tag{1}$$

$f_{b,c,t}$ are deposit account fees and minimum required balances needed to avoid the fee for bank b in county c in year t, $Large_{b,t}$ is an indicator for whether the bank's assets exceed $10 billion, and $\lambda_{c,t}$ are county-year fixed effects. By including county-year fixed effects, I compare a bank's deposit account to those of other banks nearby, thus ruling out that the results are driven by market structure or differences in the economic characteristics of the areas where small and large banks have branches.[9] I include the interaction between $Large_{b,t}$ and $After2011_t$, an indicator for the post-2011 period, because fees and minimum balances on large banks' deposit accounts increase around the passage of the Durbin Amendment (See Figure 1). The Durbin Amendment to the Dodd-Frank Wall Street Re-form and Consumer Protection Act, passed in 2010 and implemented in 2011, capped the interchange fee that large banks with more than $10 billion in assets could charge on debit card transactions. Because it decreased the profitability of deposit accounts for these large banks, as a response, these banks increased fees and minimum balances on their accounts.[10]

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[7] The Federal Reserve calls banks with less than $10 billion in assets "community banks."

[8] Many previous studies of bank size split banks by whether they are single market or multi-market, rather than by size, e.g. Hannan (2006), Park and Pennacchi (2009), among others. My preferred specifications use size since the higher fees large banks charge are due to size-related advantages such as access to wholesale funding. In addition, the set of multi-market banks used by Hannan (2006) is highly correlated with the set of large banks used in this paper and my results are robust to defining large as multi-market.

[9] There is an existing literature on the effect of market structure on bank fees. Hannan (2006) finds that large, multi-market banks have higher fees than single-market banks, and a higher concentration of multi-market banks increases the fees the single-market banks charge. Azar et al. (2016) argue that local deposit market concentration, measured taking into account cross-ownership of banks, explains the cross-section of bank fees and interest rate spreads.

Table 1 presents the results of equation (1): large banks have higher fees and higher required minimum balances relative to small banks, and this difference is not driven by the Durbin Amendment increase. In columns 1 and 2, the dependent variables are checking account fee and minimum balance to avoid the fee; in columns 3 and 4, I repeat the analysis for interest checking accounts. Standard errors are clustered at the state level, but double-clustering at both state and bank levels produces similar standard errors. For the purposes of this paper, I focus on transaction account fees, but the results are similar for savings and money market deposit accounts (Panel A of Table A1). As a summary, Figures A1 and A2 of Appendix A plot the estimates and standard errors on the coefficient for $Large_{b,t}$ from equation (1) for fees and required minimum balances on checking, interest checking, savings, and money market deposit accounts (MMDA).

What drives these differences in fees and minimum balances? As prior literature has pointed out, large banks are able to access wholesale funding sources, such as large uninsured deposits and the Federal Funds and repo markets, and thus have a funding advantage over small banks, since wholesale funding is cheaper than equity funding. This cheaper funding can explain the lower rates large banks offer on their interest-bearing deposits (Hannan and Prager, 2004; Park and Pennacchi, 2009). [11] In section D of the Appendix, I present a modified version of the models from Park and Pennacchi (2009) and Bord (2018), in which banks offer both interest-bearing deposit accounts and transaction accounts that provide liquidity services. I show that large banks' access to wholesale funding implies that they charge higher fees on transaction accounts. [12]

In Table A2 of Appendix A, I present evidence that access to wholesale funding drives the difference in fees established in Table 1. As my proxy for access to wholesale funding, I use an indicator for whether the bank has a public debt rating from Standard & Poor's. Rated banks have higher fees, and this explains much of the correlation between fees and bank size. In columns 2-4, I show that access to wholesale funding explains the difference in fees, even when controlling for differences in the bank account services and amenities large and small banks provide. In column 2, I control for the average distance between each establishment in the county and the bank's closest branch. This is a measure of how centrally located the bank's branch network is. I also include indicator variables for whether the bank has branches in other counties and other states (column 3), and measures of the number of other products the bank offers and of its customer service (column 4).

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[10] See Kay et al. (2014) for a further discussion of the effects of the Durbin Amendment.

[11] Because large banks have lower funding costs, they also charge lower rates on loans. Erel (2011) provides evidence that commercial loan rates decrease after mergers.

[12] In Bord (2018), I show that in addition to access to wholesale funding, the ability to cross-sell new products to existing transaction account holders helps explain the fees banks charge on these accounts.

The wholesale funding explanation for the difference in fees implies that any impact large banks' higher fees have on lower-income depositors is due to intrinsic differences between banks, and is not related to depositors' incomes. Both types of banks value the marginal dollar of deposits from a high-income depositor and from a low-income depositor the same. An alternative explanation is that large banks have higher fees to price discriminate against lower-income depositors, either because they are more costly to large banks, or because they are more costly generally, relative to high-income households. This explanation is unlikely for two reasons. First, existing literature suggests that large banks are not less efficient than small banks, and have economies of scale in terms of infrastructure, salaries, and other costs (Kovner et al., 2014; Wheelock and Wilson, 2012). This suggests that the cost to a large bank of a lower-income depositor should not be higher than the cost to a small bank. Second, if lower-income depositors are more costly than high-income depositors, then the question arises of why small banks do not also increase fees in order to discriminate against low-income households. Existing literature suggests that small banks do not have systematically lower profits, which would occur if they accepted costlier low-income depositors (DeYoung and Rice, 2004). In addition, as I show in Table A3 of Appendix A, branches of small banks bought by other small banks are not more likely to fail or undergo subsequent mergers, relative to branches of small banks bought by large banks.

Thus, large banks' higher fees are likely driven not by differences in the costs of lower-income depositors, but by differences in account amenities and funding costs. For the rest of the paper, I take the difference in fees as given and examine its effects on depositors.

2.2 Bank Fees and the Unbanked

In this section, I discuss the prevalence of unbanked and financially fragile households in the U.S.. Survey evidence suggests that despite the high costs of not having a bank account, some lower-income depositors already on the margin of staying in the formal banking system decide to leave the banking system altogether due to high fees and high required minimum balances.

According to the FDIC National Survey of Unbanked and Underbanked Households (henceforth FDIC Survey), approximately 7% to 8% of US households are unbanked: they do not have any bank or credit union deposit accounts.[13] Lower-income households are more likely to be unbanked, with approximately 28% of households with an annual income of less than $15,000 and 20% of those with an annual income of less than $30,000 without bank accounts. Similarly, unbanked rates are higher among households with a single female head of household (18%) and minority households (17%).[14]

Households without access to the formal banking system have to instead utilize alternative financial services, also called fringe banking services. These products, which are essentially bank deposit account substitutes, include check cashing facilities, prepaid cards, money orders, and bill pay outlets. Check cashing facilities are establishments that immediately cash a consumer's checks, for a 3-5% fee. The unbanked use stores with bill pay centers (such as Wal-Mart) to pay their credit card or utility bills and turn to wire transfers and money orders in order to pay individuals or transfer money. Note that these deposit account alternatives are distinct from fringe banking services that are loan alternatives, such as pawn shops and payday lenders. Estimates of the monetary cost of fringe banking services range considerably but most estimates are on the order of $200 to $400 per year (Barr, 2004; Good, 1999).[15]

Although some unbanked households have never had bank accounts, the FDIC Survey suggests that many used to be part of the formal banking system. Almost 50% of unbanked households surveyed by the FDIC had a bank account at some point in the past, and 30% of them mentioned high account fees and minimum balances as one of the reasons for leaving the banking system. Another 23% offer the unpredictability of fees as a reason for being unbanked. These statistics are consistent with the finding that a large percentage of the US population is financially fragile, unable to come up with even a relatively small sum of money if it were necessary. For example, using data from the TNG Global Economic Crisis Survey, Lusardi et al. (2011) find that 25% of Americans cannot come up with $2,000 within 30 days at all, and another 20% would have to sell some possession or turn to payday lending. Similarly, a Federal Reserve survey conducted in 2014 found that 44% of households would either be unable to produce $400 immediately or would have to borrow the money or pawn some possessions (of the Federal Reserve System, 2017). The growing presence of large banks with high fees and minimum balances may mean that these households can no longer afford their bank accounts.

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[13] An additional 5-8% are underbanked, which means they have a bank account but still use some deposit account alternatives such as check cashing, money orders, or prepaid cards.

[14] All calculations reflect data from the FDIC Unbanked/Underbanked Surveys of 2009-2015. These estimates are consistent with prior surveys (Rhine et al., 2006).

[15] Even though the costs of using these services may be higher than the costs of maintaining a deposit account, households may still choose to be unbanked due to: convenience of fringe banking services' hours of operation (Dove Consulting, 2000); unpredictability and high cost of other bank account fees such as overdraft fees (Melzer and Morgan, 2015; Servon, 2013); and decisions based on irrational or incorrect/incomplete information, similar to Agarwal et al. (2009) and Bertrand and Morse (2011).

At first glance, the high percentage of unbanked households who used to have bank accounts seems to contradict the general decrease in the fraction of households that are unbanked. According to the Federal Reserve Survey of Consumer Finances, the percentage of households without a transaction account decreased from 15% in 1989 to 7% by 2013. Celerier and Matray (2017) find that following banking deregulation laws, banks expanded their branch networks and more individuals entered the banking system. However, these findings are complementary, not contradictory, and show the counteracting forces that impact the unbanked. As the total number of bank branches increased from 64,000 in 1994 to 84,000 by 2016, the share owned by large banks increased from 30% to 56%. The growth and expansion of the banking industry that Celerier and Matray (2017) examine led to increased competition and reduced the unbanked population. At the same time, consolidation and growth of the largest banks provides a countervailing force that pushes some depositors out of the banking system. An increase in competition without the accompanying consolidation may have reduced the percentage of households without bank accounts even further. In Table A5 of Appendix A, I use the FDIC Survey data to show that at the MSA-level, the presence of large banks is positively correlated with the probability of being unbanked. I also confirm the Celerier and Matray (2017) finding that increased branch density leads to a lower probability of being unbanked, and show that this effect is driven mainly by small banks. Higher branch density by large banks *increases* the probability of being unbanked.

3 Empirical Design and Identification

Having discussed the survey evidence, I next turn to a causal estimation of the effects of bank consolidation on depositors. To test whether large banks' high fees and required minimum balances cause depositors to leave the banking system, I examine the effects of mergers in which a large acquirer buys a small target bank. Because banks that are acquired might differ from the general population of banks, I implement a difference-in-differences methodology and compare these acquisitions to similar cases in which the acquirer is another small bank.

An acquisition of a small bank by a large bank, relative to by another small bank, is an exogenous shock to the acquired institution only if whether the acquirer is large or small is randomly assigned. The threat to exogeneity is that large and small banks have different types of acquisition targets, in which case comparing the two types of acquisitions would be invalid. I address this threat to exogeneity in three ways. First, throughout the analysis, I show that there are no pre-trends in the main outcome variables and that it is only after their acquisitions, that small banks acquired by large banks and those acquired by other small banks experience differences in outcomes. Next, in Section 3.2, I show that at the time of the acquisition, the household characteristics of the zip codes where the branches of the two types of banks are located are very similar. This suggests that acquirers are not targeting certain banks based on the different customers of those banks. Finally, in Section 3.3, I discuss my instrument for whether the acquirer is a large bank, and in Section 4.1, I show that my results are robust to restricting my analysis to peripheral branches that are arguably unrelated to the merger.

3.1 Empirical Methodology and Data

In this section, I lay out my difference-in-differences methodology and discuss the advantages of using a control group of acquisitions by small banks.

The core of my identification is a comparison of small banks that are acquired by large banks ("treatment" group) to those that are acquired by other small banks ("control" group). Figure 2 graphically presents the benefits of using small banks acquired by other small banks as a control group in a simplified, univariate context. It is a plot of branch-level *forward-looking* deposit growth: the growth at time 0 is calculated from the year before to the year after the merger. I use the branch-level deposit growth from the FDIC as a proxy for changes in depositor entry into and exit from the acquired bank, since I do not have data on individual depositors' banking decisions.

Figure 2 illustrates two notable advantages to using acquisitions by other small banks as the control group. First, deposit growth begins decreasing two years prior to the acquisition, suggesting that whether a bank is acquired or not is endogenous. Thus, comparing the treatment group to non-acquired banks would give biased results. Second, prior to the acquisition, the treatment and control banks experience fairly parallel trends in deposit growth. This suggests that the acquisitions are unlikely to be endogenously driven by differences in deposit growth. As I discuss later in Section 4.1, Figure 2 also previews my finding that branches of treated banks, those acquired by large institutions, experience lower growth rates in the 4-5 years following the merger, relative to branches of control banks.

To test how bank consolidation effects depositors, I perform a difference-in-differences analysis comparing, within the same year and county, branches of treated banks (small banks bought by large banks) with branches of control banks (small banks bought by other small banks), before and after the merger. Specifically, I run regressions of the form:

$$Y_{i,b,c,t} = \alpha_{c,t} + \beta_i + \tau_{b,t} + \delta \text{Bought by Large}_b \times \text{Post}_{b,t} + \epsilon_{i,b,c,t} \tag{2}$$

$Y_{i,b,c,t}$ is an outcome variable such as account maintenance fees or deposit growth, which is calculated as the change in log deposits for branch i of bank b in county c at time t. $\alpha_{c,t}$ are county-year fixed effects and β_i are branch fixed effects, which I include to capture any time-invariant branch characteristics. $\tau_{b,t}$ are event-time fixed effects, included to control for any general pre- and post-merger trends. My main coefficient of interest is δ, the coefficient on Bought by $\text{Large}_b\times$ $\text{Post}_{b,t}$, the interaction between the indicator for a small bank bought by a large bank (the treatment group) and the indicator for the post-merger period. δ captures the difference, within the same county and year, between the treatment and control group, after the merger relative to before.

I obtain bank branch location and deposit information from the FDIC Summary of Deposits, fee and minimum balance data from RateWatch, and bank financial statement data from the FFIEC's call reports. I supplement this with zip code characteristics from the Census, zip code level income data from the IRS's Statistics of Income, and data from the Census's County Business Patterns (CBP) and from Infogroup on the number of check cashing facilities, payday lenders, pawnshops, and total number of establishments in each zip code.

Using the FDIC's Summary of Deposits and the Chicago Federal Reserve's Bank Merger datasets, I create a panel dataset of bank and thrift branches and identify all ownership changes that occurred. Due to some inconsistencies in the Summary of Deposits branch-level identifier, I supplement this dataset with branch-level data from SNL Financial, as well as my own algorithm that matches branches by address. The end result is a panel dataset that tracks characteristics of each branch over time, for the time period 1994-2016. Detailed information about the creation of this dataset can be found in Appendix B. The advantage of this dataset, and of using the FDIC Summary of Deposits data, is that it provides yearly branch-level deposits. There is no public dataset on depositor banking relationships, so I proxy for depositor behavior by the deposit growth rates at the branch-level following the merger. Using this dataset, I am able to track ownership changes of each branch, as well as changes in address. I include branch divestitures in my sample, although limiting my analysis strictly to cases when a whole bank is bought does not change my results. I only consider mergers in which the target was a small bank with inflation-adjusted assets of less than \$10 billion, and discard all cases in which the target was a failed bank.[16] I am left with 3,753 mergers, 680 in which the acquirer is a large bank and 3,073 in which the acquirer is a small bank. These mergers correspond to 15,139 branches.

3.2 Exogeneity and Summary Statistics

Having described my methodology, I now examine whether there are differences between treatment and control groups either at the bank-level or in the economic or demographic characteristics of the zip codes where the banks operate. Although small banks acquired by large banks differ from those acquired by small banks, these differences are unlikely to be a threat to exogeneity.

Table 2 presents summary statistics for the target banks, as of the year prior to their acquisition. Column 1 shows the difference between treated and control branches and banks; column 2 presents the t-statistics of the difference; and column 3 presents the mean for the control sample. Since my analysis includes county-year fixed effects, I include county-fixed effects when calculating the branch-level statistics.[17] Small banks bought by large banks differ from those bought by small banks on several dimensions. First, they tend to be bigger. Branches of the treatment group are bigger in terms of deposits, and these banks have more branches and more assets. In addition, treated banks have a lower ratio of deposits to assets and a lower tier 1 capital ratio. The fact that large banks acquire bigger small banks is consistent with Granja, Matvos, and Seru (2017), who find that acquirers of failed banks buy banks that are similar to themselves in terms of geographic footprint and business lines.

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[16] I winsorize the branch data at the 1% level to exclude outliers. I also drop observations which have a low quality of the identifier I created to track each branch over time, as well as reorganizations—acquisitions of banks by other banks in the same bank holding company. I limit the analysis to branches that have deposit growth data for the time period from two years before the merger to two years after.

[17] Specifically, the summary statistics are calculated as $y_{i,b,c,t} = \alpha + \text{Bought by Large}_{b,t} + \lambda_c + \epsilon_{i,b,c,t}$, where λ_c are county fixed effects and Bought by Large_b is the indicator for treatment. I do not include fixed effects for the bank-level summary statistics.

However, Table 2 shows little evidence of differences that would be a threat to exogeneity. For example, one threat to exogeneity would arise if large acquirers bought banks that perform worse, and the worse performance of these banks drove depositors to leave. If this were the case, any difference in subsequent outcomes between my treatment and control group would be to due to selection rather than the treatment effect of having been bought by a large bank. Table 2 suggests that this is not the case. There is no evidence that the treatment group performs worse before the merger; in fact, the treatment group has higher income and lower net charge-offs than the control group.

A related threat to exogeneity is the possibility that the two types of banks have different types of customers or experience differential local macroeconomic shocks that drive both the acquisitions and the subsequent outcomes. Although I cannot rule this out completely since I do not have data on each bank's customers, evidence on observable zip code level characteristics suggests that this is not the case. Table 3 presents summary statistics on the zip codes where the branches of the treatment and control banks are located and reveals few differences.[18] First, in Panel A, I examine yearly zip code level measures of income and economic activity and show that there is no difference in these observable characteristics across the locations of the two types of branches. To capture demographic and socio-economic data, in Panel B, I examine differences based on zip code data from the 2000 Census. Small bank branches acquired by large banks tend to be located in more populated urban areas. However, there is no evidence that these areas have more lower-income households, a higher ratio of unemployed households, or that the change from 2000 to 2010 in unemployment, median income, or other characteristics is higher in treated zip codes (Panel C). As I show in Table 11 and discuss further in section 4.3, measures of local economic activity and economic characteristics of the households experience no trends around mergers. These results suggest that based on observable characteristics, the zip codes where the branches of the two types of banks are located are comparable in both levels and trends.

3.3 Instrumental Variables

Although the treatment and control acquisitions are similar based on observable characteristics, the possibility of unobserved selection remains a concern. In this section, I discuss the instrumental variables I use and present the first-stage results.

It is possible that although there are no differences in zip code economic and demographic characteristics of the two types of acquisitions, there may be still be differences in the characteristics of the customers of the specific institutions since banking markets are highly localized (Gilje et al., 2016; Nguyen, 2017). Consider the following hypothetical scenario: customers of small banks acquired by large banks are in areas—neighborhoods within zip codes—that are becoming poorer. For instance, unemployment due to local establishment closures may lead a bank's customers to leave the banking system because they feel that they cannot afford to keep their accounts. This bank would then be bought by a large bank since the acquirer knows that the higher fees it charges will have little impact on the depositor base; the low-income depositors are leaving anyway. By contrast, customers of small banks acquired by small banks are in areas that are well-off financially, and these small banks are not acquired by large banks because the large banks know that depositors may react negatively to the higher fees. In this hypothetical example, the acquisition decision and the difference in depositor outcomes is driven by differences in the customer bases of the two acquired banks; acquisition by a large bank is correlated with, but does not cause, depositor exit.

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[18] As above, the summary statistics account for county fixed effects.

To rule out endogeneity similar to this example, I turn to instrumental variables based on geographic proximity and similarity of loan portfolios. As Granja, Matvos, and Seru (2017) show, acquirers of failed banks are similar to the acquired banks based on geography and business strategy. This is also the case for non-failure bank mergers. Many acquirers in my sample have branches in close proximity to the target: 58% (28%) of acquirers have branches in at least one of the counties (zip codes) the target bank is located in. Relying on this fact, I use as my instrument the percentage of large banks near the acquired bank. Because contemporaneous proximity to large banks might also be endogenous, I calculate this measure as of 1994. Specifically, for each zip code where the target bank has branches, I first calculate the percentage of branches owned by large banks in 1994. Next, I weigh each zip code by the percentage of acquired bank branches located there. This weighted average is a bank-level measure of the presence of large bank branches in 1994.

Thus, I estimate the effect on deposit growth of mergers that have a large acquirer due to the target's proximity to large banks. The exclusion restriction is that the percent of nearby branches owned by large banks in 1994 affects deposit growth only through its effects on the acquisition decision. The instrument would fail to address the threat to exogeneity only if areas with more large banks in 1994 are also associated with other demographic or economic changes in the late 1990s and 2000s that drive deposit outflow. This is unlikely, especially for the latter half of my sample, and restricting my analysis to the 2000-2016 period does not change my results (see Table A6). This instrument solves the endogeneity problem of the above example because it only captures the part of the acquisition decision driven by proximity, rather than the customer base.

Columns 1 and 2 of Table 4 present the results of the first stage regressions using the geographic proximity instrument. Because the treatment indicator Bought by Large_b is a binary variable, I follow Wooldridge (2010) and first estimate the probability of treatment using a probit (Column 1). I then use the predicted value from the probit as an instrument for treatment using two stage least squares (2SLS). Column 2 presents the first stage, which is strong, with an F statistic greater than 10, so I can reject the possibility of a weak instrument.

I also use an alternative instrumental variable based on potential acquirer loan portfolio characteristics. For each target bank, I calculate the Euclidian distance between its loan portfolio and a weighted portfolio of all large banks with branches in the same county. Similar to Granja et al. (2017), the Euclidian distance is calculated over the share of real estate, consumer, and commercial and industrial loans held by the bank as of June prior to the merger. This distance is a measure of similarity between the acquired institution and possible large acquirers. If a potential acquirer has a similar loan portfolio to the target, it is more likely to acquire the target due to potential synergy in lending. The probit and first stage regressions using this instrument are presented in columns 3 and 4 of Table 4, and are similar to the results in columns 1 and 2. Because the first instrument is based on geographic proximity, is as of 1994, and uses zip code level variation, it is my preferred specification.[19]

4 Results

In this section, I estimate the causal effect of bank consolidation using my difference-in-differences methodology and the geographic proximity instrument. I first establish that immediately after the acquisition, more deposits flow out of treated branches than out of control branches. Consistent with higher fees and higher minimum balances being a driver of this outflow, fees and minimum required balances increase at treated branches after acquisitions. In addition, deposit outflow is stronger in areas where households are more likely to respond to higher fees and required minimum balances by leaving the bank. Finally, using a proxy for the presence of unbanked households, I present evidence consistent with some of these depositors leaving the banking system altogether.

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[19] The first-stage F-statistic for the second instrument is 11. Using both instruments results in an F-statistic of 12.7. Although in all cases, the F-statistic is greater than the rule of thumb of 10 suggested by Angrist and Pischke (2001), using the second instrument by itself or with the first is more likely to result in problems of weak instruments. Because just-identified instrumental variable analysis is median-unbiased, I present my results using just the first instrument and use the second instrument separately as a robustness check. My results are similar when using both instruments, but the magnitude of the coefficients is slightly larger.

4.1 Deposit Growth

I first examine the impact of bank consolidation on depositors at the acquired branches, using the forward-looking branch-level deposit growth rate as a proxy for changes in depositor entry into and exit from each branch. If some depositors respond to acquisitions of small banks by large banks by leaving the bank—for whatever reason—then relative to deposit growth at control branches, growth at treatment branches should be lower after the merger.

In Table 5, I implement the difference-in-differences methodology of equation (2), and find that, consistent with Figure 2, deposit growth decreases at treated branches, relative to control branches, after acquisitions. All regressions include county-year fixed effects, so that the main variables of interest measure the differential decrease in deposit growth for treated banks compared to control banks after, relative to before, the merger in the same county and year. Standard errors are clustered at the county level, but are robust to clustering at both the county and merger level. Column 1 presents the OLS result. In column 2, my instrument is the percentage of large bank branches in each bank's zip codes in 1994. In column 3, I use my alternate instrument, based on the Euclidian distance between the bank's lending portfolio and a weighted average lending portfolio for large banks. The results are very similar in all cases and show that a treatment merger causes deposit growth rates to be lower by approximately 1.5-1.8 percentage points per year. The fact that the IV results are larger in magnitude than the OLS results is likely due to the OLS result not taking into account that small banks acquired by large tend to be in slightly more urban areas, which are likely to have generally higher deposit growth. Figure 3 presents the full set of yearly coefficients and 95% confidence intervals from a fully-saturated regression using my preferred instrumental variable. Consistent with the univariate analysis in Figure 2, Figure 3 shows no pre-trends and finds that the effect on deposit growth is concentrated in the first few years after the merger. Cumulatively, the first four years after the merger account for a 12 percentage point difference in deposit growth. As expected, the difference in growth rates does not persist long-term; after the initial adjustment period of 4 years, the difference between the two groups disappears. This is consistent with similar long-run depositor entry into, and exit from, acquired banks for both the treated and control groups. Small and large banks are both viable and in equilibrium, depositors choose which bank best suits their needs. The only changes happen around mergers, when some depositors leave the treated banks.

In columns 4 and 5, I show that my results are robust to limiting to subsamples for which the concern of endogeneity is mitigated. First, in column 4, I restrict my sample to peripheral branches, which are branches located in zip codes in which the bank has less than 5% of its deposits.[20] Even if large banks choose which small banks to acquire based on the consumer profiles of those banks, focusing on peripheral branches, whose consumers would not have an effect on the overall strategy or operations of the bank, should avoid this issue. For these peripheral branches, the merger is plausibly exogenous since the acquirers are not selecting based on the characteristics of these branches. Finally, in column 5, I limit the analysis to a propensity-score matched sample of mergers. Using the bank characteristics from Table 2, I estimate a propensity score of being acquired by a large bank, and match each bank bought by a large acquirer to a nearest neighbor, a similar bank undergoing a merger in the same year that is bought by a small bank. Following Crump et al. (2009), I keep only observations with a propensity score between 0.1 and 0.9. Comparing acquisitions that are similar in characteristics mitigates the concern that selection on pre-merger characteristics by the acquirers drives the results. Although the sample sizes in columns 4 and 5 are smaller, the results are consistent with the OLS and IV analysis.[21]

Robustness and Alternative Explanations

I perform several further robustness tests in Table 6 to rule out alternative explanations of my results. First, I show that the results are not driven by differential increases in market power by large banks nor by differences in regulatory approaches to approving mergers (e.g. regulators approving a large bank's purchase of a small bank only in economically dire situations). In column 1, I exclude counties in which the acquirer had a branch prior to the merger, and in column 2, I restrict the sample to mergers for which there was no increase in average concentration across the counties where the branches of the target bank were located. I measure concentration by the Herfindahl-Hirschman Index

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[20] The results are robust to using 2% or 1% of deposits, or 5% of branches.

[21] The larger magnitude of the coefficient on Bought by $\text{Large}_b \times \text{Post}_{b,t}$ is likely due to the fact that large acquirers tend to target better-performing banks, whereas in the matched sample, I compare banks of similar performance prior to the merger.

(HHI) of deposits, calculated as the sum of the squared market shares of each bank in the county. The coefficients are similar to the baseline results of Table 5 and highlight that my results capture not the effects of increased market power due to consolidation, but the effects of underlying differences between large and small banks. Next, in column 3, I exclude branches that changed address after the merger to rule out that branch relocations, rather than acquisitions by large banks, drive depositor exit. Furthermore, I address the concern that my regressions over-estimate the true coefficient because some depositors leave treated branches for control branches, thus inflating my coefficient. In column 4, I restrict the sample so as to remove any zip codes in which both treated and control branches are present. Finally, in column 5, I confirm that access to wholesale funding is the underlying driver for the findings of Table 5. I redefine my treatment sample as those banks without a Standard and Poor's Rating that were acquired by a bank with a rating. The control sample is similarly based on a bank's rating rather than size. The results in column 5 suggest that deposit outflow is even higher, consistent with wholesale finding as the driver for large banks' higher fees. In Table A6 of Appendix A, I perform further robustness checks and show that the results are not driven by a specific time period, nor by inclusion of branches that undergo multiple mergers during the sample period.

4.2 Deposit Account Fees and Required Minimum Balances

Having established that branches of treated banks experience lower deposit growth after the merger, I next examine why this happens. Although there are multiple factors that may drive outflow, in this section I focus on higher fees and required minimum balances, as discussed in Section 2. Not only do fees and required minimum balances increase after treatment acquisitions, but the deposit outflow is strongest in a) low-income areas, where households are more likely to respond to these higher account prices; and b) for mergers taking place after a plausibly exogenous increase in large bank fees and required minimum balances. In addition, these results cannot be explained by customer exit due to decreased customer service. I cannot rule out, nor do I maintain, that other factors such as depositor preferences do not play a role in the deposit outflow. However, taken together, the evidence I present is consistent with higher fees and higher required minimum balances driving at least part of the outflow.

Table 7 repeats the difference-in-differences analysis using checking account fees (column 1), checking minimum balances (column 2), interest checking fees (columns 3), and interest checking minimum balances (column 4) as the dependent variables. The main variable of interest, as before, is Bought by $\text{Large}_b\times$ $\text{Post}_{b,t}$, the interaction between the treatment indicator and the post period indicator. The table presents the results using the geographic proximity instrument, and in all cases, the coefficient is positive and significant; small banks bought by large banks experience fee increases after the merger, relative to the control group. Figure 4 shows that just as with the deposit growth coefficients in Figure 3, there is no evidence of pre-trends prior to the merger. However, unlike the deposits growth coefficients, there is no time variation in the coefficients: fees and minimums increase after the merger and remain increased. Table A7 of Appendix A shows that the results are robust to limiting to acquisitions by out-of-county banks (Panel A) and to using fees and required minimum balances on other types of deposit accounts (Panel B). The results are also robust to using the alternative instrument and restricting to peripheral branches or the propensity-matched sample.

On average, the regular (interest) checking account fee increases by approximately \$12 (\$34) per year and minimum balances increase by \$200 (\$600). The increase in mimimum balances is relatively similar to the types of financial shocks that many households state they would not be able to overcome without difficulty (of the Federal Reserve System, 2017). Although a yearly increase in deposit account fees of just \$15-\$30 a year seems small, this is probably an underestimate, especially for poorer households. Lower income households tend to overuse fee-based bank services such as overdrafts and these service fees also tend to be higher for large banks.

Fees at treatment branches increase after mergers because they converge to the fees of the acquirers. Figure 5 illustrates this in a univariate setting, plotting in the left panel checking account fees at treated branches and at branches owned by their acquirers in the same state as the treated branch. The right panel similarly plots checking account fees at control branches and at branches owned by their acquirers in the same state as the control branches. Fees at treated branches are low prior to the acquisition, but increase afterwards and converge to the fees of the acquiring institutions. By contrast, fees at control branches remain low; there is little difference between the fees of control branches and their acquirers before or after the acquisitions.

To test the hypothesis that depositors leave due to increased fees, I next examine whether the results are stronger in areas with more low-income households, who are less likely to be able to bear the increased cost of a deposit account. I run a triple difference regression of the form:

$$Y_{i,b,c,z,t} = \delta \text{Bought by Large}_b \times \text{Post}_{b,t} + \chi \text{LowInc}_z \times \text{Bought by Large}_b \times \text{Post}_{b,t} \\ + \phi \text{LowInc}_z \times \text{Post}_{b,t} + \alpha_{c,t} + \beta_i + \tau_t + \epsilon_{i,b,c,z,t} \tag{3}$$

As before, $Y_{i,b,c,z,t}$ is the deposit growth of branch i of bank b in zip code z and county c at time t. LowInc_z is an indicator for whether z is a low-income zip code. If depositors in low-income areas are more likely to leave the acquired branch, then χ should be negative and significant. Table 8 presents the results of this triple-difference regression using different measures of low income zip codes. These measures are indicators for whether the branch is in a zip code that is *above* the median of the distribution of: the percent of households living below the poverty line in 2000 ($\text{I}\{\text{Pct Poverty}\}_z$; column 1); the percent of households with less than \$30,000 in income in the year prior to the merger ($\text{I}\{\text{Pct AGI} < \$25000\}_z$; column 2); and the percent of households receiving the Earned Income Tax Credit (EITC), a government subsidy mainly aimed at working single mothers, in the year prior to the merger ($\text{I}\{\text{Pct EITC}\}_z$; column 3). In all cases, the interaction terms are negative and significant—deposit outflow is higher in lower-income neighborhoods, which are more likely to have trouble meeting the increased fees and minimum balances.

Next, I exploit a plausibly exogenous variation in fee increases caused by the implementation of the Durbin Amendment to the Dodd-Frank Act in 2011. The Durbin Amendment limited the debit card interchange fees for banks with more than \$10 billion in assets, and in response, many of these banks increased account fees (Figure 1).[22] In column 4 of Table 8, I test whether the post-merger deposit outflow at treated branches is stronger after passage of the Durbin Amendment by implementing a triple-difference with an indicator for the period after 2011, After2011_t. As expected, the coefficient on Bought by $\text{Large}_b \times$ $\text{Post}_{b,t} \times$ After2011_t is negative and significant.

Alternative Explanations

In this section, I address the possibility that low-income households may prefer small banks to large banks for reasons unrelated to deposit account fees, such as convenience and dislike of unknown banks.

First,differential changes in hours or customer service at acquired and control branches could explain my results. Survey evidence suggests that households sometimes switch banks due to a lack of convenience or customer service (Kiser, 2002), and it is possible that large banks have worse customer service and curtailed hours. To rule out this explanation, in column 1 of Table 9, I restrict my sample to mergers that likely did not result in changes to customer service. I measure the level of customer service as the number of full-time bank employees divided by the number of branches, and I only include mergers for which the customer service level at the acquirer was higher than at the target bank. The results are again similar to those of Table 5, which suggests that changes in customer service probably do not drive my findings.

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[22] See Kay et al. (2014) and Sarin (2018) for more on the Durbin Amendment.

Second, large banks are more likely to be out-of-county or out-of-state acquirers, and so it is more likely that consumers have never heard of the acquirer before when it is a large bank than when it is a small bank. In column 2 of Table 9, I restrict the analysis only to in-state acquirers, that is acquisitions in which the buyer had branches in the same state as the target bank. The results are slightly weaker, both economically and statistically, but consistent with those of Table 5.

Although I have focused on fees and required minimum balances, large banks also pay lower deposit rates relative to small banks (Hannan and Prager, 2004; Park and Pennacchi, 2009). In Table A8, I confirm that rates decrease at treated branches after acquisition using my difference-in-differences methodology. The decrease in rates is unlikely to matter for retail depositors since survey evidence and prior literature suggests that households respond to fees, not rates, when deciding to switch banks or leave the banking system (Amel and Hannan, 1999; Federal Deposit Insurance Corporation, 2015; Kiser, 2002). It is possible that large depositors or firms respond to the lower rates, however. In unreported results, I show that the deposit runoff is larger in magnitude in head-office branches, which are more likely to house the deposits of firms and large depositors. However, if all of the deposit outflow I document is driven by firms and large depositors, then I would not expect to find that deposit outflow is higher in low-income areas and after the passage of the Durbin Amendment, as in Table 8. Thus, while it is possible that large depositors' or firms' responses to lower deposit rates explain some of the deposit runoff, this explanation does not fully account for my results.

4.3 Where do the Depositors Go?

Having established that some depositors leave treated branches after the acquisitions, and that this deposit outflow is at least partially driven by higher fees and higher required minimum balances, I next show that acquisitions of small banks by large banks cause an increase in the number of check cashing facilities in the zip code. This is consistent with consolidation driving some depositors out of the banking system. This result is not driven by selection or differential trends in economic characteristics, nor by branch closures.

A novel dataset from Infogroup allows me to proxy for the percentage of unbanked individuals by the number of check cashing facilities per capita in the zip code. The disadvantage of this dataset is that, as with all the other data I use, I cannot track individuals' decisions. The advantages of the Infogroup dataset are two-fold. First, it allows me to identify the number of check-cashing facilities, which is a good proxy for the number of unbanked households. Check cashing facilities are substitutes for deposit account alternatives—unbanked households turn to check cashers to cash their employment, government assistance, and other checks. In the FDIC Survey, more than 45% of unbanked households, and more than 50% of unbanked households who used to have a bank account, use check cashing facilities. Second, as I discuss further in Appendix B, the Infogroup dataset allows me to distinguish between check cashing facilities and payday lenders, even though both types of establishments are in the same 6-digit NAICS code. Whereas check cashing outlets are substitutes for bank deposit account services, payday lenders are substitutes for bank personal loans, and a bank account is often necessary to receive a payday loan. In the FDIC Survey, only 8% of unbanked households use payday lending services. If bank consolidation pushes some depositors out of the banking system due to higher deposit account fees, the zip code should experience an increase in demand for check-cashing facilities, but not in demand for payday lenders. I perform this robustness test later in this section.

Using the proxy from Infogroup, I test whether the number of check cashing facilities increases after bank mergers using a zip code level version of equation (2). Specifically, I run regressions of the form:

$$CC_{z,c,t} = \alpha_{c,t} + \beta_z + \tau_t + \delta \text{Bought by Large}_z \times \text{Post}_{z,t} + \epsilon_{i,z,t} \qquad (4)$$

As before, I include county-year fixed effects, $\alpha_{c,t}$, zip code fixed effects β_z and event-time fixed effects $\tau_{z,t}$. Bought by Large_z is an indicator for whether the zip code had a treatment branch or a control branch.[23] The dependent variable is the number of check cashing facilities per 10,000 residents. Columns 1 of Table 10 presents the OLS results and column 2 presents the IV. The magnitude of the coefficient on Bought by $\text{Large}_z \times Post_{z,t}$ is small, but in absolute terms, treated zip codes increase their ratio of check cashing facilities per 10,000 residents by approximately 0.045 more than control zip codes. Figure 6 presents the full set of yearly coefficients and 95% confidence intervals from a fully-saturated regression that checks for pre-trends. Since there are costs to opening a new check cashing facility, new entry takes time.[24] By 5 years after the acquisition, the difference between treated and control zip codes is responsible for approximately a 0.075 increase in the number of check cashing facilities per 10,000 residents, representing an increase of one check cashing facility per 7 zip codes, on average. Further robustness checks, including the results with the alternative instrument and restricting to the plausibly exogenous subsamples of the previous section are presented in Table A9 of Appendix A.

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[23] There are few zip codes with both types of branches and they are excluded from my analysis.

Next, I test whether the increase in the number of cash-checking facilities is larger in areas where more households are affected by the merger and where there are more lower-income households. In column 3, I run a triple difference, interacting my main variables with Big Merger$_z$, an indicator for whether the number of branches involved in the merger is greater than 1, the median. In column 4, I interact with Few Small Br$_z$, an indicator for whether the percent of other small bank branches, those uninvolved in any mergers, is lower than the median. Finally, I follow Table 8 and interact Bought by Large$_z$ $\times$ $Post_{z,t}$ with indicators for whether the zip code is above the median in the percentage of households living in poverty (I{Pct Poverty}$_z$; column 5) and percentage of households receiving the EITC (I{Pct EITC}$_z$; column 6).[25] In all cases, the interaction term is positive and significant, and the coefficient on Bought by Large$_z$ $\times$ $Post_{z,t}$ is generally not significant. Thus, the increase in check cashers is concentrated in areas where more depositors were affected by the merger, where there are few other small bank branches for depositors to go to, and where there are more low-income households who find it more difficult to pay the increased fees and minimums.

Robustness and Alternative Explanations

I examine two possible alternate explanations for the increase in unbanked households following treatment acquisitions. The first potential concern, as before, is selection; namely, it is possible that both consolidation and the number of check cashing facilities are driven by trends in economic characteristics. If zip codes that experience higher growth rates of low-income households also experience higher rates of treatment mergers, this could explain the results I find above. Table 3 shows that based on cross-sectional observable characteristics, this alternative explanation does not seem to hold, and the instrumental variables analysis also helps address this concern. However, to further resolve this issue, in Table 11, I run the difference-in-differences methodology on several economic and demographic variables to show that they reveal no trends around the time of the mergers. In column 1, I use as my dependent variable the number of payday lending stores and pawnshops per 10,000 residents. If the increase in check cashing facilities is driven by higher percentages of low-income households, then I should also observe an increase in payday stores and pawnshops. However, this is not the case. In column 2, the dependent variable is the number of other establishments—excluding check cashers, payday lenders, and pawnshops—per 10,000 residents. In column 3, the dependent variable is log amount of mortgages originated. In column 4-6, I use as the dependent variable the average zip code average adjusted gross income (AGI), the percentage of filers with income of less than \$25,000, and the percentage of filers that receive the EITC, respectively. In all cases, the coefficient on Bought by $\text{Large}_z \times \text{Post}_{z,t}$ is not significant.

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[24] The increase in year 0 coefficient relative to the year -1 coefficient is likely due to the way the number of check cashing facilities is measured. Unlike deposits, which are as of June 30, the number of check cashing facilities is as of December 31st of each year.

[25] The results are similar when using indicators for a zip code above the median in percent of households with adjusted gross income (AGI) less than $25,000 or zip codes with below median AGI.

A second alternative explanation for my results is that demand for check cashing facilities increases because of branch closures, rather than higher fees and required minimum balances (Nguyen, 2017). This explanation is unlikely because, although large acquirers do tend to close more branches than small acquirers, all the target banks in my sample are small and few branches are closed in these cases. In the first year after a merger, treatment zip codes experienced an average of 0.09 branch closures, as opposed to 0.08 for control zip codes. By three years after the merger, these numbers rise to 0.25 and 0.20 branches, and by five years after the merger to 0.35 and 0.28, respectively. This is equivalent to 0.3 branch closures *per merger* in the first year after the merger, 0.7 branch closures by 3 years after the merger, and 1 branch closure by 5 years after the merger. By contrast, the average merger between two large banks with more than \$10 billion in assets each results in 13 branch closures in the first year. In Table 12, I rerun the difference-in-differences regressions limiting to zip codes that did not experience a branch closure 3 years after the merger (column 1) and 5 years after the merger (column 2). Next, I test a related explanation—that individuals leave the banking system due to recession-related job-losses. In column 3, I drop recession years and the years immediately following (2001-2002, 2008-2010) from my analysis. In all three cases, the results are very similar to those in column 2 of Table 10.

Depositor Switching Behavior

Next, I further examine the finding that the increase in check-cashing facilities is lower in areas with more branches of unacquired small banks. If depositors leave acquired banks due to the higher fees and minimum balances, one would expect that some of those who leave go to other nearby small banks. In Table A10 of Appendix A, I test this hypothesis by repeating the difference-in-differences analysis of equation (4) using as the dependent variable deposit growth at branches of banks that do not undergo mergers or acquisitions. In each zip code, I calculate, separately, the zip code level deposit growth of small and large banks that do not experience a merger. As before, I conduct my analysis at the zip code level. I define treated zip codes as zip codes that contain at least one branch of a small bank acquired by a large bank, whereas control zip codes are those that contain at least one branch of a small bank acquired by another small bank and

After the acquisition, deposit growth at branches of other small banks increases slightly in treated zip codes, relative to control zip codes. By contrast, there is no effect on branches of large banks or branches of banks further away from the merger. First, in column 1, I use as the dependent variable the deposit growth at other small banks located in the same zip code as an acquisition. In column 2, the dependent variable is deposit growth at branches of large banks. In columns 3 and 4, the the dependent variable is zip code level deposit growth at branches of small and large banks, respectively, in zip codes adjacent to ones in which a merger take place. The results are generally consistent with some depositors leaving small banks after they are acquired by large banks, and going to branches of unacquired small banks. Figure A4 presents the set of yearly coefficients and 95% confidence intervals from a fully-saturated regression that checks for pre-trends. The coefficients are noisy, but the first panel—correponding to branches of other small banks— shows a general increase after the merger, whereas there is no effect on other sets of branches.

Because the average zip code has 3 branches of other small banks and 1.8 branches of acquired banks, the results suggest that the deposit growth at other small bank branches in the same zip code corresponds to approximately 50% of the deposit outflow estimated in Table 5.[26] In addition to going to other small, depositors may also leave acquired institutions for credit unions. I do not have branch-level data on credit union deposits, but on average, credit union branches are one-third the size of commercial bank branches, and the average zip code has 2-3 credit union branches. Thus, accounting for depositors who switch to credit unions is approximately equivalent to accounting for an extra small bank branch in the zip code. If this assumption is correct, even accounting for credit unions, depositor switching behavior only corresponds to 60% of the deposit outflow from acquired branches.

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[26] Acquired branches also tend to be larger than branches of unacquired banks.

5 Real and Financial Consequences of Becoming Unbanked

Having documented the effects of mergers on depositors, I examine the consequences of becoming unbanked due to bank consolidation. There is evidence from developing countries that having a bank account improves a household's ability to save (Ashraf et al., 2006; Burgess and Pande, 2005; Prina, 2015). Thus, those without bank accounts are less likely to save and less likely to be able to smooth temporary shocks to earnings (Barr and Blank, 2008).

In this section, I test the mechanism of unbanked status causing a decreased ability to smooth consumption and withstand a financial shock. My hypothesis is that a personal financial shock, such as unemployment or a natural disaster, has a larger effect on unbanked households, who have had an impaired ability to save due to their unbanked status and hence have less savings to rely on. Using an extensive dataset of individual credit reports from TransUnion and a novel dataset of evictions from AIRS, I run a difference-in-differences analysis testing whether households in treated zip codes that experience an unemployment-related zip code shock during the Great Recession are more likely to undergo financial hardship than similar households in control zip codes, who also experience the shock. I find evidence supporting this hypothesis: households in treated zip codes that experience the shock are more likely to have debt sold to a collection agency and are more likely to become evicted during the Great Recession.

5.1 Data and Methodology

My datasets on households' real and financial consequences come from two sources. First, I use data from TransUnion, one of the three major credit reporting agencies. The data are a random sample of 4 million credit account records, and contain information on a household's zip code, credit score, age, outstanding credit from almost all financial institutions, and negative indicators such as loan delinquency, debt in collection, foreclosure, bankruptcy, etc.[27] According to the Federal Deposit Insurance Corporation (2015), only 6% of unbanked households have a credit card. Therefore, I use information on whether the household has a debt sold to a debt collection agency as a measure of financial delinquency.[28] Although they may not have formal banking system credit, unbanked households have utilities and medical bill payments, which if delinquent for long enough, are often sold to debt collection agencies. In fact, 52% of all debts sold to collection agencies are unpaid medical bills and 23% are utility bills (Avery et al., 2003). My second dataset comes from American Information Research Services, Inc. and contains the number of evictions by zip code for each year from 2009 to 2016.

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[27] See Avery et al. (2003) for a detailed description of the data.

Using these datasets, I test whether households in treated zip codes—zip codes in which a large bank bought a small bank—were more likely to have debt sold to a collection agency after a personal financial shock than households in control zip codes that also experience a personal financial shock. I run regressions of the form:

$$f_{i,z} = \beta Shock_z + \gamma Treated_z + \delta Shock_z \times \text{Bought by Large}_z + \chi_i \phi X_z + \lambda_c + \epsilon_{i,z} \tag{5}$$

$f_{i,z}$ is an indicator for whether individual i in zip code z had any debts sold to a debt collection agency between 2008 and 2010, and χ_i are fiexed effects for age by credit score buckets.[29] Z_z are zip code level controls and λ_c are county fixed effects. Zip code controls include log number of households, population density, median income, whether the zip code is urban or rural, and percentages of households that are: black, Hispanic, aged 25-34, living in owner-occupied housing, in the labor force, unemployed, have earnings, and living in poverty. All zip code controls are as of the 2000 Census.

The ideal measure of a personal financial shock would be an individual's exogenous and unexpected unemployment. Because I do not have individual-level employment, I instead use zip code (or county)-level shocks related to the Great Recession. $Shock_z$ is one of the following measures of unemployment associated with the Great Recession: 1) whether the county level increase in unemployment was above the median between 2006-2010 or 2) whether the zip code level increase in unemployment was above the median between 2000 and 2010. Bought by Large_z is the zip code level treatment indicator from the previous sections. My main coefficient of interest is δ, the coefficient on the interaction term between treatment and the personal financial shock.

The advantage of using measures of unemployment related to the Great Recession is that this was a very powerful shock, and business cycles have a stronger negative effect on lower-income and unskilled workers (Krusell et al., 2009; Mukoyama and Sahin, 2006). Unskilled workers have a higher risk of becoming unemployed in recessions than skilled workers (Mincer, 1991). Thus, the employment shock is more likely to have a stronger effect on the same households who are at risk of leaving the banking system after acquisitions by large banks. This reduces concern that the set of households that drive the results below are not the same set of households who leave the banking system in Table 10. The disadvantage of using shocks related to the Great Recession is that small and large banks' differential responses to the financial crisis may contaminate my results. I address this further in the Robustness section below.

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[28] 7% of unbanked households have some sort of bank credit, either a credit card or a bank loan. Of unbanked households who used to have a bank account, 8% have a credit card and 10% have some type of bank credit. These calculations are the author's, using data from the FDIC Survey.

[29] I split age into buckets of 5 years each and credit score into buckets of 10 points each.

Because the financial crisis began in earnest in 2008, I limit my analysis to mergers that take place between 2002 and 2007 and consider debt collection outcomes in the 2008-2010 time period. I also limit the sample to households who are more likely to be compliers of the treatment: namely, households with a credit score lower than the median credit score[30]), and young or middle-aged (age less than 55). Further, to focus on areas where more households are likely to have become unbanked, I limit the sample to zip codes with more than 1 branch involved in the merger (the median number of acquired branches per zip code is 1).

5.2 Results

Households in zip codes that experience both bank consolidation and a financial shock are more likely to have debt sold to collection agencies, and this effect is not caused by the expansion of credit during the housing boom. These findings are mainly driven by medical debt, consistent with the prevalence of medical debt and the impact of such debts on household financial well-being. Similarly, households in these zip codes are more likely to become evicted, relative to households in control zip codes that experience a shock.

I begin by examining the effects of consolidation and the financial shock on debt sold to collection agencies. Columns 1 and 2 of Table 13 show that using either measure of financial shock, households in treated zip codes have higher rates of debt sold to debt collection agencies than similar shocked households in control zip codes. In column 1, the measure of shock is based on county level unemployment and in column 2, it is based on zip code level unemployment. The coefficient is approximately 0.001 and the baseline rate of debt sold to collection agencies is 0.2,[31] so financial fragility due to being forced out of the banking system by acquisitions by large banks explains 5% of all debt sold to debt collection agencies.[32] Since I focus on zip codes where more than one branch was involved in the merger, this translates to approximately 2,000 debts. As expected, the coefficient on the zip code measure of financial shock is also positive and significant. The high magnitude of the coefficient on the interaction of the shock and treatment, relative to magnitude on the coefficient on Shock_z, is due to the fact that I limit the analysis to zip codes where more than 1 branch undergoes a merger. In zip codes where only 1 branch is acquired, the effect of the shock remains strong, but the effect of the interaction term is smaller in magnitude and not statistically significant. In column 3 and 4, I run a falsification test using as my dependent variable debt sold to debt collection agencies in the 2002-2007 period. The coefficient on the interaction term is not significant, which suggests that it is the combination of treatment and financial shock, and not trends in debt sold to collection agencies, that drives the result.

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[30] The credit score I use is similar to the more popular FICO score but is calculated differently and on a 501-990 scale, rather than the FICO 300-850 scale.

[31] The baseline rate is consistent with Avery (2003), who finds that 30% of all households have debts sold

An alternative explanation for these results is that large banks increased lending during the credit boom of 2002-2006. If households in zip codes in which large banks expanded their presence had been able to borrow more during the boom, once the banks stopped extending credit in 2007, these households would have face difficulty paying off their debts. In Table A11 of Appendix A, I test this explanation. Instead of the measures of unemployment increase discussed above, I use as my county level shocks indicators for whether measures of the boom and bust of real estate prices (columns 1 and 2) and zip code- level measures of credit growth from 2002-2006 (columns 3 and 4) are above the median. Table A11 finds no evidence for this alternative explanation. Although the extension of credit by large banks may have played a role in whether households later faced financial difficulty, there was no additional effect in treated zip codes.

Next, I examine whether medical or non-medical debt drives these results. In Table A12, I split the type of debt into non-medical (columns 1-2) and medical (columns 3-4). Using either measure of financial shock, it is medical debt that drives my findings. This is consistent with anecdotal evidence of households facing financial difficulty and

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to collection agencies. He also finds that for 10% of the households, debts sold to collection agencies are the only data from the credit reporting agency. Due to my sample construction, I exclude some households with only data on debts sold to collection agencies in their credit report files. See next footnote.

[32] To rule out that the effect is driven by households moving into the merger zip codes, I restrict my analysis to households who exist in the dataset during the 2002-2007 and the 2008-2010 period. Unbanked households are less likely to appear in the data due to new accounts; they often appear for the first time due to derogatory marks on their account such as debt sold to collection agencies. Since I do not include in my analysis households who appear for the first time in the 2008-2010 period, the coefficient of 0.001 is likely an underestimation of the true effect.

being unable to pay off their existing medical debt once they became unemployed during the Great Recession (eg: "Medical Debts Put Patients at Risk of Financial Collapse", Washburn (2012)). My results suggest that this effect was even stronger for households who became unbanked following acquisitions by large banks. Although medical debts are sometimes very large sums, the ones sold to collection agencies are often small, with a median of \$207 and a mean of \$579 (Consumer Financial Protection Bureau, 2014). These small sums are consistent with survey findings regarding many American households being unable to come up with \$400 immediately (of the Federal Reserve System, 2017).

In addition to debt sold to collection agencies, another sign of financial hardship is eviction. In Table 14, I use zip code level eviction data and test whether treatment mergers coupled with personal financial shocks lead to higher evictions. To do so, I rerun equation (4) at the zip code level. The dependent variable in columns 1 and 2 is zip code level evictions from 2009-2012 as a fraction of the total number of households. Using either measure of financial shock, households in treatment zip codes are more likely to be evicted during the Great Recession, by 0.004-0.005 evictions per household.[33] This corresponds to approximately 9,000 evictions in my sample. In column 3 and 4, I rule out that the increase in evictions is driven by higher rent prices. To do so, I use as my dependent variable average rent prices in the zip code during the 2009-2012 period. The interaction between Bought by Large_z and the measures of financial shock is not statistically significant, which shows that higher rent prices in treated zip codes experiencing the shock did not drive higher evictions.

Robustness

In this section, I discuss two different, plausibly exogeneous measures of shock that I use as robustness checks for the results. The first measure relies on shocks due to natural disasters. The second is a Bartik-type constructed measure of yearly local employment growth The results are similar for both measures, and are consistent with my main findings.

One concern with using increases in unemployment during the Great Recession as a measure of shock that this was a one-time shock which may have affected large banks and small banks differently. For example, large banks were more likely to have exposure to mortgage-backed securities. Lending reductions by banks in poor financial health during the Great Recession contributed significantly to decreases of employment at firms with borrowing relationships to these banks (Chodorow-Reich, 2014). This would suggest that areas with a higher presence of large banks may have experienced stronger unemployment shocks and this correlation may drive my results.[34]

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[33] Relative to an estimate of approximately 6% of households who faced eviction during the time period (Desmond and Shollenberger, 2015).

To address these concerns, as a further robustness check, in Table A13 of Appendix A, I use two plausibly exogenous, time-varying measures of financial shock. First, I obtain data on disasters from the Spatial Hazard Events and Losses Database for the United States (SHELDUS) at the University of South Carolina, and use as my measure of financial shock an indicator for a weather-related disaster in the top 5% of disasters.[35] This variable captures the effect of hurricanes, tornadoes, and wildfires, which should have a plausibly exogenous effect on debt delinquency. Because this variable is time-varying, in column 1 of Table A13, I run a full triple differences analysis similar to equation (3) for the 2003-2010 period. I find that households in treated zip codes that experience a natural disaster are subsequently more likely to subsequently have debt sold to a collection agency. In columns 2 and 3, I consider medical and non-medical debt separately and find no difference between the two. This is to be expected since my previous measure of financial shock—unemployment—reduces a household's ability to service existing debt; by contrast, natural disasters create an additional financial burden which is sometimes, but not always, medical.

Although natural disasters are plausibly exogenous, this instrument is less likely to capture the effect on recently unbanked households. Much of the financial burden of natural disasters stems from property damage, and most unbanked households do not own property. As a second, alternative measure, I follow Bartik (2003) and Maggio and Kermani (2017), among others, and use as my measure of financial shock a Bartik unemployment shock. I calculat the Bartik measure as the national employment growth in each 2-digit SIC industry multiplied by the zip code share of employment in each industry. This results in a zip code level measure of employment changes that is driven by overall national trends and differences in industry shares across zip codes. I define my personal financial shock as an indicator for whether the Bartik employment measure is in the *bottom* 5% of the distribution.[36] In columns 4-6, I repeat the analysis of columns 1-3 using this alternate measure of financial shocks and find similar results. Households in treated zip codes that experience the unemployment shock are more likely to have debt sold to collection agencies, and this result is mainly driven by medical debt.

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[34] Summary statistics in Table 3 suggest that this is not the case and that the zip code unemployment growth between 2000 and 2010 is not correlated with treatment, but it is possible that the ten-year growth masks underlying short-run trends.

[35] These disasters cause on average $130 million of damage per county. Using other cutoffs produces qualitatively similar results. SHELDUS data were previously used by Morse (2011), among others.

[36] The results of Table A13 are qualitatively similar when using other cutoffs to define financial shocks.

6 Conclusion

I estimate the effects of bank consolidation on depositors and find that the higher fees and required minimum balances that large banks charge on their accounts cause lower-income depositors to leave the banking system. I find that relative to acquisitions of small banks by small banks, acquisitions by large banks lead to increased fees and minimum balances and deposit runoff at target institutions. Increases in the number of check cashing facilities several years after the merger suggest that some depositors leave the banking system altogether and instead turn to fringe banking services, such as check cashers. There are economically significant real and financial consequences to this phenomenon; households in areas affected by bank consolidation are more likely to have debts sold to collection agencies and to be evicted when faced with an unemployment-related personal shock.

My results deal with a portion of the population that has only recently been studied in the finance literature. Low income households, and especially unbanked households, do not participate much in the traditional financial system and so have little effect on it. Yet there are almost 9 million unbanked households in the U.S., and the fringe banking industry is popular and growing. In 2016, consumers spent \$5.6 billion in fees on check cashing, prepaid cards, billpay and money orders, which suggests that low-income and unbanked households constitute important markets.[37] Understanding how these groups respond to changes in the financial system may help explain why they avoid playing a larger role in it.

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[37] See Schmall and Wolkowitz (2016).

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Figure 1: Checking Account Fees and Minimums by Bank Size

This figure shows the average annualized fees (left panel) and average minimum required balances needed to avoid the fee (right panel) for regular (non-interest bearing) checking accounts. All data are from RateWatch.

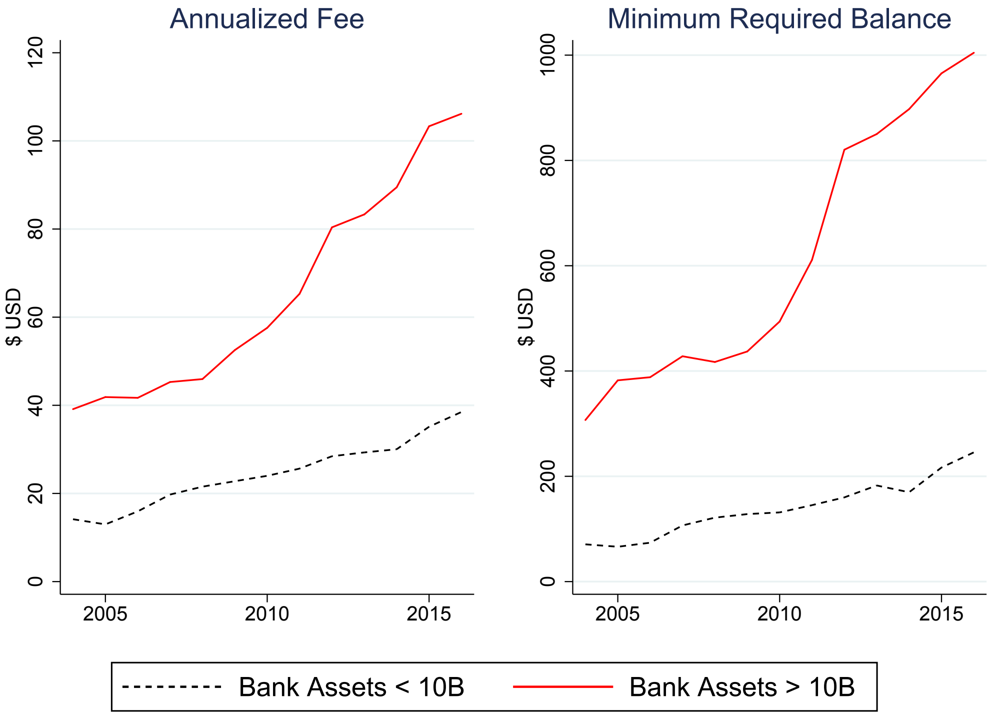


Figure 2: Deposit Growth after Mergers

This figure plots deposit growth for small banks of less than $10 billion in assets that are acquired either by large banks with more than $10 billion in assets (“treated” group) or by other small banks (“control group”). All assets are inflation-adjusted to 2016 dollars. Deposit growth is calculated as the growth from the current period to the next period. Year 0 corresponds to June 30th prior to the merger.

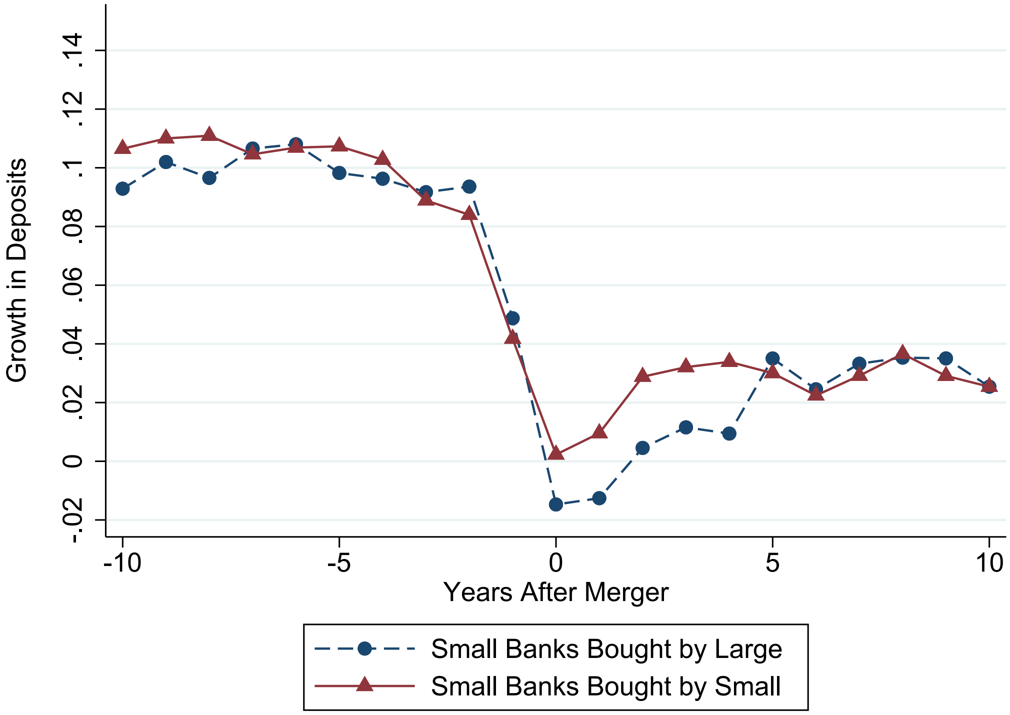


Figure 3: The Effect of Consolidation on Deposit Growth

This figure plots the yearly coefficients from a difference-in-difference regression estimating the effect of consolidation on deposit growth. The solid line shows the coefficients on the interactions between treatment, whether the bank was acquired by a large bank, and indicators for each year after the merger. Dashed lines capture the 95% confidence intervals. Year 0 corresponds to June 30th prior to the merger.

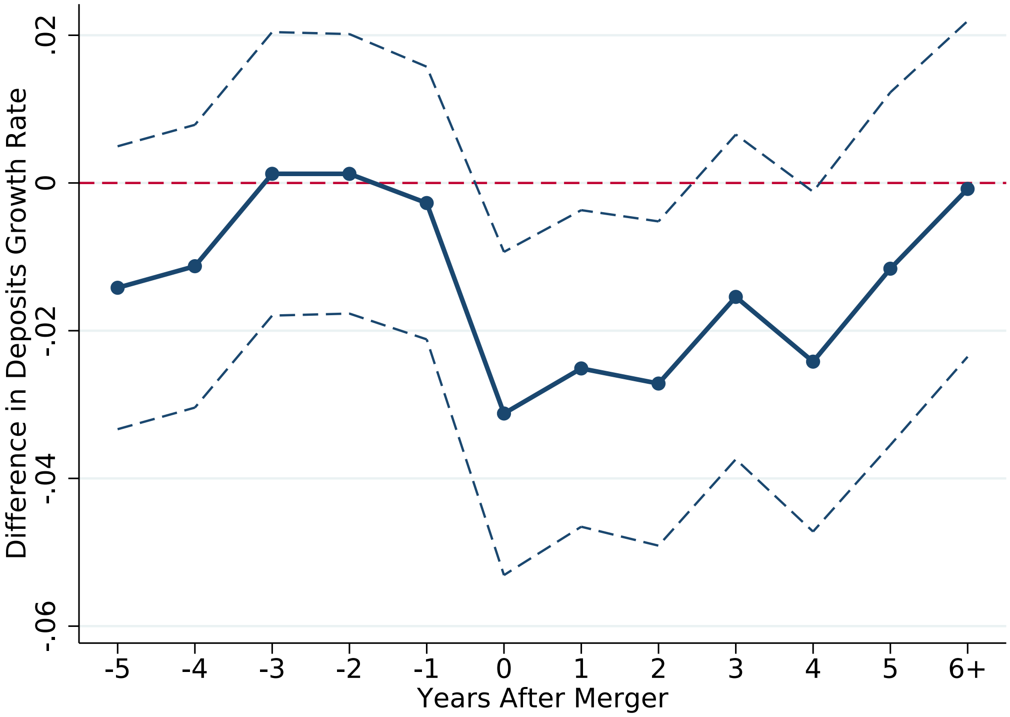
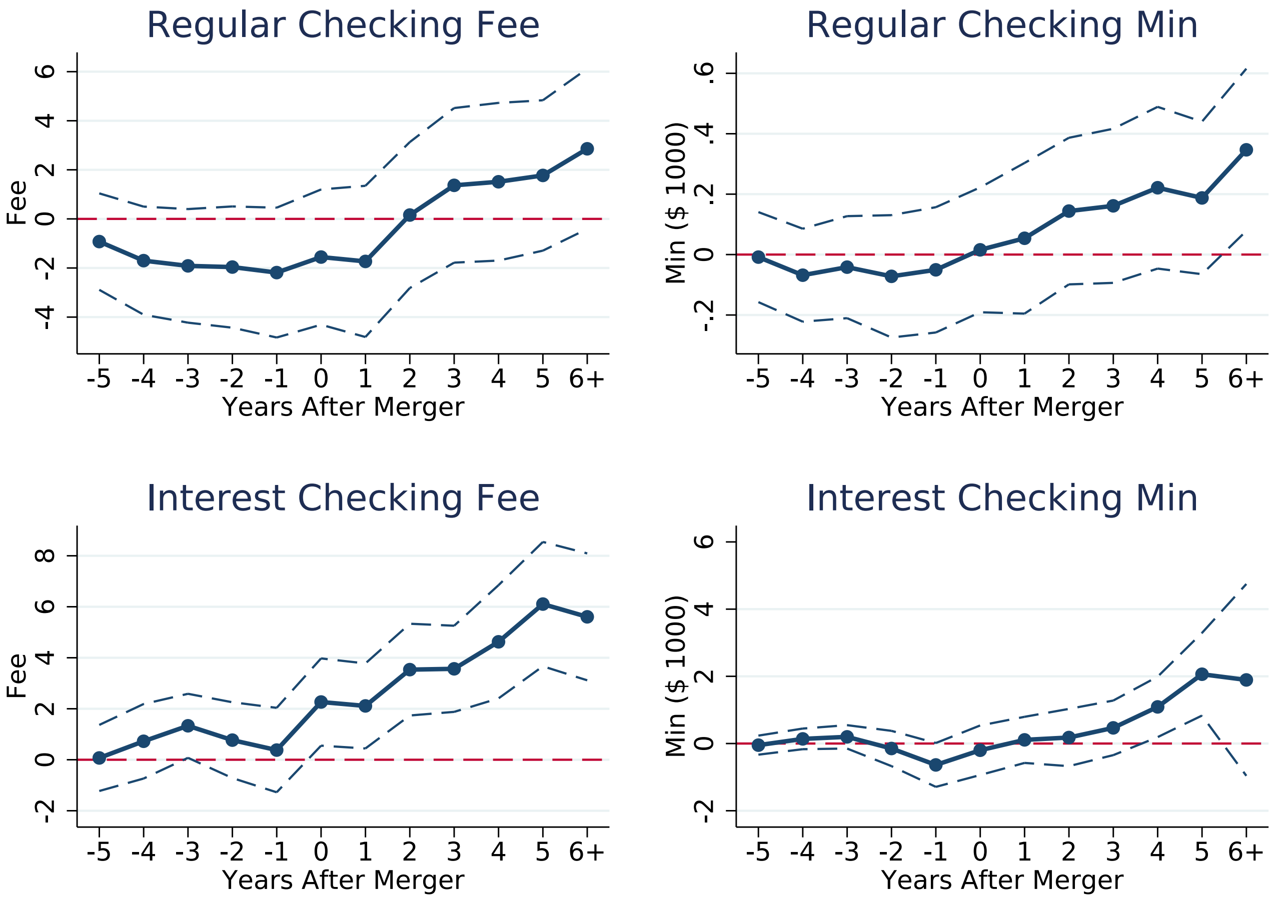


Figure 4: The Effect of Consolidation on Fees and Minimum Balances

This figure plots the yearly coefficients from a difference-in-difference regression estimating the effect of consolidation on deposit account fees and required minimum balances. The dependent variables are checking account fees (top left), checking account minimum balances (top right), interest checking account fees (bottom left) and interest checking account minimum balances (bottom right). The plot shows the coefficients on the interactions between treatment, whether the bank was acquired by a large bank, and indicators for each year after the merger. Dashed lines capture the 95% confidence intervals. Year 0 corresponds to June 30th prior to the merger.



Regular Checking Fee

Regular Checking Min

Interest Checking Fee

Interest Checking Min

Figure 5: Convergence of Checking Account Fees after Mergers

The left panel of the figure plots checking account fees for small banks acquired by other small banks (control group) and their acquirers. The right panel plots checking account fees for small banks acquired by large banks (treated group) and their acquirers. Large banks are defined as those with more than $10 billion in inflation-adjusted 2016 dollars; small banks are defined as those with less than $10 billion in inflation-adjusted assets. Year 0 corresponds to June 30th prior to the merger.

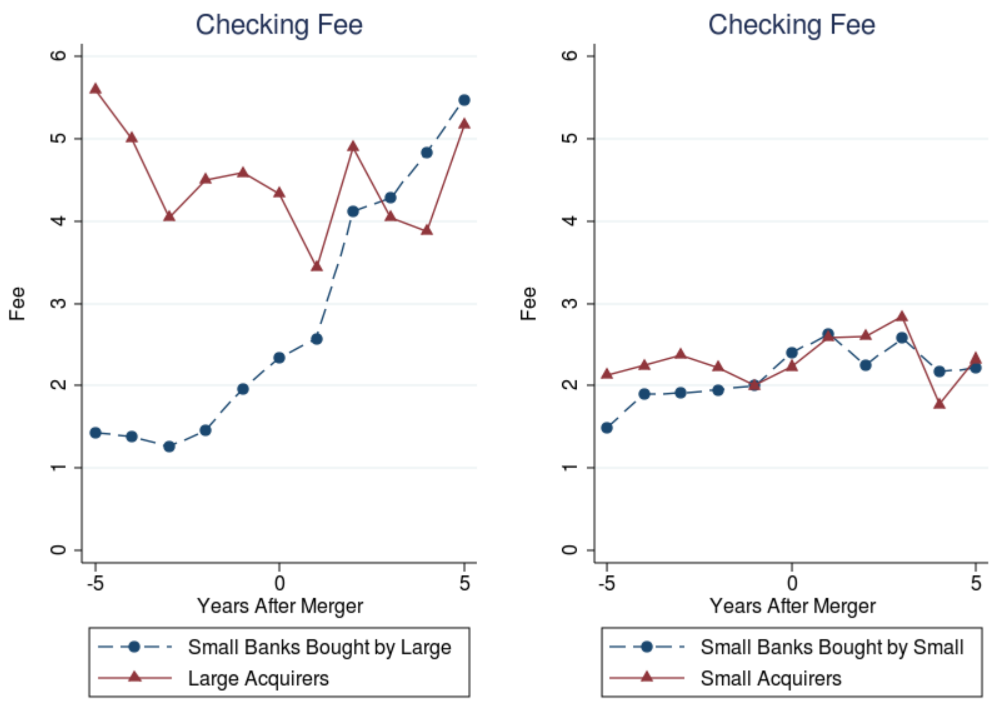


Figure 6: The Effect of Consolidation on Check Cashing Facilities

This figure plots the yearly coefficients from a difference-in-difference regression estimating the effect of consolidation on check cashing facilities. The dependent variable is the number of check cashing facilities per 10,000 residents. The plot shows the coefficients on the interactions between treatment, whether the bank was acquired by a large bank, and indicators for each year after the merger. Dashed lines capture the 95% confidence intervals. All variables are measured as of December 31st, and Year 0 corresponds to the year prior to the merger.

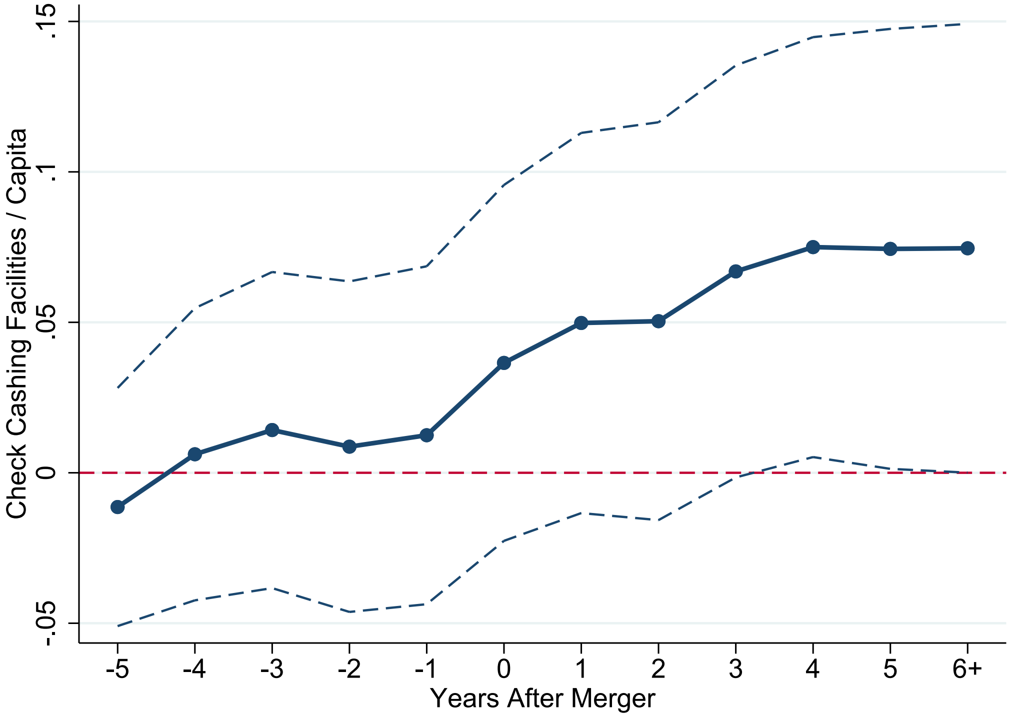


Figure 7: Trends in Zip Code Characteristics Around Consolidation

This figure plots the yearly coefficients from difference-in-difference regressions estimating the effect of consolidation on zip code characteristics. The dependent variables are number of payday lenders and pawnshops per 10,000 residents (top left), number of all other establishments–not including check cashers, payday lenders, and pawnshops– per 10,000 residents (top right), log mortgage lending originated (bottom left), and average zip code adjusted gross income (AGI; bottom right). The plot shows the coefficients on the interaction terms between treatment, whether the bank was acquired by a large bank, and indicators for each year after the merger. Dashed lines capture the 95% confidence intervals. All variables are measured as of December 31st, and Year 0 corresponds to the year prior to the merger.

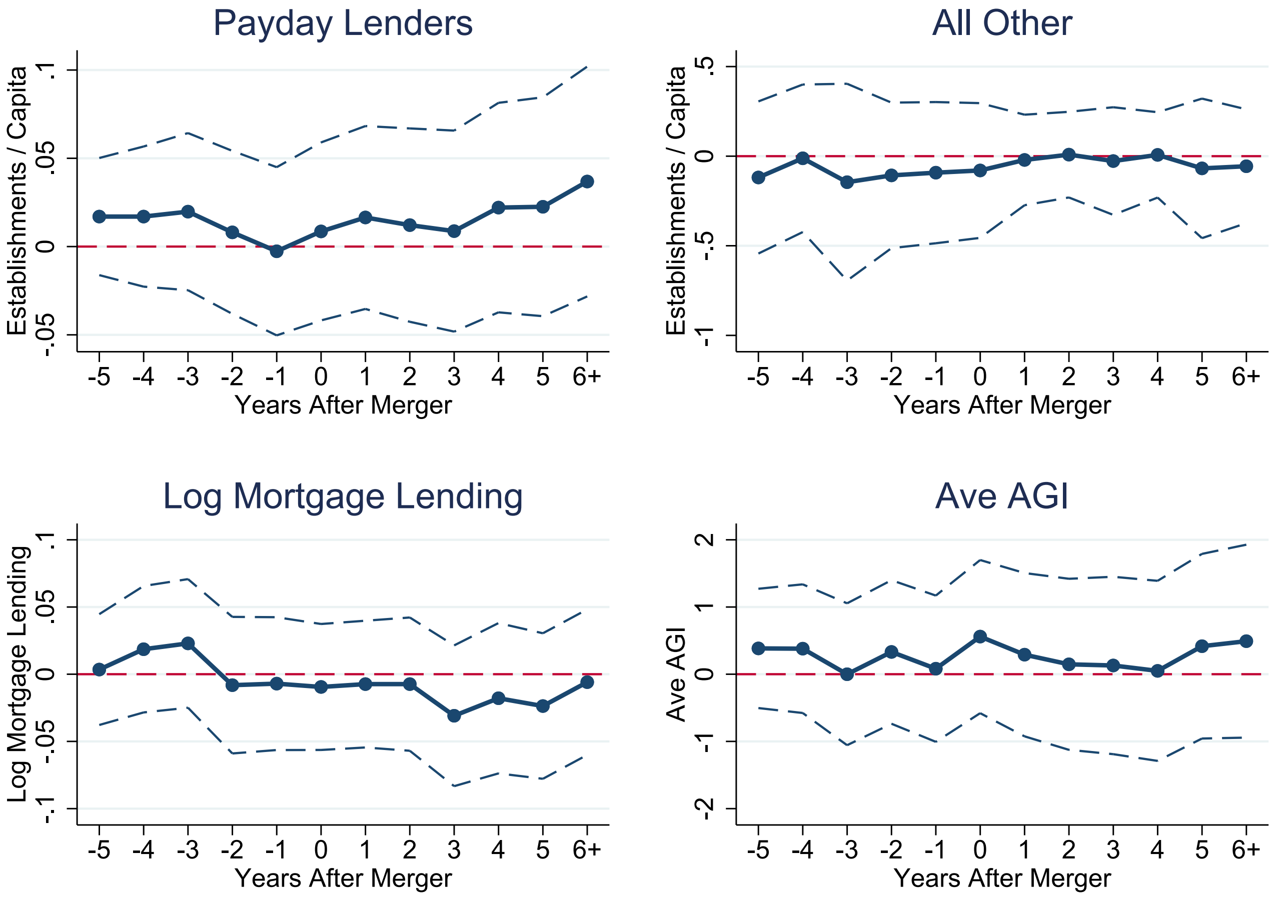


Table 1: Deposit Account Fees

This table shows the results of a regression of equation (1) from section 2.1, estimating the difference in deposit account fees and minimum balances between small banks with less than \$10 billion in assets and large banks with more than \$10 billion in assets. The dependent variables are the fee on checking accounts (column 1), the average minimum balance needed to avoid the fee (column 2), fee on interest checking accounts (column 3), and the minimum balance on interest checking accounts (column 4). Each observation corresponds to a bank-county-year triple and I include county-year fixed effects. $\text{Large}_{b,t}$ is an indicator for whether the bank has more than \$10 billion in assets, in inflation-adjusted 2016 dollars. $\text{Large}_{b,t} \times \text{After2011}_t$ is the interaction between this indicator and an indicator for the 2011-2016 period. Standard errors are shown in parentheses and are clustered at the state level. * p<0.10, ** p<0.05, *** p<0.01.

Dependent Variable:	Regular Checking		Interest Checking	
	Fee (1)	Min (2)	Fee (3)	Min (4)
$\text{Large}_{b,t}$	15.155***	157.919***	40.623***	1624.443***
	(5.194)	(33.687)	(5.065)	(174.050)
$\text{Large}_{b,t} \times \text{After2011}_t$	32.290***	432.083***	37.016***	2806.347***
	(3.339)	(41.254)	(5.110)	(462.633)
County-Year FE	Yes	Yes	Yes	Yes
Observations	100192	96322	104116	102644
Within R-squared	0.128	0.201	0.237	0.165

Table 2: Merger Target Summary Statistics

This table shows the summary statistics for “treated” and “control” mergers. Column 1 presents the difference between treated and control branches and banks (treated minus control); column 2 presents the t-statistics of the difference; and column 3 shows the mean for the control sample. Treated banks are small banks with less than \$10 billion in inflation-adjusted assets that are acquired by large banks with more than \$10 Billion in assets. Control banks are small banks with less than \$10 billion in inflation-adjusted assets that are acquired by other small banks. The branch-level summary statistics in Panel A are reported after adjusting for county fixed effects. Pct Cons Loans is the percent of the bank’s portfolio in consumer loans. Core Deposits are the sum of demand deposits, deposits in NOW and ATS accounts, money market deposit accounts (MMDA), other savings deposits, and time deposits under \$100,000 (FDIC (2013)). Pct Pastdue and NonAcc Loans is the total of loans pastdue and nonaccrual loans as a fraction of all loans. Tier1 Ratio is the bank’s Tier 1 capital ratio. All data are from the FDIC’s Summary of Deposits and the FFIEC call reports.

Panel A: Branch Variables	Difference	T-stat	Control Mean
Deposits in MM	9.347	5.461***	41.558
Checking Acct Fee	1.000	0.601	3.344
Checking Acct Minimum	89.375	0.450	356.523
Treated Branches	5636		
Control Branches	9503		

Panel B: Bank Variables	Difference	T-stat	Control Mean
Infl-adj Assets in MM	732.221	5.708***	712.365
Number of Branches	5.196	6.487***	3.092
Number of Counties	1.051	4.877***	1.634
Loans/Assets	0.013	1.294	0.625
Pct Cons Loans	0.013	1.207	0.101
Pct Real Estate Loans	0.025	1.600	0.664
Deposits/Liabilities	-0.024	4.333***	0.929
Core Deposits/Liabilities	-0.016	1.840*	0.783
Net Income/Assets	0.004	5.372***	0.006
Pct Pastdue and NonAcc Loans	-0.004	1.298	0.026
Net Chargeoffs/Loans	-0.002	3.521***	0.006
Tier 1 Ratio	-1.580	2.478**	15.170
Treated Banks	680		
Control Banks	3073		

Table 3: Branch Location Summary Statistics

This table shows the summary statistics for the demographic and economic characteristics of zip codes in which “treated” and “control” banks have branches. Column 1 shows the difference between treated and control zip codes (treated minus control); column 2 presents the t-statistics of the difference; and column 3 presents the mean for the control sample. Treated banks are small banks with less than \$10 billion in inflation-adjusted assets that were acquired by large banks with more than \$10 billion in assets. Control banks are small banks with less than \$10 billion in inflation-adjusted assets that were acquired by other small banks. All summary statistics are reported after adjusting for county fixed effects. The characteristics in Panel A are time-varying and are reported as of the year prior to the acquisition. Panel B presents characteristics as of the 2000 Census, and Panel C presents changes in characteristics between 2000 and 2010. Data on number of establishments and number of check-cashing facilities come from Infogroup. Deposit and branch information comes from the FDIC’s summary of deposits. Data on the percent with AGI< \$25,0000 and percent receiving the Earned Income Tax Credit (EITC) come from the IRS Summary of Statistics. Average credit score and percent of households with debt at collection agencies come from TransUnion. Data for Panels B and C come from the Census.

Panel A: As of Merger Year	Difference	T-stat	Control Mean
Num. Check Cashers/Num. Households	0.041	0.879	0.677
Num. Establishments/Num. Housholds	0.425	0.999	0.305
Num. Branches/Num. Households	0.181	0.308	2.858
Deposits	51,862.021	1.624	431,570.775
Pct with AGI < 25K	-0.000	0.085	0.420
Pct receiving EITC	0.001	0.143	0.157
Pct with Debt at Collections Agencies	-0.009	1.149	0.134
Panel B: As of 2000 Census	**Difference**	**T-stat**	**Control Mean**
Num. of Households	192.642	1.666*	7,795.345
Pop. Density	0.040	1.067	0.849
Urban Areas	0.005	1.646*	0.134
Pct Black	0.199	0.885	8.539
Pct Hispanic	0.137	0.506	8.203
Pct under Age 25	-0.013	0.093	33.361
Pct Age 25-34	-0.049	0.741	13.485
Pct Age 35-44	-0.010	0.197	16.043
Pct 45-64	-0.093	1.169	23.121
Pct 65+	0.168	1.467	13.990
Pct High School Graduates	-0.143	1.131	29.866
Pct with Associate Degree	0.000	0.011	6.196
Pct with Bachelors Degree	0.034	0.217	16.020
Pct of Owner Occupied Housing	-0.008	0.027	69.088
Pct of Renter Occupied Housing	0.008	0.027	30.912
Household Median Income	-31.727	0.099	45,367.934
Pct in Labor Force	-0.081	0.640	63.599
Pct Unemployed	0.008	0.203	3.267
Pct Living below Poverty Level	0.032	0.225	11.126
Pct of Households with Earnings	-0.119	0.850	79.680
Pct of Households with Public Assistance	0.008	0.165	2.805
Panel C: Changes from 2000 to 2010	**Difference**	**T-stat**	**Control Mean**
Pct Unemployment	0.029	0.392	4.749
Pct in Labor Force	0.105	1.052	-4.829
Median Income	-128.890	0.925	11,965.554
Pct with Earnings	0.046	0.642	-1.705
Pct with Public Assistance	-0.027	0.954	-0.548
Pct Living in Poverty	0.048	0.606	-1.524
Treated Zip Codes	5991		
Control Zip Codes	11901		

Table 4: IV First Stage

This table presents the first stages of the instrumental variables regressions. Column 1 displays the results of the probit regression of treatment on the geographic proximity instrument: the percentage of branches in each zip code owned by large banks in 1994, weighted by the percentage of the bank's branches in each zip code. Column 2 presents the first stage of 2SLS, instrumenting for Bought by Large$_b \times$ Post$_{b,t}$ by the predicted value of Bought by Large$_b$ interacted with $Post_{b,t}$, e.g. $\widehat{BoughtbyLarge}_b \times Post_{b,t}$. Column 3 and 4 use the Euclidian distance between the target bank's and potential acquirers' loan portfolios. Standard errors are clustered at the county level (Columns 2 and 4).

Instrument:	Large Br Density 94		Euclidian Dist. to Large	
Dependent Variable:	Bought by Large$_b$ Probit (1)	Bought by Large$_b$x Post$_{b,t}$ OLS: First Stage (2)	Bought by Large$_b$ Probit (3)	Bought by Large$_b$x Post$_{b,t}$ OLS: First Stage (4)
Branch Density 1994	0.580***			
	(0.102)			
$\widehat{\text{Bought by Large}_b}$ x Post$_{b,t}$		1.368***		
		(0.127)		
Euclidian Dist. to Large			5.108**	
			(2.179)	
$\widehat{\text{Bought by Large}_b}$ x Post$_{b,t}$				1.367***
				(0.122)
County-Year Fixed Effects		Yes		Yes
Branch Fixed Effects		Yes		Yes
Observations	3753	186564	3753	186564
Within R-squared	0.007	0.417	0.004	0.406

Table 5: The Effect of Consolidation on Deposit Growth

This table presents the results of the difference-in-differences specification of equation (2) of Section 3.1, estimating the effect of bank consolidation on deposit growth. The dependent variable is the branch-level deposit growth rate, calculated as the growth from the current year to the following year. Bought by $\text{Large}_b \times \text{Post}_{b,t}$ is the interaction between the treatment effect, whether the acquirer is a bank with more than $10 billion in inflation-adjusted assets, and the post-merger indicator. Column 1 presents the results from the OLS regression. In Column 2, I use as my instrument the percent of nearby branches that were owned by large banks in 1994. In Column 3, I use as my instrument the distance between the target bank's and possible acquirers' loan portfolios. Column 4 limits the sample to peripheral branches, which are in zip codes with less than 5% of the bank's deposits. Column 5 restricts the analysis to a propensity-score matched sample of mergers. County-year fixed effects and branch fixed effects are included in each regression. Standard errors are shown in parentheses and are clustered at the county level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Dependent Variable: Branch Deposit Growth

	OLS	IV		Sub-sample	
		Large Br Density 1994	Euclidian Dist to Large	Peripheral Branches	Matched Sample
	(1)	(2)	(3)	(4)	(5)
Bought by $\text{Large}_b \times \text{Post}_{b,t}$	-0.015***	-0.018**	-0.018**	-0.021***	-0.022***
	(0.004)	(0.008)	(0.008)	(0.007)	(0.008)
County-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Branch Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	186564	186564	186564	97099	102827
Within R-squared	0.149	0.149	0.149	0.170	0.163

Table 6: The Effect of Consolidation on Deposit Growth - Robustness

This table presents several robustness check for the results in Table 5. The dependent variable is the branch-level deposit growth rate, calculated as the growth from the current year to the following year. Bought by $\text{Large}_b \times \text{Post}_{b,t}$ is the interaction between the treatment effect, whether the acquirer is a bank with more than $10 billion in inflation-adjusted assets, and the post-merger indicator. Column 1 includes only instances in which the acquirer did not have a branch in the same county as the target branch. Column 2 limits the analysis to mergers that did not result in an increase in the average HHI across the counties in which the target bank has branches. Column 3 excludes observations corresponding to address changes after the merger. Column 4 restricts the analysis to zip codes that had only treatment branches or control branches, but not both. Column 5 redefines treatment and control mergers based on whether the bank has a Standard and Poor's rating, rather than whether it has more than $10 billion in inflation-adjusted assets. All regressions use my preferred instrument, the percent of nearby branches that were owned by large banks in 1994. County-year fixed effects and branch fixed effects are included in each regression. Standard errors are shown in parentheses and are clustered at the county level. * p<0.10, ** p<0.05, *** p<0.01.

Dependent Variable:	Branch Deposit Growth				
	Out of County Acquirer (1)	No HHI Increase (2)	Excluding Address Chgs (3)	No Other Mergers Near (4)	Large = with Public Rating (5)
Bought by Large x Post	-0.019*	-0.027*	-0.016**	-0.024**	-0.029**
	(0.011)	(0.016)	(0.008)	(0.010)	(0.013)
County-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Branch Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	107526	78162	163448	132442	131139
Within R-squared	0.148	0.148	0.149	0.149	0.142

Table 7: The Effect of Consolidation on Bank Fees

This table presents the results of the difference-in-differences specification of equation (2) in Section 3.1, estimating the effect of bank consolidation on deposit account fees. The dependent variable are: branch-level annualized checking account fee (column 1); checking account minimum balance needed to avoid the fee (column 2); annualized interest checking account fee (column 3); and interest checking account minimum balance needed to avoid the fee (column 4). Bought by $\text{Large}_b \times \text{Post}_{b,t}$ is the interaction between the treatment effect, whether the acquirer is a bank with more than $10 billion in assets, and the post-merger indicator. All regressions use my preferred instrument, the percent of nearby branches that were owned by large banks in 1994. County-year fixed effects and branch fixed effects are included in each regression. Standard errors are shown in parentheses and are clustered at the county level. * p<0.10, ** p<0.05, *** p<0.01.

Dependent Variable:	Regular Checking		Interest Checking	
	Fee	Min	Fee	Min
	(1)	(2)	(3)	(4)
Bought by $\text{Large}_b \times \text{Post}_{b,t}$	12.130***	228.498***	34.609***	623.131**
	(3.561)	(73.190)	(8.502)	(274.437)
County-Year Fixed Effects	Yes	Yes	Yes	Yes
Branch Fixed Effects	Yes	Yes	Yes	Yes
Observations	28341	26738	31598	30845
Within R-squared	0.002	0.087	0.039	0.051

Table 8: The Effect of Consolidation on Deposit Growth, by Area Income

This table presents the results of the triple differences specification of equation (3) of Section 4.2, estimating how the effect of bank consolidation on deposit growth differs by income and by degree of fee increase. The dependent variable is branch-level deposit growth rate, calculated as the growth from the current year to the following year. Bought by $\text{Large}_b \times \text{Post}_{b,t}$ is the interaction between the treatment effect, whether the acquirer is a bank with more than \$10 billion in assets, and the post-merger indicator. $\text{I}\{\text{Pct Poverty}\}_z$ is an indicator variable for whether the percentage of households in poverty is above the median of the distribution. $\text{I}\{\text{Pct AGI} < \$25000\}_z$ is an indicator variable for whether the percentage of households with income of less than \$25,000 is above the median of the distribution. $\text{I}\{\text{Pct EITC}\}_z$ is an indicator variable for the percentage of households who receive the Earned Income Tax Credit (EITC), a tax credit mainly aimed at working single female heads of household, is above the median of the distribution. After2011_t is an indicator for the period 2011-2016, when large bank fee and required minimum balances increased substantially due to the Durbin Amendment. All regressions use my preferred instrument, the percent of nearby branches that were owned by large banks in 1994. County-year fixed effects and branch fixed effects are included in each regression. Standard errors are shown in parentheses and are clustered at the county level. * p<0.10, ** p<0.05, *** p<0.01.

Dependent Variable:	Deposit Growth			
	(1)	(2)	(3)	(4)
Bought by $\text{Large}_b \times \text{Post}_{b,t}$	-0.011**	-0.008	-0.016**	-0.015*
	(0.005)	(0.007)	(0.008)	(0.008)
Bought by $\text{Large}_b \times \text{Post}_{b,t} \times$ Post $\times$ I{Pct Poverty}	-0.011***			
	(0.004)			
Bought by $\text{Large}_b \times \text{Post}_{b,t} \times$ I{Pct AGI < \$25000}		-0.009*		
		(0.005)		
Bought by $\text{Large}_b \times \text{Post}_{b,t} \times$ I{Pct EITC}			-0.012**	
			(0.006)	
Bought by $\text{Large}_b \times \text{Post}_{b,t} \times$ After2011				-0.021*
				(0.011)
County-Year Fixed Effects	Yes	Yes	Yes	Yes
Branch Fixed Effects	Yes	Yes	Yes	Yes
Observations	183432	115507	183432	88723
Within R-squared	0.15	0.15	0.14	0.06

Table 9: The Effect of Consolidation on Deposit Growth: Customer Service and In-State Mergers

This table presents a robustness check for the results of Table 5. The dependent variable is branch-level deposit growth rate, calculated as the growth from the current year to the following year. Bought by $\text{Large}_b \times \text{Post}_{b,t}$ is the interaction between the treatment effect, whether the acquirer is a bank with more than $10 billion in assets, and the post-merger indicator. In column 1, I restrict the sample to mergers in which the acquiring bank has a higher percentage of full time employees divided by total number of branches than the target. In column 2, I include only mergers for which the acquirer and the target have branches in the same state prior to the merger. County-year fixed effects and branch fixed effects are included in each regression. Standard errors are shown in parentheses and are clustered at the county level. * p<0.10, ** p<0.05, *** p<0.01.

Dependent Variable: Branch Deposit Growth

	Increased Empl per Br	In-State Acquirer
Bought by $\text{Large}_b \times \text{Post}_{b,t}$	-0.020*	-0.010*
	(0.011)	(0.006)
County-Year FE	Yes	Yes
Branch FE	Yes	Yes
Observations	82406	163584
Within R-squared	0.153	0.149

Table 10: The Effect of Consolidation on Check-Cashing Facilities

This table presents the results of the zip code-level difference-in-differences specification described in section 4.3, estimating the effect of bank consolidation on the number of check cashing facilities. The dependent variable is the number of check-cashing facilities per 10,000 residents. Bought by Large$_z\times$ Post$_{z,t}$ is the interaction between the treatment effect, whether the acquirer is a bank with more than $10 billion in assets, and the post-merger indicator. Big Merger$_z$ is an indicator for whether the number of branches involved in the merger was above the median (which is 1). Few Small Br$_z$ is an indicator for whether the percent of other small bank branches in the zip code at the time of the merger was below the median. See Table 8 for definitions of I{Pct Poverty}$_z$ and I{Pct EITC}$_z$. All regressions use my preferred instrument, the percent of nearby branches that were owned by large banks in 1994. County-year fixed effects and zip code effects are included in each regression. Standard errors are shown in parentheses and are clustered at the county level. * p<0.10, ** p<0.05, *** p<0.01.

Dependent Variable: Num Check-Cashing Facilities / Population

	OLS	IV				
	(1)	(2)	(3)	(4)	(5)	(6)
Bought by Large$_z\times$ Post$_{z,t}$	0.042**	0.045**	0.036	0.012	0.011	-0.001
	(0.017)	(0.023)	(0.023)	(0.026)	(0.024)	(0.035)
Bought by Large$_z\times$ Post$_{z,t}\times$ Big Merger$_z$			0.087***			
			(0.030)			
Bought by Large$_z\times$ Post$_{z,t}\times$ Few Small Br$_z$				0.061**		
				(0.024)		
Bought by Large$_z\times$ Post$_{z,t}\times$ I{Pct Poverty}$_z$					0.065***	
					(0.023)	
Bought by Large$_z\times$ Post$_{z,t}\times$ I{Pct EITC}$_z$						0.079**
						(0.033)
County-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Zip Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	123118	123118	123118	123118	123079	67125
Within R-Squared	0.002	0.002	0.002	0.002	0.002	0.003

Table 11: Trends in Zip Code Characteristics around Consolidation

This table presents the results of the zip code-level difference-in-differences specification described in section 4.3, estimating the effect of bank consolidation on other demographic and economic trends. The dependent variable in column 1 is the number of payday lenders per 10,000 residents. In column 2, the dependent variable is the total number of other establishments, excluding check cashing facilities and payday lenders, per 10,000 residents. In column 3, the dependent variable is log mortgages originated, and in column 4, it is the average adjusted gross income (AGI) from the IRS's Statistics of Income. In column 5, the dependent variable is the percentage of households with AGI less than \$25,000. In column 6, the dependent variable is the percentage of households receiving unemployment benefits. Bought by $\text{Large}_z \times \text{Post}_{z,t}$ is the interaction between the treatment effect, whether the acquirer is a bank with more than \$10 billion in assets, and the post-merger indicator. All regressions use my preferred instrument, the percent of nearby branches that were owned by large banks in 1994. County-year fixed effects and zip code fixed effects are included in each regression. Standard errors are shown in parentheses and are clustered at the county level. * p<0.10, ** p<0.05, *** p<0.01.

	Other Zip Code Characteristics					
Dependent Variable:	Payday Lenders (1)	All Other Establishments (2)	Log Mtg Orig (3)	Ave AGI (4)	Pct AGI < \$25,000 (5)	Pct Unemployed (6)
Bought by $\text{Large}_z \times \text{Post}_{z,t}$	0.011	0.064	-0.011	-0.064	0.001	-0.012
	(0.013)	(0.071)	(0.013)	(0.279)	(0.001)	(0.038)
County-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Zip Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	123118	123103	123118	104149	104149	53087
Within R-squared	0.001	0.002	0.004	0.004	0.004	0.001

Table 12: The Effect of Consolidation on Check-Cashing Facilities - Closures and Recessions

This table presents the results of the difference-in-differences specification described in section 4.3, estimating the effect of bank consolidation on the number of check cashing facilities. The dependent variable is the number of check-cashing facilities per 10,000 residents. Bought by $\text{Large}_z \times \text{Post}_{z,t}$ is the interaction between the treatment effect, whether the acquirer is a bank with more than \$10 billion in assets, and the post-merger indicator. Column 1 excludes all zip codes in which a branch of the merged institution was closed within 3 years after the merger. Column 2 excludes all zip codes in which a branch of the merged institution was closed within 5 years after the merger. Column 3 excludes recession years and the years immediately following a recession (2001-2002, 2008-2010). All regressions use my preferred instrument, the percent of nearby branches that were owned by large banks in 1994. County-year fixed effects and zip code fixed effects are included in each regression. Standard errors are shown in parentheses and are clustered at the county level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Dependent Variable: Num Check-Cashing Facilities / Population

	No Branches Closed after:		Excluding Recessions
	3 Years (1)	5 Years (2)	(3)
Bought by $\text{Large}_b \times \text{Post}_{b,t}$	0.060** (0.027)	0.057** (0.027)	0.048* (0.026)
County-Year Fixed Effects	Yes	Yes	Yes
Zip Fixed Effects	Yes	Yes	Yes
Observations	95859	89882	80148
Within R-squared	0.002	0.002	0.002

Table 13: The Effect of Consolidation and Financial Shocks: Household Delinquency

This table presents the results of the difference-in-differences specification described in section 5, estimating the effect on households of the interaction between bank consolidation and financial shocks. In columns 1 and 2, the dependent variable is an indicator for whether the household had debt sold to a debt collection agency during the 2008-2010 period. In column 3 and 4, the dependent variable is whether the household had debt sold to a collection agency during the 2002-2007 period. Bought by Large$_z$ is the treatment effect, whether the acquirer was a bank with more than \$10 billion in assets. County Unempl Shock$_c$ is an indicator for whether the county unemployment increase from 2006 to 2010 was above the median. Zip Unempl Shock$_z$ is an indicator for whether the zip code unemployment increase from 2000 to 2010 was above the median. Bought by Large$_z \times$ County Unempl Shock$_c$ and Bought by Large$_z \times$ Zip Unempl Shock$_z$ are the interaction terms between the treatment indicator and the financial shock. All regressions use my preferred instrument, the percent of nearby branches that were owned by large banks in 1994. County fixed effects, zip code controls, and age by credit score bucket fixed effects are included in each regression. Zip code controls include log number of households, population density, median income, whether the zip code is urban or rural, and percentages of households that are: black, Hispanic, aged 25-34, living in owner-occupied housing, in the labor force, unemployed, with earnings, and living in poverty. All zip code controls are as of the 2000 Census. Standard errors are shown in parentheses and are clustered at the county level. * p<0.10, ** p<0.05, *** p<0.01.

Dependent Variable:	Household Had Debt Sold to Collections Agency in:			
	2008-2010		2002-2007	
	(1)	(2)	(3)	(4)
Bought by Large$_z$	-0.006	-0.007*	0.000	-0.004
	(0.004)	(0.004)	(0.004)	(0.004)
Bought by Large$_z \times$ County Unempl Shock$_c$	0.009**		-0.001	
	(0.005)		(0.005)	
Zip Unempl Shock$_z$		0.009***		0.007*
		(0.003)		(0.004)
Bought by Large$_z \times$ Zip Unempl Shock$_z$		0.011**		0.005
		(0.005)		(0.005)
County Fixed Effects	Yes	Yes	Yes	Yes
Age by Credit Score Bucket Fixed Effects	Yes	Yes	Yes	Yes
Zip Controls	Yes	Yes	Yes	Yes
Observations	224767	224767	224767	224767
Within R-squared	0.181	0.181	0.112	0.112

Table 14: The Effect of Consolidation and Financial Shocks: Evictions

This table presents the results of the difference-in-differences specification described in section 5, estimating the effect of the interaction between bank consolidation and financial shocks on households. In columns 1 and 2, the dependent variable is the percent of households evicted during the 2009-2012 period. In column 3 and 4, the dependent variable is average zip code rent price during the 2009-2012 period. Bought by Large$_z$ is the treatment effect, whether the acquirer was a bank with more than \$10 billion in assets. County Unempl Shock$_c$ is an indicator for whether the county unemployment increase from 2006 to 2010 was above the median. Zip Unempl Shock$_z$ is an indicator for whether the zip code unemployment increase from 2000 to 2010 was above the median. Bought by Large$_z \times$ County Unempl Shock$_c$ and Bought by Large$_z \times$ Zip Unempl Shock$_z$ are the interaction terms between the treatment indicator and the financial shock. All regressions use my preferred instrument, the percent of nearby branches that were owned by large banks in 1994. County fixed effects and zip code controls are included in each regression. Zip code controls include log number of households, population density, median income, whether the zip code is urban or rural, and percentages of households that are: black, Hispanic, aged 25-34, living in owner-occupied housing, in the labor force, unemployed, with earnings, and living in poverty. All zip code controls are as of the 2000 Census. Standard errors are shown in parentheses and are clustered at the county level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Dependent Variable:	Percent Households Evicted		Rent Prices	
	(1)	(2)	(3)	(4)
Bought by Large$_z$	-0.002	-0.001	-118.741	-101.956
	(0.002)	(0.002)	(76.551)	(73.222)
Bought by Large$_z \times$ County Unempl Shock$_c$	0.005*		32.452	
	(0.003)		(87.349)	
Zip Unempl Shock$_z$		0.002**		-69.761
		(0.0002)		(48.612)
Bought by Large$_z \times$ Zip Unempl Shock$_z$		0.004*		15.237
		(0.002)		(67.959)
County Fixed Effects	Yes	Yes	Yes	Yes
Zip Controls	Yes	Yes	Yes	Yes
Observations	941	941	824	824
Within R-squared	0.263	0.263	0.446	0.446

Appendix

A Appendix Figures and Tables

Figure A1: Fees by Bank Size

This figure shows the coefficients and standard errors from a regression of different deposit accounts type fees on an indicator for whether the bank has more than $10 billion in assets and county-year fixed effects. All data are from RateWatch and the FFIEC Call Reports.

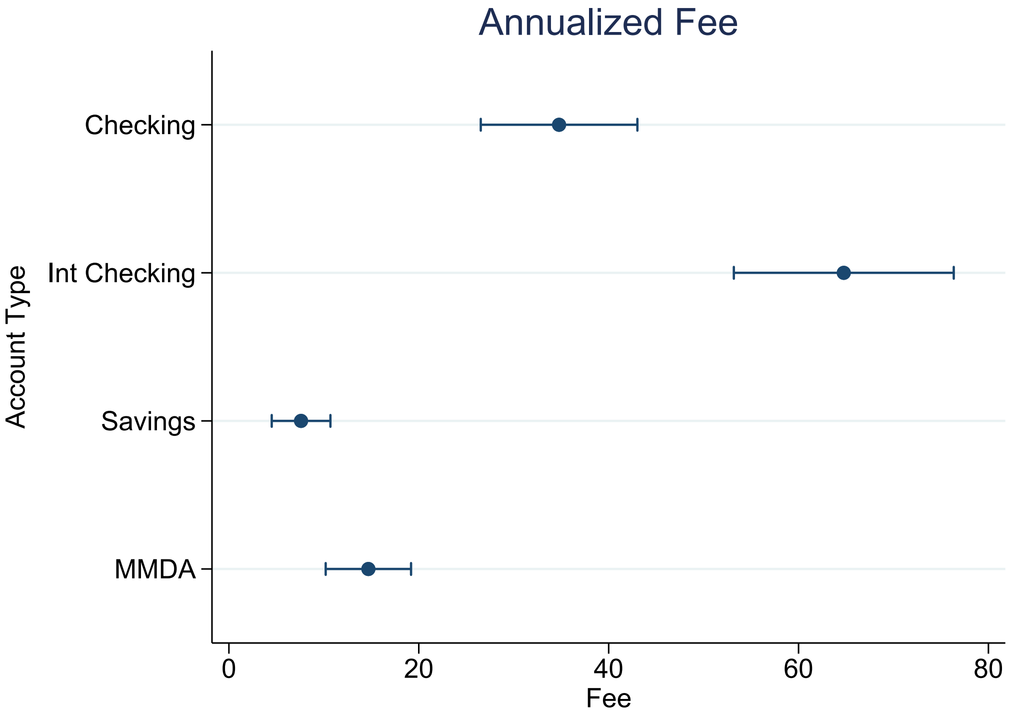


Figure A2: Rates by Bank Size

This figure shows the coefficients and standard errors from a regression of different deposit accounts type required minimum balances on an indicator for whether the bank has more than $10 billion in assets and county-year fixed effects. All data are from RateWatch and the FFIEC Call Reports.

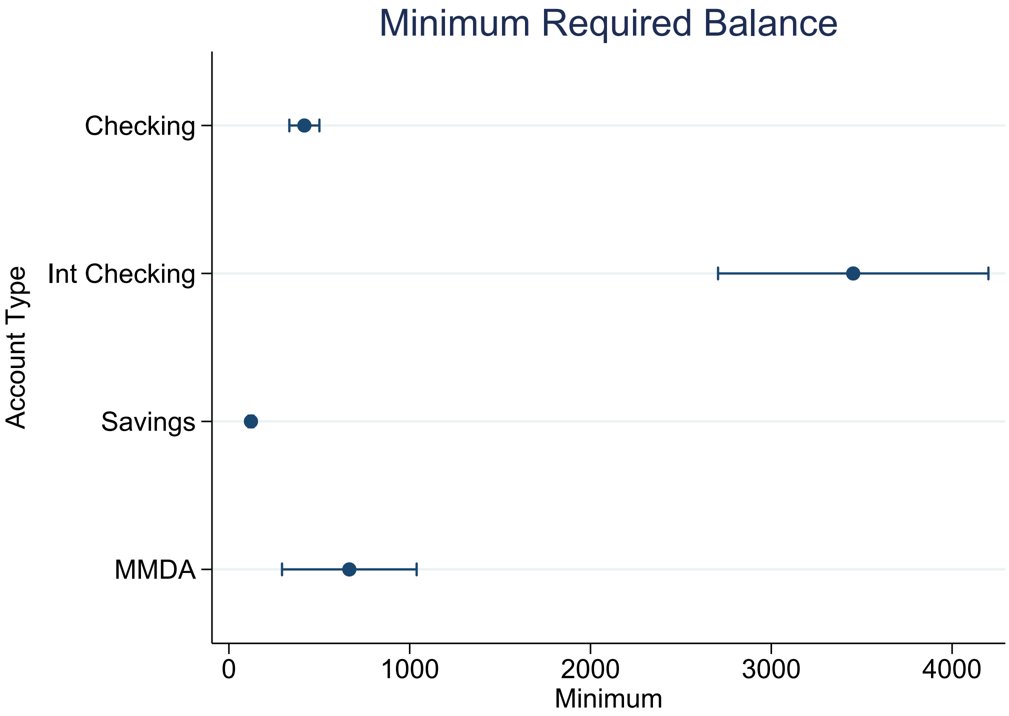


Figure A3: Rates by Bank Size

This figure shows the deposit rates on savings accounts (left panel) and money market deposit accounts (MMDA; right panel) All data are from RateWatch.

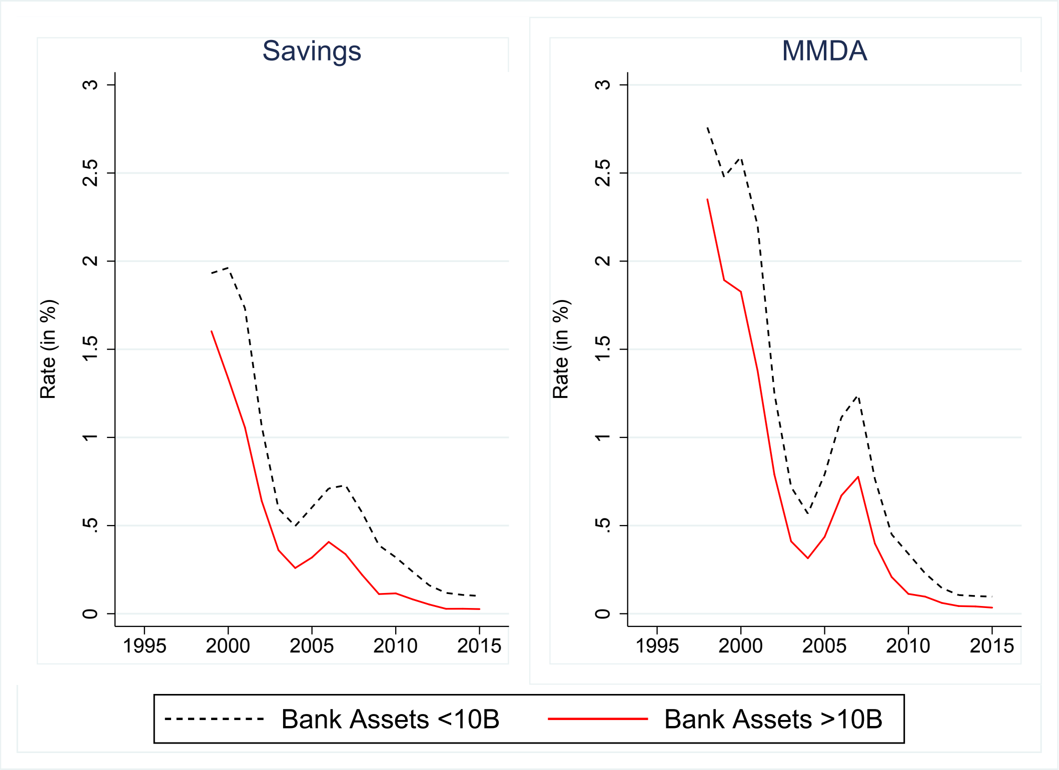


Figure A4: Deposit Growth at Other Branches

This figure plots the yearly coefficients from a difference-in-difference regression estimating the effect of consolidation on deposit growth at unacquired branches. The dependent variable is deposit growth at branches of small banks in the same zip code (top left), large banks in the same zip code (top right), small banks in the same county but not the same zip code (bottom left), and large banks in the same county but not the same zip code (bottom right). The plot shows the coefficients on the interactions between treatment, whether the bank was acquired by a large bank, and indicators for each year after the merger. Year 0 corresponds to June 30th prior to the merger.

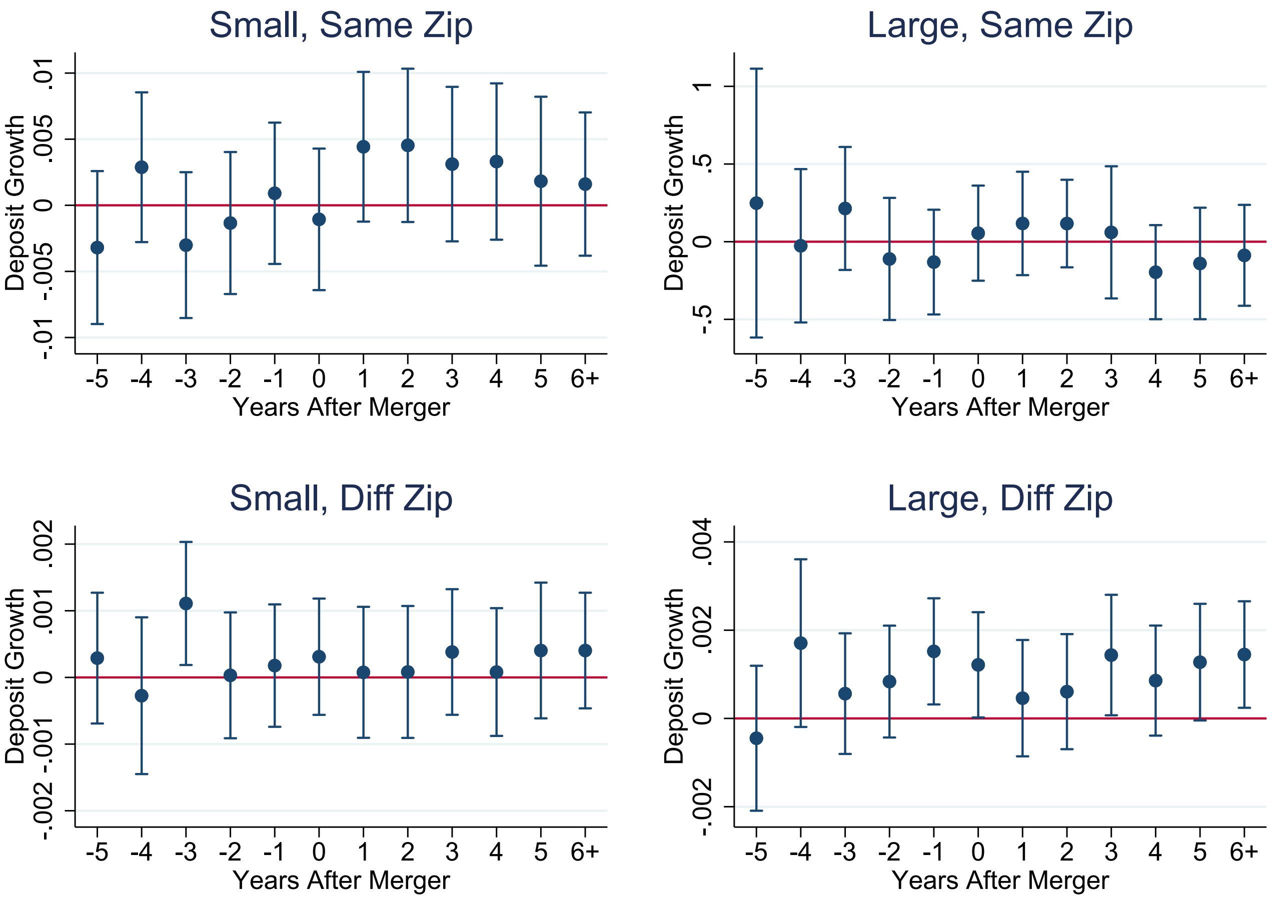


Table A1: Savings and MMDA Fees

This table shows the results of a regression of equation (1) from Section 2.1, estimating the difference in deposit account fees and minimum balances between small banks with fewer than \$10 billion in assets and large banks with more than \$10 billion in assets. The dependent variables in this panel are the annualized fee on savings accounts (column 1), the average minimum balance needed to avoid the fee (column 2), annualized fee on money market deposit accounts (MMDA; column 3), and the minimum required balance on MMDA (column 4). Each observation corresponds to a bank-county-year triple and I include county-year fixed effects. $\text{Large}_{b,t}$ is an indicator for whether the bank has more than \$10 billion in assets, in inflation-adjusted 2016 dollars. $\text{Large}_{b,t} \times \text{After2011}_t$ is the interaction between this indicator and an indicator for the 2011-2016 period. Standard errors are shown in parentheses and are clustered at the state level.

Dependent Variable:	Savings		MMDA	
	Fee (1)	Min (2)	Fee (3)	Min (4)
$\text{Large}_{b,t}$	4.757**	114.678***	9.919***	392.553**
	(2.350)	(8.789)	(2.905)	(194.234)
$\text{Large}_{b,t} \times \text{After2011}_t$	4.707**	12.049	7.889**	431.548**
	(2.257)	(7.827)	(2.985)	(166.359)
County-Year FE	Yes	Yes	Yes	Yes
Observations	118502	117594	112087	101655
Within R-squared	0.021	0.227	0.023	0.015

Table A2: What Drives Bank Account Fees?

This table runs a hedonic regression of checking account fees on account characteristics. The dependent variable is the annualized checking account fee. $\text{Rated}_{b,t}$ is an indicator for whether the bank has a rating from Standard & Poor's. Large, not $\text{Rated}_{b,t}$ is an indicator for banks without an S&P rating, but with more than $10 billion in assets, in inflation-adjusted 2016 dollars. $\text{Rated}_{b,t} \times \text{After2011}_t$ is the interaction between $\text{Rated}_{b,t}$ and an indicator for the 2011-2016 period. Large, not $\text{Rated}_{b,t} \times \text{After2011}_t$ is the interaction between Large, not $\text{Rated}_{b,t}$ and an indicator for the 2011-2016 period. Ave Dist from Bank to Other $\text{Est}_{b,t}$ is the average distance between each establishment in the county and the closest branch of the bank. It is a measure for how central the bank's branch network is to other businesses in the county. Establishment location comes from Infogroup. $\text{I}\{\text{Branches in other Counties}\}_{b,t}$ is an indicator for whether the bank has branches in another county. $\text{I}\{\text{Branches in other States}\}_{b,t}$ is an indicator for whether the bank has branches in another state. Number of $\text{Services}_{b,t}$ is the total number of other services the bank provides out of the following: billpay, person to person payments, overdraft line of credit, overdraft protection, mobile banking, domestic wire transfers, and international wire transfers. Empl per $\text{Branch}_{b,t}$ is a proxy for convenience and customer service. It is calculated as the number of full-time employees divided by the number of the bank's branches. Standard errors are shown in parentheses and are clustered at the county-year level. * p<0.10, ** p<0.05, *** p<0.01.

Dependent Variable:	Checking Account Fee			
	(1)	(2)	(3)	(4)
$\text{Rated}_{b,t}$	17.194***	16.215***	16.631**	16.969**
	(5.462)	(5.593)	(6.565)	(6.927)
Large, not $\text{Rated}_{b,t}$	-7.388	-6.826	-6.405	-6.485
	(5.893)	(6.307)	(5.997)	(5.924)
$\text{Rated}_{b,t} \times \text{After2011}_t$	36.225***	36.745***	36.585***	36.826***
	(3.730)	(3.766)	(3.851)	(3.623)
Large, not $\text{Rated}_{b,t} \times \text{After2011}_t$	13.580*	12.799*	12.608*	12.763*
	(7.283)	(7.284)	(7.412)	(7.292)
Ave Dist from Bank to Other $\text{Est}_{b,t}$		-0.283	-0.285	-0.282*
		(0.193)	(0.186)	(0.168)
$\text{I}\{\text{Branches in other Counties}\}_{b,t}$			-1.615	-2.583
			(1.723)	(2.202)
$\text{I}\{\text{Branches in other States}\}_{b,t}$			-0.356	-0.650
			(2.532)	(2.494)
Num $\text{Services}_{b,t}$				-0.195
				(0.281)
Empl per $\text{Branch}_{b,t}$				-0.000
				(0.000)
County-Year FE	Yes	Yes	Yes	Yes
Observations	100192	99619	99619	99619
Within R-squared	0.157	0.158	0.158	0.158

Table A3: Treated Banks Are Not More Likely to Be Sold

This table tests whether branches of small banks are more likely to fail, be sold or be closed than branches of large banks. The sample is treated (branches of small banks acquired by a large bank) and control (branches of small banks acquired by other small banks) branches, and the dependent variables examine their subsequent performance. Small banks are defined as those with less than $10 billion in assets and large banks are defined as those with more than $10 billion in assets. The coefficients are from a regression of subsequent branch events on county-year fixed effects and Bought by Large_b, an indicator of whether the acquirer is a bank with more than $10 billion in inflation-adjusted assets. In column 1, the dependent variable is an indicator for whether the branch is closed before the end of the sample period. In column 2, it is an indicator for whether the branch is still open 5 years after the merger. In column 3, it is an indicator for whether the branch is sold as part of a bank merger. In column 4, the dependent variable is an indicator for whether the branch subsequently fails as part of a bank failure. In column 5, the dependent variable is an indicator for whether the branch is divested. In column 6, the dependent variable is an indicator for whether the branch subsequently moves locations. I restrict the sample to the first time a branch is involved in a merger during my sample period. Standard errors are shown in parentheses and are clustered at the county level. * p<0.10, ** p<0.05, *** p<0.01.

Dependent Variable:	Indicator for Branch Event:					
	Closure (1)	Existence in 5 Yrs (2)	Sale (3)	Failure (4)	Divestiture (5)	Move out of Zip Code (6)
Bought by Large_b	-0.013	0.004	-0.028	-0.001	0.016	-0.001
	(0.028)	(0.018)	(0.037)	(0.001)	(0.016)	(0.007)
County-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	10219	10219	10219	10219	10219	10219
R-squared	0.320	0.727	0.774	0.361	0.603	0.222

Table A4: Robustness: Checking Account Fees

This table shows the results of a regression of equation (1) from Section 2.1, estimating the difference in deposit account fees and minimum balances between small banks with fewer than $10 billion in assets and large banks with more than $10 billion in assets. The sample is limited to counties in which the average large bank market share is less than the average small bank market share. The dependent variables in this panel are the annualized fee on checking accounts (column 1), the average minimum balance needed to avoid the fee (column 2), the annualized fee on interest checking accounts (column 3), and the minimum balance on interest checking accounts (column 4). Each observation corresponds to a bank-county-year triple and I include county-year fixed effects. $\text{Large}_{b,t}$ is an indicator for whether the bank has more than $10 billion in assets, in inflation-adjusted 2016 dollars. $\text{Large}_{b,t} \times \text{After2011}_t$ is the interaction between this indicator and an indicator for the 2011-2016 period. Standard errors are shown in parentheses and are clustered at the state level.

Dependent Variable:	Regular Checking		Interest Checking	
	Fee	Min	Fee	Min
$\text{Large}_{b,t}$	12.625**	123.924***	43.377***	1789.669***
	(5.276)	(36.745)	(5.000)	(231.646)
$\text{Large}_{b,t} \times \text{After2011}_t$	34.221***	469.489***	35.975***	2673.824***
	(3.958)	(52.687)	(5.841)	(503.992)
County-Year FE	Yes	Yes	Yes	Yes
Observations	26961	25943	27978	27638
Within R-squared	0.141	0.233	0.304	0.207

Table A5: Large Banks and the Unbanked - FDIC Survey

This table tests the relationship between a household banked status and the presence of large banks in the MSA using data from the FDIC Survey. The dependent variable in columns 1, 3 and 4 is whether the household was unbanked. The dependent variable in column 2 is whether the household used deposit alternative financial services (AFS), namely check cashing facilities, money orders, or prepaid cards. Large Bank Presence is the share of branches that are owned by large banks with more than $10 billion in inflation-adjusted assets. Branch Density is the number of bank branches divided by the total number of households. The number of households is as of the 2000 Census. I include region-year fixed effects and household and MSA controls. Household controls include indicators for whether the household is: in an urban MSA, black or Hispanic, foreign born, aged 65 or older, unemployed, a homeowner, married, and a single female head of household. MSA-level controls include housing density (number of households per square mile), log of total number of households, average family size, log of median income, and percentage of households that are: urban, black or hispanic, living below the poverty rate, unemployed, households aged 65 or older, with income less than $10 thousand, and with income between $10 and $35 thousand. Standard errors are shown in parentheses and are clustered at the MSA level. * p<0.10, ** p<0.05, *** p<0.01.

Dependent Variable:	Unbanked	AFS Use	Unbanked	
	(1)	(2)	(3)	(4)
Large Bank Presence	0.021*	0.064**		0.015**
	(0.012)	(0.025)		(0.007)
Branch Density			-0.014***	-0.010**
			(0.004)	(0.005)
MSA Controls	Yes	Yes	Yes	Yes
Year-Region Fixed Effects	Yes	Yes	Yes	Yes
Observations	158459	155922	158459	158459
R-Squared	0.132	0.193	0.132	0.132

Table A6: The Effect of Consolidation on Deposit Growth: Robustness

This table presents further robustness checks for the results of Table 5. The dependent variable is branch-level deposit growth rate, calculated as the growth from the current year to the following year. Bought by $\text{Large}_b \times \text{Post}_{b,t}$ is the interaction between the treatment effect, whether the acquirer is a bank with more than $10 billion in assets, and the post-merger indicator. column 1 restricts the sample to the 1994-2010 period, and column 2 restricts the sample to the 2000-2016 period. In column 3, I only use the first merger for each branch and discard any subsequent mergers. In column 4, I use branches that undergo exactly 1 merger during my sample period. In column 5, I follow the methodology of Sandler and Sandler (2014) to adjust for multiple mergers by including the sum of post merger indicators. County-year fixed effects and branch fixed effects are included in each regression. Standard errors are shown in parentheses and are clustered at the county level. * p<0.10, ** p<0.05, *** p<0.01.

Dependent Variable:	Branch Deposit Growth				
	1994-2010 (1)	2000-2016 (2)	First Merger (3)	Only Merger (4)	Mult Merger (5)
Bought by $\text{Large}_b \times \text{Post}_{b,t}$	-0.025***	-0.022*	-0.021**	-0.018**	-0.019***
	(0.008)	(0.012)	(0.009)	(0.009)	(0.006)
County-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Branch Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	136833	95282	159853	128984	181378
Within R-squared	0.162	0.132	0.149	0.144	0.147

Table A7: The Effect of Consolidation on Fees: Robustness

This table presents robustness checks for Table 7. Bought by $\text{Large}_b \times \text{Post}_{b,t}$ is the interaction between the treatment effect, whether the acquirer is a bank with more than $10 billion in assets, and the post-merger indicator. County-year fixed effects and branch fixed effects are included in each regression. Standard errors are shown in parentheses and are clustered at the county level. * p<0.10, ** p<0.05, *** p<0.01.

Panel A: No Effect on Concentration

This panel estimates the effect of consolidation on deposit account fees, limiting to mergers that did not lead to an increase in the average concentration of the counties in which the target banks operated (as measured by deposit HHI). The dependent variable is branch-level annualized checking account fee (column 1), checking account required minimum balance (column 2), annualized interest checking account fee (column 3), interest checking account required minimum balance (column 4).

Dependent Variable:	Regular Checking		Interest Checking	
	Fee (1)	Min (2)	Fee (3)	Min (4)
Bought by $\text{Large}_b \times \text{Post}_{b,t}$	19.091***	151.997**	40.280***	576.910**
	(4.071)	(71.774)	(10.054)	(285.360)
County-Year Fixed Effects	Yes	Yes	Yes	Yes
Branch Fixed Effects	Yes	Yes	Yes	Yes
Observations	24069	22757	26864	26389
Within R-squared	0.011	0.061	0.033	0.062

Panel B: Other Deposit Account Fees

This panel estimates the effect of consolidation on other types of deposit account fees. The dependent variables in this panel are the annualized fee on savings accounts (column 1), the required minimum balance on savings accounts (column 2), annualized fee on money market deposit accounts (MMDA; column 3), and the required minimum balance on MMDA (column 4).

Dependent Variable:	Savings		MMDA	
	Fee (1)	Min (2)	Fee (3)	Min (4)
Bought by $\text{Large}_b \times \text{Post}_{b,t}$	30.039***	64.236***	18.159**	843.533
	(10.506)	(21.930)	(8.056)	(627.076)
County-Year Fixed Effects	Yes	Yes	Yes	Yes
Branch Fixed Effects	Yes	Yes	Yes	Yes
Observations	31783	31458	30284	26981
Within R-squared	-0.018	0.081	0.018	0.011

Table A8: The Effect of Consolidation on Rates

Panel A: Deposit Spreads

This panel presents the results of the difference-in-differences specification of equation (2) in Section 3.1, estimating the effect of bank consolidation on deposit account spreads. The dependent variable in column 1 is the spread on the 12-month Certificate of Deposit (CD), calculated as the 12 month Treasury rate minus the 12 month CD rate. In column 2, the dependent variable is the spread on the 3-month CD, over the 3-month Treasury Bill rate. In columns 3 and 4, the dependent variables are the spreads on savings accounts and money market deposit accounts (MMDA), both calculated as the spread over the 3-month Treasury Bill rate. Bought by $\text{Large}_b\times$ $\text{Post}_{b,t}$ is the interaction between the treatment effect, of whether the acquirer is a bank with more than $10 billion in assets, and the post-merger indicator. County-year fixed effects and branch fixed effects are included in each regression. Standard errors are shown in parentheses and are clustered at the county level. Data on deposit rates are from RateWatch. * p<0.10, ** p<0.05, *** p<0.01.

	CD Rates		Other Rates	
	12 Month (1)	3 Month (2)	Savings (3)	MMDA (4)
Bought by $\text{Large}_b\times$ $\text{Post}_{b,t}$	0.222***	0.115***	0.061***	0.004
	(0.021)	(0.020)	(0.010)	(0.015)
County-Year Fixed Effects	Yes	Yes	Yes	Yes
Branch Fixed Effects	Yes	Yes	Yes	Yes
Observations	119324	115448	74707	101933
Within R-squared	0.01	0.00	0.00	0.00

Panel B: Loan Rates

This panel presents the results of the difference-in-differences specification of equation (2) in Section 3.1, estimating the effect of bank consolidation on loan rates. The dependent variable in the column 1 is the spread on the 30-year fixed rate mortgage, calculated as the mortgage rate minus the 30-year Treasury Bond rate. In column 2, the dependent variable is the spread on a 5-year adjustable rate mortgage (ARM), over the 5-year Treasury Bond rate. In columns 3 and 4, the dependent variables are the spreads on a 5-year auto loan and a home equity line of credit, respectively, both calculated as the spread over the 5-year Treasury Bond. Bought by $\text{Large}_b\times$ $\text{Post}_{b,t}$ is the interaction between the treatment effect, of whether the acquirer is a bank with more than $10 billion in assets, and the post-merger indicator. County-year fixed effects are included in each regression. I do not include branch fixed effects due to the low number of observations. Robust Standard errors are shown in parentheses. Data on loan rates are from SNL Financial. * p<0.10, ** p<0.05, *** p<0.01..

Dependent Variable: Spread on	30 Year Fixed Mtg (1)	5/1 ARM (2)	5 Year Auto (3)	HELOC (4)
Bought by $\text{Large}_b\times$ $\text{Post}_{b,t}$	-0.002	-0.019***	-0.015	-0.007*
	(0.003)	(0.006)	(0.010)	(0.004)
County-Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	4626	3124	4265	4059
Within R-squared	0.012	0.248	0.020	0.029

Table A9: Check-Cashing Facilities

This table presents further robustness checks for the results of Table 10. The dependent variable is the number of check cashing facilities per 10,000 residents. Bought by $\text{Large}_z \times \text{Post}_{z,t}$ is the interaction between the treatment effect, whether the acquirer is a bank with more than \$10 billion in assets, and the post-merger indicator. In column 1, I use the alternate IV based on the Euclidian distance between the target's loan portfolio and that of the potential acquirers. Column 2 limits the sample to peripheral branches, which are in counties with less than 5% of the bank's deposits. Column 3 restricts the analysis to a propensity-matched sample of mergers. In column 4, I use as my dependent variable the number of check cashing facilities or payday lenders from the Census County Busines Patterns dataset. County-year fixed effects and zip code effects are included in each regression. Standard errors are shown in parentheses and are clustered at the county level. * p<0.10, ** p<0.05, *** p<0.01.

Dependent Variable: Num Check-Cashing Facilities / Population

	Alternative Specifications			Census CBP
	Alt IV (1)	Peripheral (2)	Propensity (3)	Check Cashers (4)
Bought by $\text{Large}_z \times \text{Post}_{z,t}$	0.045**	0.040*	0.042*	0.160***
	(0.023)	(0.020)	(0.025)	(0.050)
County-Year Fixed Effects	Yes	Yes	Yes	Yes
Zip Fixed Effects	Yes	Yes	Yes	Yes
Observations	123118	69995	55057	114125
Within R-Squared	0.002	0.002	0.002	0.001

Table A10: Bank-Switching Behavior

This table tests whether deposit growth increases at unacquired branches in close geographic proximity to treated branches, consistent with some depositors going to other small banks, after the acquisition. The dependent variable is the zip code level deposit growth for different samples of branches. Column 1 uses as the dependent variable the average deposit growth at branches of other small banks in the same zip code as an acquisition. Column 2 uses deposit growth at branches of large banks in the same zip code as an acquisition. Columns 3 and 4 use branches of small and large banks, respectively, in zip codes that do not experience an acquisition, but are adjacent to ones that do. County-year fixed effects and zip code fixed effects are included in each regression. Standard errors are shown in parentheses and are clustered at the zip code level. * p<0.10, ** p<0.05, *** p<0.01.

	Same Zip Code		Adjacent Zip Codes	
	Small	Large	Small	Large
Bought by Large x Post	0.008*	-0.006	-0.002	-0.007
	(0.005)	(0.005)	(0.004)	(0.006)
County-Year Fixed Effects	Yes	Yes	Yes	Yes
Zip Fixed Effects	Yes	Yes	Yes	Yes
Observations	88573	68350	440580	206460
Within R-squared	0.001	0.000	0.000	0.000

Table A11: The Effect of Consolidation and Financial Shocks: Household Delinquency- Robustness

This table presents robustness checks of the results of Table 13 and shows that the findings are not driven by increased credit during the 2002-2006 boom. The dependent variable is an indicator for whether the household has debt sold to a debt collection agency during the 2008-2010 period. In column 1, County RE Boom$_c$ is an indicator for whether the county real estate price growth from 2002 to 2006 was above the median. In column 2, County RE$_c$ Bust is an indicator whether the county real estate price growth from 2006 to 2010 was below the median. In column 3, Zip Credit Inc 2002-2006$_z$ is an indicator whether the zip code credit growth from 2002 to 2006 was above the median. In column 4, Zip Credit Inc excl Mtg$_z$ is an indicator whether the zip code credit growth from 2002 to 2006, excluding mortgage debt, was above the median. All regressions use my preferred instrument, the percent of nearby branches that were owned by large banks in 1994. County fixed effects, zip code controls, and age by credit score bucket fixed effects are included in each regression. Zip code controls include log number of households, population density, median income, whether the zip code is urban or rural, and percentages of households that are: black, Hispanic, aged 25-34, living in owner-occupied housing, in the labor force, unemployed, with earnings and living in poverty. All zip code controls are as of the 2000 Census. Standard errors are shown in parentheses and are clustered at the county level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Dependent Variable:	Household Had Debt Sold to Collections Agency in 2008-2010			
	(1)	(2)	(3)	(4)
Bought by Large$_z$	-0.005	0.002	-0.002	-0.000
	(0.004)	(0.006)	(0.003)	(0.004)
Bought by Large$_z \times$ County RE Boom$_c$	0.008			
	(0.005)			
Bought by Large$_z \times$ County RE Bust$_c$		-0.001		
		(0.006)		
Zip Credit Inc 2002-2006$_z$			-0.004	
			(0.004)	
Bought by Large$_z \times$ Zip Credit Inc$_z$			0.003	
			(0.004)	
Zip Credit Inc excl Mtg 2002-2006$_z$				-0.000
				(0.003)
Bought by Large$_z \times$ Zip Credit Inc excl Mtg$_z$				0.001
				(0.004)
County Fixed Effects	Yes	Yes	Yes	Yes
Age by Credit Score Bucket Fixed Effects	Yes	Yes	Yes	Yes
Zip Controls	Yes	Yes	Yes	Yes
Observations	213989	214922	224767	224767
Within R-squared	0.181	0.181	0.181	0.181

Table A12: The Effect of Consolidation and Financial Shocks: Household Delinquency-Medical vs Non-Medical Debt

This table tests whether the results of Table 13 are driven by medical or non-medical debt. In columns 1 and 2, the dependent variable is whether the household has a non-medical debt sold to a collection agency from 2008 to 2010. In columns 3 and 4, the dependent variable is whether the household has a medical debt sold to a collection agency from 2008 to 2010. County Unempl Shock$_c$ is an indicator for whether the county unemployment increase from 2006 to 2010 was above the median. Zip Unempl Shock$_z$ is an indicator for whether the zip code unemployment increase from 2000 to 2010 was above the median. All regression use my preferred instrument, the percent of nearby branches that were owned by large banks in 1994. County fixed effects and age by credit score bucket fixed effects are included in each regression. Zip code controls include log number of households, population density, median income, whether the zip code is urban or rural, and percentages of households that are: black, Hispanic, aged 25-34, living in owner-occupied housing, in the labor force, unemployed, with earnings and living in poverty. All zip code controls are as of the 2000 Census. Standard errors are shown in parentheses and are clustered at the county level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Dependent Variable:	Household Had Debt Sold to Collections Agency in:			
	Non-Medical		Medical	
	(1)	(2)	(3)	(4)
Bought by Large$_z$	-0.001	-0.003	-0.009*	-0.006
	(0.004)	(0.004)	(0.005)	(0.004)
Bought by Large$_z \times$ County Unempl Shock$_c$	0.001		0.015**	
	(0.004)		(0.006)	
Zip Unempl Shock$_z$		0.010***		0.004
		(0.003)		(0.005)
Bought by Large$_z \times$ Zip Unempl Shock$_z$		0.004		0.011**
		(0.005)		(0.005)
County Fixed Effects	Yes	Yes	Yes	Yes
Age by Credit Score Bucket Fixed Effects	Yes	Yes	Yes	Yes
Zip Controls	Yes	Yes	Yes	Yes
Observations	224767	224767	224767	224767
Within R-squared	0.181	0.194	0.042	0.044

Table A13: The Effect of Consolidation and Financial Shocks: Household Delinquency- Alternative Approaches

In this table, I run the full triple-difference methodology with a time-varying personal financial shock. In columns 1-3, I use as my measure of shock whether the zip code experienced a natural disaster (storm, tornado, hurricane, earthquake, or flood) in the top 5% of the distribution of property damage. In columns 4-6, I use as my measure of personal financial shock a Bartik-type unemployment shock. I follow Di Maggio and Kermani (2017) and construct unemployment growth as a zip-code industry-weighted measure of nation-wide changes in industry employment. I calculate for each industry, the nationwide yearly changes in employment excluding the zip code at hand, and weight these employment changes by the shares of each industry in the zip code. This results in a zip-code level measure of employment changes which is driven by overall national trends and differences in industry shares across zip codes. I define my shock as an unemployment increase in the top 5% of the distribution of employment changes. In columns 1 and 4, the dependent variable is whether the household has debt sold to a collection agency from 2008 to 2010. In columns 2 and 5, the dependent variable is whether the household has a non-medical debt sold to a collection agency from 2008 to 2010. In columns 3 and 6, the dependent variable is whether the household had a medical debt sold to a collection agency from 2008 to 2010. All regression use my preferred instrument, the percent of nearby branches that were owned by large banks in 1994. County fixed effects and zip code fixed effects are included in each regression. Standard errors are shown in parentheses and are clustered at the county level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

	Disasters			Bartik Shock		
	Any Debt (1)	Non-Medical (2)	Medical (3)	Any Debt (4)	Non-Medical (5)	Medical (6)
Bought by Large$_z$× Post$_{z,t}$	-0.005	-0.002	-0.006**	-0.000	0.002	-0.007**
	(0.005)	(0.005)	(0.003)	(0.006)	(0.005)	(0.003)
Bought by Large$_z$× Post$_{z,t}$ × Disaster$_{z,t}$	0.041**	0.020	0.027			
	(0.017)	(0.017)	(0.017)			
Bought by Large$_z$× Post$_{z,t}$ × Bartik$_{z,t}$				0.025*	0.011	0.019**
				(0.013)	(0.014)	(0.009)
County-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Zip Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	652167	652167	652167	652167	652167	652167
Within R-squared	0.021	0.021	0.003	0.021	0.021	0.004

B Datasets and Data Creation

B.1 Deposits: Creating a Branch-level Dataset Consistent over Time

For the purposes of my analysis, it is important to have a panel dataset of branches and follow each branch over time. Although the FDIC summary of Deposits (SOD) dataset provides the variable uninumbr, which is meant to create a consistent time-series for each branch, this variable has several drawbacks. First, it is not defined for most branches of thrifts, prior to 2011. Second, there are instances of 2 branches within the same bank ”swapping” uninumbrs. For example, from 2003-2008, uninumbr 13448 corresponds to a branch located at 901 North Boll Weevil Circle in Enterprise, Alabama, with deposits of \$120-\$140 million. Uninumbr 249140 corresponds to a branch at 3680 West Main Street in Dothan, Alabama, with deposits of approximately \$50 million. However, in 2009, the branches “swap places” and from 2009-2016, uninumbr 13448 corresponds to a branch at 3680 West Main Street in Dothan, Alabama, with deposits of approximately \$50 million and uninumbr 249140 corresponds to a branch in Enterprise, Alabama with deposits of \$120-\$140 million.

Third, the uninumbr seems to correspond to a bank's office rather than a fixed geographic location. Offices may move around and switch locations with other offices, especially immediately after mergers. There are many instances of an office (uninumbr) of the target bank closing and an office of the acquiring bank moving to that same location. For the purposes of my paper, however, and for the local population who have accounts at this branch, the closing is irrelevant, since the branch remains open and accessible to depositors.

Most papers either aggregate the SOD data to the county level (e.g. Bord et al. (2017)) or use the SOD data to identify branches at a single point in time (e.g. Nguyen (2017)). For these papers, most inconsistencies regarding the time series of each branch are irrelevant. However, because I use branch-level deposit and fee data and track each branch over time, these inconsistencies may introduce much more noise into the analysis.

To adjust for these differences between what uninumbr tracks and what is required for my paper, I use the SOD data, and augment it with both an algorithm that matches branches based on location and data from SNL Financial, which also has data on bank branches and has its own internal identifier. The algorithm matches branches 1) first based on address within the same identifier (first Federal Reserve RSSD, then FDIC cert), and 2) then based on address accounting for bank mergers. I use fuzzy string matching to account for typos and changes in the address, as well as differences in zip code definitions over time. I check this algorithm and the original uninumbr against the identifier snlbranchkey, the internal SNL identifier for bank branches. Since there is always the possibility of both type I and type II error, for the analysis in this paper, I only keep branch matches which have a very high confidence of being correct.

The final outcome is a dataset that is better lined up than either the SOD or the SNL branch datasets. First, whereas the SOD dataset contains approximately 131,600 unique uninumbrs, they correspond to 128,600 unique branches in my dataset. Second, yearly deposit growth is less noisy in my dataset, with a lower standard deviation and interquartile range.

B.2 Check-Cashing Outlets: County Business Patterns and Infogroup

The County Business Patterns (CBP) dataset contains information on the number of establishments by zip code and year for each six digit NAICS category. The NAICS category 522390 comprises "establishments primarily engaged in facilitating credit intermediation (except mortgage and loan brokerage; and financial transactions processing, reserve, and clearinghouse activities)." Prior papers have used this NAICS category to identify payday lenders (Bhutta, 2014; Melzer, 2011), but the category also includes check cashing facilities. Many check cashing facilities engage in payday lending, and vice versa, but the two types of activities are distinct and serve as substitutes for two different types of bank services. Payday lending is a substitute for bank consumer lending, whereas check cashing is a substitute for bank deposit account services. For this paper, it is important to disentangle the two types of establishments since I focus on depositors and their demand for deposit account alternatives, not credit alternatives. In addition, having a bank account is a pre-requisite for most types of payday lending.[38]

To distinguish these two types of establishments, I turn to data from Infogroup. Infogroup collects and verifies establishment location data from thousands of yellow and white pages books around the country. For each establishment, it reports the address and name, as well as the detailed NAICS and SIC codes the establishment falls under. I identify check cashing outlets as those in SIC code 609903, "Check Cashing Services," as well as those that have both "Check" and "Cash" in their names. I identify payday lenders as those in SIC code 614113, "Payday Loans", as well as establishments that have the word "Cash" in their name, but are neither pawnshops, gold stores, nor check cashing facilities. The zip code level number of payday stores from CBP and from Infogroup have a correlation of 0.8.

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[38] Most banks do not engage in small value consumer lending, instead satisfying demand for these loans by extending credit cards. Since large banks are more likely to issue credit cards, demand for payday lending should remain constant or decrease after mergers involving large banks.

C Cross-Sectional Results from the FDIC Survey

In this section, I use the FDIC's National Survey of Unbanked and Underbanked Households (FDIC Survey) to examine the correlation between the presence of large banks and the prevalence of unbanked households. Because the FDIC survey contains geographical data only at the MSA level, I am not able to address issues of causality using the survey data. Instead, I show cross-sectional correlations that are suggestive evidence of a link between the presence of large banks and the prevalence of unbanked households.

FDIC Survey Data

The FDIC Survey dataset is a nationally representative survey that contains information on households' banking status and use of altenative financial services. The survey is conducted as part of the Consumer Population Survey (CPS) by the census, and was conducted 4 times—in January 2009, June 2011, June 2013, and June 2015. Approximately 47,000 individuals filled out the survey each time, answering questions on whether they have a bank account, whether they use alternative financial services (AFS) and what types, and various socio-economic questions. The detailed, individual-level information on banking status and AFS use is the main advantage of the data. The main limitation of these data is that the geographic identifier for the household's location is available only at the MSA level in the public data. In addition, there are only 4 years of repeated cross-sectional data.

Methodology

Using data from the FDIC survey and FDIC's Summary of deposits, I regress an individual's banking status on the presence of large banks in the MSA and controls. Specifically, I run a regression of the form:

$$Y_{i,m,r,t} = \alpha + \beta \text{Large Bank Presence}_{m,r,t} + \gamma I_{i,m,r,t} + \delta M_{m,r,t} + \lambda_{r,t} + \epsilon_{i,m,r,t} \tag{A1}$$

The dependent variable, $Y_{i,m,r,t}$, is either an indicator for whether the household i in MSA m in region r surveyed at time t has a bank account or an indicator for whether the household has ever used different types of alternative financial services (AFS), such as check cashing facilities, money orders, or prepaid cards. The main variable of interest is Large Bank Presence$_{m,r,t}$, which is a measure of the presence of large banks in MSA m in region r at time t. $I_{i,m,r,t}$ are individual-level controls for individual i, and $M_{m,r,t}$ are MSA-level controls. I discuss the specific dependent and independent variables I use below. $\lambda_{r,t}$ are region-year fixed effects. The regions in the survey are Northeast, Midwest, South, and West and time periods are 2009, 2011, 2013 and 2015. Each observation is weighted by its household survey sampling weight, provided by the CPS, to account for the sampling methodology of the survey. Throughout this section, standard errors are clustered at the MSA level.

From the FDIC Survey, I include as household-level controls the following indicator variables corresponding to whether the household: lives in an urban part of the MSA, is black or Hispanic, foreign born, aged 65 or older, unemployed, a homeowner, married, single female head of household. In addition, since the FDIC Survey and CPS include a limited number of household characteristics and because whether a household is unbanked may also be influenced by other MSA-level factors (such as availability of AFS, the banking status and AFS use of the household's network), I also include MSA-level controls. From the 2000 census, I include the housing density (number of households per square mile), the log of total number of households, average family size, the log of median MSA income, and percentages of households that are: living in the urban part of MSA, black, Hispanic, living below the poverty line, aged 65 or older, unemployed, income less than \$10 thousand, and with income between \$10 and \$35 thousand.[39] I also include the yearly MSA deposit HHI and the MSA debt to income ratio as of 2006.[40] Most of these controls have the expected signs. As previous literature has found, households located in urban areas, minority households, and unemployed households, and households with a single female head of household are more likely to be unbanked. Older individuals and those that own their house are less likely to be unbanked.

Results

Table A5 presents the results of the regression described above, showing the correlation between the presence of large banks and the prevalence of unbanked households. In column 1, the dependent variable is an indicator for whether the household has a bank account.

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[39] I use 2000 census data since my first of data is 2009. Using the 2010 data for the variables available from the 2010 Census does not change the results

[40] I compile the MSA-level debt to income data from the county data available on Amir Sufi's website.

My main variable of interest is the presence of large banks in the MSA, which is calculated as the ratio of the branches in the MSA that belong to large banks.[41] The coefficient is positive and significant which implies that individuals in MSAs with a higher presence of large banks are more likely to be unbanked. In column 2, I use as the dependent variable an indicator for whether the household has ever used any deposit alternative financial services; namely: check cashing facilities, prepaid cards or money orders. Since the bank accounts and deposit AFS use are substitutes, households in areas with a higher presence of large banks are both more likely to be unbanked and more likely to use AFS. These results are both statistically and economically significant. Individuals in an MSA with a Large Bank Presence one standard deviation higher than the mean are approximately 0.5% more likely to be unbanked and 1.4% more likely to use AFS.

Comparison to Celerier and Matray (2017)

At first glance, these results are inconsistent with those of Celerier and Matray (2017), who argue that interstate branching deregulation has decreased, not increased, the percent of unbanked households. Yet the changes in the banking industry have resulted in two counteracting forces that impact the unbanked. On the one hand, as Celerier and Matray (2017) show, the increase in the number of bank branches has decreased the number of unbanked households. On the other hand, the consolidation that followed increased the proportion of large banks, which I argue increase the percent of unbanked households. To clarify this distinction, I follow Celerier and Matray and in column 3, I repeat the regression of column 1 using as the independent variable Branch Density$_{m,r,t}$, calculated as the number of branches per household in the MSA. Consistent with the results of Celerier and Matray, the coefficient is negative and significant, suggesting that in MSAs with more branches per household, households are less likely to be unbanked. In column 4, I include both branch density and the presence of large banks. Both variables maintain their signs from the previous columns. This suggests that the positive relationship between the existence of bank branches and the banked status that Celerier and Matray (2017) find is driven mostly by small banks. A higher presence of large banks, on the other hand, increases the percentage of unbanked households. Since large banks tend to have higher fees, I check in unreported results that MSAs with a higher presence of large banks also tend to have higher checking and savings fees and that individuals in MSAs with higher average fees are more likely to be unbanked.

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[41] Although the existing literature often measure bank presence using share of deposits, share of branches is more applicable in this analysis since it is the existence of a branch, and not its size, that is relevant for a lower-income depositor's decision to open an account. Using a share of deposits would overestimate the presence of large banks because large banks' deposits are driven to a large extent by the large deposit accounts of firms and wealthy individual rather than retail deposits. Using the share of deposits produces qualitatively similar results.

D A Model of Bank Pricing

In this section, I present a simplified model of bank pricing. I make the following assumptions. First, as in Somaini and Einav (2013), I model the market as an N-1 dimensional simplex with the banks located at the N vertices and consumers located along each edge. This assumption is essentially a generalization of the standard Hotelling line and it makes sure that all banks compete directly with all other banks. Second, consumers have access to three products–loan accounts, savings deposit accounts and checking accounts, decide (separately) which of their top two banks to use for each product. Third, as in prior literature such as Barros (1999) and Park and Pennacchi (2009), there are two types of banks–large and small–which differ in their ability to access wholesale funding. I describe these assumptions in detail below.

D.1 Competition

I follow von Ungern-Sternberg (1991) and Somaini and Einav (2013) in considering a modification to the standard Salop (1979) circle model of competition. In the Salop model, both banks and consumers are distributed around a circle, and each consumer chooses between the two banks closest to him. By contrast, I model the market as an n-dimensional simplex, with banks located at the N vertices and consumers located along the the N(N-1)/2 edges. As in the circle model, each consumer chooses only between the two banks at either end of the edge, but each pair of banks has consumers who choose between them. Customer location and distance from banks can be interpreted as either physical distance, or a measure of how different each bank's product is from a customer's preferred product, based on physical distance, idiosyncratic preferences, and other factors. The advantage to this model, over the standard Salop circle, is that each bank competes with all others for consumers, and this greatly simplifies the analytic complexity of the model. [42]

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[42] As I discuss in Bord (2018), the assumption of a Salop circle model imposes a strong constraint on the choice set of consumers. If consumers choose only between their top two banks–for example due to limited attention–then the Salop circle constrains the $\frac{N(N-1)}{2}$ pairs to just N-1 possible choices. Second, as shown in prior literature, the Salop circle assumption results in a cascading effect when firms have heterogeneous costs (Park and Pennacchi, 2009). I argue in Bord (2018) that this effect is at odds with the reality of the banking industry.

D.2 Consumers

I assume that consumers are spread uniformly along the N(N-1) edges of the simplex, and that each edge has length 1. Consumers are of two types–savers and borrowers. I abstract away from the overlapping generations model underlying this assumption and assume that a fraction π^ℓ of consumers are borrowers and π^d are savers. Savers deposit an amount D into a savings account, which pays interest rate r^d. Borrowers borrow an amount L from the bank, at interest rate r^ℓ. Both savers and borrowers face a per-unit of distance, per dollar 'transportation' cost t, which represents the cost of dealing with the bank, getting to a branch or ATM, and so on.[43]

In addition, all consumers have access to a checking account that has cost f and provides liquidity services. The liquidity services provided by the checking account are assumed high enough that all consumers choose to have the account.[44] For simplicity, I abstract away from the existence of the unbanked, households that do not have any type of bank account and who comprise 6-8% of households.

First, consider a consumer choosing between banks i and j for a checking account. If she is located between banks i and j at a distance $d_{i,j} \in [0,1]$ from bank i, she is indifferent between the two banks if:

$$-f_i - td^c_{i,j}K = -f_j - t(1 - d^c_{i,j})K$$

Therefore, the demand for checking accounts from bank i is given by:

$$X^c_i = \sum_{j \neq i} d^c_{i,j} = \sum_{j \neq i} (\frac{1}{2} + \frac{f_j - f_i}{2tK})$$

A borrower and a saver located between banks i and j face similar choices.[45] Therefore, the demand for loans from bank i is:

$$X^\ell_i = \sum_{j \neq i} d^\ell_{i,j} = \sum_{j \neq i} \frac{1}{2} + \frac{(r^\ell_j - r^\ell_i)}{2t}$$

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[43] Alternatively, the transportation cost can be interpreted as the difference between the products the banks offer, and the ideal product the consumer would want, in terms of various qualities unrelated to the costs of the account, such as customer service.

[44] According to the Federal Reserve's Survey of Consumer Finances and the FDIC's Survey of Banked and Unbanked Households, more than 90% of households have some type of bank account.

[45] I assume that the per-dollar, per-unit of distance transportation cost is the same for all consumers. I also assume that borrowers have the cash on hand to pay the interest on the loan.

And the demand for savings funds from bank i is given by:

$$X_i^d = \sum_{j \neq i} d_{i,j}^d = \sum_{j \neq i} \frac{1}{2} + \frac{r_i^d - r_j^d}{2t}$$

D.3 Banks

A bank has cost of issuing a loan of c^ℓ, cost of maintaining deposits of c^d and cost of maintaining a checking account of c^c. Note that all three costs also include the cost of acquiring new customers for that product.[46]

As in Park and Pennachi (2009), I assume that there are two types of banks. The market contains R large banks, which have access to wholesale funding and are less reliant on (checking and savings) deposits, and N-R small banks that do not have access to wholesale funding and are thus more reliant on (checking and savings) deposits for funding.

A small bank chooses r_i^d, r_i^ℓ, f_i, equity E with cost r_E to maximize profit:

$$\max_{r_i^d, r_i^\ell, f_i, E_i} \pi^\ell X_i^\ell (r_i^\ell - c^\ell) L - \pi^d X_i^d)(r_i^d + c^d) D + X_i^c (f_i - c^c) - r_E E_i$$

subject to balance sheet constraint: $\pi^\ell X_i^\ell)L = \pi^d X_i^d D + X_i^c K + E_i$, where X_i^ℓ, X_i^d, and X_i^c are the total demand for loans, deposits, and checking accounts of bank i.

A large bank differs from a small bank only in that large banks are able to access wholesale funding markets F, and the cost of raising wholesale funds is lower than the cost of equity: $r_E > r_F$.[47] A large bank thus chooses r_i^d, r_i^ℓ, f_i, E_i and wholesale funding F_i with cost r_F to maximize profit:

$$\max_{r_i^d, r_i^\ell, f_i, E_i, F_i} \pi^\ell X_i^\ell (r_i^\ell - c^\ell) L - \pi^d X_i^d)(r_i^d + c^d) D + X_i^c (f_i - c^c) - r_E E_i - r_F F_i$$

subject to the balance sheet constraint $\pi^\ell X_i^\ell)L = \pi^d X_i^d D + X_i^c K + E_i + F_i$. Large banks also face a capital constraint ρ such that the large bank's equity must be higher than at least ρ of its other liabilities: $E_i \geq \rho(F_i + \pi^d X_i^d + X_i^c K)$.

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[46] For simplicity, I assume that the cost of maintaining the accounts is the same for all banks.

[47] See Bord (2018) for a more detailed discussion of differences between small and large banks, and a model in which all banks are able to cross-sell new products to existing customers, and large banks set prices uniformly across multiple markets.

D.4 Equilibrium

Note that when solving the first-order conditions, each price is independent of all other prices. Since there are R large banks and N-R small banks, the best response of a small bank i to the fees f_L and f_S, set by all other large and small banks is given by the first order condition:

$$\frac{N-1}{2t}(f_i - c^c - r_E K) + (N-R-1)(\frac{1}{2} + \frac{f_S - f_i}{2t}) + R(\frac{1}{2} + \frac{f_L - f_i}{2t}) = 0$$

Similarly, the best response of a large bank j is given by:

$$\frac{N-1}{2t}(f_j - c^c - r_F K) + (N-R)(\frac{1}{2} + \frac{f_S - f_j}{2t}) + (\frac{1}{2} + \frac{f_L - f_j}{2t}) = 0$$

In equilibrium $f_i = f_S$ and $f_j = f_L$, so solving the two equations above, we get:

$$\begin{aligned} f_S &= c^c - r_E K + tK + \frac{R}{2N-1}(r_E - r_F)K \\ f_L &= c^c - r_F K + tK - \frac{N-R}{2N-1}(r_E - r_F)K \\ f_L - f_S &= \frac{N-1}{2N-1}(\frac{r_E - r_F}{1+\rho}K) > 0 \end{aligned} \tag{A2}$$

Similarly, the deposit and loan rates are given by:

$$\begin{aligned} r_S^D &= c^d + r_E - t + \frac{R}{2N-1}(r_E - r_F) \\ r_L^D &= c^d + r_F - t - \frac{N-R}{2N-1}(r_E - r_F) \\ r_L^D - r_S^D &= -\frac{N-1}{2N-1}(r_E - r_F) < 0 \\ r_S^L &= c^\ell + r_E + t + \frac{R}{2N-1}\frac{r_E - r_F}{1+\rho} \\ r_L^L &= c^\ell + \frac{r_E + \rho r_F}{1+\rho} + t - \frac{N-R}{N-1}\frac{r_E - r_F}{1+\rho} \\ r_L^L - r_S^L &= \frac{N-1}{2N-1}\frac{r_E - r_F}{1+\rho} < 0 \end{aligned} \tag{A3}$$

In all cases, equilibrium prices depend on the cost of the product to the bank, taking into account the bank's access to funding markets, and the transportation cost, which is a measure of the bank's monopoly pricing. The last term, which reflects the effect of competing against a different-sized bank, depends on the differences in the cost of the product for each type of bank and the presence of each type of bank in the market. Similarly to Park and Pennacchi (2009), the model explains why large banks offer lower deposit and loan rates, and why they charge higher fees on their transaction accounts.