Do Bank Branches Still Matter? The Effect of Closings on Local Economic Outcomes

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Abstract

This paper studies branch closings to assess whether physical bank branches still play an important role in facilitating credit access in the U.S. The identification strategy exploits within-county, Census tract level variation in exposure to post-merger consolidation. Closings lead to a prolonged decline in local small business lending, which in turn translates into a reduction in employment growth rates. The effects are very localized, dissipating within 6-8 miles, and are most severe in low-income neighborhoods. These results show branches are still important in markets where lending is information-intensive and lender-specific relationships, once broken, are difficult to replace.

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1 Introduction

Banks are among the most heavily regulated industries in the economy, and a primary policy objective is to ensure local access to the banking system. The FDIC, for example, requires that banks provide 90-day notice ahead of any intention to close a branch; this is intended to facilitate public discussion of "the adverse effect the closing may have on the availability of banking services in the affected area."[1] However, one might question the extent to which these concerns are still relevant. In a world where online and mobile banking are widely used, and technological advancements have vastly increased the reach of arms-length financing, do bank branches still matter?

This question is particularly timely given the dramatic increase in branch closings in the wake of the Great Recession. Figure 1 shows that after fifteen years of uninterrupted expansion, the U.S. branch network has been shrinking since 2010. This trend is widely expected to continue, and has generated widespread concern regarding the impact on local communities. The most highly publicized cases have been those in which closings have led to the emergence of "banking deserts": neighborhoods or towns that are left without ready access to another branch.[2] Data from the FDIC show that 20% of branch closings since 2010 have been cases in which the closed branch was the only one in its Census tract (the median tract is 2 square miles).[3]

Mirroring this emphasis, existing regulation vis-à-vis branch closings is geared almost exclusively toward helping communities where closings lead to a substantial decline in the number of local branches. The FDIC's 90-day rule, for example, is waived in cases of consolidation where the branches involved are "within the same neighborhood." Yet according to the FDIC data, at least 80% of closings occur in areas where there is no meaningful impact on physical access as measured by the number of remaining branches. Could closings still have an impact in these cases?

[1] See https://www.fdic.gov/regulations/laws/rules/5000-3830.html. Similarly, under the Community Reinvestment Act, regulators may organize public forums between banks and community groups in low- and middle-income areas when there is concern regarding the effect of a closing on local accessibility to bank services (Barr (2005), Skillern (2002)).

[2] See the March 31, 2013, *Wall Street Journal* article titled "After Years of Growth, Banks are Pruning Their Branches," and the November 13, 2013, story from NPR titled "'Banking Deserts' Spread Across Low-Income Neighborhoods."

[3] This figure is obtained by geocoding branch locations and closings as reported in the FDIC Summary of Deposits and the FDIC Report of Changes.

This paper evaluates that question by estimating the local economic effects of bank branch closings in areas where the branch network is dense. I estimate the impact of closings on local credit supply as measured by the volume of small business and mortgage loans originated to borrowers located in areas where branches close. The empirical challenge is that banks choose which branches to close, and those decisions are related to local economic conditions that are correlated with credit demand. Branches will close in areas where current or forecasted profitability is expected to be low, and a naïve comparison between areas where branches close and areas where they do not would likely overestimate the impact of the closing itself.

As a solution to this endogeneity problem, I use exposure to post-merger consolidation as an instrument for branch closings. Many mergers are followed by a period of retrenchment during which branches are closed in areas where the two previously-separate networks overlap. I therefore define "exposure" to be a binary variable equal to 1 for neighborhoods that had branches from both Buyer and Target banks prior to the merger, and 0 for neighborhoods that had branches from only one or neither. To use only plausibly exogenous variation, I focus on mergers between large banks (i.e., banks with at least \$10 billion in pre-merger assets) and use Census tract level data that allow me to exploit within-county variation in exposure to consolidation. Since the median tract is only 1.5 square miles – compared to 586 square miles for the median county – this level of geographic disaggregation allows me to compare economically similar areas with and without closings and to measure the effects of a closing at a very local level.

Figure 2 illustrates this identification strategy for a sample merger and a sample county in the data. The empirical framework compares the pre- and post-merger level of lending in "exposed" tracts relative to a set of control tracts that (i) are located in the same county and (ii) had branches belonging to at least two large banks who did not merge with one another. The spirit of this approach is to compare tracts that, *a priori*, were equally likely to have been exposed to a large bank merger. This instrument identifies the effect of closings that occur in substantially crowded markets (the average Exposed tract has 6 branches prior to the merger) and are precisely those excluded from the FDIC's 90-day rule. As such, the results are informative for whether closings have disruptive effects even when the local banking market is very dense.

This paper yields three primary findings. First, closings are associated with a substantial and prolonged decline in credit supply to local small businesses. Small business lending is 8% lower for several years after a closing and remains depressed despite the entry of new banks, which shows the decline is not driven by the competitive effects of the merger. Moreover, closings have essentially no effect on local mortgage lending, indicating that the decline in small business lending is unlikely to be driven by demand-side factors. Second, the decline in lending is more severe in low-income and high-minority tracts, indicating that closings are most disruptive in disadvantaged neighborhoods. Third, the impact of a closing is very localized: the magnitude of the effect decreases monotonically as distance from the closed branch increases, and ultimately dissipates 6-8 miles out.

These results show that, even in crowded markets, closings have large effects on local credit supply when lending is information-intensive and lender-specific relationships are difficult to replace. These dynamics are less important in the mortgage market, where rates of securitization are very high and the process of loan approval has become largely automated. Small business lending, on the other hand, is an information-intensive market. If personnel-specific soft information is destroyed when a branch is closed, borrowers may face a prolonged decline in credit supply until they are able to build a relationship with a new lender. This is consistent with the finding that the effects of closings are more severe in marginalized neighborhoods, where borrowers may be particularly dependent on soft information and lender-specific relationships. I also show the decline in small business lending translates into a 2 percentage point reduction in local employment growth rates, all of which is concentrated amongst the establishments most likely to be reliant on relationship-dependent bank lending (e.g., small, single-unit establishments and private establishments).

The welfare implications of this decline in local credit supply hinge on the characteristics of the marginal borrower. If closings restrict credit access for positive NPV borrowers, then the decline in lending is welfare-reducing. If, however, these are negative NPV borrowers, then the decline in lending may be welfare-enhancing. The data sources used in this paper do not include borrower and loan characteristics, such as default rates, that can distinguish empirically between these possibilities, and so the welfare implications are ultimately ambiguous.

Nevertheless, this paper speaks to several important policy issues. First, I show closings are more disruptive in disadvantaged neighborhoods even though the number of branches does not vary systematically between upper- and lower-income tracts in my sample. This is highly policy-relevant as existing regulation is heavily concerned with lending in low-income and minority neighborhoods, where borrowers have historically faced high barriers to credit access. The focus on dense banking markets also highlights that, when lender-specific relationships are difficult to replace, restricting physical access is not the only channel through which closings can have substantial impacts on local communities. This indicates that the current focus of banking regulation vis-à-vis branch closings and bank mergers may be overly narrow. More broadly, this paper shows that bank branches do still matter. Despite technological advances that have seemingly rendered physical branches increasingly irrelevant, local credit markets still play a meaningful role in determining local credit supply.

This paper complements a rich body of work that has explored how the infrastructure of local banking markets matters for local outcomes (Jayaratne and Strahan (1996), Black and Strahan (2002), Burgess and Pande (2005), Cetorelli and Strahan (2006), Kerr and Nanda (2009), Gilje (2012), Gilje, Loutskina and Strahan (2013)), but, to the best of my knowledge, is the first to study the effects of branch closings. Papers that have studied the effects of bank consolidation on small business lending have found either negative or neutral effects (Strahan and Weston (1996), Strahan and Weston (1998), Berger, Saunders, Scalise and Udell (1998), Peek and Rosengren (1998), Sapienza (2002)). These papers are motivated by the concern that lending will fall when large banks acquire smaller ones since large banks are less well-suited to relationship-intensive lending (Stein (2002), Berger, Miller, Petersen, Rajan and Stein (2005)). Relative to these, this paper shows that the destruction of branch-level soft information is an important factor even in mergers between large banks. Several papers have explored the extent to which small business lending markets are still highly localized despite technological changes that facilitate long-distance lending (Petersen and Rajan (2002), Amel and Brevoort (2005), Brevoort, Holmes and Wolken (2010)). The strong, and very localized, response of small business lending to branch closings found in this paper suggests that while these advances may have lessened the importance of physical distance in small business lending, they have not managed to eradicate it entirely. Finally, while an existing literature has used state- or county-level data to estimate the effects of negative local credit supply shocks in the U.S. (Peek and Rosengren (2000), Ashcraft (2005), Greenstone, Mas and Nguyen (2015)), the sources of variation used in these papers cannot identify the effects of branch-level shocks. This paper provides an identification strategy leveraged with tract-level data that have not previously been used in this context to show that branch closings have a significant impact on their local communities.

The paper proceeds as follows. Section 2 describes the data. Section 3 discusses the identification strategy and empirical framework. Section 4 presents and discusses the results. Section 5 concludes.

2 Data

The primary unit of observation in this paper is the Census tract. These are defined by the U.S. Census Bureau to be small, relatively permanent statistical subdivisions of a county. Tracts are defined to optimally contain 4,000 inhabitants, and therefore vary in size across urban and rural areas. As discussed in greater detail in Section 3, I construct a sample of tracts based on exposure to large bank mergers. The median tract in this sample is 1.5 square miles, while the median county is 586 square miles (these numbers are comparable to those for the U.S. overall). Tract boundaries are slightly revised with each Census, and this paper uses boundaries as of the 2000 Census.[4]

To construct the exposure instrument, I use the FDIC Summary of Deposits, which provides an annual enumeration of all branches belonging to FDIC-insured institutions. These data link each branch to its parent bank, and provide a limited amount of branch-level information including deposits, street address, and, since 2008, the branch's latitude and longitude. I use data from 1999-2012, and map branch locations to their Census tract using GIS software. Some observations are dropped because their latitude and longitude data are missing and their

[4]For variables reported using 2010 boundaries, the Census provides a set of relationship files that allows researchers to merge geographic entities over time.

recorded street address is either invalid or incomplete. Appendix Table A.1 provides summary statistics for this geocoding procedure: the percentage of unmapped observations is 7.5% in 1999 and declines to 0.6% in 2012.

Data on merger activity and branch closings are from the FDIC Report of Changes. To gauge the impact of closings on local lending, I use Community Reinvestment Act (CRA) and Home Mortgage Disclosure Act (HMDA) data published by the Federal Financial Institutions Examination Council (FFIEC). Under the CRA, all banks with assets greater than $1 billion are required to disclose annual tract-level data on the number and dollar volume of loans originated to businesses with gross annual revenues less than or equal to $1 million.[5] While these data only capture small business loans originated by CRA-eligible banks, Greenstone et al. (2015) estimate that these institutions account for 86% of total lending in this market. To measure small business lending by institutions excluded from CRA reporting requirements, I use call report data from the Federal Reserve Bank of Chicago and the National Credit Union Administration (NCUA).

Under HMDA reporting criteria, financial institutions are also required to publish data on their local mortgage lending activity.[6] HMDA data are at the loan application level and include not only the Census tract associated with the application, but also its amount, whether it was approved/denied, its type (i.e., home purchase / home equity / refinancing), and applicant characteristics such as income. I drop mortgages subsidized by the Federal Housing Authority, the U.S. Department of Veterans Affairs, or other government programs, which constitute approximately 10% of the full HMDA sample, and aggregate the remaining data to create an annual measure of tract-level mortgage originations. Both tract-level small business loan and mortgage originations are winsorized at the 1% level.

It is important to note that both CRA and HMDA data are based on the *location of the borrower*, as opposed to the location of the bank. For a given tract, the data measure the total number of loans made to borrowers located in that tract, regardless of the location of the originating branch. This allows me to estimate the impact of a branch closing on total

[5]Before 2005, the asset threshold for CRA reporting was $250 million.

[6]According to the 2014 reporting criteria published by the FFIEC, institutions required to disclose under HMDA are banks, credit unions, and savings associations that have at least $43 million in assets, have a branch office in a metropolitan statistical area or metropolitan division, originated at least one home purchase loan or refinancing of a home purchase loan in the preceding calendar year, and are federally insured or regulated.

credit supply to borrowers located in the same tract. Call report data are, unfortunately, not available at a geographically disaggregated level and can only be used to approximate measures of tract-level lending by non-CRA lenders, as described in greater detail in Section 4.

Finally, to provide evidence on the real economic effects of branch closings, I use establishment-level data from the National Establishment Time-Series (NETS), which is compiled by Walls and Associates using Dun and Bradstreet's Market Identifier files. Tract-level demographic characteristics such as population and median family income are from the 2000 Census. All other data are for the 1999-2012 period.

3 Identification and Empirical Framework

The structural relationship of interest is the effect of a branch closing on local credit supply:

$$y_{it} = \alpha_i + \gamma_t + \lambda \mathbf{X_{it}} + \beta_c Close_{it} + \epsilon_{it}, \tag{1}$$

where y_{it} is total lending to borrowers located in tract i in year t, α_i are tract fixed effects, γ_t are year fixed effects, $\mathbf{X_{it}}$ is a vector of tract characteristics, and $Close_{it}$ is an indicator equal to 1 if a branch closes in tract i in year t. The OLS estimate for β_c is unbiased if $Close_{it}$ is orthogonal to ϵ_{it}: i.e., if the incidence of the closing is unrelated to local factors that would also affect the level of lending. In general, this assumption is unlikely to hold as shocks to credit demand will affect both the level of lending as well as the profitability of local bank branches.

To generate plausibly exogenous variation in the incidence of branch closings, I use exposure to post-merger consolidation as an instrument for closings. Bank mergers are often followed by a period of retrenchment during which the merged institution closes branches in areas where the two previously-separate networks overlap. This implies that areas with both Buyer and Target bank branches are at greater risk of a post-merger closing. I therefore supplement Equation 1 with the following first stage regression:

$$Close_{it} = \kappa_i + \psi_t + \rho \mathbf{X_{it}} + \beta_e Expose_{it} + \omega_{it}, \tag{2}$$

where $Expose_{it}$ is an indicator equal to 1 if two banks with branches in tract i undergo a merger in year t.

Mergers themselves are motivated by several considerations, including expansion into new markets, the synthesis of complementary business functions, an increase in market power, or cost savings from consolidation. In the context of this identification strategy, this may be problematic if the incidence of the merger is itself driven by factors specific to areas where Buyer and Target branches overlap. To address this concern and to use only plausibly exogenous exposure to consolidation, I focus on mergers where both Buyer and Target banks held at least $10 billion in pre-merger assets, which roughly corresponds to the top 1% of the size distribution of U.S. banks. For mergers in this category, only 1.4% (3.5%) of Buyer (Target) banks' deposits, on average, are located in Exposed tracts prior to the merger. This represents such a small fraction of the merging banks' overall businesses that it is unlikely any factors specific to these areas would be an important determinant in the decision to merge.

The full set of criteria for inclusion in my merger sample are those that (i) occurred between 2001-2010, (ii) involved Buyer and Target banks that each held at least $10 billion in pre-merger assets, and (iii) where the merging institutions had overlapping retail branch networks in at least one Census tract. This yields a sample of 20 mergers. To further minimize the possibility that the decision to merge may be related to a decline in economic conditions specific to areas where the banks' branches are located, I also drop mergers that were either classified as failing (i.e., they required financial assistance from the FDIC) or that occurred during the financial crisis.[7] The final sample comprises the 13 mergers listed in Table 1. The failing / crisis mergers are listed in Appendix Table A.2.

Table 2 reports summary statistics for the Buyer and Target banks in the merger sample. By construction, these are very large institutions (the median Buyer holds $82 billion in assets, while the median Target holds $26 billion) with very extensive branch networks (the median Buyer controls 721 branches and operates in 8 states, while the median Target controls 292 branches and operates in 7 states). For comparison, the median bank in the U.S. holds $100 million in assets and controls only 3 branches.

[7] The results are qualitatively similar when these mergers are included but, consistent with these concerns, the outcomes display pre-trends that are absent in the primary sample.

For each of these 13 mergers, I define Exposed tracts to be those that had branches from both Buyer and Target banks in the year prior to the merger. Figure 2 shows how these tracts are identified for a sample merger and a sample county in the data. The left panel shows a map of Wake County, NC with Census tracts delineated and the geographic distribution of bank branches in the year prior to the 2004 Wachovia - SouthTrust merger. Red squares denote Wachovia branches, green triangles denote SouthTrust branches, and any tract containing both is an Exposed tract, as shown in red in the right panel.[8]

Figure 2 shows that Wachovia and SouthTrust branches tend to be clustered around urban centers, which suggests that using the rest of the county as a Control group would amount to a comparison between urban and rural areas. Column 1 of Table 3 confirms this, and shows that Exposed tracts differ significantly from all other tracts in the county along many dimensions: they have higher populations, a higher fraction of white and college-educated households, higher incomes, and banking markets that are both larger and growing more quickly.[9]

To identify tracts that are more comparable to Exposed tracts, I therefore map the locations of branches belonging to other large banks - i.e., other banks that also held at least \$10 billion in assets. These are denoted with blue circles in the left panel of Figure 2. As my Control group, I take any tract that did not have both a Wachovia and a SouthTrust branch, but did have branches from at least two large banks who did not merge with one another. These tracts are shown in grey in the right panel of Figure 2.

The spirit of this approach is to define a set of tracts that, *a priori*, had similar potential to be exposed to a large bank merger. Column 2 of Table 3 shows that Control tracts are, indeed, much more similar to Exposed tracts than all other tracts in the county. As some significant differences remain, I use a differences-in-differences (DD) framework to compare lending in Exposed and Controls tracts in the same county, before and after a merger, and allow for

[8]Tract boundaries are often determined by major roads and so branches are often located on, or very near, boundaries. The geocoding procedure maps each branch to a unique tract, which introduces some measurement error to the definition of the instrument, but should, if anything, reduce the magnitude of the first stage estimates.

[9]As the identification is based on within-county comparisons, I present summary statistics by estimating regressions of the form:

$$f_{ic} = \alpha + \beta Expose_{ic} + \sigma_c + \epsilon_{ic},$$

where f_{ic} is a pre-merger characteristic for tract i in county c, and $Expose_{ic}$ is a dummy equal to 1 if tract i is an Exposed tract. Conditional on purging county fixed effects, α is the Control group mean and β is the difference in means between Exposed and Control.

time-varying trends based on pre-merger tract characteristics. The primary specification is

$$y_{icmt} = \alpha_i + \eta_m + (\gamma_t \times \sigma_c) + \mathbf{X_i}\beta_t + \sum_{\tau} \delta_\tau \left(D^{\tau}_{mt} \times Expose_{icm}\right) + \epsilon_{icmt}, \tag{3}$$

where y_{icmt} is an outcome for tract i in county c for merger m in year t; α_i are tract fixed effects; η_m are merger fixed effects; $(\gamma_t \times \sigma_c)$ are county-by-year fixed effects; $\mathbf{X_i}$ is a vector of pre-merger tract characteristics whose effects are allowed to vary by year; D^{τ}_{mt} is a dummy equal to 1 if year t is τ years after merger m is approved by federal regulators; and $Expose_{icm}$ is a dummy equal to 1 if tract i is an Exposed tract for merger m. The pre-merger tract characteristics in $\mathbf{X_i}$ are population, population density, fraction minority, fraction college-educated, median family income, the number of branches as of the year preceding the merger, and average annual growth in the number of branches for the two years preceding the merger. τ ranges from -8 to 10, and standard errors are clustered at the tract level. The coefficient of interest is δ_τ, which measures the difference, conditional on controls, in outcome y between Exposed and Control tracts τ years after the merger. As the validity of the DD framework hinges on the assumption of parallel trends, I present event study plots in Section 4 that allow for visual examination of pre-trends in the data.

3.1 External Validity

The internal validity of the DD framework hinges on the assumption of parallel trends, but assessing external validity is also informative in the context of this identification strategy. While the set of tracts exposed to post-merger consolidation may be exogenously determined, banks still choose which branches to close. This does not invalidate the instrument, which requires that *exposure* to consolidation is as good as randomly assigned. It does, however, affect the interpretation of the local average treatment effect (LATE) identified by the merger instrument.

In a general framework with heterogeneous treatment effects, the LATE identified by a particular instrument is the effect of treatment on compliers, where compliers are observations whose treatment status is changed by the instrument. In other words, compliers are neither "always-takers" (tracts where a branch would have closed regardless of whether or not there was any merger) nor "never-takers" (tracts where no branch is closed even when a merger occurs). Instead, compliers are tracts where a branch closes if and only if there is a merger. To interpret the LATE identified by the merger instrument, we need to know who the compliers are.

Table 4 shows the complier characteristics for my sample.[10] Relative to the median tract in the sample, compliers tend to be less densely populated, have a lower median income, and have a higher number of pre-merger branches, all of which suggests that banks tend to concentrate their closings in areas deemed to be "overbranched." This emphasizes that the merger instrument does not identify the effect of closings that move neighborhoods from 1 to 0 branches. It identifies the effect of taking an already-crowded market and removing one branch from it.

4 Results

4.1 Exposure to Consolidation and Branch Closings

This section presents evidence for the first stage relationship between exposure to consolidation and the incidence of branch closings. Figure 3 provides the template used for the event study results. It plots the δ_τ estimated from Equation 3, where the dependent variable is the number of branch closings in tract i in year t. The bars show the 95% confidence intervals, and the lines at $\tau = -4$ and $\tau = 6$ denote the range over which there is a balanced panel. $\delta_\tau > 0$ indicates a higher incidence of branch closings in Exposed tracts relative to Controls τ years after a merger.

Figure 3 shows that up to several years prior to the merger, Exposed tracts are no more likely than Controls to experience a closing. However, the relative incidence increases in the year the merger is approved, spikes in the year after, and then falls back to zero. Column 1 of Table 5 presents the corresponding point estimates, and shows the sum of δ_0 and δ_1 is 0.284. There is generally a maximum of one closing per tract, so this can be roughly interpreted as a

[10] While it is not possible to identify the compliers in the sample, Angrist and Pischke (2009) describe a procedure for summarizing their characteristics. Briefly, the first step is to calculate the proportion of Always-Takers (π^A) and Never-Takers (π^N) in the data. In the context of this paper, the former is calculated by estimating the fraction of Control tracts who experienced a closing after the merger, while the latter is calculated by estimating the fraction of Exposed tracts who did not experience a closing. From these two numbers, one can calculate the proportion of compliers $\pi^C = 1 - \pi^A - \pi^N$. With this information, one can back out the average characteristics of compliers by first estimating the average characteristics over the set of Always-Takers and compliers (i.e., Exposed tracts that did experience a closing) and then the average characteristics over Always-Takers only (i.e., Control tracts that had closings).

28 percentage point increase in the relative probability of a closing in Exposed tracts in the 2 years following the merger.

Since the Control group includes tracts that have branches from only the Buyer or the Target along with another large bank, the results in Figure 3 are not driven by a tendency for merged banks to close branches across the board. Appendix Figure A.1 confirms this directly by showing the merger has no effect on the incidence of branch closings in Buyer and Target Only tracts relative to Unexposed tracts (those that did not have branches from either the Buyer or the Target, but did have branches belonging to two other large banks).[11] This confirms that physical proximity between merging branches matters for determining where closings occur.

Figure 4 shows the higher incidence of closings in Exposed tracts translates into a decline in the total number of branches, and illustrates the importance of estimating the year-by-year coefficients. There is no evidence of pre-trends, and the plot reveals that the post-merger decline is only temporary. By $\tau = 4$, the number of branches in Exposed tracts is again level with Control tracts. The corresponding point estimates are shown in Column 2 of Table 5. The dependent variable is the total number of branches, but the results are similar when using the total number of banks. These results are consistent with Garmaise and Moskowitz (2006), who find the market structure effects of mergers last approximately 3 years before other banks enter.[12] This pattern suggests that while it is in the merged bank's best interest to consolidate on its fixed costs by closing an overlapping branch, profits are then high enough to accommodate a new entrant.

4.2 Closings and Local Credit Supply

The previous section showed that exposure to consolidation increases the probability of a branch closing. Do closings, in turn, have an impact on local credit supply? In this section, the dependent variables are drawn from the FFIEC data, and measure the volume of new small business and mortgage loans made to borrowers located in tract i in year t, regardless of the

[11] I look at both Buyer Only and Target Only tracts since the data indicate that post-merger closings are split fairly evenly between Buyer and Target bank branches. 60% of post-merger closings involve a Target branch, while 40% involve a Buyer branch.

[12] Results not shown here confirm that this pattern is driven by a higher rate of branch openings in Exposed tracts 3-4 years after the merger, rather than by a higher rate of branch closings in Control tracts during the same period.

location of the originating branch.

Figure 5 shows the reduced form relationship between exposure to consolidation and the volume of new lending. The left panel shows a large and significant decline in new loans to local small businesses. Relative to Controls, Exposed tracts experience a decline in small business lending that persists up to 6 years after the closing. In contrast, the right panel shows very little effect on local mortgage lending; a slight dip coincides with the timing of the branch closing, but none of the year-by-year coefficients are statistically significant.

This comparison suggests closings have a more substantial effect in the small business lending market, but the contrast becomes especially striking when we compare the reduced form estimates in both markets with the first stage relationship between exposure to consolidation and the total number of branches. Figure 6 superimposes the reduced form estimates from Figure 5 over the first stage coefficients from Figure 4. The right panel shows the decline in mortgage lending is temporary and recovers even before the number of branches. The left panel, however, shows closings have a much longer-term impact on credit supply to local small businesses. Small business lending declines when a branch closes, and remains depressed even after the entry of new banks.[13]

To more easily interpret the magnitude of these effects, Table 6 provides estimates from less flexible versions of the DD. I estimate:

$$y_{icmt} = \alpha_i + \eta_m + (\gamma_t \times \sigma_c) + \mathbf{X_i}\beta_t + \delta_{POST}\left(POST_{mt} \times Expose_{icm}\right) + \epsilon_{icmt}, \tag{4}$$

where $POST_{mt}$ is a dummy equal to 1 if year t occurs after merger m is approved by federal regulators and all other variables are as previously defined. δ_{POST} measures the post-merger mean shift in the level of lending. Given the patterns observed in Figure 5, I also allow a post-merger linear trend in event year for the mortgage results by estimating:

$$\begin{aligned} y_{icmt} &= \alpha_i + \eta_m + (\gamma_t \times \sigma_c) + \mathbf{X_i}\beta_t + \delta_{POST}\left(POST_{mt} \times Expose_{icm}\right) \\ &\quad + \delta_\tau\left(POST_{mt} \times Expose_{icm} \times \tau\right) + \epsilon_{icmt}, \end{aligned} \tag{5}$$

[13]Roughly 50% of the time, entrants are banks above the CRA asset threshold.

where τ is the event year.

The reduced form estimates in Column 1 of Table 6 show the decline in the number of new loans is mirrored by a decline in the dollar volume of new lending. The point estimates in Panel A show closings are associated with a statistically significant 17% annual decline in the dollar volume of new small business loans. To provide a sense of scale, the average Buyer (Target) branch in this sample controls approximately 29% (27%) of tract-level deposits. Over the six years following the closing, this decline amounts to over $4.5 million in forgone loans to local small businesses, which is roughly equivalent to total tract-level lending in a given year. Panel B shows closings have no significant impact on local mortgage lending.

4.2.1 Do Borrowers Substitute toward Other Lenders?

Table 6 shows closings lead to a substantial decline in small business lending, but the dependent variable is small business loans extended by banks above the CRA reporting threshold. If borrowers substitute toward non-CRA lenders, namely small community banks and credit unions, only a portion of the 17% would represent an actual loss in local credit supply.

Gauging the magnitude of this substitution is complicated by the fact that the CRA is the only source of geographically-disaggregated information on small business loan originations. In the absence of comparable data for small banks and credit unions, I use an approximation based on call report data, which are reported at the bank level. To generate tract-level measures, I define small business loans to be the sum of "Commercial and industrial loans" and "Loans secured by nonfarm or nonresidential real estate" whose original amounts are $1 million or less.[14] I then divide the bank-level totals across all tracts where the institution's branches are located. For banks, each tract's share of an institution's total lending is determined by the share of total deposits held by branches in that tract, which is obtained from the FDIC Summary of Deposits. For credit unions, bank totals are divided evenly across all branches.[15] Estimates for the amount of lending done by each bank in a given tract are aggregated together

[14] In the CRA data, I define small business loans to be "Loans extended to businesses with annual revenues of less than $1 million," a classification that does not exist in the call report data. However, the CRA results still hold when using "Loans with loan amount at origination less than $1 million."

[15] The NCUA only started publishing information on credit union branch locations in 2011. Therefore, institution totals are divided across tracts based on each credit union's geographic footprint in 2011. The results are similar when all lending is attributed to the tract in which the headquarters are located.

to generate a single tract-level measure of small business loans extended by non-CRA entities. Since quantities in the call report are stocks, while the CRA reports the flow of new loan originations, the magnitude of these results will not be directly comparable to those in Table 6.

Panel A of Table 7 shows the results of estimating Equation 4 with the measures of tract-level lending derived from bank and credit union call reports. As the purpose of this exercise is to approximate the extent of substitution between different lenders, I focus on the magnitude of the point estimates rather than on their statistical significance. The first row shows that, consistent with the CRA results, total lending from banks above the CRA reporting threshold declines after the closing. The second row suggests that there is a corresponding increase in lending from smaller banks that absorbs approximately 23% of the decline from larger banks. The third row suggests that credit unions further absorb approximately 16% of the original decline. Netting these effects from the 17% estimate from Table 6 leaves a remaining 8% decline in lending that is not absorbed by small banks or credit unions.

An alternative source of credit is home equity (HE) loans. Appendix Table A.3, however, shows no evidence of a compensating increase in these loans after a closing. The Small Business Administration also reports that while many small businesses use credit cards extensively, credit card debt accounts for only a small portion of small business financing relative to bank loans and retained earnings.[16] Nevertheless, without tract-level data to measure this substitution explicitly, the 8% decline can be treated as an upper bound for the total loss in credit.

It is worth emphasizing that, due to the data limitations described above, this is necessarily a back-of-the-envelope approximation. However, it suggests that the decline in lending from CRA banks is not entirely absorbed by other lenders and that there may, in fact, be a substantial restriction in local credit supply following a branch closing.

4.2.2 Interpretation

What are the mechanisms through which closings may lead to a restriction in credit supply for local small businesses? One possibility is that lending falls because reducing the number of local competitors from n to $n-1$ places upward pressure on prices. Garmaise and Moskowitz (2006)

[16]See https://www.sba.gov/sites/default/files/2014_Finance_FAQ.pdf.

provide empirical evidence that merger-induced increases in local concentation lead to higher prices and less credit, and Scharfstein and Sunderam (2015) show they reduce the sensitivity of local mortgage rates to MBS yields. The patterns in Figure 6, however, show that the direct effects of a change in tract-level concentration are empirically negligible. The slight dip in mortgage lending recovers before new banks enter, which shows the initial (statistically insignificant) decline cannot be attributed to the change in local market structure. Similarly, small business lending does not respond to the entry of new banks: the decline in lending persists even after the competitive environment has returned to its previous equilibrium. The effects of increased concentration may be limited in this particular context because, as shown in Table 3, the average Exposed tract has 6 branches prior to the merger. This instrument identifies the effect of closings that occur in very crowded markets, and where the direct effect of a shift from n to $n-1$ lenders may not be very large.

An alternative explanation is that the decline in lending may be driven by institutional changes induced by the merger. Peek and Rosengren (1998) show that Buyers tend to recast Targets in their own image, which leads to post-merger convergence toward the behavior of the Buyer. If Buyers in this sample engage in less small business lending than Targets, lending may decline in Exposed tracts after a merger.[17] Alternatively, Target banks may engage in more risky lending (hence, contributing to their eventual acquisition), which is eliminated after they are acquired.

To evaluate the importance of this channel, I estimate the effect of closings on lending in Target Only tracts: i.e., tracts that have branches from the Target, but not the Buyer. Branches in these areas are affected by any institutional change resulting from the merger, but are not exposed to the greater risk of a post-merger closing. While not statistically significant, the point estimate in Panel B of Table 7 show there is some decline in lending in Target Only tracts, but by a much lesser amount than what is observed in Exposed tracts. Moreover, while institutional change may contribute to the initial decline in lending, it is not sufficient for explaining the *persistence* of the decline, which indicates that borrowers find it difficult to substitute toward

[17] Call report data actually indicate that Buyers in this sample are often *more* engaged in small business lending than Targets (as measured by the ratio of the dollar volume of small business loans over total assets in the year prior to the merger), which would lead any post-merger convergence to run in the direction opposite to the results.

other lenders even over the long-term. This suggests the presence of an additional channel that can explain the decline in lending in Exposed tracts.

The contrast between small business and mortgage lending shown in Figure 6 strongly suggests that this additional channel is the destruction of lender-specific relationships. A large literature in finance has studied the role of soft information and relationships; in particular, Drexler and Schoar (2012) provide evidence that severing the relationship between an individual borrower and her loan manager can lead to disruptions in credit access. In cases of post-merger consolidation, the staff at the closed branch are often let go while the accounts are transferred to the neighboring branch of the merged bank. To the extent this process destroys personnel-specific soft information that is difficult to transfer, borrowers may face a prolonged restriction in credit supply until they are able to establish new relationships.

Relationship-specific capital is less important in the mortgage market where rates of securitization are very high and the process of loan approval has become largely automated.[18] In contrast, small business lending is the prototypical example of an information-intensive market where borrowers are heavily reliant on lender-specific relationships.[19] The prolonged decline in small business lending displayed in Figure 6 – and, importantly, its persistence despite the entry of new banks – suggests closings disrupt lending relationships in that market that take time to rebuild.[20]

If broken relationships are the key channel through which closings disrupt local small business lending, the consequences should be especially severe for the most relationship-dependent segments of the market. I estimate the differential effect of branch closings along three dimensions that previous papers have shown to be correlated with information-intensiveness: income, minority status, and distance from the branch. Munoz and Butcher (2013) show that credit

[18] To wit, an October 2014 *New York Times* article reported that Ben Bernanke had recently been unable to refinance his mortgage because the program used to screen his application detected that he had had a recent change in employment.

[19] Petersen and Rajan (1994) and Berger and Udell (1995) both emphasize the importance of relationship lending for small businesses. Amel and Brevoort (2005) and Brevoort et al. (2010) show small business lending markets tend to be very local, and Agarwal and Hauswald (2010) argue this is because geographic proximity facilitates the collection of soft information. Greenstone et al. (2015) provide evidence that small businesses who faced restrictions in credit supply during the Great Recession were unable to substitute toward other lenders.

[20] In addition to the price effects, several papers have shown that a change in the competitive environment can have a direct impact on the amount of relationship lending banks choose to engage in (Petersen and Rajan (1995), Boot and Thakor (2000)). Again, however, the fact that lending does not respond to the entry of new banks suggests these competitive effects are negligible in this context.

histories for low-income borrowers tend to be thinner and patchier, meaning there is less hard information available to evaluate a borrower's creditworthiness. Bond and Townsend (1996) provide evidence that borrowers in low-income and minority neighborhoods rely more heavily on informal sources of credit, and posit this may be because informal lenders have cheaper access to relevant information about borrowers within the same community.[21] For these reasons, we would expect closings to be especially disruptive in low-income and high-minority neighborhoods. Petersen and Rajan (2002) argue firms differ on a number of dimensions that render them more or less transparent to their creditors - in their paper, they measure transparency using possession of a business credit card, existence of financial records, franchise status, and the dispersion of shareholders. As the CRA data do not identify individual borrowers, the same measures cannot be used in this context. However, Petersen and Rajan (2002) show that information transparency is correlated with a firm's distance from its lender (more transparent firms are able to borrow at greater distances), which is also correlated with local branch density. To the extent that closings sever lending relationships, we would expect the declines in lending to be most severe in areas of high branch density where firms are located closer to their lender.

Table 8 shows IV estimates of:

$$\begin{aligned} y_{icmt} &= \alpha_i + \eta_m + (\gamma_t \times \sigma_c) + \mathbf{X_i}\beta_t + \delta_{POST}\,(POST_{mt} \times Expose_{icm}) \\ &\quad + \delta_{POST\times Below}\,(POST_{mt} \times Expose_{icm} \times Below_{ic}) + \epsilon_{icmt}, \end{aligned} \tag{6}$$

where $Below_{ic}$ is a dummy equal to 1 if the tract is in the lowest tercile of the distribution for median income or percent white, or below the median distance from the branch.[22] The latter is approximated by dividing total tract area by the number of branches in the tract. In all cases,

[21] These issues are not particular to the U.S., and resonate throughout the literature on barriers to credit in developing countries. Fisman, Paravisini and Vig (2012), for example, use data from India to show that soft information (transferred, in their case, via cultural proximity between borowers and lenders) can be important in ensuring access to credit in settings where problems of asymmetric information would otherwise give rise to substantial credit rationing. Banerjee and Duflo (2010) provide a broader overview of the development literature on this topic.

[22] By construction, tracts in this sample have branches from at least two banks with assets of greater than \$10 billion, which automatically excludes the most marginalized neighborhoods who may have little, if any access, to the mainstream banking system. Therefore, I use the lowest tercile of the distribution for median income and percent white in my sample, rather than just below median, to get a meaningful sample of "marginalized" areas. Tracts in the lowest tercile have median income below \$40K and the percent of white households is less than 0.76.

we expect $\delta_{POST \times Below} < 0$ if the effects of closings are more severe for information-intensive borrowers.

Table 8 confirms that this is the case for all three measures of information-intensiveness, though the coefficients range from marginally significant to insignificant. Post-closing declines in lending are more severe in in tracts with lower median income, a higher fraction of minority households, and where firms, on average, are closer to their lender. Note that the baseline specification controls for total branches, and so Column 3 compares areas with equal branch coverage but different geographic densities.[23] These results are consistent with the assertion that broken relationships are the primary channel through which closings lead to declines in local credit supply.

It is also worth noting that Columns 1 and 2 show that the post-closing decline in lending is more severe in low-income and high-minority tracts, even though the baseline level of lending is also lower in these areas. This is true despite the fact that the correlation between the number of branches and tract-level median income and percent white is extremely low (only 0.0171 and 0.1035, respectively) in this sample. Conditional on having branches from at least two large banks, banking markets in low-income neighborhoods are just as crowded as those of wealthier neighborhoods in this sample. The results in Table 8 indicate that marginalized neighborhoods not only face a larger absolute decline in lending after a branch closing, they actually suffer a larger proportional hit to credit supply.

4.2.3 Robustness

The standard practice in much of the finance literature is to define local banking markets at the level of the MSA or non-MSA county. Garmaise and Moskowitz (2006) argue that this convention has been driven by data availability, and that evidence suggests local markets are likely to be much smaller. As this paper's identification strategy relies on within-county comparisons, this may be a concern if the results are driven by comparisons between tracts located very far apart. To address this, I re-estimate the reduced form results for small business

[23] The distances in this context are much smaller than those in Petersen and Rajan (2002): in their data, the mean distance between a firm and its lender was 115 miles, and the median was 9 miles. In Table 8 the difference in average distance for tracts above and below the median is just 2.5 miles versus 0.2 miles. Nevertheless, the results are in the expected direction.

lending using varying definitions for the size of sub-county local banking markets. For each Exposed tract, I define the market to be all Control tracts located within 10, 15, or 25 miles.[24] Identification is then based on within-market comparisons between Exposed and Control tracts.

Panel C of Table 7 shows the estimate for the post-merger decline in small business lending is robust to these variations. The estimate obtained when the market is defined using a 15-mile radius (the definition used by Garmaise and Moskowitz (2006)) is -2.337 compared to -2.507 when the market is defined at the county level. Even with a 10-mile radius, the estimate is still -2.040. This suggests the results are not affected in any meaningful way by treating counties as the local market.

Another concern may be that the reporting threshold for CRA was increased from $250 million to $1 billion in 2005, which falls in the middle of my sample period. If the share of banks within the $250 million - $1 billion range differs systematically between Exposed and Control tracts, this may contribute to the post-merger decline in CRA-reported lending documented in Figure 6 and Table 6. Panel D of Table 7, however, shows the results are robust to controlling for the tract-level market share of banks who were excluded from CRA starting in 2005.

4.3 Geographic Spillovers

The results so far have shown there is a substantial decline in credit supply to small businesses located in the same tract where a closing occurs, but surrounding areas are likely to be affected as well. The median tract in this sample is only 1.5 miles, and survey evidence shows small businesses search up to several miles away for a credit provider (Amel and Brevoort (2005), Brevoort et al. (2010)).

To measure these geographic spillovers, I categorize tracts according to their distance from a branch closing. For each Exposed tract, let R^x denote the set of tracts located between $x-1$ and x miles away. R^0 contains only the Exposed tract. R^1 consists of all tracts whose centroids are located at most 1 mile away from the Exposed tract, but excludes the Exposed tract itself. R^2 consists of all tracts whose centroids are located at most 2 miles away, but excludes all tracts contained in R^1 and R^0. And so on and so forth.

[24]Distances are measured based on tract centroids.

I define R^x for all $x \in \{0, 10\}$. For each x, I estimate Equation 4 where the dependent variable is the number of new small business loans, R^x is the "exposed" group, and the Control group consists of all tracts located in the same county but at least 10 miles away from the branch closing. δ_{POST} measures the post-merger decline in lending observed in tracts who did not themselves experience a closing, but who were located x miles away from one.

Figure 7 plots the δ_{POST} for each $x \in \{0, 10\}$, and shows that the effects of a closing are very localized. The impact is most severe in the tract where the branch is located and, strikingly, the magnitude of the effect decreases nearly monotonically as distance from the closed branch increases. Ultimately, the impact on lending dissipates around 6-8 miles from the Exposed tract. This pattern is remarkably consistent, both qualitatively and quantitatively, with existing evidence on the local nature of small business lending markets. Amel and Brevoort (2005) and Brevoort et al. (2010) use survey evidence to show the median distance between small firms and their supplier of credit is around 3-5 miles. Figure 7 uses actual firm behavior and provides a measure that falls exactly within that range. These results indicate that while technological advances may have lessened the "tyranny of distance" in small business lending, as shown in Petersen and Rajan (2002), they have not managed to eradicate it entirely.

4.4 Real Economic Effects

Finally, it is natural to ask the extent to which these declines in local small business lending have real economic effects. To answer this question, I use establishment-level data from NETS to construct tract-level measures of annual employment growth. The employment growth rate for tract i in year t is:

$$\begin{aligned} EmpGr_{it} \quad = \quad & [jobs\, created\, by\, new\, establishments_{it} - jobs\, lost\, from\, closing \\ & establishments_{it} + employment\, in\, continuing\, establishments_{it} - \\ & employment\, in\, continuing\, establishments_{i,t-1} + inmigration_{it} - \\ & outmigration_{it}] / \left[0.5 \times employment_{i,t-1} + 0.5 \times employment_{it}\right]. \end{aligned}$$

This is a symmetric growth rate, ranging between -2 and 2, and is a second-order approximation to log differences.

Table 9 shows the results of estimating Equation 4 where the dependent variable is the tract-level employment growth rate. More specifically, these are IV estimates. Column (1) shows that closings lead to a 2 percentage point reduction in the employment growth rate.[25] Columns (2) and (3) show that these effects are entirely concentrated amongst establishments who are more heavily dependent upon bank credit and who, *ex ante*, we would expect to face greater difficulty in finding substitute sources of credit. Row (2) shows closings have the greatest impact on establishments in industries with a high dependence on external finance, as defined in Rajan and Zingales (1998). As the Rajan and Zingales (1998) measure is computed using data on large, publicly-traded companies and may not translate exactly to the financing needs of small firms, Row (3) shows that the same qualitative results hold when I define capital intensity using the measure from Hurst and Lusardi (2004), which uses data on starting capital requirements from the Survey of Small Business Finances. Rows (4) and (5) show the effects of closings are most severe for small standalones (defined to be single-unit establishments with fewer than 20 employees) and privately-owned establishments. This is consistent with the effects of closings being most severe on information-intensive borrowers.

5 Conclusion

Do bank branches still matter? This paper argues that they do. I show that, even in crowded banking markets, closings have large effects on local credit supply when lending is information-intensive and lender-specific relationships are difficult to replace. Closings have only a minimal impact on mortgages, but lending to local small businesses falls substantially and remains persistently low even after the entry of new banks. The effects are very localized, and are most severe in marginalized neighborhoods where relationship-specific capital is likely to be especially important. Ultimately, the decline in local lending translates into a 2 percentage point reduction in employment growth rates.

[25] In results not shown here, I also look at the effect of closings on establishment growth rates. The point estimates are negative, but statistically insignificant.

From the perspective of assessing the welfare implications of branch closings, the first-order issue is: who is the marginal borrower who loses access to credit? If closings sever relationships that facilitate credit access for positive NPV borrowers, then the decline in lending is welfare-reducing.[26] In general, this need not be the case. Hertzberg, Liberti and Paravisini (2010) show that when loan managers are responsible not only for maintaining a relationship with their borrowers, but also for monitoring their repayment prospects, they may suppress negative signals about the firm's ability to repay since it will reflect negatively on their own reputation. If managers siphon funds to borrowers with negative NPV projects, the observed decline in lending may be welfare-enhancing. The data sources used in this paper do not include borrower and loan characteristics, such as default rates, that can distinguish empirically between these possibilities (the overall decline in lending likely includes both cases), and so the welfare implications are ultimately ambiguous.

Nevertheless, this paper has several important policy implications. First, it suggests that the same informational frictions that lead borrowers in disadvantaged neighborhoods to face high barriers to credit access can also make it harder for them to adjust to subsequent credit market shocks. This implies that financial shocks, even those that affect only the largest institutions, may filter down to have substantial distributional effects at the local level. This is highly relevant from a policymaking perspective given the heavy focus on credit access in low-income and minority neighborhoods.

Second, these results suggest that the current approach to regulating branch closings and evaluating the impact of bank mergers may be overly narrow. The focus on the availability of other branches fails to recognize that if closings destroy lender-specific information, borrowers will be unable to obtain credit at equal terms even in dense banking markets. The number of branches that are geographically close is an insufficient measure for actual credit access.

Finally, this paper shows that technological advances have not eradicated the importance of physical proximity in lending. Even in the U.S. banking system today, there are some markets

[26]This depends, of course, on properly accounting for banks' forgone costs. If banks fully internalize the social value of their lending, then closings are socially efficient even when they result in a loss of credit for positive NPV borrowers. If, however, there are market imperfections that drive a wedge between the social and private values of capital, then there will be scenarios where banks close branches even though that results in a loss of social welfare.

and some segments of the population for whom local bank branches still play a crucial role in determining local access to credit.

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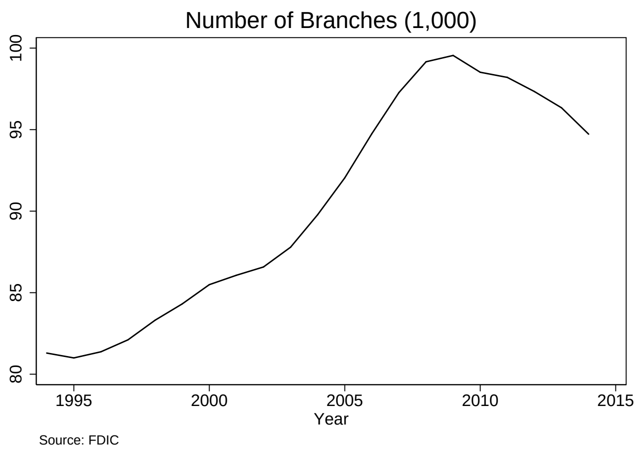
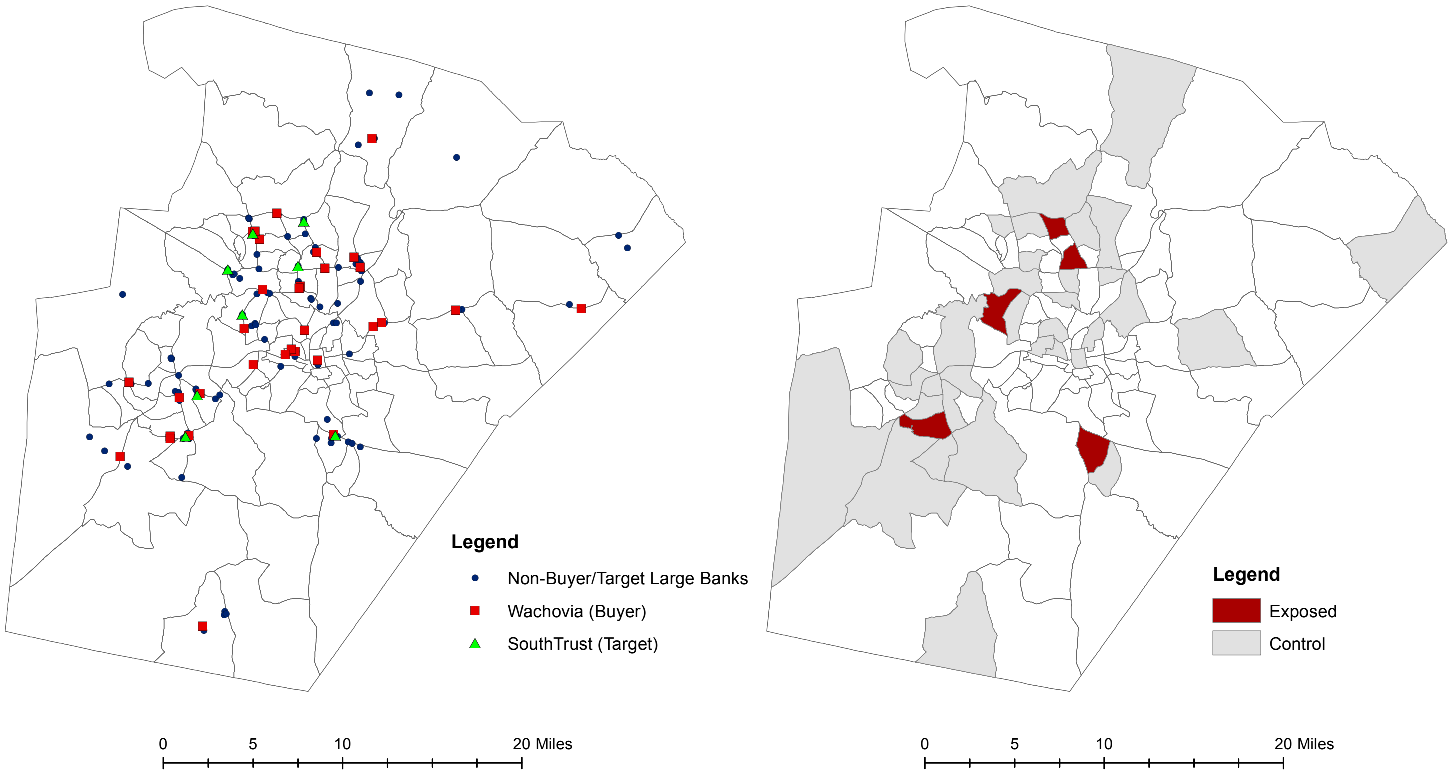
Figure 1: U.S. Bank Branches, 1994-2014



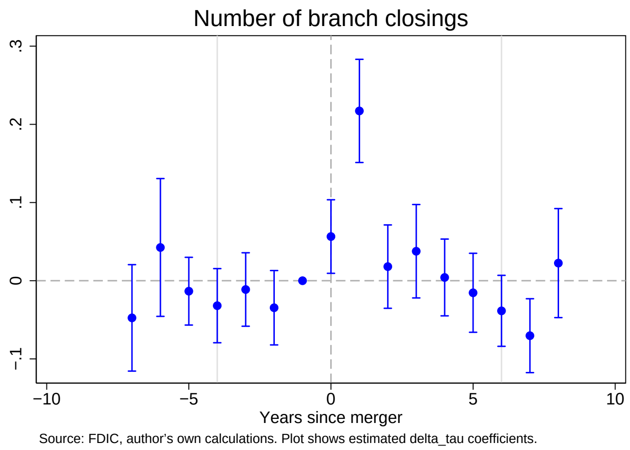
Source: FDIC. Figure displays the total number of bank branches reported in the FDIC Summary of Deposits from 1994-2014.

Figure 2: Defining Exposed and Control Tracts - Wake County, NC



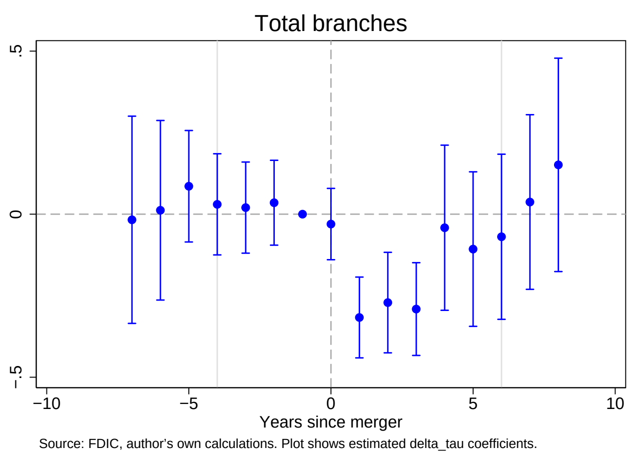
Source: FDIC, U.S. Census. Figure shows how Exposed and Control tracts are defined in Wake County, NC for the 2004 merger between Wachovia and SouthTrust. Left panel shows the Census tract boundaries along with the geographic distribution of bank branches in the year prior to the merger. Red squares are Wachovia (Buyer) branches, green triangles are SouthTrust (Target) branches, and blue circles are branches belonging to other banks with at least $10 billion in assets. Right panel shows Exposed tracts (those with both a Wachovia and a SouthTrust branch) and Control tracts (those that had branches from at least two large non-merging banks).

Figure 3: Exposure to Consolidation and the Incidence of Branch Closings



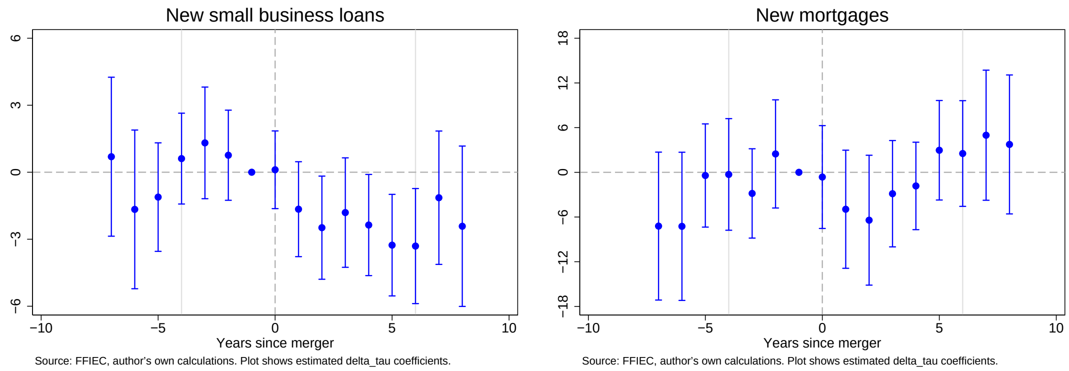
Source: FDIC, author's own calculations. Figure plots the δ_τ from estimating Equation 3 where the dependent variable is the number of branch closings in tract i in year t. Bands show 95% confidence intervals. $\tau = 0$ is the year the merger was approved by federal regulators, and and all coefficients are normalized relative to $\tau = -1$. The vertical lines at $\tau = -4$ and $\tau = 6$ denote the range over which the panel is balanced. Robust standard errors are clustered at the tract level.

Figure 4: Exposure to Consolidation and Total Branches



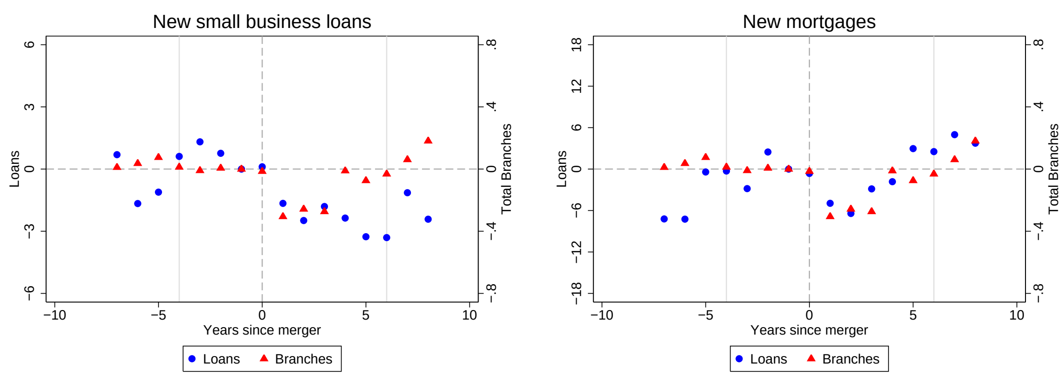
Source: FDIC, author's own calculations. Figure plots the δ_τ from estimating Equation 3 where the dependent variable is the total number of branches in tract i in year t. Bands show the 95% confidence intervals. $\tau = 0$ is the year the merger was approved by federal regulators, and and all coefficients are normalized relative to $\tau = -1$. The vertical lines at $\tau = -4$ and $\tau = 6$ denote the range over which the panel is balanced. Robust standard errors are clustered at the tract level.

Figure 5: Exposure to Consolidation and the Volume of New Lending



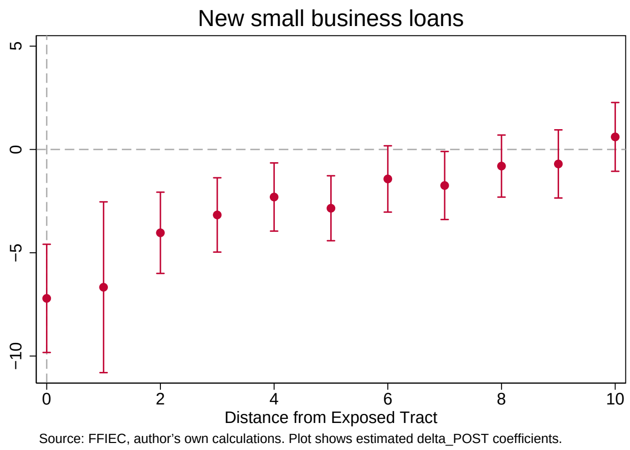
Source: FFIEC, author's own calculations. Figures plot the δ_τ from estimating Equation 3 where the dependent variables are the number of new small business loans (left) and new mortgages (right) made to borrowers located in tract i in year t. Bands show the 95% confidence intervals. $\tau = 0$ is the year the merger was approved by federal regulators, and and all coefficients are normalized relative to $\tau = -1$. The vertical lines at $\tau = -4$ and $\tau = 6$ denote the range over which the panel is balanced. Robust standard errors are clustered at the tract level.

Figure 6: The Effect of Closings on Branch Levels and Local Credit Supply



Source: FDIC, FFIEC, author's own calculations. Figures plots the δ_τ from estimating Equation 3. For red triangles, the dependent variable is the total number of branches in tract i in year t. For blue circles, the dependent variables are the number of new small business loans (left) and new mortgages (right) made to borrowers located in tract i in year t. $\tau = 0$ is the year the merger was approved by federal regulators, and all coefficients are normalized relative to $\tau = -1$. The vertical lines at $\tau = -4$ and $\tau = 6$ denote the range over which the panel is balanced. Robust standard errors are clustered at the tract level.

Figure 7: The Geographic Spillover of Bank Branch Closings



Source: FFIEC, author's own calculations. Figure displays the δ_{POST} from estimating successive iterations of Equation 4 where the dependent variable is new small business loans in tract i in year t and treated tracts are sorted according to their distance from the Exposed tract. See Section 4.3 for details. Bands show the 95% confidence intervals. Robust standard errors are clustered at the tract level.

Table 1: Merger Sample

Buyer	Target	Year Approved
Manufacturer and Traders Trust Company	Allfirst Bank	2003
Bank of America	Fleet National Bank	2004
National City Bank	The Provident Bank	2004
Regions Bank	Union Planters Bank	2004
JPMorgan Chase Bank	Bank One	2004
North Fork Bank	Greenpoint Bank	2004
SunTrust Bank	National Bank of Commerce	2004
Wachovia Bank	SouthTrust Bank	2004
Sovereign Bank	Independence Community Bank	2006
Regions Bank	AmSouth Bank	2006
Bank of America	United States Trust Company	2007
The Huntington National Bank	Sky Bank	2007
Bank of America	LaSalle Bank	2007

Source: FDIC. Table shows the 13 mergers included in the merger sample and the year they were approved by federal regulators. The criteria for inclusion in this sample are described in detail in Section 3.

Table 2: Merger Summary Statistics

	Median	Min	Max
	Panel A: Buyer		
Total assets (billion $)	82	26	1,250
No. of branches	721	259	5,781
States of operation	8	1	31
Counties of operation	183	18	694
	Panel B: Target		
Total assets (billion $)	26	10	246
No. of branches	292	29	1,563
States of operation	7	1	13
Counties of operation	54	7	204

Source: FDIC. Table displays summary statistics for the Buyer and Target banks in the merger sample. All variables are as of the year in which the intention to merge was announced.

Table 3: Summary Statistics for Exposed and Control Tracts

Variable	(1) Exposed Minus All Other	(2) Exposed Minus Control	(3) Control Mean
Population	951.2***	307.5*	5,408
	(152.0)	(180.8)	
Population Density	-179.2	-2.931	5,826
	(206.4)	(316.5)	
Fraction Minority	-0.079***	-0.005	0.238
	(0.011)	(0.012)	
Fraction College-Educated	0.082***	0.0242**	0.333
	(0.008)	(0.010)	
Percent MSA Median Income	13.15***	3.712	118.3
	(2.296)	(2.667)	
Median Income (000s)	2.448**	-0.135	51.3
	(0.952)	(1.135)	
Fraction Mortgage	0.006	0.005	0.715
	(0.007)	(0.008)	
Pre-Merger Branches	4.651***	2.069***	3.845
	(0.186)	(0.216)	
Pre-Merger Branch Growth	0.021***	-0.007	0.058
	(0.007)	(0.009)	
Joint F-test	76.02	17.53	
p-value	0.00	0.00	
Number Exposed	394	394	
Number Comparison	18,046	3,129	

Source: FDIC, U.S. Census, author's own calculations. Table provides tract-level summary statistics obtained by estimating $f_{ic} = \alpha + \beta Expose_{ic} + \sigma_c + \epsilon_{ic}$, where f_{ic} is a pre-merger characteristic for tract i in county c and $Expose_{ic}$ is a dummy equal to 1 if tract i is an Exposed tract. Column (1) reports the estimated β coefficients when comparing Exposed tracts with all other tracts in the same county. Columns (2) and (3) report the estimated β and α coefficients, respectively, when comparing Exposed tracts with the Control group, which is all tracts in the same county that contain branches from at least two large non-merging banks. Population density is per square mile. Percent MSA median income is the ratio of tract median income to MSA median income. Demographic variables are as of the 2000 Census; "pre-merger" variables are as of the year preceding each merger. Pre-merger branch growth is the average annual growth in the number of branches for the two years preceding the merger. Robust standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 4: Complier Characteristics

Variable	(1) Compliers	(2) Ratio: Compliers to Sample
Population	0.58	1.16
Population Density	0.18	0.36
Fraction Minority	0.58	1.16
Fraction College-Educated	0.48	0.96
Percent MSA median income	0.44	0.88
Median Income (000s)	0.29	0.58
Fraction Mortgage	0.45	0.90
Pre-Merger Branches	0.89	1.78
Pre-Merger Branch Growth	0.39	1.10

Source: FDIC, FFIEC, U.S. Census, author's own calculations. Table uses the methodology outlined in Angrist & Pischke (2009) to show how Complier tracts compare to the median tract in the sample. For more details, see Footnote 10. Column 1 shows the fraction of Compliers who lie above the median tract in the sample. For example, the first row shows 58% of Compliers are more populated than the median tract in the sample. Column 2 calculates the ratio of Compliers to Sample by dividing each entry in the second column by 0.50, since 50% of tracts in the sample will, by definition, lie above the median tract.

Table 5: Exposure to Consolidation and Branch Closings

	(1) Number of Closings	(2) Total Branches
$\delta_{<0}$	-0.018	0.031
	(0.018)	(0.056)
δ_0	0.060**	-0.028
	(0.025)	(0.067)
δ_1	0.224***	-0.318***
	(0.034)	(0.086)
δ_2	0.021	-0.267**
	(0.028)	(0.111)
δ_3	0.041	-0.293***
	(0.031)	(0.100)
$\delta_{>3}$	-0.016	-0.003
	(0.013)	(0.135)
2Y Cum. Effect	0.284***	
	(0.042)	
Baseline Mean	0.168	5.906
Obs.	49,630	49,630

Source: FDIC, author's own calculations. Table shows the δ_τ estimated from Equation 3 where the dependent variable is the number of closings and the total number of branches, respectively, in tract i in year t. All coefficients are normalized relative to $\tau < -3$, where $\tau = 0$ is the year in which the merger was approved by federal regulators. The 2Y Cumulative Effect is the sum of δ_0 and δ_1. Baseline Mean is calculated for Exposed tracts in $\tau = -1$. Robust standard errors are clustered at the tract level and are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 6: The Effect of Closings on Local Credit Supply

	Coefficient	(1) Reduced Form	(2) IV	(3) 6Y Cum. Effect	(4) Baseline Mean
			Panel A: Small Business Loans		
# Loans	δ_{POST}	-2.507***	-9.372***	-56.23***	102.92
		(0.899)	(3.282)	(19.69)	
	Obs.	46,007	46,007		
$ Volume (000s)	δ_{POST}	-202.2***	-773.4***	-4,640***	4,667
		(63.00)	(239.1)	(1,434)	
	Obs.	43,921	43,921		
			Panel B: Mortgages		
# Loans	δ_{POST}	-7.010	-20.91	12.09	274.11
		(4.796)	(13.82)	(14.61)	
	δ_τ	1.680	5.499		
		(1.041)	(3.374)		
	Obs.	47,253	47,253		
$ Volume (000s)	δ_{POST}	-1,007	-2,932	1,487	38,773
		(961.7)	(2,732)	(2,756)	
	δ_τ	232.3	736.7		
		(203.7)	(639.8)		
	Obs.	47,251	47,251		

Source: FFIEC, author's own calculations. Panel A (B) presents estimates of Equation 4 (5) where the dependent variable is either the number or dollar volume of new small business loans (mortgages) in tract i in year t. Column 1 provides the reduced form estimates, Column 2 provides the IV estimates where tract-level exposure to consolidation instruments for the incidence of branch closings, Column 3 provides the cumulative effect over the 6 years following the merger ($6 \times \delta_{POST}$ for small business lending; $\delta_{POST} + 6 \times \delta_\tau$ for mortgages), Column 4 is the pre-merger average in Exposed tracts. Robust standard errors are clustered at the tract level and are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 7: Robustness Checks

	(1) δ_{POST}	(2) Obs.
Baseline	-2.507***	46,007
	(0.899)	
	Panel A: Call Report Data	
CRA Banks	-14,691	48,115
	(14,544)	
Small Banks	3,402	48,115
	(2,258)	
Credit Unions	2,367	16,575
	(1,173)	
	Panel B: Target Only Tracts	
Target Only	-1.005	31,132
	(0.760)	
	Panel C: Size of the Banking Market	
25-Mile	-2.445***	90,655
	(0.897)	
15-Mile	-2.337**	74,239
	(0.909)	
10-Mile	-2.040**	54,934
	(0.910)	
	Panel D: 2005 Reporting Change	
Control: Share Excl. Banks	-2.515***	45,830
	(0.898)	

Source: Chicago Fed, FDIC, FFIEC, NCUA, author's own calculations. Table shows the δ_{POST} estimated from several iterations of Equation 4. Except in Panel A, the dependent variable is the number of small business loans in tract i in year t from CRA data. Baseline is the reduced form estimate from Table 6. Panel A: dependent variables are approximations of tract-level small business lending obtained from call report data. See Section 4.2.1 for details. Note these are stock variables rather than flows, as measured in CRA. Panel B: treated group is Target Only tracts, i.e., those that only had branches from the Target and not the Buyer prior to the merger. See Section 4.2.2 for details. Panel C: rather than using the County as in Baseline, I define the market for each Exposed tract to be all Controls located with a 25-, 15-, or 10-mile radius. See Section 4.2.3 for details. Panel D: I control for the market share of banks with assets in the $250 - $1 billion in each tract. See Section 4.2.3 for details. In all specifications, robust standard errors are clustered at the tract level and are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 8: Variation by Information Intensity

Coefficient	(1) Median Income	(2) Percent White	(3) Distance from Branch
δ_{POST}	-5.88*	-8.03**	-4.54
	(3.40)	(3.33)	(3.83)
$\delta_{POST \times Below}$	-8.93*	-4.64	-10.54**
	(4.70)	(5.07)	(4.11)
Baseline Mean (Below - Above)	-20.37***	-10.97*	1.97
Obs.	46,007	45,991	46,007

Source: FFIEC, U.S. Census, author's own calculations. Table presents IV estimates of Equation 6 where the dependent variable is the number of new small business loans in tract i in year t. Median Income and Percent White are from the 2000 Census. Distance from Branch is tract area (in miles) divided by the number of bank branches. "Below" indicates the lowest tercile for Median Income (<\$40K) and Percent White (<0.76), and denotes below median for Distance from Branch (<0.5 miles). Baseline Mean shows the difference in mean levels of lending between tracts in the Below versus Above group along each dimension. Robust standard errors are clustered at the tract level and are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

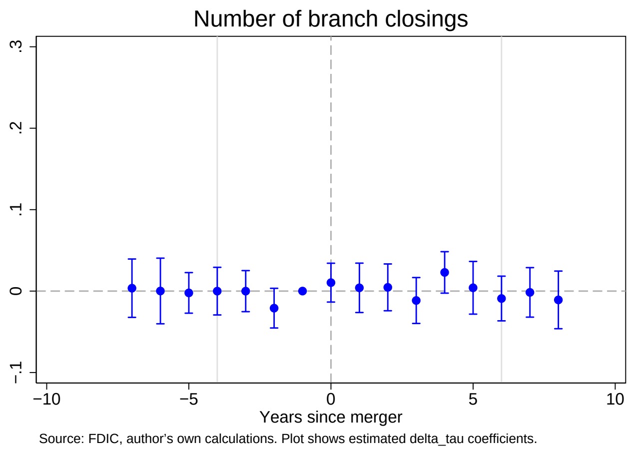
Table 9: The Effect of Branch Closings on Employment

Establishment Type	(1)	(2) Included	(3) Excluded
All	-0.022**		
	(0.009)		
Dependent on External Finance		-0.026*	-0.016
		(0.014)	(0.011)
Capital Intensive		-0.025**	-0.019
		(0.011)	(0.012)
Small Standalone		-0.020**	-0.017
		(0.008)	(0.012)
Private		-0.020**	-0.026
		(0.009)	(0.021)
Baseline Mean	0.006		
Obs.	45,357		

Source: NETS, U.S. Census, author's own calculations. Table presents IV estimates of Equation 4 where the dependent variable is employment growth rates as defined in Section 4.4. Industry Dependence on External Finance is defined according to the Rajan & Zingales (1998) measure, while Capital Intensity is defined according to the Hurst & Lusardi (2004) measure. Small Standalones are single-unit establishments with fewer than 20 employees, and Private denotes privately-held establishments. Baseline Mean is for Exposed tracts in $\tau = -1$. Robust standard errors are clustered at the tract level and are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Appendix

Figure A.1: Branch Closings in Buyer Only and Target Only Tracts



Source: FDIC, author's own calculations. Figure plots the δ_τ from estimating Equation 3 where the dependent variable is the number of branch closings in tract i in year t. The treated group is tracts that only had branches from either the Buyer or the Target (but not both) prior to the merger, and the control group is Unexposed tracts (those that had branches from neither the Buyer nor the Target). Bands show 95% confidence intervals. $\tau = 0$ is the year the merger was approved by federal regulators, and all coefficients are normalized relative to $\tau = -1$. The vertical lines at $\tau = -4$ and $\tau = 6$ denote the range over which the panel is balanced. Robust standard errors are clustered at the tract level.

Table A.1: Geocoding Summary Statistics

Year	Total Branches	Mapped	Unmapped	% Unmapped
1999	84,312	77,971	6,341	7.5
2000	85,492	79,713	5,779	6.8
2001	86,069	80,919	5,150	6.0
2002	86,578	82,001	4,577	5.3
2003	87,790	85,297	2,493	2.8
2004	89,784	87,598	2,186	2.4
2005	92,042	90,083	1,959	2.1
2006	94,752	93,016	1,736	1.8
2007	97,274	95,847	1,427	1.5
2008	99,163	98,211	952	1.0
2009	99,550	98,856	694	0.7
2010	98,520	97,812	708	0.6
2011	98,204	97,657	547	0.6
2012	97,337	96,774	563	0.6

Source: FDIC, author's own calculations. Table shows summary statistics for the geocoding procedure used to map branch locations from the FDIC Summary of Deposits to their Census tract. Branch locations can be geocoded either by plotting their latitude and longitude, or by matching their street address to those stored in a GIS repository. I rely on the former whenever possible as it is the most reliable, but latitude and longitude data are only available beginning in 2008 and can only be matched to a limited number of observations prior to that. As a result, in every year there are observations that cannot be mapped because they have no lat/long data and their street address was either incomplete or invalid and could not be matched to an address in the GIS repository.

Table A.2: Failing/Crisis Mergers

Buyer	Target	Year Approved	FDIC Assistance
TD BankNorth	Commerce Bank	2008	
JPMorgan Chase Bank	Washington Mutual Bank	2008	X
Wells Fargo Bank	Wachovia Bank	2008	
U.S. Bank	Downey Savings and Loan	2008	X
PNC Bank	National City Bank	2008	
Branch Banking and Trust Company	Colonial Bank	2009	X
East West Bank	United Commercial Bank	2009	X

Source: FDIC. Table shows the mergers excluded from the primary sample because they were either classified as failing (i.e., they required financial assistance from the FDIC) or they occurred during the 2008 financial crisis.

Table A.3: Effects on Mortgages, by Mortgage Type

Coefficient	(1) Purchase	(2) HE	(3) Refi	(4) Purchase	(5) HE	(6) Refi
	# Loans			$ Volume (000s)		
δ_{POST}	-10.65	-1.383	-9.029	-2,313	-345.7*	163.5
	(10.61)	(0.873)	(6.771)	(1,945)	(176.5)	(1,996)
δ_{τ}	3.084	0.386*	2.033	581.6	59.81	58.57
	(2.262)	(0.217)	(1.966)	(417.5)	(46.83)	(481.9)
6Y Cum. Effect	7.852	0.930	3.167	1,177	13.18	514.87
	(8.360)	(0.881)	(8.659)	(1,679)	(166.15)	(1,926)
Baseline Mean	107.99	10.69	155.08	15,541	580.74	21,360
Obs.	47,253	47,253	47,253	46,246	46,246	46,246

Source: FFIEC, U.S. Census, author's own calculations. Table presents IV estimates of Equation 5 where the dependent variable is mortgage originations, by mortgage type: Purchase are home purchase mortgages, HE are home equity loans, and Refi are refinancings. Baseline Mean is the pre-merger average in Exposed tracts. Robust standard errors are clustered at the tract level and are in parentheses. *** p<0.01, ** p<0.05, * p<0.1