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

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Abstract

I study the relationship between bank-specific capital and credit access in a new setting: bank branch closings in markets where the branch network is dense. Existing regulation in the U.S. is targeted toward areas with few branches where closings inhibit physical access to the branch network. I show that, even in crowded markets, closings can have large effects on local credit supply. To generate plausibly exogenous variation in the incidence of closings, I use Census tract level data paired with a novel identification strategy that exploits within-county variation in exposure to post-merger consolidation. This instrument identifies the effect of closings that occur in close proximity to other branches. I find that closings have a prolonged negative impact on credit supply to local small businesses, but only a temporary effect on local mortgage lending. The number of new small business loans is 13% lower for several years, and this decline persists even after the entry of new banks. The decline in lending is highly localized, dissipating 8 miles out, and is concentrated in low-income and high-minority neighborhoods. These results show closings have large effects on local credit supply when lending is information-intensive and lender-specific relationships are difficult to replace. I provide a framework for discussing the welfare implications, which depend on the characteristics of the marginal borrower.

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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] This and other policies focus on closings that hinder the physical accessibility of the branch network, but much less emphasis is placed on the disruptive effects of closings that occur in crowded banking markets.

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 the wave of closings has prompted widespread concern regarding the impact on local communities. In large part, this discussion has emphasized the role of closings in reducing physical access to the banking system: commentators have detailed the emergence of "banking deserts" and chronicled the effect of closings that leave neighborhoods or towns 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 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 on lending in these cases?

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 lending. 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

[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, for example, 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.

do not would likely overestimate the impact of the closing itself.

As a solution to the 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. The number of new small business loans is 13% lower for several years after a closing. Notably, lending remains depressed despite the entry of new banks, which shows the decline is not driven by the competitive effects of the merger. In constrast, there is only a temporary decline in mortgage lending. Second, the decline in lending is concentrated within low-income and high-minority tracts, indicating that closings are most disruptive in disadvantaged neighborhoods. Third, I provide evidence that 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 8 miles out.

These results suggest that, even in crowded markets, closings can have large effects on local credit supply when lending is information-intensive and lender-specific relationships are difficult to replace. These dynamics are relatively 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 can face a prolonged decline in credit supply until they are able to build a relationship with a new lender.[4] Similarly, low-income and minority borrowers may be particularly dependent on soft information and lender-specific relationships.

The welfare implications of this decline hinge on the characteristics of the marginal borrower. If closings restrict credit access for positive NPV borrowers, then the decline in local credit supply is welfare-reducing. If, however, these are negative NPV borrowers, then the decline in lending may actually 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. However, I provide a conceptual framework to illustrate that, even if borrowers with positive NPV projects lose access to credit, closings may still be efficient from the viewpoint of total welfare once we account for the banks' forgone costs.

This paper has several important policy implications. First, understanding the heterogeneous effects of branch closings is highly policy-relevant. Existing regulation is heavily focused on lending to low-income and minority borrowers, who generally face high barriers to credit access. I show that closings are more disruptive in disadvantaged neighborhoods even though the number of branches does not vary systematically between lower- and upper-income tracts in my sample. This suggests that the same factors that are believed to restrict credit supply in marginalized neighborhoods (i.e., a greater dependence on soft information and lender-specific relationships) may also increase their vulnerability to adverse shocks. These findings also show that physical access is not the only dimension along which closings can have substantial impacts on local communities, suggesting that the current focus of banking regulation vis-à-vis branch closings may be overly narrow. More broadly, this paper shows that there are segments of the U.S. banking system today where a borrower's access to credit is still defined by her local credit market.

This paper makes several contributions relative to the existing literature. A rich body of work has explored how the infrastructure of the local banking market matters for local outcomes in both developing and developed countries (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), Townsend and Zhorin (2014)). To the best of my knowledge, this paper is the first to study the local effects of branch closings and, in particular, their effects in already-crowded markets.

[4] Drexler and Schoar (2012) provide evidence that soft information is difficult to transfer even between employees in the same institution. They show that shocking the relationship between an individual borrower and her loan manager can disrupt credit access.

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 small business 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)). This paper shows the destruction of branch-level soft information is an important factor even in mergers between large banks. Finally, while an existing literature has used state- or county-level data to estimate the effects of negative local credit supply shocks (Peek and Rosengren (2000), Ashcraft (2005), Greenstone, Mas and Nguyen (2014)), the sources of variation used in these papers cannot identify the effects of branch-level shocks. This paper provides a novel identification strategy paired with tract-level data that have not previously been used in this context to show that branch closings have large effects 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 the empirical findings, and Section 5 interprets them as part of a broader framework that considers the welfare impact of branch closings. Section 6 concludes with policy implications.

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

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 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.

As the only bank-level information available in the Summary of Deposits is total assets, I also use balance sheet data on total lending from the FDIC Report of Conditions and Income. 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. While these data only capture small business loans originated by CRA-eligible banks, Greenstone et al. (2014) use call report data to estimate that these account for 86% of all loans under $1 million.

Under similar HMDA reporting criteria, financial institutions are also required to publish data on their local mortgage lending activity.[5] 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 data structure allows me to estimate the impact of a branch closing on total credit supply to borrowers located in the same tract.

Finally, to provide evidence on the real economic effects of branch closings, I use the ZIP Business Patterns data published by the U.S. Census. These provide annual, ZIP-level measures of total establishments, employment, and annual payrolls. I also discuss results obtained using measures of household credit outcomes (such as bankruptcy and delinquency rates) constructed from the Federal

[5]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.

Reserve Bank of New York / Equifax Consumer Credit Panel. 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 lending:

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

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 geographic or product 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 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 are located in Exposed tracts prior to the merger. It is unlikely that any factors specific to these areas would be an important determinant in Buyer and Target banks' 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.[6] 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.

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. Control tracts are those that did not have branches from both the Buyer and the Target, but did have branches from at least two large banks that did not merge with one another. The identification strategy is based on within-county comparisons between Exposed and Control tracts.

Figure 2 shows how Exposed and Control tracts are defined for a sample merger and a sample county in my 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 merger between Wachovia and SouthTrust. Red squares are Wachovia branches, green triangles are SouthTrust branches, and any tract containing both is an Exposed tract.[7] These branches tend to be clustered around the two urban centers of the county, which suggests that using all other tracts as a Control would amount to a comparison between urban and rural areas. To identify tracts that are more comparable to Exposed tracts, I map the locations of branches belonging to other large banks (i.e., other banks that also held

[6] 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.

[7] The figure shows branches are often located on, or very near, tract boundaries, even though the geocoding procedure maps each branch to a unique tract. This is because boundaries are often determined by major roads where branches are also likely to locate. This introduces some measurement error to the definition of the instrument, but should, if anything, reduce the magnitude of the estimate.

at least \$10 billion in assets). 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. This group consists of three different kinds of tracts: Buyer Only tracts who only had a Wachovia branch, but not a SouthTrust; Target Only tracts who only had a SouthTrust branch, but not a Wachovia; and Unexposed tracts who had neither a Wachovia nor a SouthTrust, but did have branches belonging to two other large banks.

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. This translates into the set of Exposed and Control tracts shown in the right panel of Figure 2. I use a difference-in-differences (DD) framework to compare lending in Exposed and Control tracts within the same county, before and after a merger.

Table 3 provides summary statistics for the tracts in my sample. The set of 13 mergers shown in Table 1 translates into a sample of 394 Exposed tracts and over 3,000 Control tracts. As the identification strategy is based on within-county comparisons, I present summary statistics by estimating regressions of the form:

$$f_{ic} = \alpha + \beta Expose_{ic} + \sigma_c + \epsilon_{ic}, \tag{3}$$

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 (shown in Column 2 of Table 3), and β is the difference in means between Exposed and Control (shown in Column 1).

Table 3 shows the average Exposed tract in the sample has roughly 6 branches prior to the merger, which indicates that the instrument identifies the effect of closings that occur in crowded markets. Exposed and Control tracts are similar on most dimensions, but Exposed tracts tend to be more populated, have a higher fraction of college-educated inhabitants, and have a higher number of pre-merger branches. In all specifications, I allow for differential trends based on these pre-merger characteristics.

While Exposed and Control tracts differ on levels, the validity of the DD framework hinges on the assumption of parallel trends. I therefore estimate a year-by-year version of the DD, and present event study plots that allow for visual examination of pre-trends in the data. The primary specification is:

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

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.

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.[8] 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

[8] 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).

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 4, 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.

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. As there is generally a maximum of one closing per tract, this can be roughly interpreted as a 28 percentage point increase in the relative probability of a closing in Exposed tracts in the 2 years following the merger.

Note that because the Control group includes Buyer Only and Target Only tracts, 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.[9]

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.

The results in Figure 4 are consistent with Garmaise and Moskowitz (2006), who find the market structure effects of mergers last approximately 3 years before other banks enter. This pattern suggests that while it is in the merged bank's interest to consolidate on its fixed costs by closing an overlapping branch, profits are then high enough to accommodate a new entrant. The fact that we observe subsequent

[9] 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.

entry in these tracts will play an important role in interpreting the credit supply results presented in Section 4.2.

4.2 Closings and Local Credit Supply

I now address the question of interest: do closings in dense banking markets have an impact on local credit supply? The dependent variables are drawn from the FFIEC data, and measure the total number of new small business and mortgage loans made to borrowers located in tract i in year t, regardless of the 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 that, coincident with branch closings, there is a decline in new mortgages that lasts approximately 3 years, though the year-by-year coefficients are not significant. The right panel reveals a larger decline in the small business lending market. Relative to Controls, Exposed tracts experience a decline in the number of new small business loans that persists up to 6 years after the closing.

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 plots the reduced form estimates from Figure 5 overlaid with the first stage coefficients from Figure 4. The left panel shows the decline in mortgage lending is temporary and recovers before the number of branches. The right 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.

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

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

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:

$$y_{icmt} = \alpha_i + \eta_m + (\gamma_t \times \sigma_c) + \mathbf{X_i}\beta_t + \delta_{POST}\left(POST_{mt} \times Expose_{icm}\right) + \delta_\tau\left(POST_{mt} \times Expose_{icm} \times \tau\right) + \epsilon_{icmt}, \tag{6}$$

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 in both markets. While not statistically significant, the point estimates in Panel A indicate that mortgage lending declines temporarily following the closing. Column 3 shows the decline has dissipated by 6 years after the merger. In contrast, Panel B shows closings are associated with a statistically significant 13% annual decline in new small business loans. Over the six years following the closing, this amounts to a total of nearly $2 million in forgone loans. To provide a sense of scale, the average closing involves a branch that controls 16% of tract-level deposits.

The contrast between small business and mortgage lending suggests closings are more disruptive in markets where lending is information-intensive. A large literature in finance has studied the role of soft information and relationships, and 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.

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.[10] The fact that lending in this market recovers even before the number of branches suggests the decline is driven by the short-term disruptive effects of the closing. Borrowers may delay their applications until any uncertainty over consolidation is resolved, or there may be administrative delays due to the process of transferring accounts. In contrast, small business lending is typically seen as the prototypical example of an information-intensive market where borrowers are heavily reliant on lender-specific relationships.[11] The prolonged decline in small business lending displayed in Figure 6 – and, importantly, its persistence despite the entry of new banks

[10] 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.

[11] 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, Holmes and Wolken (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. (2014) provide evidence that small businesses who faced restrictions in credit supply during the Great Recession were unable to substitute toward other lenders.

– suggests closings disrupt lending relationships in that market that take time to rebuild.

4.2.1 Alternative Explanations

Appendix Section A outlines a model of spatial competition in local banking markets, and shows that lending may decline after a merger or closing if reducing the number of competitors from n to $n-1$ places upward pressure on prices. Indeed, Garmaise and Moskowitz (2006) provide empirical evidence that merger-induced increases in local concentation lead to higher prices and less credit. Under this hypothesis, the results in Figure 5 may reflect not that small business lending is more relationship-dependent than mortgage lending, but that it tends to be more locally concentrated and, therefore, more sensitive to changes in local market structure.[12]

While plausible in theory, the patterns in Figure 6 provide evidence that the direct effects of a change in tract-level concentration are empirically negligible. Mortgage lending recovers before the number of branches, which shows the initial 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. One reason the competitive effects may be limited in this context is that, as discussed in Section 3, this instrument identifies the effect of closings that occur in very crowded markets.

An alternative explanation for the results in Section 4.2 is that the decline in small business lending is driven by a change in organizational focus induced by the merger. Peek and Rosengren (1998) show that Buyer banks tend to recast Targets in their own image, which leads to post-merger convergence toward the behavior of the Buyer. If Buyers engage in less small business lending than Targets, this may be one reason small business lending declines in Exposed tracts after a merger. A related possibility is that Target banks may engage in more risky lending than Buyers (hence, contributing to their eventual acquisition), which is eliminated after they are acquired.

There are several pieces of evidence that refute this hypothesis. First, Appendix Table A.3 shows the lending intensity of each Buyer and Target bank in the sample, as measured by the ratio of the dollar volume of small business loans over total assets in the year before the merger. In most cases, Buyer and Target intensities are of similar magnitude. If not, Buyers are often *more* engaged in small business lending than Targets, which would lead any post-merger convergence to run in the direction

[12] 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. However, the direction of the effect is ambiguous. Petersen and Rajan (1995) argue increased credit market competition will impose constraints on the strength of lending relationships since banks are less able to extract rents from future surplus. Conversely, Boot and Thakor (2000) argue increased competition will lead banks to engage in more relationship lending since this will insulate them from pure price competition.

opposite to the results.

Moreover, Appendix Table A.4 shows there is no evidence of a decline in lending in Target Only tracts. Branches in these tracts would be affected by any organizational change resulting from the merger, but are not exposed to the greater risk of post-merger closings.

4.2.2 Varying the Size of the Local Banking Market

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 my identification strategy relies on within-county comparisons, this may be a concern if my results are driven by comparisons between tracts located very far apart. To address this, I re-estimate the reduced form results for small business 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.[13] Identification is then based on within-market comparisons between Exposed and Control tracts.

Appendix Table A.5 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 -2.414 compared to -2.504 when the market is defined at the county level. Even with a 10-mile radius, the estimate is still -2.051. This suggests the results are not affected in any meaningful way by treating counties as the local market.

4.3 Heterogeneity Across Borrowers

Section 4.2 provided evidence that the impact of a branch closing varies according to the information-intensity of different loan products. This section addresses whether there are heterogeneous effects across different borrowers. This is highly policy-relevant given that U.S. banking regulation is heavily geared toward lending to low-income and minority borrowers. These policies, as evidenced by the Community Reinvestment Act, the Equal Opportunity Credit Act, and the Home Mortgage Disclosure Act, are primarily focused on increasing the level of lending in disadvantaged neighborhoods. However, it is important to know whether the sensitivity of lending to adverse shocks may also be higher in these areas.

I split my sample into terciles based on tract-level median family income, and separately estimate Equations 5 and 6 for each one. The thresholds are chosen so as to ensure a near equal distribution of

[13] Distances are measured based on tract centroids.

observations across each group: Low-Income tracts are those with median income below $40K, Middle-Income are those between $40-58K, and High-Income are those with median income greater than $58K.

Columns (1) through (3) of Table 7 present the corresponding IV estimates, and show the post-closing declines in lending are entirely concentrated amongst the lowest-income tracts in the sample. In fact, Panel B shows closings have no statistically significant effect on credit supply to small businesses in Middle- and High-Income tracts, but Low-Income tracts experience a nearly 40% decline in new small business loans. Columns (4) through (6) show the results of splitting the sample according to fraction minority. While the confidence intervals are larger, the point estimates suggest a similar story: the decline in lending is most severe in tracts with the highest fraction of minority households.

What might explain these heterogeneous effects? One possibility is that there may be fewer branches located in low-income and minority tracts. In this case, each closing will represent a more substantial decline in the availability of bank services. However, Appendix Table A.6 shows the correlation between the number of branches and tract-level median income and fraction minority is extremely low (only 0.0079 and -0.0984, 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.

An alternative explanation is that closings are more disruptive in disadvantaged neighborhoods because these are precisely the borrowers for whom soft information and relationships are most important. Munoz and Butcher (2013) show that credit 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 in Chicago 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. These issues are not particular to the U.S. context and resonate throughout the literature on barriers to credit access in developing countries.[14] In this sense, the same factors that are believe to restrict credit supply in low-income and minority neighborhoods may also increase their vulnerability to adverse shocks.

4.4 Geographic Spillovers

How local are the effects of a branch closing? The results have shown there is a substantial decline in credit supply to small businesses located in the same tract, but surrounding areas are likely to be

[14] 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.

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.

I define R^x for all $x \in \{0, 10\}$. For each x, I estimate Equation 5 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 at about 8 miles.

These results are 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.

4.5 Real Economic Effects

Finally, there is a larger question of the extent to which the decline in local lending has real economic effects. Greenstone et al. (2014) show that county-level declines in small business lending during the Great Recession led to lower employment growth amongst small establishments, which suggests there may be similar dynamics in the context of branch closings.

The ideal dataset would provide data on real outcomes at the tract level. This is especially important given the evidence from Section 4.4 that the effects of a closing are very localized. Unfortunately, the most finely disaggregated, publicly available data on business outcomes are the ZIP Business Patterns data published by the U.S. Census (the median ZIP in the sample is 9 square miles). I use these data to provide suggestive evidence for the effect of branch closings on the number of establishments and total employment.[15] Future work will use confidential Census microdata to construct tract-level measures of establishment entry, exit, and employment.[16]

For each merger, I define Exposed ZIPs to be those that contain at least one Exposed tract, while Control ZIPs are those that contain only Control tracts.[17] In the majority of cases, an Exposed ZIP contains only one Exposed tract. Appendix Table A.7 provides summary statistics for the Exposed and Control ZIPs. Appendix Figures A.2 and A.3 show the tract-level results on branch closings and small business lending hold qualitatively at the ZIP level, albeit more noisily.

Figure 8 shows the reduced form relationship between exposure to consolidation and log establishments and log employment at the ZIP level.[18] There is no notable change immediately following the merger, and the plots reveal substantial pre-trends that were absent in the tract-level results. Panel A of Table 8 estimates the less flexible version of the DD, allowing for both a mean shift and trend break in the post-merger period. The results are only marginally significant for log employment, and indicate that annual growth in employment is approximately 4 percent lower in Exposed ZIPs relative to Controls 6 years after the merger. This is a very large effect for a single branch closing and likely reflects other differences between Exposed and Control ZIPs.

While the effects of a single closing may be too diluted to be reflected in ZIP-level aggregates, we can also exploit within-ZIP comparisons across different industries. To the extent there are real economic effects of branch closings, these should be most pronounced in industries that are heavily reliant on bank credit. Rajan and Zingales (1998) provide a method for classifying industries according to their dependence on external finance. I use the classification provided in Gilje (2012) (and reported in Appendix Table A.8), who shows that industries with a high dependence are more sensitive to local credit supply shocks. I estimate the following triple-difference specification:

$$\begin{aligned} y_{izcmt} &= \alpha_z + \eta_m + (\gamma_t \times \sigma_{ic}) + \mathbf{X_z}\beta_t + \delta_1 High_i + \delta_2 Treat_z + \delta_3 POST_{mt} && (7)\\ &\quad + \delta_4 \left(POST_{mt} \times Treat_z\right) + \delta_5 \left(POST_{mt} \times High_i\right) + \delta_6 \left(High_i \times Treat_z\right) && (8)\\ &\quad + \delta_7 \left(POST_{mt} \times Treat_z \times High_i\right) + \epsilon_{izcmt}, && (9) \end{aligned}$$

[15] Appendix Section B discusses the impact of closings on household credit outcomes such as delinquency rates and credit scores.

[16] Kerr and Nanda (2009) use these data to examine the impact of branching deregulation in the U.S. on entrepreneurship and incumbent firm displacement.

[17] This is not an exact match since tract boundaries, which are defined by the U.S. Census, do not correspond to ZIP boundaries, which are defined by the U.S. Postal Servce. In practice, if a tract is located in more than one ZIP code, I assign it to the ZIP in which the majority of its population lives.

[18] Results on annual payrolls are not shown, but are consistent with those for establishments and employment.

where y_{izcmt} is log of the total number of establishments in industry i in ZIP z (this is the only variable broken down by industry in the Census data); $High_i$ is a dummy equal to 1 if industry i is classified as having a high dependence on external finance; $Treat_z$ is a dummy equal to 1 if ZIP z is a Treatment ZIP; $POST_{mt}$ is equal to 1 if year t occurs after merger m is approved by regulators; and all fixed effects are defined as before. I estimate the year-by-year version of this triple difference. If industries with a greater dependence on external finance are more severely affected by branch closings, we would expect $\delta_7 < 0$ in the post-merger period.

Figure 9 shows that, while there are still pre-trends, there is a decline in the relative growth of industries with a high dependence on external finance that coincides with the incidence of branch closings. Panel B of Table 8 shows this translates into 3 percent lower annual growth in the number of establishments in high dependent industries relative to low dependent industries 6 years after the closing.

5 Welfare Implications

Section 4 shows that closings have large, negative effects on local credit supply in markets where lending is information-intensive. The first-order issue for determining the impact on welfare, however, is: who is the marginal borrower? If closings sever relationships that facilitate credit access for positive NPV borrowers, then the decline in lending is welfare-reducing. 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 last several years have also revealed ample evidence of lax lending standards and their role in fueling the credit boom that preceded the 2008 financial crisis. The evidence from Section 3.1 that merger-induced closings are concentrated in "overbranched" areas may suggest that the marginal borrower affected by these closings is especially likely to have benefited from overlending. 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.

Even if borrowers with positive NPV projects lose access to credit, however, closings may still be efficient from the viewpoint of total welfare once we account for the banks' forgone costs. To illustrate this, consider the following simple model. There is a banking market where banks must pay a cost e in order to enter.[19] Upon entry, banks pay a per-period fixed cost of operation F for each branch, which covers the cost of renting a storefront and hiring staff and does not vary with the number of customers served. For the moment, I assume that each bank operates only one branch. Banks are Nash price setters and engage in only one activity, which is lending. There is zero marginal cost to lending, but banks must charge an interest rate that is high enough to cover their fixed costs. Consumers borrow from banks to invest in projects with a return ω.

Let $x_i(p_i, r)$ denote the total loan demand for bank i when i charges an interest rate p_i and all other banks charge a rate r. An equilibrium consists of an interest rate r and a number of banks n such that (i) each bank i earns maximum profit:

$$p_i = \arg\max_q \left[q x_i(q, r) - F\right]$$

and (ii) there is no entry:

$$\pi(n) \geq 0 > \pi(n+1),$$

where $\pi(n)$ are per-bank profits, net of the cost of entry, when n banks are in the market. As all banks are identical, consider the symmetric equilibrium (r^*, n^*) where each bank has equal market share, L^*. Conditional on this equilibrium loan portfolio, let ρ denote the minimum interest rate a bank would have to charge in order to cover the fixed cost F. Since there are barriers to entry, banks can earn positive profits in equilibrium and the interest rate r^* will lie between banks' minimum interest rate and borrowers' maximum willingness to pay: i.e., $\rho \leq r^* \leq \omega$.

To consider the welfare implications of consolidation, suppose there is an unanticipated merger where Bank A acquires Bank B, who both operate a branch in this market. Post-merger, A has the option of closing B's branch and absorbing its loan portfolio to consolidate on the fixed cost F. However, some percent p of B's loan portfolio is lost during consolidation, and those borrowers who are dropped are shut out of the credit market (for example, due to the destruction of personnel-specific soft information). To simplify notation, let $\lambda(r) = pL^*r$ denote the revenue the bank earns from lending to this p percent of borrowers at an interest rate r.

Ceteris paribus, A will close B if the revenue lost from dropping these loans is less than the savings accrued from consolidation on the fixed cost: i.e., if $\lambda(r^*) < F$. The borrowers who would be dropped, however, are willing to pay up to ω to prevent the branch from closing. This means that as long as $\lambda(\omega) > F$, the bank can renegotiate a higher interest rate $\hat{r} \leq \omega$ where (i) those consumers who would

[19] As an example, one could consider the Salop circle model discussed in Appendix Section A. However, as the spatial component of the Salop circle is not central in this context, I opt for a more general setting.

lose access as a result of consolidation are willing to borrow at that higher rate and (ii) $\lambda(\hat{r}) \geq F$, so A prefers to keep B open.[20] This implies there is no room for policies geared at preventing branch consolidation. Banks' profit-maximizing behavior will dictate that a branch closes only when the cost savings from doing so exceed consumers' maximum willingness to pay to keep the branch open: i.e., when $\lambda(\omega) < F$.

This argument relies, however, on the implicit assumption that banks can bargain with borrowers over the full social surplus from their lending. To the extent this is violated, consolidation may occur even when that is the socially *in*efficient outcome. As a simple example, suppose there are positive spillovers to bank lending. These may take the form of agglomeration economies where the profits of non bank-dependent firms are positively correlated with those of bank-dependent firms.[21] In this scenario, the social value of capital, ω^{SP}, will exceed the private value, ω, and there may be cases where $\lambda(\omega) < F < \lambda\left(\omega^{SP}\right)$ and the merged bank consolidates its branches even though this results in a loss of social surplus.

This discussion has focused on welfare in the market where the branch closing occurs. At a more aggregate level, banks may reallocate the resources from a closed branch across their remaining network, and so the within-market decline in lending may be perfectly offset by an increase in lending elsewhere. From the viewpoint of social efficiency, however, what matters is the net change in welfare and not the net change in lending. In that vein, it is important to emphasize that Section 4.3 shows the decline in lending is concentrated in low-income areas where the marginal utility of consumption is high. Unless banks reallocate their resources to equally poor neighborhoods, and conditional on the earlier discussion regarding the characteristics of the marginal borrower, this would imply a net reduction in welfare.

6 Conclusion

This paper uses a novel identification strategy paired with Census tract level data to estimate the local economic effects of bank branch closings. 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. The effects are concentrated in low-income and minority neighborhoods, which are areas that have historically faced high barriers to credit access and are highly relevant in the context of U.S. banking regulation. I also characterize the geographic spillovers of branch closings and show

[20] The model implicitly assumes loan contracts are characterized by one-sided commitment. Banks cannot break their *ex ante* contract with customers, except by closing a branch. However, customers can ask to renegotiate the contract if they are want to pay a higher rate. In this case, only those borrowers whose credit access is threatened by the closure are willing to bargain. All others have nothing to gain from doing so since, post-consolidation, they can still borrow at r^*.

[21] Pashigian and Gould (1998), Gould, Pashigian and Prendergast (2005), and Benmelech, Bergman, Milanez and Mukharlyamov (2014) provide evidence of local agglomeration economies.

their effects are very localized.

There are two important policy implications. First, existing regulation in the U.S. is heavily focused on increasing the availability of banking services in low-income and minority neighborhoods, which tend to be less heavily-branched than wealthier areas. However, I show that closings are more disruptive in these disadvantaged areas even though the number of branches does not vary systematically between lower- and upper-income tracts in my sample. This suggests that the same factors that are believed to restrict credit supply to marginalized borrowers may also make it harder for them to adjust to credit market disruptions. This implies that financial shocks, even those that affect only the largest financial institutions, may ultimately have disproportionate effects on already-disadvantaged groups.

Second, these findings also suggest 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. More broadly, these conclusions show that in the U.S. banking system today, there are some markets and some segments of the population for whom local credit markets still play an important role in determining local credit access.

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Townsend, Robert M. and Victor V. Zhorin, "Imperfect Competition among Financial Service Providers: A Framework Connecting Contract Theory, Industrial Organization, and Development Economics," 2014.

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

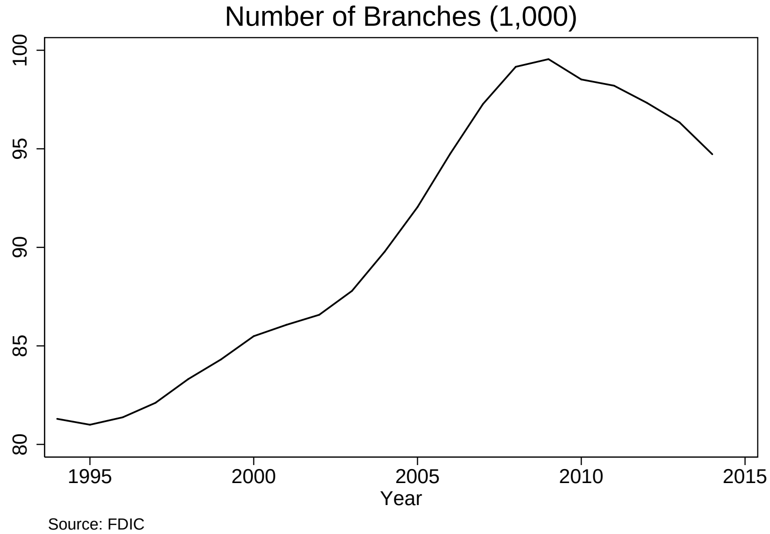
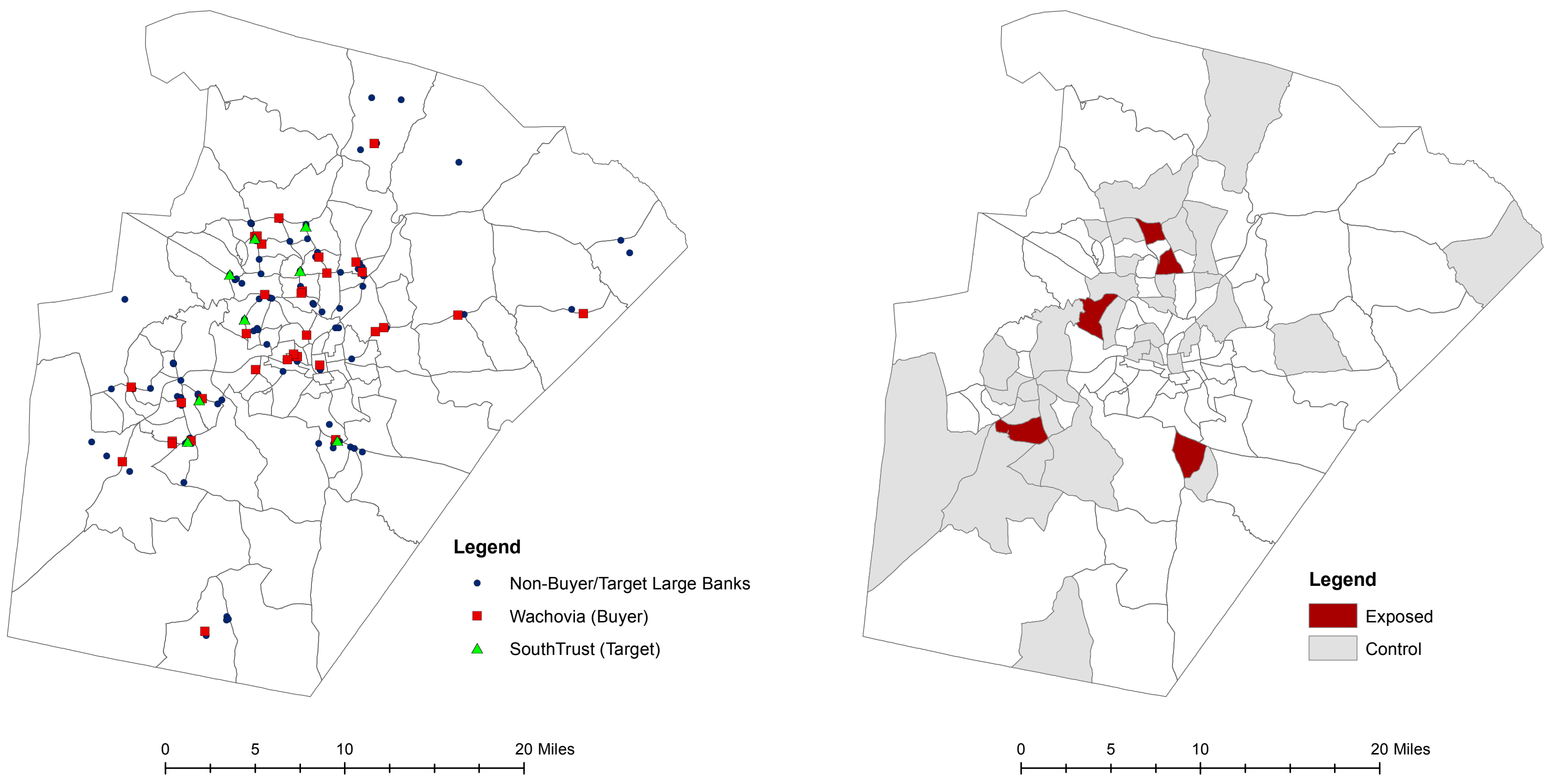


Figure displays the total number of bank branches reported in the FDIC Summary of Deposits from 1994-2014. These are annual data that enumerate all branches belonging to FDIC-insured institutions.

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



Source: FDIC. Figure uses the example of Wake County, NC, to show how Exposed and Control tracts are defined. The left panel shows the Census tract boundaries in Wake County along with the geographic distribution of bank branches in the year prior to the 2004 merger between Wachovia and SouthTrust. Red squares are Wachovia (Buyer) branches, green triangles are SouthTrust (Target) branches, and blue circles are branches belonging to other large banks (i.e., other banks with at least $10 billion in assets). Tracts with both a Wachovia and a SouthTrust branch are Exposed tracts. Tracts that did not have both a Wachovia and a SouthTrust branch, but did have branches belonging to at least two large banks are the Control group. This corresponds to the set of Exposed and Control tracts shown in the right panel.

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

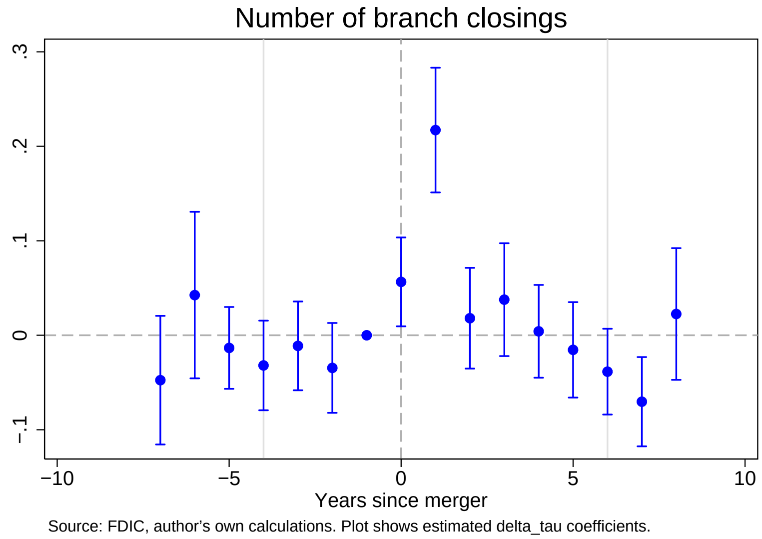


Figure shows the first stage relationship between exposure to consolidation and the incidence of branch closings. The figure plots the δ_τ estimated from the event study specification, along with the 95% confidence intervals. The dependent variable is the number of branch closings in tract i in year t. $\tau = 0$ 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 Local Branch Levels

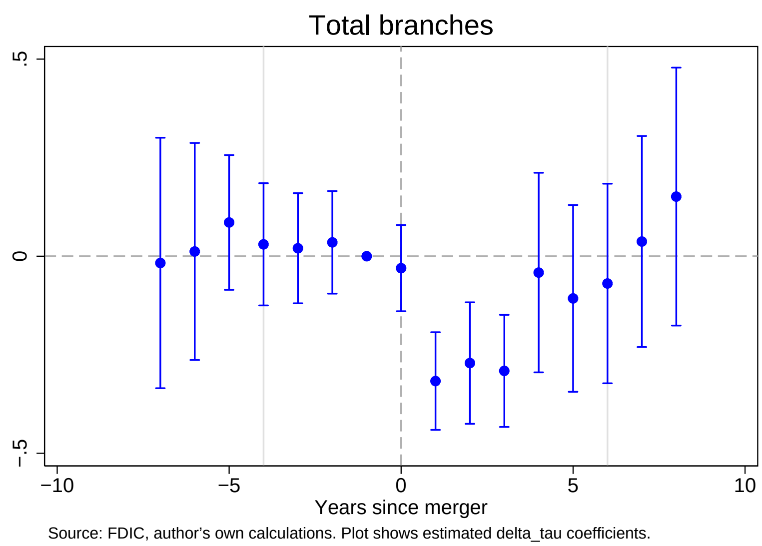


Figure plots the δ_τ estimated from the event study specification, along with the 95% confidence intervals. The dependent variable is the total number of branches in tract i in year t. $\tau = 0$ is the year the merger was approved by federal regulators, and and all coefficients are normalized relative 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

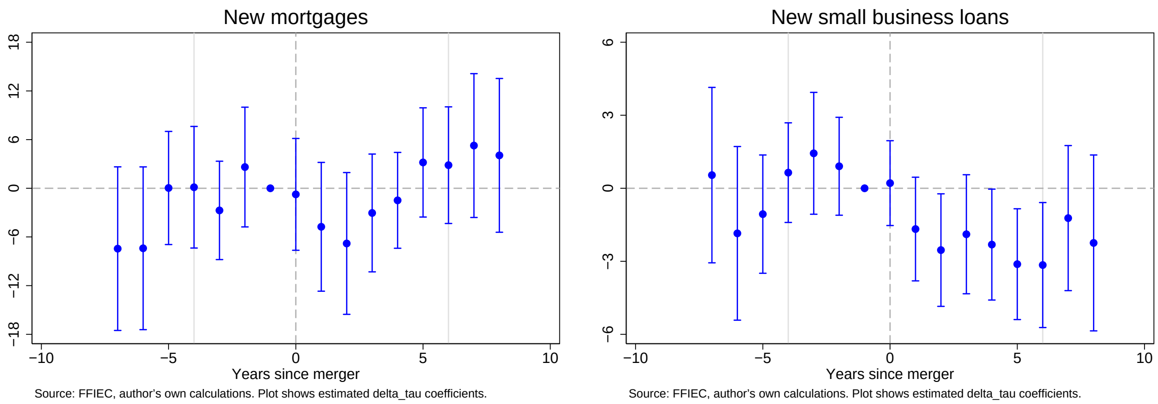
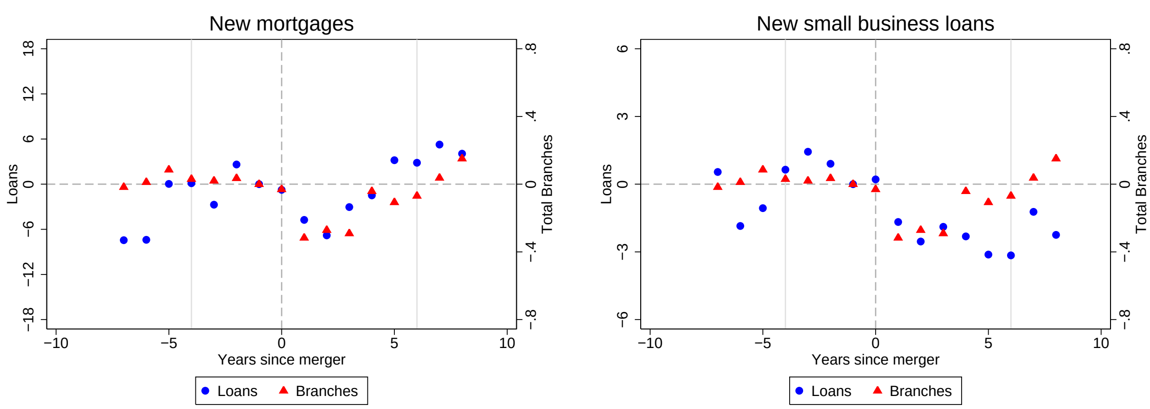


Figure displays reduced form estimates of the relationship between exposure to consolidation and lending to borrowers located in that tract. The figure plots the δ_τ estimated from the event study specification, along with the 95% confidence intervals. The dependent variables are, respectively, the number of new mortgages and new small business loans 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 6: The Effect of Subsequent Bank Entry on Local Credit Supply



Source: FDIC, FFIEC, author's own calculations. Figure plots the δ_τ estimated from the event study specification. 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, respectively, the number of new mortgages (left panel) and new small business loans (right panel) 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

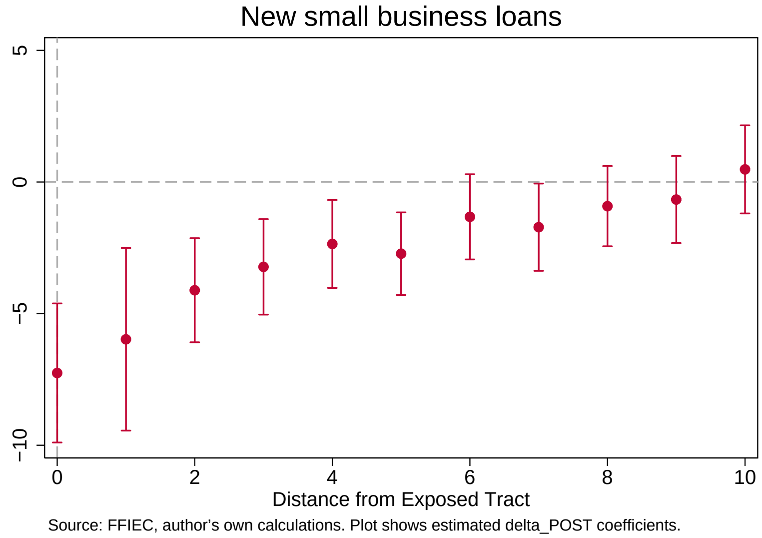


Figure displays reduced form estimates of the post-merger decline in new small business loans in tracts sorted according to their distance from an Exposed tract. The Control group is tracts located at least 10 miles away from an Exposed tract. Section 4.4 provides more details. Estimates are from the version of the difference-in-differences that allows for a single post-merger mean shift in the level of lending. The bars show the 95% confidence intervals. Robust standard errors are clustered at the tract level.

Figure 8: Exposure to Consolidation and ZIP-Level Business Outcomes

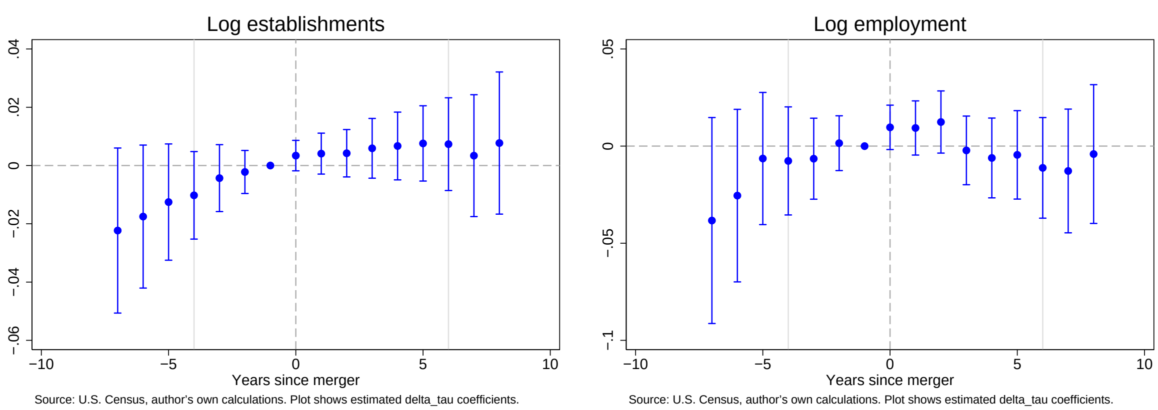


Figure shows the reduced form relationship between exposure to consolidation and ZIP-level log establishments and log employment. The figures plot the δ_τ estimated from the event study specification, along with the 95% confidence intervals. $\tau = 0$ is the year the merger was approved by federal regulators, 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 ZIP level.

Figure 9: The Effect of Closings on Growth in Industries with a High Dependence on External Finance

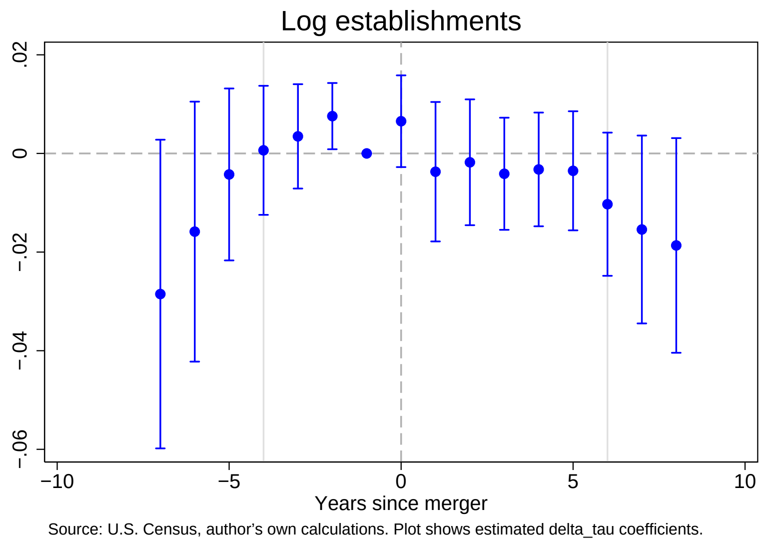


Figure shows the results of estimating the triple difference specification described in Section 4.5. The figure plots the coefficients on the triple interactions between indicators for the post-merger period, being located in an Exposed ZIP, and belonging to one of the industries classified as having a high dependence on external finance in Appendix Table A.8. The dependent variable is log establishments in industry i in ZIP z 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 ZIP 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 primary merger sample and the year they were approved by federal regulators. The criteria for inclusion in this sample are all mergers that (i) occurred between 2001-2010, (ii) involved Buyer and Target banks with at least $10 billion each in pre-merger assets, and (iii) where both banks had overlapping retail branch networks in at least one Census tract. Of the remaining 20 mergers, I drop those that were either classified as failing (i.e., they required assistance from the FDIC) or occurred during the 2008 financial crisis. These excluded mergers are listed in Appendix Table A.2.

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 13 Buyer and 13 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 Control	(2) Control Mean
Population	307.5*	5,408
	(180.8)	
Population Density	-2.931	5,826
	(316.5)	
Fraction Minority	-0.005	0.238
	(0.012)	
Fraction College-Educated	0.0242**	0.333
	(0.010)	
Percent MSA Median Income	3.712	118.3
	(2.667)	
Median Income (000s)	-0.135	51.3
	(1.135)	
Fraction Mortgage	0.005	0.715
	(0.008)	
Pre-Merger Branches	2.069***	3.845
	(0.216)	
Pre-Merger Branch Growth	-0.007	0.058
	(0.009)	
Joint F-test	17.53	
p-value	0.00	
Number Exposed	394	
Number Control	3,129	

Source: FDIC, U.S. Census, author's own calculations. Table provides summary statistics for Exposed and Control tracts. Values are obtained from a regression of each tract-level characteristic on an indicator for being an Exposed tract and county fixed effects. 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 8. 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)	
Control Mean	0.129	4.048
Obs.	49,630	49,630

Source: FDIC, author's own calculations. Table shows estimates of the first stage relationship between exposure to consolidation and the incidence of branch closings. 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. The control mean is calculated for $\tau = 1$. 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) Control Mean
			Panel A: Mortgages		
# Loans	δ_{POST}	-7.234	-21.49	12.71	157.26
		(4.807)	(13.80)	(14.86)	
	δ_{τ}	1.743*	5.701*		
		(1.049)	(3.401)		
	Obs.	47,931	47,931		
$ Volume (000s)	δ_{POST}	-1,308	-3,823	1,614	30,690
		(968.5)	(2,758)	(2,815)	
	δ_{τ}	288.4	906.1		
		(205.8)	(652.1)		
	Obs.	47,975	47,975		
			Panel B: Small Business Loans		
# Loans	δ_{POST}	-2.504***	-9.373***	-56.24***	67.87
		(0.903)	(3.291)	(19.75)	
	Obs.	46,631	46,631		
$ Volume (000s)	δ_{POST}	-83.91*	-318.6*	-1,912*	2,505
		(49.61)	(179.8)	(1,078)	
	Obs.	46,601	46,601		

Source: FFIEC, author's own calculations. Table presents difference-in-differences estimates where the dependent variable is either the number or dollar volume of new loans in tract i in year t. Based on the patterns observed in Figure 6, I allow for a post-merger mean shift in the level of lending in both mortgage and small business lending markets, and include a linear trend in event year for the former. Column 1 provides the reduced form estimates, Column 2 the IV estimates, Column 3 the cumulative effect over the 6 years following the merger ($\delta_{POST} + 6 \times \delta_{\tau}$ for mortgages; $6 \times \delta_{POST}$ for small business lending), Column 4 is the per-year control group mean averaged over the post-merger period. Robust standard errors are clustered at the tract level and are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 7: Heterogeneity by Tract-Level Income and Fraction Minority

Coefficient	(1) Low	(2) Middle	(3) High	(4) Low	(5) Middle	(6) High
	Median Income			Fraction Minority		
			Panel A: Mortgages			
δ_{POST}	-55.42**	-39.74*	1.665	-13.14	-9.531	-78.67**
	(24.21)	(20.43)	(32.81)	(15.17)	(25.77)	(33.07)
δ_{τ}	11.23**	5.977	0.145	5.060	-4.034	14.66*
	(5.276)	(4.495)	(8.816)	(3.813)	(4.997)	(8.446)
Control Mean	86.28	154.74	227.09	183.72	180.58	108.18
Obs.	16,383	16,165	15,383	16,179	15,836	15,916
			Panel B: Small Business Loans			
δ_{POST}	-19.53**	-3.264	-3.773	-4.397	-8.790	-11.84
	(9.478)	(4.012)	(7.270)	(4.416)	(6.203)	(8.156)
Control Mean	53.09	66.10	84.60	68.16	77.33	58.44
Obs.	15,956	15,749	14,926	15,826	15,340	15,465

Source: FFIEC, U.S. Census, author's own calculations. Table presents IV estimates of the effect of closings on local credit supply in tracts split according to median family income and fraction minority households. The dependent variable in Panel A is the number of new mortgages in tract i in year t; in Panel B, it is the number of new small business loans. Estimates are based on the same specifications used for Table 6. Control Mean is the per-year control group mean averaged over the post-merger period. Tercile thresholds are chosen to ensure a nearly equal distribution of observations across each group. Low-Income tracts have median family income in 2000 less than $40K, Middle- have $40-$58K, and High- have greater than $58K. Low-Minority tracts have less than 0.09, Middle- have 0.09-0.24, and High- have greater than 0.24. Robust standard errors are clustered at the tract level and are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 8: Exposure to Consolidation and ZIP-Level Outcomes

Coefficient	(1) Log Establishments	(2) Log Employment
	Panel A: Difference-in-Differences	
δ_{POST}	0.0019	0.0052
	(0.0040)	(0.0084)
δ_{τ}	-0.0027	-0.0073*
	(0.0022)	(0.0040)
6Y Cum. Effect	-0.0142	-0.0386*
	(0.0108)	(0.0203)
Obs.	25,295	25,295
	Panel B: Triple Difference: Post $\times$ *Exposed* $\times$ $High$	
δ_{POST}	-0.0038	
	(0.0093)	
δ_{τ}	-0.0050*	
	(0.0027)	
6Y Cum. Effect	-0.0336**	
	(0.0160)	
Obs.	51,072	

Source: U.S. Census, author's own calculations. Table presents reduced form estimates of the effect of exposure to consolidation on ZIP-level outcomes. Panel A shows the estimates from the diff-in-diff framework, while Panel B shows the result of the triple difference described in Section 4.5. The dependent variables are log establishments and log employment in ZIP z in year t. The total number of establishments is the only variable available at the industry-level and, therefore, the only outcome used for the triple difference. Robust standard errors are clustered at the tract level and are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

A Salop Circle Model

Following the framework of Salop (1979), let a local banking market be represented as a circle of length 1, where borrowers are uniformly distributed along the circle and have linear transportation costs. There are n banks, spaced evenly along the circle as shown in the left panel of Appendix Figure A.4. Papers that have used Salop circles to model banking markets typically interpret them as representing geographic space (see, for example, Chiappori, Perez-Castrillo and Verdier (1995) and Park and Pennacchi (2009)). Following the example of Barros (1999), however, I interpret positions along the circle as locations in characteristics space. This reflects the fact that distance, while important, is not the only dimension over which consumers have preferences when choosing a credit provider: factors such as services offered and customer satisfaction will also matter. This implies that while consumers have a central tendency to borrow from a bank that is located nearby (since distance is one of the characteristics that determine borrowing preferences), there is no reason to expect they will always borrow from the bank that is geographically closest. This is a more accurate description of actual borrowing patterns than a model where only physical distance matters.

As the empirical analysis is based on the location of borrowers and lenders in geographic, not characteristics, space, it is worth discussing how one maps into the other. In the data, I observe locations at the Census tract level, and compare the evolution of lending in tracts where branches close with that of similar, and often neighboring, tracts located in the same county. Let ν denote the percentage of a tract's potential borrowers who borrow from a given bank A. If the distribution of borrower preferences is identical across tracts, the framework implies ν will be highest in the tract where A is located, since distance is one of the factors that borrowers care about. But we would also expect that ν is (i) greater than 0 in tracts neighboring A, since some borrowers are willing to travel if A is closely aligned with their other preferences and (ii) decreasing monotonically in distance from A, since preference for A decreases as distance from it increases. At the end of this section, I discuss what implications this will have for the spillover effects if A closes.

First, to illustrate the more general impact of a closing in this market, suppose that lending is relationship-dependent and consumers must invest in building relationship capital with a specific lender before they are able to borrow. This process is costless, but time-consuming. It takes $k > 0$ periods to build a relationship, and this is the sense in which relationships are "sticky" and difficult to replace. Relationship capital is lender-specific, nontransferable, and permanent (i.e., having established a relationship with a given lender, a firm never has to repeat the process of building a relationship with that lender even if they temporarily stop borrowing).

Conditional on having a relationship with a bank located x away along the circle, firms borrow if the net surplus from doing so is positive: i.e., if

$$F(x, p) = s - p - tx > 0,$$

where s is the surplus from borrowing, p is the price of the loan, and t is the "transportation" cost, which can be interpreted as the cost of borrowing from a bank located farther from one's ideal preference.

Banks are Nash price setters. They pay a fixed cost f to enter the market, and have marginal cost of lending equal to c. Restricting attention to configurations where banks are spaced evenly around the circle, as in the left panel of Appendix Figure A.4, a consumer located $x \in (0, {}^1\!/\!{}_n)$ from bank i is indifferent between borrowing from i or its neighbor if $F(x, p) = F({}^1\!/\!{}_n - x, p)$ - i.e., if

$$x = \frac{p - p_i + {}^t\!/\!{}_n}{2t}.$$

In other words, x is the location of the consumer who is indifferent between borrowing from i or its neighbor. Given the symmetric configuration, bank i's demand is equal to $2x$.

An equilibrium consists of a price p and number of banks n such that (i) each bank i earns maximum profit:

$$p_i = \arg\max_q \left[(q - c)\left(\frac{p - q + {}^t\!/\!{}_n}{t}\right) - f\right]$$

and (ii) there is no entry:

$$\pi(n) \geq 0 > \pi(n + 1),$$

where $\pi(n)$ are per-bank profits, net of the fixed cost of entry, when n banks are in the market.

The market begins in an equilibrium (p^*, n^*) where all borrowers have stable, established relationships and borrow from their closest bank. The market is fully covered, banks are spaced ${}^1\!/\!{}_{n^*}$ apart, and the marginal consumer is located ${}^1\!/\!{}_{2n^*}$ from the closest bank.

Suppose a bank is closed at time $t = 0$, and the locations of remaining banks are fixed. Ignoring, for a moment, the possible effect on prices, the right panel of Figure A.4 illustrates the impact on lending. The dashed segment shows consumers who previously borrowed from the closed bank: those on the thick, blue segment switch to borrowing from one of the neighboring banks, while those on the thin, red segment exit the market as they are too far to earn positive surplus. Lending declines by the length of the dashed segment: those on the thin, red segment are inactive, and those on the thick, blue segment *appear* inactive while in the process of building a new relationship.

Note that this model provides positive rather than normative predictions, and that the scenario depicted in Appendix Figure A.4 represents just one possible outcome of the branch closing. In particular, it requires the assumption that $F\left(\frac{1}{2n^*}, p^*\right) > 0$ and so the market is initially covered, but $F\left(\frac{1}{n^*}, p^*\right) < 0$ and so some borrowers exit the market after their branch closes.

Given that positions along the circle represent locations in characteristics space, how does the pattern depicted in Appendix Figure A.4 translate to the tract-level identification strategy? The dashed segment of the circle represents consumers who previously borrowed, and had a relationship with, the closed bank. As previously discussed, if distance is one of the factors that determine firms' borrowing preferences, the fraction of borrowers with a relationship should be higher in tracts located closer to the bank branch. Geographic proximity therefore serves as an imperfect proxy for having a relationship, and the decline in lending represented by the dashed segment in Appendix Figure A.4 should be most concentrated in the tract where the branch is located. This will be reflected in a lower level of aggregate lending when we compare Exposed tracts with Control tracts in the same county.

Up to this point, I have ignored how prices in the market may be affected by the closing, but remaining banks may respond to the change in market structure. As I show below, the pattern of competition suggests the effect on prices will vary according to remaining banks' locations (in characteristics space) relative to the closed branch.

To illustrate this, consider one of the bank branches directly neighboring the closed one - for clarity, call this bank A. After the closing, A's market share expands to $M = \frac{1}{2n^*} + \bar{x}$, where $\bar{x}$ is defined by:

$$\begin{aligned} F\left(\bar{x}, p^*\right) &= 0 \\ s - p^* - t\bar{x} &= 0 \\ \bar{x} &= \frac{s - p^*}{t}. \end{aligned}$$

If A increases its price to $p^* + \epsilon$, it earns ϵ more on each of its inframarginal consumers, but loses those at the margin who drop out due to the higher price. The loss in market share corresponding to an ϵ increase in the price is $\frac{\epsilon}{t}$. To see this: at p^*, the marginal consumer is located $\frac{s-p^*}{t}$ away from A. At $p^* + \epsilon$, the marginal consumer is located $\frac{s-p^*-\epsilon}{t}$ away. Increasing the price by ϵ therefore decreases A's market share by $\frac{s-p^*}{t} - \frac{s-p^*-\epsilon}{t} = \frac{\epsilon}{t}$.

This means the range of ϵ for which the benefit of increasing the price outweighs the cost is given by:

$$\begin{aligned}
\epsilon\left(M-\frac{\epsilon}{t}\right) &> (p^*-c)\left(\frac{\epsilon}{t}\right) \\
\epsilon &< Mt-(p^*-c)\,.
\end{aligned}$$

That is, it is optimal for A to increase its price if $Mt > (p^* - c)$. I derive the parameter restrictions under which this condition is satisfied by substituting in the equilibrium expressions $n^* = \sqrt{t/f}$ and $p^* = c + \sqrt{tf}$ (these are derived in Salop (1979)). $\bar{x}$ is then given by:

$$\begin{aligned}
\bar{x} &= \frac{s-p^*}{t} \\
&= \frac{s-c-\sqrt{tf}}{t}
\end{aligned}$$

A's market share after the closing is:

$$\begin{aligned}
M &= \frac{1}{2n^*}+\bar{x} \\
&= \frac{1}{2\sqrt{t/f}}+\frac{s-c-\sqrt{tf}}{t}.
\end{aligned}$$

Which means it is optimal for A to increase its price if:

$$\begin{aligned}
Mt &> p^*-c \\
\frac{t}{2\sqrt{t/f}}+s-c-\sqrt{tf} &> c+\sqrt{tf}-c \\
\frac{\sqrt{tf}}{2}-2\sqrt{tf} &> c-s,
\end{aligned}$$

which can be rearranged as:

$$\frac{3}{2}\sqrt{tf} < s-c.$$

In other words, it is optimal for A to increase its price if firms' surplus from borrowing is high enough relative to banks' marginal costs.

The same analysis applies to the bank neighboring the closed branch on the other side. As these banks increase their prices, this provides their upstream neighbors with flexibility in increasing their prices (since some of their customers now face a more expensive outside option), which in turn allows their neighbors to increase prices, and so on and so forth. Ultimately, this can lead to a cascade of price increases across the market, with the magnitude of the increase being larger for banks located closer to the closed branch. This can lead to the scenario depicted in Appendix Figure A.5, where the orange dotted segments show that consumers who are not directly exposed to the closing may nonetheless drop out in response to the post-closing higher prices.

It is not entirely straightforward to map the pattern displayed in Appendix Figure A.5 to the tract-level identification strategy, but to the extent that distance in geographic space is roughly correlated with distance in characteristics space, we would expect that tracts very far from branch closings are less affected by changes in local concentration than tracts located nearby. This suggests that lending may be lower in Exposed tracts relative to Controls after a closing because these are the areas where prices increase more. While this is a concern in theory, I show in Figure 6 that lending does not respond to the entry of new banks in Exposed tracts. This suggests that, in this context, these concentration effects are empirically negligible.

B Household Credit Outcomes

In this section, I use data from the Federal Reserve Bank of New York/Equifax Consumer Credit Panel (CCP) to gauge the impact of branch closings on household credit outcomes such as bankruptcy and delinquency rates. These are quarterly, individual-level panel data provided by Equifax, and constitute a 5% random sample of all individuals with a credit history, along with all members of their household.[22] For each individual, the data include the information contained in their history including loan account data, public record and collection agency data, and a limited amount of individual background data. I use only fourth-quarter data to generate an annual dataset, and aggregate variables to the household level.

Households in the CCP data are linked to their Census tract of residence, so these results rely on the tract-level identification strategy described in Section 3. Approximately 20% of households in the sample move at least once over the sample period so, for each merger, Exposed and Control tracts are associated with households living there in the year before the intention to merge was announced. The results are robust to including/excluding those who subsequently move. I estimate a household-level version of Equation 4, where the right hand side includes a vector of household-level pre-merger characteristics, including whether the household has a mortgage, any delinquent accounts (those at least 30 days past due), any bankruptcy or foreclosure on file, and a proxy for holding a small business credit card.

The full CCP sample consists of over 13 million households. Once restricted to those living in Exposed and Control tracts, the sample shrinks to 233,701 households. Summary statistics are shown in Appendix

[22] For a detailed explanation of the randomization procedure, see Lee and van der Klaauw (2010).

Table A.9. Households living in Exposed and Control tracts are similar along most dimensions, though those living in Exposed tracts are slightly more likely to hold a small business credit card. These cards are not directly identified in the data, but, as they are characterized by high limits, I proxy for them by identifying households where the average credit limit over all open credit cards is at least $20,000.

Appendix Figure A.6 shows closings have no impact on the financial stability of surrounding households, as measured by delinquency rates, collection rates, credit scores, and bankruptcy rates. The results are similar when the sample is restricted to low-income tracts, and when the Control group consists of tracts located at least 5 miles away from an Exposed tract. There are several possible explanations for this. The first is that Section 4.2 shows closings are associated with a prolonged decline in credit supply to local small businesses, and not to local consumers. Data from the 2010 Survey of Consumer Finances show that only 4% of households took out a loan to finance a business they owned in that year. Second, given the CCP consists of only a 5% random sample, the number of observations in any tract may not be large enough to pick up the direct effects of a decline in small business lending. Third, to the extent there are indirect effects stemming from depressed local economic activity, this may be better reflected in the financial stability of households who work, rather than live, in these areas.

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

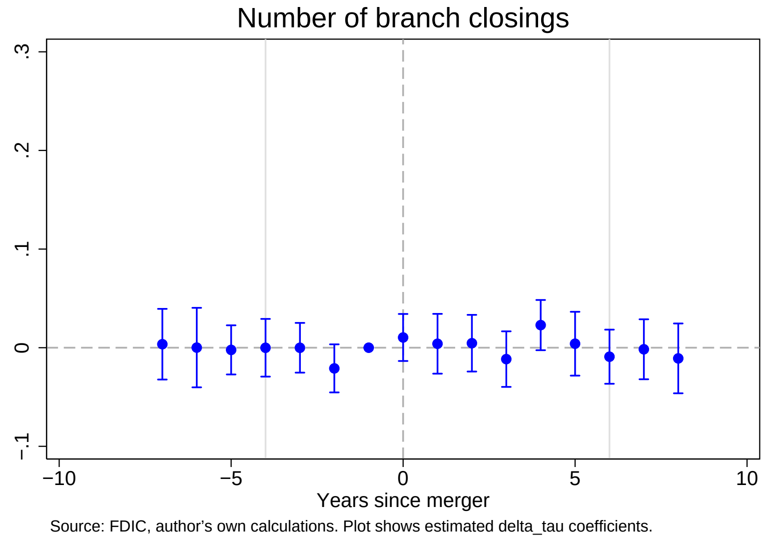


Figure plots the δ_τ estimated from the event study specification, along with the 95% confidence intervals. The treated group is tracts that only had branches from either the Buyer or the Target bank (but not both) prior to the merger, and the control group is unexposed tracts. $\tau = 0$ is the year 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 A.2: Exposure to Consolidation and the Incidence of Branch Closings, ZIP-Level

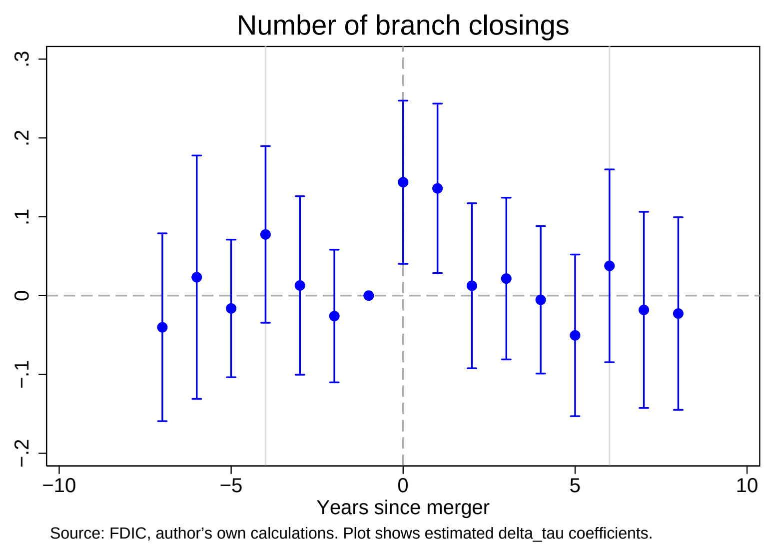


Figure shows the first stage relationship between exposure to consolidation and the incidence of branch closings at the ZIP level. The figure plots the δ_τ estimated from the event study specification, along with the 95% confidence intervals. The dependent variable is the number of branch closings in ZIP z 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 ZIP level.

Figure A.3: Exposure to Consolidation and the Volume of New Small Business Lending, ZIP-Level

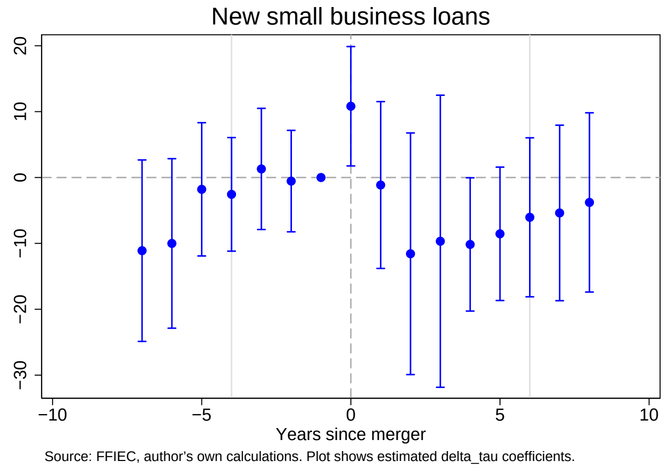


Figure shows the reduced form relationship between exposure to consolidation and small business lending at the ZIP level. The figure plots the δ_τ estimated from the event study specification, along with the 95% confidence intervals. The dependent variable is the number of new small business loans made to borrowers located in ZIP z 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 ZIP level.

Figure A.4: Salop Circle Model: Effect of a Branch Closing on Lending to Local Borrowers

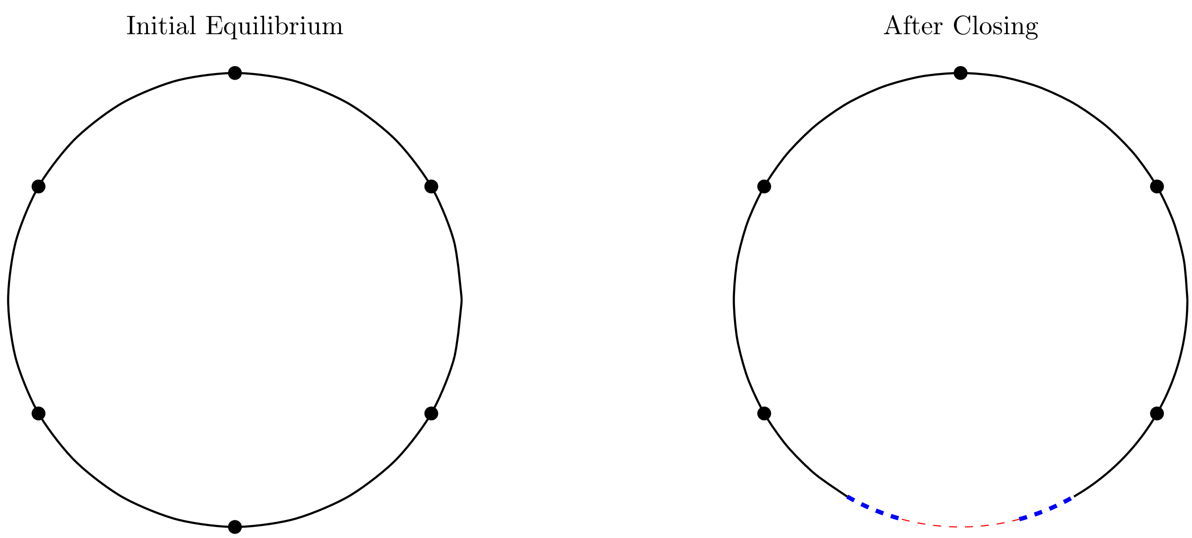


Figure illustrates the effect of a branch closing on lending to local borrowers following the Salop circle framework described in Appendix Section A. The left panel shows the initial equilibrium with banks spaced evenly along the circle. The right panel shows the effect of closing a branch. The dashed segment shows consumers who previously borrowed from the closed bank: those located on the thick, blue segment switch to borrowing from one of the neighboring banks, while those on the thin, red segment exit the market as they are too far away to earn positive surplus. Lending declines by the length of the dashed segment immediately after the closing, since those on red exit and those on blue appear inactive while in the process of building a new relationship.

Figure A.5: Salop Circle Model: Price Effects Following a Branch Closing

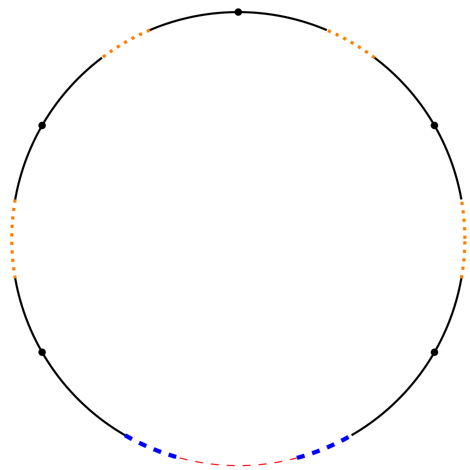


Figure displays the potential market-wide price effects of a branch closing using the Salop circle framework presented in Appendix Section A. As described in greater detail in that section, a single closing can result in a cascade of increasing prices across the market, with the largest increases occurring for banks located closest to the location of the closing. Consumers on the thick, blue, dashed segment are those stranded by the closing who switch to borrowing from one of the neighboring banks; those on the thin, red, dashed are stranded by the closing but are located too far away from neighboring banks to continue borrowing; those on the dotted orange segments are not directly exposed to the closing, but drop out in response to higher prices.

Figure A.6: Impact on Household Financial Stability

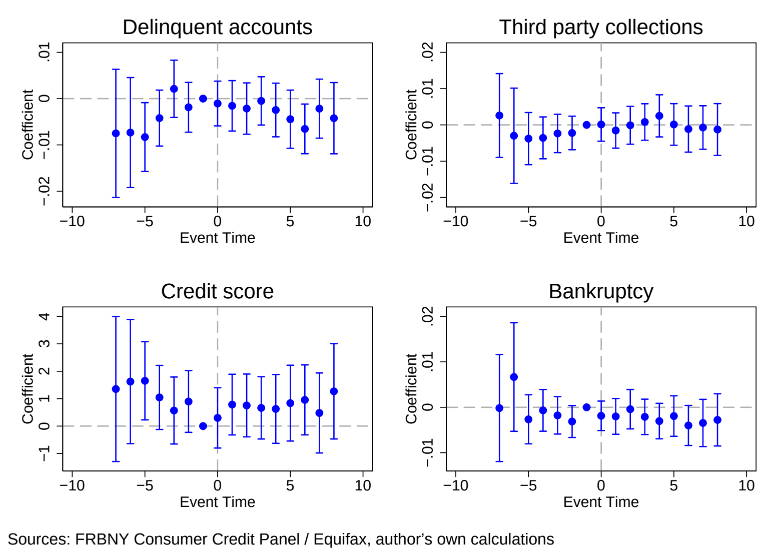


Figure shows the reduced form relationship between living in a tract that is exposed to post-merger consolidation and various measures of household financial stability. The figure plots the δ_τ estimated from the event study specifications, along with the 95% confidence intervals. The dependent variables are indicators for having at least one delinquent account (i.e., an account at least 30 days past due), having any third party collections on file, and having either a bankruptcy or foreclosure on file. $\tau = 0$ is the year the merger was approved by federal regulators, and all coefficients are normalized relative to $\tau = -1$. 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 7 mergers excluded from the primary sample because they were either classified as failing (i.e., they required financial assistance from the FDIC) or occurred during the 2008 financial crisis.

Table A.3: Buyer and Target Small Business Lending Intensity

Buyer	Target	Year Approved	Buyer Intensity	Target Intensity
Manufacturer & Traders	Allfirst	2003	6.5	5.5
Bank of America	Fleet	2004	1.5	4.3
National City	Provident	2004	5.0	0.8
Regions	Union Planters	2004	37.6	14.2
JPMorgan Chase	Bank One	2004	1.6	1.8
North Fork	Greenpoint	2004	12.9	0
SunTrust	National Bank of Commerce	2004	11.0	10.2
Wachovia	SouthTrust	2004	4.9	13.7
Sovereign	Independence Community	2006	5.2	2.0
Regions	AmSouth	2006	22.2	30.0
Bank of America	United States Trust	2007	1.5	0.3
Huntington National	Sky	2007	9.5	11.8
Bank of America	LaSalle	2007	1.2	0.8

Source: FDIC, FFIEC, author's own calculations. Table shows the intensity of small business lending for each Buyer and Target bank in the merger sample. Intensity is defined as the ratio of the dollar volume of small business loan originations (as reported through CRA disclosures) over total assets in the year prior to the merger.

Table A.4: The Effect of Closings on Credit Supply in Target Only Tracts

		(1)	(2)
	Coefficient	Exposed Tracts	Target Only Tracts
# Loans	δ_{POST}	-2.504***	-1.032
		(0.903)	(0.727)
	Obs.	46,631	35,517
$ Volume (thousands)	δ_{POST}	-83.91*	-37.87
		(49.61)	(42.23)
	Obs.	46,601	35,801

Source: FFIEC, author's own calculations. Table compares reduced form estimates of the post-merger decline in new small business loans in Exposed tracts (Column 1) versus tracts that only had branches from the Target bank prior to the merger (Column 2). Robust standard errors are clustered at the tract level and are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A.5: Robustness to Varying Size of the Local Banking Market

Variable	(1) County	(2) 25-Mile	(3) 15-Mile	(4) 10-Mile
δ_{POST}	-2.504***	-2.474***	-2.414***	-2.051**
	(0.903)	(0.895)	(0.908)	(0.916)
Control Mean	90.70	89.5	89.3	89.7
Obs.	46,631	95,177	77,643	57,436

Source: FDIC, FFIEC, author's own calculations. Table shows reduced form estimates of the post-merger mean shift in the level of new small business loans for different definitions of the local banking market. Column 1 is the baseline estimate based on within-county comparisons between Exposed and Control tracts. Column 2 defines the local market for each Exposed tract to be all Control tracts located within a 25-mile radius. Columns 3 and 4 use analogous definitions for markets of 15- and 10-mile radii. The number of observations is higher in Columns 2 through 4 since the same Control tract may be defined as part of the local market for several 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 A.6: Correlation between Tract Demographics and Branch Levels

Variable	Branches	Median Income	Fraction Minority
Branches	1.00		
Median Income	0.0079	1.00	
Fraction Minority	-0.0984	-0.4369	1.00

Source: FDIC, U.S. Census, author's own calculations. Table presents the correlation matrix between tract-level median income and fraction minority (as of the 2000 Census) and the number of pre-merger branches.

Table A.7: Summary Statistics for Exposed and Control ZIPs

Variable	Exposed Minus Control	Control Mean
Population	1,622	28,538
	(1,111)	
Population Density	210.6	4,477
	(235.5)	
Fraction Minority	-0.037***	0.280
	(0.014)	
Fraction College-Educated	0.051***	0.300
	(0.011)	
Median Income (000s)	2.928**	49.06
	(1.154)	
Fraction Mortgage	0.018**	0.721
	(0.009)	
Pre-Merger Branches	2.721***	9.040
	(0.459)	
Pre-Merger Branch Growth	-0.001	0.057
	(0.015)	
Joint F-test	7.10	
p-value	0.00	
Number Exposed	353	
Number Control	1,588	

Source: FDIC, U.S. Census, author's own calculations. Table provides summary statistics for Exposed and Control ZIPs. The former are ZIPs that contain at least one Exposed tract; the latter are those that contain only Control tracts. Table values are obtained from a regression of each ZIP-level characteristic on an indicator for being an Exposed ZIP and county fixed effects. Population density is per square mile. Demographic characteristics 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 A.8: Industry Dependence on External Finance

Two Digit NAICS	Two Digit NAICS Name	External Dependence Measure	External Dependence Flag
62	Health Care and Social Assistance	-0.87	0
42	Wholesale Trade	-0.79	0
11	Agriculture, Forestry, Fishing, and Hunting	-0.43	0
61	Educational Services	-0.43	0
81	Other Services (except Public Administration)	-0.29	0
44	Retail Trade	-0.20	0
22	Utilities	-0.14	0
56	Administrative and Support and Waste Management and Remediation Services	-0.01	1
48	Transportation and Warehousing	-0.01	1
31	Manufacturing	0.16	1
72	Accommodation and Food Services	0.18	1
71	Arts, Entertainment, and Recreation	0.45	1
54	Professional, Scientific, and Technical Services	0.81	1
51	Information	0.95	1

Source: Gilje (2012). Table shows the industry-level measure of dependence on external finance computed by Gilje (2012) using Compustat data for the period 1999-2008 and the methodology outlined by Rajan and Zingales (1998). Industry groups are based on the two digit North American Industry Classification System. The external dependence measure is the industry median dependence on external finance, and industries with above median dependence on external finance have an External Dependence Flag equal to 1.

Table A.9: Summary Statistics for Households Living in Exposed and Control Tracts

Variable	Exposed Minus Control	Control Mean
Has a mortgage	0.00486	0.508
	(0.00317)	
Credit score	0.990	683.3
	(0.617)	
Any delinquent accounts	0.00287	0.273
	(0.003)	
Any third-party collections	-0.00108	0.220
	(0.003)	
Bankruptcy on file	-0.0027	0.127
	(0.002)	
Foreclosure on file	0.00135	0.0242
	(0.0010)	
Small business credit card	0.00475***	0.0300
	(0.0011)	
Joint F-test	3.85	
p-value	0.0003	
Number Exposed	32,285	
Number Control	199,416	

Source: FRBNY Consumer Credit Panel / Equifax, author's own calculations. Table provides summary statistics for households living in Exposed and Control tracts prior to each merger. Table values are obtained from a regression of household-level characteristics on an indicator for living in an Exposed tract and county fixed effects. Credit score is the Equifax risk score, which is correlated with the FICO score and has the same range. Delinquent accounts are those that are at least 30 days past due. I proxy for having a small business credit card by using a dummy equal to one if the average limit over all open credit cards is at least $20,000. The median number of households in each tract is 245, and the median length of time a household appears in the data is 13 years. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1