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Are Credit Markets Still Local? Evidence from Bank Branch Closings†

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This paper studies whether distance shapes credit allocation by estimating the impact of bank branch closings during the 2000s on local access to credit. To generate plausibly exogenous variation in the incidence of closings, I use an instrument based on within-county, tract-level variation in exposure to post-merger branch consolidation. Closings lead to a persistent decline in local small business lending. Annual originations fall by $453,000 after a closing, off a baseline of $4.7 million, and remain depressed for up to six years. The effects are very localized, dissipating within six miles, and are especially severe during the financial crisis. (*JEL* G21, G34, L22, R12, R32)

Stretching back to Marshall (1890), a rich literature studies how distance shapes investment (Helpman 1984, Brainard 1997), trade (Tinbergen 1962; Krugman 1991a; Helpman, Melitz, and Rubinstein 2008), and economic activity more generally (Krugman 1991b, Glaeser et al. 1992). Indeed, the fundamental driver of agglomeration economies is the idea that geographic proximity reduces the costs of transferring labor, goods, and importantly, information (Duranton and Puga 2004). Over the last few decades, however, technological changes have dramatically lowered the costs of transmitting and processing information. This raises the question of whether distance still matters for informationally intensive activities.

The banking sector is a natural environment for assessing this question since an extensive body of research holds that informational asymmetries are central to credit allocation (e.g., Akerlof 1970 and Stiglitz and Weiss 1981) and information technologies have had an especially pronounced impact there. Kroszner and Strahan (1999) argue that, starting in the 1970s, innovations in information technology and credit scoring reduced geographic ties between customers and banks and laid the groundwork for subsequent branch deregulation; Petersen and Rajan (2002) show that the

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same forces have gradually eroded the local nature of small business lending; and over the last few years, billions of dollars have been invested in online financial technology services, while in-branch visits now account for only a fraction of banking transactions.[1] This suggests that improvements in information technology may have reduced the role of distance in shaping credit outcomes.

In this paper, I combine a quasi-experimental research design with nationally representative data on small business lending in the United States to evaluate whether distance matters for credit allocation. Previous papers have documented correlations between distance and credit outcomes in either survey data or data obtained from a single lender (Petersen and Rajan 2002; Degryse and Ongena 2005; Agarwal and Hauswald 2010; Brevoort, Holmes, and Wolken 2010). I take a novel approach and estimate the causal impact of bank branch closings during the 2000s on local access to credit. While many papers have studied the effects of large, aggregate shocks to the banking system, much less attention has been paid to shocks at the branch level. Each year, hundreds of branches are opened and closed in the United States as banks adjust their physical footprints in response to changing local market conditions and firm objectives. While the ebb and flow of these adjustments occur well below the surface of the aggregate health of the banking system, they may have substantial effects at the local level. In recent years, this issue has become especially important as the incidence of branch closings has increased dramatically in the wake of the Great Recession. This trend has generated concerns about diminished access to financial services and credit, even as banks argue that technology has rendered the traditional model of branch banking increasingly irrelevant.

The empirical challenge in estimating the local effects of branch closings is that the closing decision is endogenous to local economic conditions that are correlated with credit demand. Banks are likely to close branches in areas where actual or expected profitability is low, and a naïve comparison between areas where branches close and areas where they do not would produce a biased estimate of the impact of the closing itself. As a solution to this endogeneity problem, I use exposure to merger-induced consolidation as an instrument for branch closings. Bank mergers are often followed by a period of retrenchment in which branches are closed in neighborhoods where the two previously separate buyer and target branch networks overlap. As the motivation for closing is strongest in areas where the geographic redundancy is highest, I rely on within-county comparisons between census tracts that have branches from both merging banks prior to the merger and those that do not. 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. The identifying assumption is that the incidence of the merger is plausibly exogenous to local economic conditions in census tracts where both merging banks have a branch. To ensure this is the case, I focus exclusively on mergers between very large banks: i.e., those where both the buyer and target banks held at least \$10 billion in premerger assets.

[1] See Sidel (2013) and Pignal (2015).

Existing papers have used mergers as an instrument for changes in the concentration of local markets: Hastings and Gilbert (2005) in gasoline markets; Dafny, Duggan, and Ramanarayanan (2012) in health insurance; and most relevant for this paper, Garmaise and Moskowitz (2006), who study the effects of merger-induced changes in banks' local market power on real activity and crime. This paper adopts a similar strategy to study the effect of physical branch closings on local credit supply, but makes the critical contribution of defining merger exposure at the tract level and using tract-level data. Using a disaggregated geographic level allows me to separate the impact of closings from the aggregate market-level effects of a merger, including the competition channel studied by Garmaise and Moskowitz, whereas these effects are potentially confounded by previous work in which exposure is defined at the market level.

Figure 1 illustrates the 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 (those that had branches from both merging banks prior to the merger) relative to a set of control tracts that are located in the same county and had branches belonging to at least two large non-merging banks. The spirit of this approach is to compare tracts that, a priori, were equally likely to have been exposed to a large bank merger. The average exposed tract in my sample has six branches prior to the merger, indicating that the instrument identifies the effects of closings that occur in substantially crowded markets. Though the policy discussion around branch closings focuses on those that lead to the creation of "banking deserts," data from the FDIC show that only 20 percent of closings since 2010 have been cases where the closed branch was the only one in its census tract.[2] My results are informative for whether closings have disruptive effects even when the local banking market is very dense and likely underestimate the impact of banking deserts.

I show closings lead to a sharp and persistent decline in credit supply to local small businesses. Annual, tract-level small business loan originations decline by $453,000 after a closing, off a baseline of $4.7 million, and remain depressed for up to 6 years. This amounts to a cumulative loss of $2.7 million in forgone loans. These effects are very localized, dissipating within six miles of the tract where the closing occurs. To rule out alternative channels related to the merger, I provide evidence that the decline in small business lending cannot be attributed to either changes in local concentration or changes in banks' internal processes for approving loans. Ultimately, the decline in lending leads to a 2 percentage point reduction in local employment growth rates, driven primarily by tighter constraints on the size of entering firms.

These findings indicate that local branches play a crucial role in providing access to credit. This is striking given that technology has drastically widened the reach of arm's length financing in the United States. To shed light on why distance still matters, I provide suggestive evidence that the negative effects of closings can be attributed to the disruption of branch-specific relationships for information-intensive borrowers. For one, lending declines despite the fact that these closings occur in

[2] This 20 percent figure is obtained by geocoding branch locations and closings as reported in the FDIC Summary of Deposits and the FDIC Report of Changes.

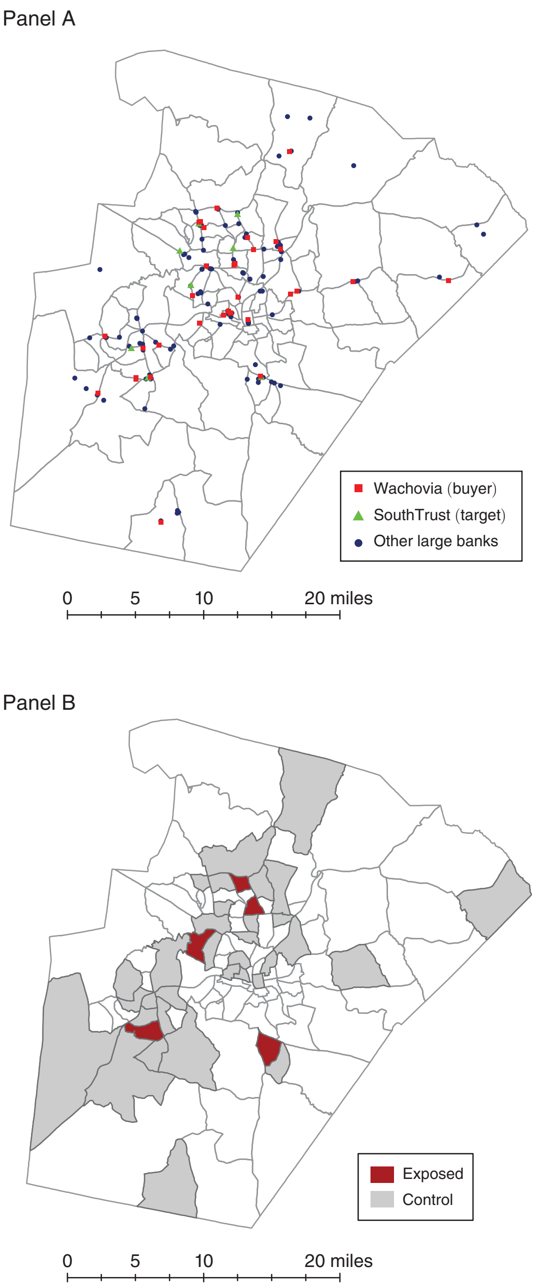

FIGURE 1. DEFINING EXPOSED AND CONTROL TRACTS—WAKE COUNTY, NC

Notes: Panel A shows the census tract boundaries in Wake County, North Carolina, along with the geographic distribution of bank branches in the year prior to the 2004 Wachovia-SouthTrust merger. Red squares are Wachovia (buyer) branches, green triangles are SouthTrust (target) branches, and blue circles are branches belonging to other banks with at least $10 billion in assets. 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. Exposed (red) and control (gray) tracts are shown in panel B.

Source: FDIC, author's own calculations very crowded markets where there is no meaningful impact on a borrower's ability to access another nearby branch. Lending also remains low despite the entry of new banks in these areas. The decline is observed in small business lending, the prototypical example of an information-intensive market, and is absent in mortgage lending, which is more transactional in nature. Finally, I also show that the negative effects of closings are most severe in cases where we would expect credit allocation to be more heavily contingent on soft information about the borrower: in particular, during the period of tightened lending standards that coincided with the financial crisis. These results suggest that distance matters because technology has yet to supplant the role of geographic proximity in facilitating the transfer of soft information.

These findings have several implications for merger policy and for banking regulation more generally. While mergers are already evaluated on the basis of their local impact, at the branch level, the focus is on banking deserts: will closings leave some communities completely unbanked?[3] In this respect, banks are treated similarly to other services, such as grocery stores and hospitals, where accessibility is thought to be important. What this approach misses is the key additional element that sets banking and credit provision apart: the importance of relationships. In this setting, distance matters not only because it improves accessibility, but also because it reduces the costs of transmitting information and facilitates the forging of long-term relationships. Thus, closings can have large effects, even in dense banking markets, if they disrupt lender-specific relationships that are difficult to replace.

This paper shows that geographic proximity and distance still matter for shaping credit allocation in the United States. The banking sector has undergone a series of vast changes over the last several decades, all of which have reduced the importance of distance and threatened the role of local branches. Yet, even in the 2000s, there are some markets and some segments of the population for whom local credit markets play a crucial role in facilitating access to credit and financial services. These results resonate with the conclusion of Glaeser (1998) that technology is not an adequate substitute for all forms of interaction. While technology has relaxed many geographic constraints for information-intensive activities, such as lending, it has not managed to eradicate them entirely.

The paper proceeds as follows. Section I reviews this paper's contributions to the existing literature. Section II describes the data. Section III discusses the identification strategy and empirical framework. Section IV presents and discusses the results. Section V concludes.

I. Related Literature

This paper is aligned with a broad literature that has studied the importance of physical location and geographic proximity in a wide range of contexts outside of banking. These include investment (Moretti 2004, Giroud 2013), innovation

[3] This focus on the margin between banked and unbanked is not unique to merger policy and characterizes branch regulation more generally. For example, the FDIC requires a 90-day notice ahead of any intention to close a branch, but waives this requirement in cases of consolidation where the branches involved are "within the same neighborhood." See https://www.fdic.gov/regulations/laws/rules/5000-3830.html.

(Carlino and Kerr 2015), and access to services (Card 1995; Rossin-Slater 2013; Avdic (2016); Handbury, Rahkovsky, and Schnell 2017). By demonstrating that distance is still important in a sector as radically transformed by technology as banking, my results suggest that technology has yet to eradicate the importance of geographic proximity for information-intensive activities more generally.

I also contribute to the literature on the importance of distance in banking, showing in particular that geographic proximity shapes credit allocation to small businesses, even in the 2000s. Many papers in this literature focus on small business lending, as information asymmetries are especially severe in this market (Petersen and Rajan 1994, Berger and Udell 1995). Previous work has studied the relationship between distance and credit outcomes by analyzing correlations in either survey data (Petersen and Rajan 2002; Amel and Brevoort 2005; Brevoort, Holmes, and Wolken 2010) or data obtained from a single lender (Degryse and Ongena 2005, Agarwal and Hauswald 2010) and has reached differing conclusions. Most starkly, Petersen and Rajan (2002) argue that technology is "slowly breaking the tyranny of distance," while Brevoort, Holmes, and Wolken (2010) argue that distance still matters. I improve upon previous approaches by exploiting quasi-experimental variation induced by very local credit supply shocks combined with comprehensive data on small business lending across the United States. Outside of the context of small business lending, my results are consistent with those of Gilje, Loutskina, and Strahan (2016), who show that branch networks continue to play an important role in integrating the most information-intensive segments of the mortgage market.

This paper provides evidence on a novel channel through which bank mergers effect change at the local level: the closure of individual bank branches. Previous papers emphasize market-level or aggregate impacts of mergers: Garmaise and Moskowitz (2006) focus on the effects of changes in market concentration; Strahan and Weston (1998), Berger et al. (1998), and Peek and Rosengren (1998) emphasize changes in banks' post-merger internal organization; Sapienza (2002) explores both. The tract-level identification strategy used in this paper allows me to separate the impact of closings from the aggregate market-level effects of a merger, and I show that the post-merger decline in my sample cannot be attributed to either of the channels emphasized in the existing literature. This suggests that estimates of the market-level impacts of mergers potentially confound their effects with those of branch closings.

Finally, I find persistent effects of closings on local small business lending, which provides evidence that information-intensive borrowers are sensitive to local credit supply shocks and are unable to seamlessly substitute toward other lenders. While many papers have explored the implications of this stickiness for understanding the effects of negative bank lending shocks, exploiting either variation in the bank-level incidence of aggregate shocks (Peek and Rosengren 2000; Khwaja and Mian 2008; Chodorow-Reich 2014; Jiménez et al. 2014; Greenstone, Mas, and Nguyen 2017) or idiosyncratic, bank-level failures (Ashcraft 2005), I use detailed microdata to tie these dynamics to branch-level shocks that occur regularly throughout the banking system.

II. Data

The primary unit of observation in this paper is the census tract. These are defined by the US 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 III, 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 United States overall). Tract boundaries are slightly revised with each census, and this paper uses boundaries as of the 2000 census.[4]

To construct the exposure instrument, I use the FDIC Summary of Deposits (SOD), 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. Online Appendix Table A.1 provides summary statistics for this geocoding procedure: column 5 shows the percentage of unmapped observations in the full SOD data is 7.5 percent in 1999 and declines to 0.6 percent in 2012. Column 6 shows the corresponding figures for the subset of counties included in the estimation sample is only 4.6 percent in 1999, declining to 0.1 percent in 2012.

Data on merger activity and branch closings are from the FDIC Report of Changes. To gauge the impact of closings on local lending, I use Community Reinvestment Act (CRA) and Home Mortgage Disclosure Act (HMDA) data published by the Federal Financial Institutions Examination Council (FFIEC). Under the CRA, all banks with assets greater than \$1 billion are required to disclose annual tract-level data on the number and dollar volume of loans originated to businesses with gross annual revenues less than or equal to \$1 million.[5] While these data only capture small business loans originated by CRA-eligible banks, Greenstone, Mas, and Nguyen (2017) estimate that these institutions account for 86 percent of total lending in this market. To measure small business lending by institutions excluded from CRA reporting requirements, I use call report data from the Federal Reserve Bank of Chicago and the National Credit Union Administration (NCUA).

Under HMDA reporting criteria, financial institutions are also required to publish data on their local mortgage lending activity.[6] HMDA data are at the loan application level and include not only the census tract associated with the application, but

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

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

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

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 US Department of Veterans Affairs, or other government programs, which constitute approximately 10 percent of the 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 percent level.

It is important to note that both CRA and HMDA data are based on the location of the borrower, as opposed to the location of the bank. For a given tract, the data measure the total number of loans made to borrowers located in that tract, regardless of the location of the originating branch. This allows me to estimate the impact of a branch closing on total credit supply to borrowers located in the same tract. Call report data are, unfortunately, not available at a geographically disaggregated level and can only be used to approximate measures of tract-level lending by non-CRA lenders, as described in greater detail in Section IV.

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

III. Identification and Empirical Framework

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

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

where y_{it} is total lending to borrowers located in tract i in year t, α_i are tract fixed effects, γ_t are year fixed effects, $\mathbf{X}_{it}$ is a vector of tract characteristics, and $Close_{it}$ is an indicator equal to one 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 one if two banks with branches in tract i undergo a merger in year t.

The key identifying assumption is that tract-level exposure to bank mergers is as good as randomly assigned. That is, there is no systematic difference between exposed and control tracts that would make pairs of banks with branches in exposed tracts more likely to merge with one another than pairs of banks with branches in control tracts. This assumption is violated if the buyer and target banks' decision to merge is driven by factors specific to tracts where their branch networks overlap. For example, banks may choose to merge precisely when local economic conditions in exposed tracts are poor, and therefore, the cost savings from merging and consolidating branches is highest. In this case, even absent the merger, we would expect exposed tracts to differ systematically from control tracts in ways that would impact both the incidence of closings as well as local lending.

The requirement that the decision to merge is plausibly exogenous with respect to the exposed tracts is necessary for the *internal* validity of the exposure instrument and is distinct from the concern that, conditional on a merger occurring, buyer and target banks will select which branches to close and where. Endogeneity of the ultimate closing decision does not imply endogeneity of the instrument, but is relevant for evaluating the *external* validity of the identification strategy and is discussed in greater length at the end of Section III.

To address the concern that banks' decision to merge may not be plausibly exogenous with respect to local economic conditions in areas where their branch networks overlap, I focus on mergers between very large banks—i.e., those where both buyer and target banks held at least $10 billion in premerger assets, which roughly corresponds to the top 1 percent of the size distribution of US banks. Mergers between large national banks are generally motivated by several considerations, including expansion into new markets, the synthesis of complementary business functions, and an increase in market power. The cost savings from consolidation may also play a role, but it is unlikely that mergers of this scale are decided on the basis of tract-level differences in local economic conditions. Indeed, for mergers in my sample, the median percentage of the buyer (target) banks' deposits held in exposed tracts prior to the merger is only 1.4 percent (3.5 percent). This represents such a small percentage of the merging banks' overall businesses that it is unlikely that any factors specific to these areas would influence the decision to merge.

Table 1 lists the mergers used in the baseline sample. These are mergers that occurred during the 2000s but before the financial crisis, involved buyer and target banks that each held at least $10 billion in premerger assets, and where the merging institutions had overlapping retail branch networks in at least one census tract. Table 2 reports summary statistics for the buyer and target banks involved in these mergers. By construction, these are very large institutions (the median buyer holds $82 billion in assets, while the median target holds $26 billion) with extensive branch networks (the median buyer controls 696 branches and operates in 8 states, while the median target controls 277 branches and operates in 6 states). For comparison, the median bank in the United States holds $100 million in assets and controls only 3 branches.

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

Notes: This table shows the 13 mergers included in the primary merger sample and the year they were approved by federal regulators. See Section III for greater details on the criteria for inclusion in this sample.

Source: FDIC

Table 2—Merger Summary Statistics

	Median	Min	Max
Panel A. Buyer			
Total assets (billion $)	82	26	1,250
Branches	696	254	5,569
States of operation	8	1	31
Counties of operation	182	18	692
Panel B. Target			
Total assets (billion $)	26	10	246
Branches	277	28	1,482
States of operation	6	1	13
Counties of operation	54	7	202

Notes: This 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.

Source: FDIC

For each merger, exposed tracts are defined to be those that had branches from both buyer and target banks in the year prior to the merger. Figure 1 shows how these tracts are identified for a sample merger and a sample county in the data. The top panel shows a map of Wake County, NC with census tracts delineated and the geographic distribution of bank branches in the year prior to the 2004 Wachovia-SouthTrust merger. Red squares denote Wachovia branches, green triangles denote SouthTrust branches, and any tract containing both is an exposed tract, as shown in red in the bottom panel.[7]

Figure 1 shows that Wachovia and SouthTrust branches tend to be clustered around urban centers, which suggests that using the rest of the county as a control group would amount to a comparison between dissimilar urban and rural areas.

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

Column 3 of Table 3 confirms this and shows that exposed tracts differ significantly from all other tracts in the county along many dimensions: they have higher populations, a higher fraction of white and college-educated households, higher incomes relative to the MSA median, banking markets that are both larger and growing more quickly, a higher number of loan originations, and lower economic growth.[8]

To identify tracts that are more comparable to exposed tracts, I therefore map the locations of branches belonging to other large banks—i.e., other banks that also held at least $10 billion in assets. These are denoted with blue circles in the top panel of Figure 1. As my control group, I take any tract that did not have both a Wachovia and a SouthTrust branch, but did have branches from at least two large banks who did not merge with one another. These tracts are shown in grey in the bottom panel of Figure 1.

Column 5 of Table 3 shows that control tracts are much more similar to exposed tracts than all other tracts in the county, but some significant differences remain. I therefore use a difference-in-difference (DD) framework to compare lending in exposed and control tracts in the same county, before and after a merger, and allow for time-varying trends based on premerger tract characteristics. In the DD framework, the identification assumption becomes one of parallel trends: absent the merger, outcomes in exposed and control tracts would have evolved along the same path. To facilitate transparent examination of any pre-trends in the data, I estimate a year-by-year DD and present all my results as event study plots. The primary specification is

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

where y_{icmt} is an outcome for tract i in county c for merger m in year t; α_i are tract fixed effects; $\left(\gamma_t \times \sigma_c\right)$ are county-by-year fixed effects; $\mathbf{X}_i$ is a vector of premerger tract characteristics whose effects are allowed to vary by year; D_{mt}^{τ} is a dummy equal to one if year t is τ years after merger m is approved by federal regulators; and $Expose_{icm}$ is a dummy equal to one if tract i is an exposed tract for merger m. The premerger 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.[9] Here, τ ranges from -8 to 10, and standard errors are clustered at the county 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.

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

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

where f_{ic} is a premerger characteristic for tract i in county c, and $Expose_{ic}$ is a dummy equal to one if tract i is an exposed tract.

[9] Results are qualitatively robust to excluding these controls; however, the event studies then show evidence of pre-trends. Hence, they are included in the preferred specification.

Table 3—Summary Statistics for Exposed and Control Tracts

Variable	Exposed (1)	All other (2)	p-value on difference (3)	Control (4)	p-value on difference (5)
Population	5,761	4,572	0.000	5,388	0.054
	[3,234]	[2,366]		[2,715]	
Population density	2,575	7,206	0.392	6,106	0.970
	[7,936]	[14,577]		[13,871]	
Fraction minority	0.21	0.39	0.000	0.24	0.830
	[0.23]	[0.34]		[0.24]	
Fraction college educated	0.31	0.26	0.000	0.34	0.012
	[0.19]	[0.19]		[0.19]	
Percent MSA median income	114.5	102.0	0.000	118.6	0.232
	[46.2]	[51.4]		[54.0]	
Median income (000s)	44.22	45.45	0.008	52.17	0.999
	[20.31]	[23.29]		[24.05]	
Fraction mortgage	0.69	0.71	0.455	0.72	0.501
	[0.15]	[0.16]		[0.15]	
Total branches	5.85	1.14	0.000	3.82	0.000
	[3.93]	[1.94]		[2.39]	
Branch growth	0.05	0.03	0.000	0.06	0.204
	[0.13]	[0.17]		[0.17]	
Mortgage originations	277.2	227.1	0.000	281.0	0.214
	[203.6]	[179.0]		[189.0]	
SBL originations	103.4	54.3	0.000	89.0	0.000
	[53.5]	[44.8]		[50.9]	
Establishment growth	0.07	0.10	0.000	0.09	0.317
	[0.06]	[0.08]		[0.06]	
Employment growth	0.01	0.02	0.003	0.02	0.531
	[0.09]	[0.13]		[0.10]	
Observations	386	18,027		3,087	

Notes: Standard deviations are in brackets. Column 3 reports the p-value for the difference between columns 1 and 2. Column 5 reports the p-value for the difference between columns 1 and 4. Here, p-values are obtained from a regression of tract characteristics 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; credit and growth variables are as of the year preceding each merger. Growth rates are the average annual growth rates over the two years preceding the merger.

Source: FDIC, FFIEC, NETS, US Census, author's own calculations

External Validity.—The internal validity of the DD framework hinges on the assumption of parallel trends, but assessing the external validity of the merger instrument is also important. Is the local average treatment effect (LATE) identified from merger-induced closings informative for understanding the impact of branch closings more generally? Table 4 compares tracts in the merger sample to all branched tracts in the United States as well as to those that experienced a closing over the same period.[10] Tracts in the merger sample are more similar to tracts that

[10] Online Appendix Figure A.1 shows the geographic distribution of counties included in the merger sample. Exposed counties are located in regions with high levels of branch density and are particularly concentrated in the

Table 4—Representativeness of the Merger Sample

Variable	All branched tracts (1)	Tracts with closings (2)	Merger sample (3)
Population	4,688	4,942	5,401
	[2,194]	[2,431]	[2,703]
Population density	4,033	3,615	6,166
	[10,052]	[7,348]	[14,322]
Fraction minority	0.20	0.20	0.23
	[0.24]	[0.22]	[0.24]
Fraction college educated	0.25	0.27	0.34
	[0.17]	[0.17]	[0.19]
Percent MSA median income	104.3	106.3	118.9
	[38.2]	[41.2]	[53.6]
Median income (000s)	44.8	45.2	51.7
	[20.2]	[20.7]	[23.9]
Fraction mortgage	0.67	0.68	0.72
	[0.15]	[0.15]	[0.15]
Total branches	2.05	3.14	3.64
	[1.97]	[2.88]	[2.60]
Branch growth	0.02	0.04	0.04
	[0.14]	[0.16]	[0.15]
Mortgage originations	193.5	210.6	238.4
	[156.4]	[164.5]	[167.5]
SBL originations	45.0	58.7	70.6
	[36.6]	[43.9]	[46.9]
Establishment growth	0.04	0.04	0.05
	[0.06]	[0.05]	[0.05]
Employment growth	0.04	0.04	0.04
	[0.12]	[0.12]	[0.13]
Observations	37,053	7,768	3,003

Notes: Standard deviations are in brackets. Demographic variables are as of the 2000 census; all other variables are from 2001. Columns 1 and 2 are all tracts in the United States that were branched and had a closing, respectively, over the 2002–2007 period.

Source: FDIC, FFIEC, NETS, US Census

have experienced a closing than they are to the average branched tract in the United States, but have banking markets that are relatively larger and more wealthy. This is unsurprising given the requirement that exposed and control tracts must have branches from at least two large banks and suggests that, if anything, estimates of the effect of merger-induced closings are likely to underestimate the impact of the average branch closing in the United States.

To more closely assess the merger LATE, Table 5 shows the complier characteristics for my sample. While the set of tracts exposed to a large bank merger and to the increased risk of consolidation is exogenously determined (this is the key identifying assumption for the merger instrument), ultimately the merged bank still chooses which branches to close. This selection does not invalidate the instrument, which requires that *exposure* to the merger is as good as randomly assigned, but

eastern half of the United States.

Table 5—Complier Characteristics

Variable	Proportion of compliers above the sample median (percent) (1)	Ratio: Compliers to sample (2)
Population	58	1.15
Population density	18	0.37
Fraction minority	60	1.21
Fraction college educated	47	0.94
Percent MSA median income	43	0.87
Median income (000s)	29	0.58
Fraction mortgage	45	0.91
Total branches	88	1.75
Branch growth	49	0.98
Mortgage originations	56	1.12
SBL originations	73	1.46
Establishment growth	34	0.68
Employment growth	36	0.72

Notes: This table shows how complier tracts compare to the median tract in the sample. Complier characteristics are calculated using the methodology outlined in Angrist and Pischke (2009). Column 1 shows the proportion of compliers who lie above 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. Demographic variables are as of the 2000 census; total branches and branch growth are as of the year preceding each merger.

Source: FDIC, FFIEC, NETS, US Census, author's own calculations

does affect the interpretation of the merger LATE. 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. 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). The estimated treatment effect corresponds to the effect of closing a branch in a tract where a closing only occurred because two banks with branches in that tract underwent a merger.[11]

Table 5 shows that compliers tend to be fairly representative of the median tract in the sample, but are less densely populated, have a lower median income, a higher number of premerger branches (and correspondingly higher loan volumes), and economic growth that is slightly lower than the median. This suggests that, among the set of tracts eligible for post-merger branch consolidation, banks tend to concentrate their closings in areas deemed to be "over-branched" or over capacity. Again, this suggests that the estimated treatment effects likely underestimate the impact of the average branch closing in the United States and certainly that of closings that result

[11] While it is not possible to identify the compliers in the sample, Angrist and Pischke (2009) describes 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).

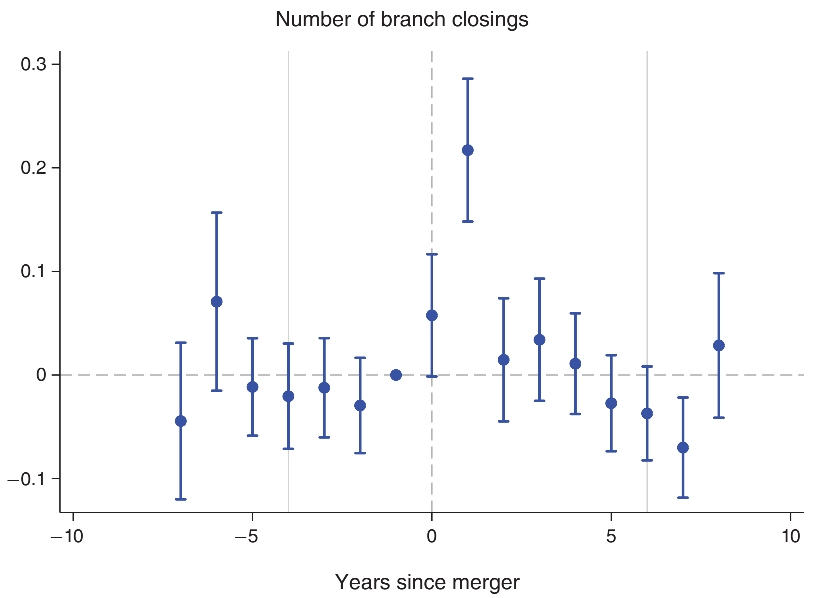

FIGURE 2. EXPOSURE TO CONSOLIDATION AND THE INCIDENCE OF BRANCH CLOSINGS

Notes: This figure plots the first-stage relationship between exposure to consolidation and the incidence of branch closings, obtained from estimating equation (3). The bars show 95 percent 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 county level.

Source: FDIC, author's own calculations

in banking deserts, as it identifies the effect of removing a branch from a crowded market.

IV. Results

A. *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 2 provides the template used for the event study results. It plots the δ_τ estimated from equation (3), where the dependent variable is the number of branch closings in tract *i* in year *t*. The bars show the 95 percent confidence intervals, and the lines at $\tau = -4$ and $\tau = 6$ denote the range over which there is a balanced panel. Notice, $\delta_\tau > 0$ indicates a higher incidence of branch closings in exposed tracts relative to controls τ years after a merger.

Figure 2 shows that up to several years prior to the merger, exposed tracts are no more likely than controls to experience a closing. However, the relative incidence increases in the year the merger is approved, spikes in the year after, and then falls back to zero. Column 1 of Table 6 presents the corresponding point estimates. There is generally a maximum of one closing per tract, so the sum of δ_0 and δ_1 can be

Table 6—First-Stage and Reduced-Form Estimates

	Number of closings (1)	Total branches (2)	Small business loan originations (3)	Mortgage originations (4)
$\delta_{<-1}$	−0.0120	0.0408	0.317	−0.993
	(0.0133)	(0.0923)	(0.776)	(2.288)
δ_0	0.0572	−0.0157	0.726	1.042
	(0.0299)	(0.0493)	(0.913)	(3.410)
δ_1	0.216	−0.301	−1.424	-4.873
	(0.0348)	(0.0729)	(1.021)	(3.691)
δ_2	0.0140	−0.241	−2.428	−5.926
	(0.0300)	(0.0881)	(1.318)	(4.108)
δ_3	0.0333	−0.258	−2.286	−3.823
	(0.0298)	(0.0705)	(1.314)	(3.504)
δ_4	0.0104	0.0169	−2.466	−2.618
	(0.0246)	(0.127)	(1.153)	(2.670)
δ_5	−0.0274	−0.00406	−2.728	1.671
	(0.0234)	(0.130)	(1.280)	(2.924)
δ_6	−0.0385	0.00320	−2.890	1.878
	(0.0230)	(0.129)	(1.399)	(3.171)
$\delta_{>6}$	−0.0236	0.0902	−1.863	2.438
	(0.0153)	(0.123)	(1.605)	(3.602)
Baseline mean	0.174	5.849	103.4	277.2
Observations	51,285	51,278	45,160	46,516

Notes: This table shows estimates of equation (3); all columns include the full set of baseline controls interacted with year dummies along with tract and county-by-year fixed effects. Columns 1 and 2 show the first-stage relationship between exposure to consolidation and branch closings and total branches, respectively. Columns 3 and 4 show the reduced-form relationship between exposure and lending. All coefficients are normalized relative to $\tau = -1$, and $\tau = 0$ is the year in which the merger was approved by federal regulators. The baseline mean is calculated for exposed tracts in $\tau = -1$. Robust standard errors are clustered at the county level and are in parentheses.

Source: FDIC, FFIEC, author's own calculations

interpreted as a 27 percentage point increase in the relative probability of a closing in exposed tracts in the 2 years following the merger. Note that, since the control group includes tracts that have branches from only the buyer or the target along with another large bank, the results in Figure 2 are not driven by a tendency for merged banks to close branches across the board. Online Appendix Figure A.2 confirms this directly by showing the merger has virtually no effect on the incidence of branch closings in buyer- and target-only tracts relative to unexposed tracts (those that did not have branches from either the buyer or the target, but did have branches belonging to two other large banks).[12] This confirms that physical proximity between merging branches matters for determining where closings occur.

Figure 3 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

[12] 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 percent of post-merger closings involve a target branch, while 40 percent involve a buyer branch.

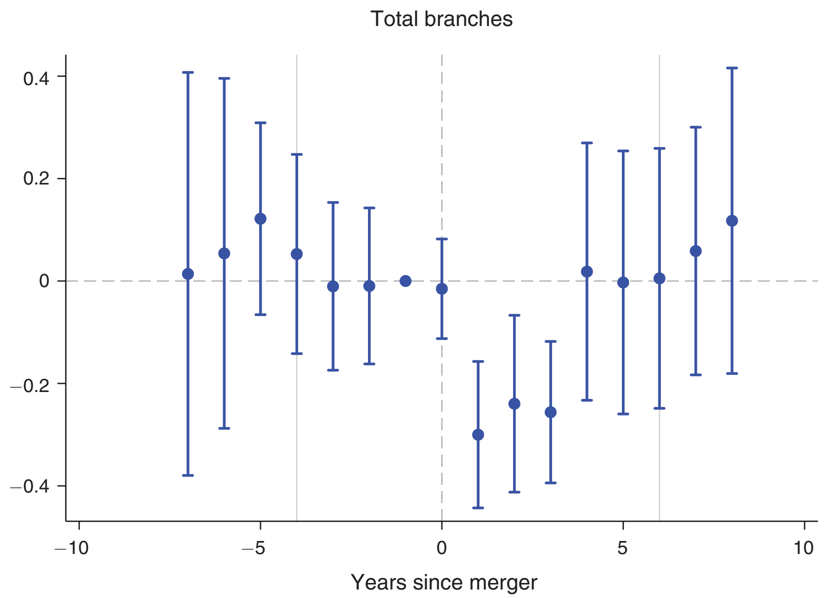

FIGURE 3. EXPOSURE TO CONSOLIDATION AND LOCAL BRANCH LEVELS

Notes: This figure plots the relationship between exposure to consolidation and the total number of branches, obtained from estimating equation (3). The bars show 95 percent 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 county level.

Source: FDIC, author's own calculations

of branches in exposed tracts is again level with control tracts. The corresponding point estimates are shown in column 2 of Table 6. The dependent variable is the total number of branches, but the results are similar when using the total number of banks. These results are consistent with Garmaise and Moskowitz (2006), who find the market structure effects of mergers last approximately three years before other banks enter.[13] This pattern suggests that while it is in the merged bank's best interest to consolidate on its fixed costs by closing an overlapping branch, profits are then high enough to accommodate a new entrant.

B. *Closings and Local Credit Supply*

The previous section showed that exposure to consolidation increases the probability of a branch closing. Do closings, in turn, have an impact on local credit supply? In this section, the dependent variables are drawn from the FFIEC data and

[13] Results not shown here confirm that this pattern is driven by a higher rate of branch openings in exposed tracts, rather than by a higher rate of branch closings in control tracts during the same period.

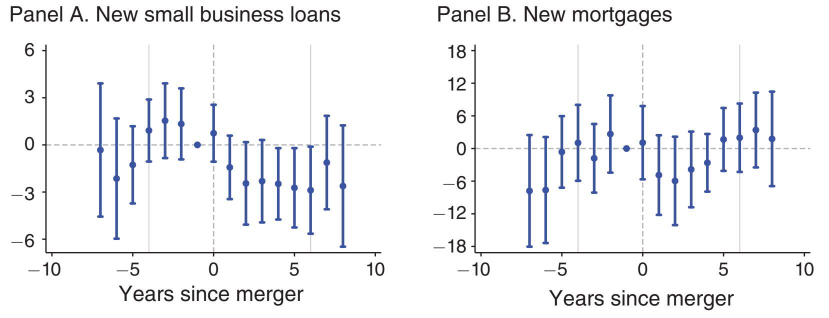

Figure 4. Exposure to Consolidation and the Volume of New Lending

Notes: This figure plots the reduced-form relationship between exposure to consolidation and lending to borrowers located in the same tract, obtained from estimating equation (3). The bars show 95 percent 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 county level.

Source: FFIEC, author's own calculations

measure the volume 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.[14]

Figure 4 shows the reduced-form relationship between exposure to consolidation and the volume of new lending. The left panel shows a large and significant decline in new loans to local small businesses. Relative to controls, exposed tracts experience a decline in small business lending that persists up to six years after the closing. In contrast, the right panel shows very little effect on local mortgage lending; a slight dip coincides with the timing of the branch closing, but none of the year-by-year coefficients are statistically significant.

This comparison suggests closings have a more substantial effect in the small business lending market, but the contrast becomes especially striking when we compare the reduced-form estimates in both markets with the first-stage relationship between exposure to consolidation and the total number of branches. Figure 5 superimposes the reduced-form estimates from Figure 4 over the first-stage coefficients from Figure 3. The right panel shows the decline in mortgage lending is temporary and recovers in line with the number of branches. The left panel, however, shows closings have a much longer-term impact on credit supply to local small businesses. Small business lending declines when a branch closes and remains depressed even

[14] The dependent variable is expressed in levels rather than logs for two reasons. First, the proportional impact of a branch closing will differ substantially across markets depending on local market shares, while the level effects may be more comparable given branch-level capacity constraints in originating loans. Second, log transformations are typically employed to reduce skewness in the dependent variable and normalize its distribution. Online Appendix Figure A.3, however, shows that taking logs in this particular context has the opposite effect; while the distribution of loan levels (net of fixed effects) is close to normally distributed, the distribution in log space is skewed due to several very negative values (1 percent of values lie between −0.67 and −4.06). Consistent with this, online Appendix Table A.4 shows the log results are consistent with the baseline results in levels after the censoring of very small values.

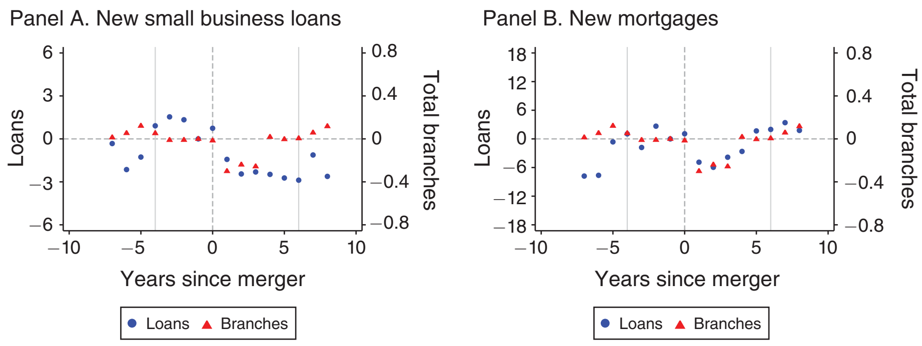

FIGURE 5. THE EFFECT OF SUBSEQUENT BANK ENTRY ON LOCAL CREDIT SUPPLY

Notes: This figure plots the first-stage relationship between exposure to consolidation and number of branches (red triangles) along with the reduced-form relationship between exposure to consolidation and lending (blue circles), both obtained from estimating equation (3). Additionally, $\tau = 0$ 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 county level.

Source: FDIC, FFIEC, author's own calculations

after the entry of new banks.[15] Columns 3 and 4 of Table 6 show the corresponding reduced-form point estimates.

To more easily interpret the magnitude of these effects, Table 7 provides reduced-form and IV estimates from a less flexible version of the DD. I estimate

$$(4) \qquad y_{icmt} = \alpha_i + (\gamma_t \times \sigma_c) + \mathbf{X}_i \beta_t + \delta_{POST}(POST_{mt} \times Close_{icm}) + \epsilon_{icmt},$$

where $POST_{mt}$ is a dummy equal to one if year t occurs after merger m is approved by federal regulators, $Close_{icm}$ is a dummy equal to one if a branch closes in tract i in county c after merger m, and all other variables are as previously defined. The instrument is $Expose_{icm}$: a dummy equal to one if tract i is an exposed tract for merger m. The term δ_{POST} measures the post-closing mean shift in the level of lending.

Column 2 of Table 7 shows the post-closing decline in the number of new loans is mirrored by a decline in the dollar volume of new lending. The point estimates in panel C show closings are associated with an \$871,000 decline in new small business loan originations. Given the baseline mean of roughly \$4.7 million, this amounts to a 19 percent annual decline in the dollar volume of new lending. Over the 6 years following the closing, this amounts to over \$5 million in forgone loans to local small businesses. Columns 3 and 4 show closings have no significant impact on local mortgage lending.[16]

[15] Note that 49 percent of the banks that enter in exposed tracts in the post-merger period are above the CRA asset threshold.

[16] Panels A and C show that OLS underestimates the IV. This may seem surprising given that banks are more likely to close branches in areas that are already trending downward, implying that OLS would overestimate the true effect. However, many closings affect branches that are smaller and relatively less active (hence, the pressure to close), while the merger LATE identifies the impact of closing branches that are much larger on average and where the closing is solely precipitated by the merger itself. Consistent with this, the median buyer/target branch in an exposed tract holds approximately \$35,000 in deposits, while the average closed branch holds only \$18,000

Table 7—IV Estimates of the Effect of Closings on Local Credit Supply

	Small business loans		Mortgages	
	Number of loans (1)	Dollar volume (000s) (2)	Number of loans (3)	Dollar volume (000s) (4)
Panel A. OLS				
δ_{OLS}	−2.143	−100.9	−0.860	−149.9
	(0.745)	(48.64)	(3.388)	(581.4)
Panel B. RF				
δ_{RF}	−2.513	−206.7	−1.263	−169.2
	(0.909)	(77.91)	(2.705)	(522.9)
Panel C. IV				
δ_{IV}	−10.41	−871.4	−5.304	−710.8
	(3.738)	(327.9)	(11.33)	(2,188)
Six-year cumulative effect	−62.47	−5,228	−31.83	−4,265
	(22.43)	(1,967)	(67.96)	(13,129)
Baseline mean	103.4	4,706	277.2	39,202
Observations	45,160	43,033	46,516	46,525

Notes: This table shows OLS, reduced-form, and IV estimates of equation (4). All columns include the full set of baseline controls interacted with year dummies along with tract and county-by-year fixed effects. The baseline mean is calculated for exposed tracts in $\tau = -1$. Robust standard errors are clustered at the county level and are in parentheses.

Source: FFIEC, author's own calculations

Online Appendix Section 1 shows these baseline results are robust to several changes including restricting the size of the local banking market to be within-county areas of 25-, 20-, or 15-mile radii, removing outliers, and accounting for missing data.

Do Borrowers Substitute toward Other Lenders?—Table 7 shows closings lead to a substantial decline in small business lending, but the dependent variable is small business loans extended by banks above the CRA reporting threshold. If borrowers substitute toward non-CRA lenders, namely small community banks and credit unions, only a portion of the 19 percent decline would represent an actual loss in local credit supply.

Gauging the magnitude of this substitution is complicated by the fact that the CRA is the only source of geographically disaggregated information on small business loan originations. In the absence of comparable data for small banks and credit unions, I use an approximation based on call report data, which are reported at the bank level. To generate tract-level measures, I define small business loans to be the sum of "commercial and industrial loans" and "loans secured by nonfarm or nonresidential real estate" whose original amounts are \$1 million or less.[17] I then divide the bank-level totals across all tracts where the institution's branches are located. For banks, each tract's share of an institution's total lending is determined

(deposits are the only measure of size observed at the branch level), and the ratio of relative loan volumes is likely to be skewed even further.

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

by the share of total deposits held by branches in that tract, which is obtained from the FDIC Summary of Deposits. For credit unions, bank totals are divided evenly across all branches.[18] Estimates for the amount of lending done by each bank in a given tract are aggregated together to generate a single tract-level measure of small business loans extended by non-CRA entities. Since quantities in the call report are stocks, while the CRA reports the flow of new loan originations, the magnitude of these results will not be directly comparable to those in Table 7.

Panel A of Table 8 shows reduced-form estimates of equation (4) where the dependent variables are measures of tract-level lending derived from bank and credit union call reports. As the purpose of this exercise is to approximate the extent of substitution between different lenders, I focus on the magnitude of the point estimates rather than on their statistical significance. The first row shows that, consistent with the CRA results, total lending from banks above the CRA reporting threshold declines after the closing. The second row suggests that there is a corresponding increase in lending from smaller banks that absorbs approximately 27 percent of the decline from larger banks. The third row suggests that credit unions further absorb approximately 21 percent of the original decline. Netting these effects from the 19 percent estimate from Table 7 leaves a remaining 10 percent, or \$453,000, decline in lending that is not absorbed by small banks or credit unions. This translates to approximately \$2.7 million in forgone loans over the 6 years following the closing.

An alternative source of credit is home equity (HE) loans. Online Appendix Table A.5, however, shows no evidence of a compensating increase in these loans after a closing. The Small Business Administration also reports that while many small businesses use credit cards extensively, credit card debt accounts for only a small portion of small business financing relative to bank loans and retained earnings.[19] Nevertheless, without tract-level data to measure this substitution explicitly, the \$453,000 decline can be treated as an upper bound for the total loss in credit.

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

Alternative Channels.—Closings lead to a sharp and persistent decline in credit supply to nearby small businesses. Before exploring potential mechanisms, I rule out alternative channels through which the merger might impact lending in exposed tracts.

One possibility is that lending falls because reducing the number of competitors from n to $n - 1$ places upward pressure on prices which, in turn, leads to a decline in borrowing. This is the mechanism highlighted in Garmaise and Moskowitz

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

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

Table 8—Extensions of Baseline Reduced-Form Results

	δ_{RF} (1)	Observations (2)
Baseline	−2.513	45,160
	(0.909)	
Panel A. Call report data		
CRA banks	−3,837	46,985
	(3,140)	
Small banks	1,034	46,985
	(735.0)	
Credit unions	792	15,736
	(271.9)	
Panel B. Target-only tracts		
Target only	−1.035	31,252
	(0.765)	
Panel C. Boom versus bust		
Boom	−1.036	24,095
	(1.251)	
Bust	−6.000	21,065
	(1.572)	
Panel D. Split by tract demographic		
Median income		
Below	−2.787	23,330
	(−1.419)	
Above	−1.419	21,355
	(1.431)	
Percent white		
Below	−3.074	22,597
	(1.289)	
Above	−1.125	21,943
	(1.256)	

Notes: This table shows reduced-form estimates of equation (4) where the dependent variable is annual, tract-level small business loan originations. Unless noted below, all specifications include the full set of baseline controls interacted with year dummies along with tract and county-by-year fixed effects. The first row is the baseline estimate from Table 7. In panel A, the dependent variables are approximations of tract-level small business lending obtained from call report data. These are stock variables rather than flows, as measured in CRA data. See Section IVB for details. Panel B shows estimates where the treated group is tracts that only had branches from the target bank and not the buyer. See Section IVB for details. Panel C shows results separately for boom and bust periods. Baseline control for fraction college educated is omitted. See Section IVB for details. Panel D shows results across subgroups based on median income and fraction of minority households. Baseline controls for median income and fraction minority are omitted. See Section IVB for details. In all specifications, robust standard errors are clustered at the county level and are in parentheses.

Source: Chicago Fed, FDIC, FFIEC, NCUA, author's own calculations

(2006).[20] They find these effects are short-lived, dissipating upon the entry of new banks approximately three years after the merger. The patterns in Figure 5 stand in stark contrast to that result: small business lending does not respond to the entry of new banks, and the decline in lending persists even after the competitive

[20] Scharfstein and Sunderam (2016) also focuses on the effects of merger-induced increases in local concentration. They use county-level variation to show this reduces the sensitivity of local mortgage rates to MBS yields.

environment has returned to its previous equilibrium.[21] This indicates that the direct effects of a change in *tract*-level concentration are empirically negligible, which is not surprising since the identification strategy leverages within-market variation at a very finely disaggregated level. Table 3 also shows that the average exposed tract has six branches prior to the merger. The instrument identifies the effect of closings that occur in very densely banked areas where there is unlikely to be a substantial effect of shifting from n to $n-1$ lenders.

A second possibility is that the decline in lending is driven by institutional changes induced by the merger. Peek and Rosengren (1998) show that buyers tend to recast targets in their own image, which leads to post-merger convergence toward the behavior of the buyer. This has motivated a large literature examining how local lending is affected when small banks are acquired by much larger ones. In the context of this paper, if buyers in my sample use different processes to approve loans or have different lending strategies than the banks they acquire, lending may decline in exposed tracts for reasons completely unrelated to the branch closing.

To evaluate the importance of this channel, I estimate the effect of closings on lending in target-only tracts: i.e., tracts that have branches from the target, but not the buyer. Branches in these areas are affected by any institutional change resulting from the merger, but are not exposed to the greater risk of a post-merger closing. The results in panel B of Table 8 suggest these tracts experience some decline in lending, but the point estimate is not statistically significant and is of a much lower magnitude than the baseline estimate. Consistent with this, Table 9 confirms that there is a large and statistically significant decline in small business lending in exposed tracts even when the control group is redefined to be target-only tracts. This indicates that post-merger institutional changes cannot account for the decline in lending observed in exposed tracts. Moreover, while institutional change may contribute to the initial decline in lending, it is not sufficient for explaining its persistence, which indicates that borrowers find it difficult to substitute toward other lenders even over the long-term.

Interpretation.—It is surprising to find that local branches still play a crucial role in facilitating access to credit given the degree to which technology has transformed the banking sector in the United States. However, there are a number of mechanisms that might explain why distance still matters. The most immediate is that borrowers may be sensitive to travel costs and, once their closest branch is shuttered, are unwilling or unable to incur the cost of traveling to a farther branch. While plausible ex ante, travel costs are unlikely to be important in this particular setting. Recall that the average exposed tract has six branches prior to the merger, indicating that these

[21] Online Appendix Figure A.4 provides direct evidence of this by showing reduced-form estimates of the effect of merger exposure on tract-level small business loan and mortgage interest rates. Both panels show prices remain flat throughout the treatment period. These results should be viewed with some reservation. The data are only available for a portion of the sample period and cover a limited segment of the small business and mortgage markets, respectively. The left panel is derived from microdata on the SBA's 7(a) loan program. Since small business loans issued under this program enjoy a government guarantee and constitute only 1 percent of all small business loans (see Brown and Earle 2017), SBA interest rates may behave differently from interest rates on conventional small business loans. The right panel uses HMDA data, which only report the spread between the APR on a loan and the Treasury rate for loans with spreads above a designated threshold.

Table 9—Using Target-Only Tracts as a Control Group

	Number loans (1)	Volume (000s in dollars) (2)
Panel A. RF		
δ_{RF}	−2.923	−206.6
	(1.207)	(96.39)
Panel B. IV		
δ_{IV}	−11.66	−837.1
	(4.662)	(391.0)
Observations	11,415	10,818

Notes: This table shows reduced-form and IV estimates of equation (4) where the control group is tracts that had branches from the target bank and not the buyer. All columns include the full set of baseline controls interacted with year dummies along with tract and county-by-year fixed effects. The dependent variables are annual, tract-level small business loan originations and the dollar volume of originations, respectively. Robust standard errors are clustered at the county level and are in parentheses.

Source: FFIEC, author's own calculations

closings occur in very crowded markets where there is no meaningful impact on a borrower's ability to access another nearby branch. Moreover, lending remains low despite the entry of new banks.

The pattern of results indicates that geographic proximity is important, but that, from a borrower's perspective, equally proximate branches are not perfect substitutes for one another. This suggests a potential explanation: distance matters because it facilitates the forging of branch-specific relationships that, once disrupted, make it difficult for borrowers to seamlessly switch to another lender. A large literature has studied the role of soft information and relationships in lending; in particular, Drexler and Schoar (2014) provide evidence that severing the relationship between an individual borrower and her loan manager can lead to disruptions in credit access. This is particularly relevant in the context of post-merger consolidation, as the staff at the closed branch are often let go. 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. This interpretation is consistent with the finding that closings are more consequential for information-intensive borrowers. While closings lead to a sharp decline in local small business lending, there is virtually no effect on local mortgage lending. Indeed, the prolonged decline in small business lending displayed in Figure 5—and importantly, its persistence despite the entry of new banks—is consistent with the idea that closings disrupt lending relationships in that market that take time to rebuild.[22]

To probe further, I evaluate whether the consequences of a closing are more severe in periods when the value of a lending relationship is highest. In particular, I study

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

how the impact of a closing depends on the broader lending environment. When banks' funding costs are low and lending standards are relatively loose, as during the credit boom of the early to mid-2000s, borrowers who lose a lending relationship will find it relatively easy to obtain credit from a new lender. This dynamic changes dramatically in times of crisis. As lending standards tighten and banks' risk tolerance falls, lenders will be increasingly unwilling to lend to an unknown borrower for whom they have limited information regarding creditworthiness.[23]

To examine how the impact of a closing varies between boom and bust periods, I separately estimate the effect of closings induced by mergers that were approved in 2003–2004 (boom) and those that were approved in 2006–2007 (bust). Figure 2 shows closings are concentrated in the first two years following the merger, so those corresponding to the 2006–2007 mergers coincide with the beginning of the financial crisis.[24] Panel C of Table 8 confirms that the negative impacts of closings are much more pronounced during the bust. In fact, the point estimate for the boom period is negative but statistically insignificant.[25]

One might be concerned that the results in panel C are driven by the fact that, even absent any closings, exposed tracts may have been more heavily affected by the aggregate boom-bust cycle than control tracts. Online Appendix Figure A.9 provides several pieces of evidence to refute this claim. First, the top left panel shows that small business lending was exactly even in exposed and control tracts in the bust sample for the four years prior to the merger, indicating that exposed tracts were not booming relative to controls. Second, the bottom panel shows no reduced-form effects on mortgage lending, which argues against the small business results being driven by differential exposure to the crisis. Third, the right panel shows the small business lending results are robust to dropping counties that experienced the largest downturns during the Great Recession.

The value of a relationship also varies across demographic groups. In particular, several papers have documented that low-income and minority borrowers are especially reliant on relationship-intensive lending.[26] Panel D of Table 8 shows that the post-closing declines in lending are more severe in tracts with lower median income and a higher fraction of minority households.[27] Even more striking, Figure 6

[23] Beck et al. (2018) provides empirical evidence that the value of lending relationships varies over the business cycle.

[24] In online Appendix Section 2, I separately examine the impact of mergers that occurred during the financial crisis (i.e., post-2007) as these differ substantially from the precrisis mergers.

[25] Estimating equation (4) with the full set of baseline controls interacted with year dummies along with tract and county-by-year fixed effects becomes infeasible once I start taking subsamples of the data. So the results in panel C of Table 8 exclude the baseline controls for median income and fraction minority, while the results in panel D exclude the baseline control for fraction college educated. The baseline results are robust to excluding these controls, and so their omission is unlikely to drive the patterns seen in Table 8.

[26] Muñoz and Butcher (2013) shows 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) provides evidence that borrowers in low-income and minority neighborhoods rely more heavily on informal sources of credit, and posit this may be because informal lenders have cheaper access to relevant information about borrowers within the same community. These issues are not particular to the United States and resonate throughout the literature on barriers to credit in developing countries. Banerjee and Duflo (2010) provides a broad overview of the development literature on this topic.

[27] This is true despite the fact that the correlation between the number of branches and tract-level median income and percent white is extremely low (only 0.0171 and 0.1035, respectively) in this sample. Moreover, the baseline level of lending is actually lower in low-income and minority tracts. Conditional on having a branch close,

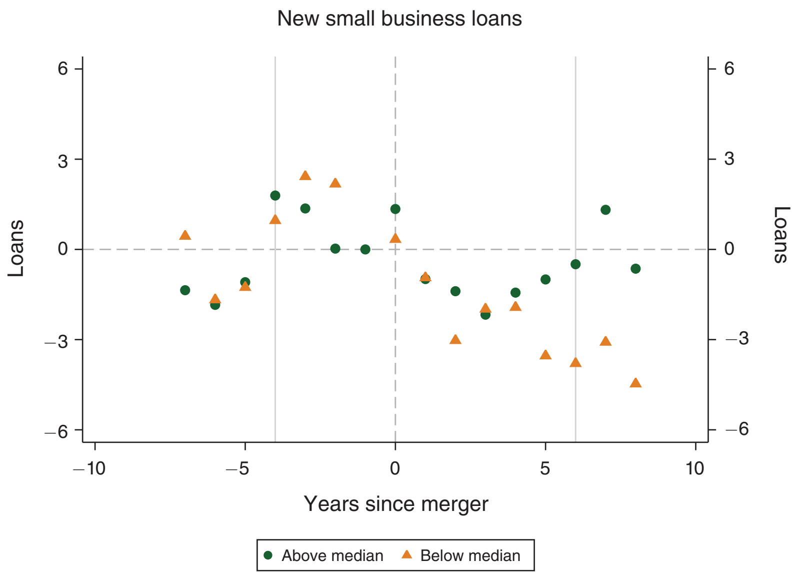

Figure 6. Differential Effect of Branch Closings, by Tract Income Level

Notes: This figure plots the reduced-form relationship between exposure to consolidation and small business loan originations for tracts whose median income is above the sample median (green circles) and for those whose median income is below the sample median (gold triangles), obtained from estimating equation (3). Additionally, $\tau = 0$ 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.

Source: FFIEC, author's own calculations

shows the persistence of the decline in lending is driven by tracts with below median income levels. It appears that local relationships, once broken, are especially difficult to rebuild for borrowers in marginalized neighborhoods.[28]

These results suggest that geographic proximity between a borrower and her lender matters because it facilitates the forging of lendings relationships that, once disrupted, make it difficult for borrowers to substitute toward other sources of credit.

C. *Geographic Spillovers*

The baseline estimates show closings restrict credit supply to small businesses located in the same tract, but surrounding areas are likely to be affected as well—recall, the median tract in this sample is only 1.5 miles. 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

these neighborhoods not only face a larger absolute decline in lending, they actually suffer a larger proportional hit to credit supply. Note that these results cannot be attributed to differences in risk management between buyer and target banks. Such institutional changes are ruled out by panel B of Table 8, as discussed in the section on alternative channels.

[28] Mortgage results by income and minority status are discussed in online Appendix Section 3.

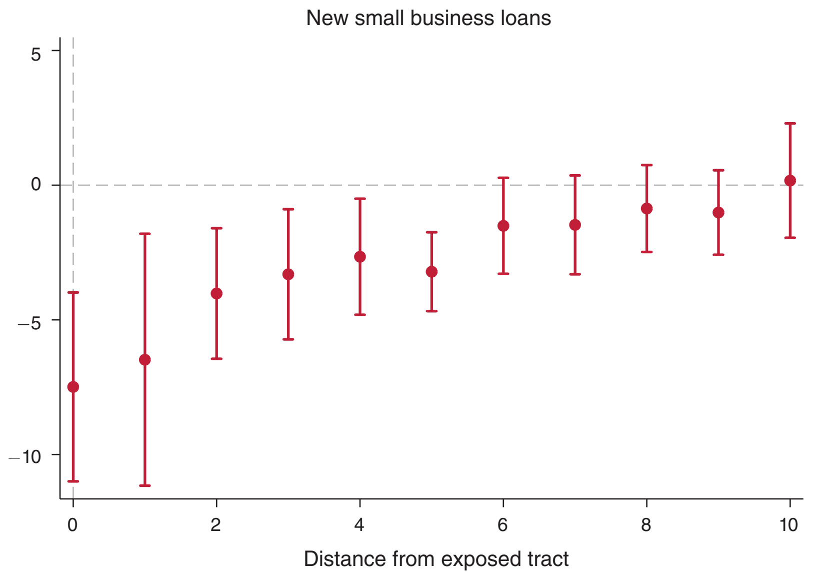

Figure 7. The Geographic Spillover of Bank Branch Closings

Notes: This figure shows how the reduced-form estimates for the post-merger decline in small business lending varies across tracts sorted according to their distance from the exposed tract. The control group is tracts located at least ten miles away from an exposed tract. See Section IVC for more detail. The bars show 95 percent confidence intervals. Robust standard errors are clustered at the county level.

Source: FFIEC, author's own calculations

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 (4) where the dependent variable is small business loan originations, R^x is the "exposed" group, and the control group consists of all tracts located in the same county but at least ten miles away from the branch closing. Here, 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_{RF} 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 six miles from the exposed tract. This pattern is remarkably consistent, both qualitatively and quantitatively, with existing evidence on the local nature of small business lending markets. Amel and Brevoort (2005) and Brevoort, Holmes, and Wolken (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.

D. *Real Economic Effects*

Finally, I investigate the extent to which the decline in small business lending has real economic effects. I use establishment-level data from NETS to construct tract-level measures of annual establishment and employment growth rates, defined below:

$$EstGr_{it} \;=\; \frac{est_{it}^{n} - est_{it}^{c}}{0.5 \times est_{i,t-1} + 0.5 \times est_{it}},$$

$$EmpGr_{it} \;=\; \frac{emp_{it}^{n} - emp_{it}^{c} + \Delta emp_{it} + in_{it} - out_{it}}{0.5 \times emp_{i,t-1} + 0.5 \times emp_{it}},$$

where est_{it}^{n} is the number of new establishments in tract i in year t, est_{it}^{c} is the number of closing establishments, $est_{i,t-1}$ is the number of establishments in period $t-1$, and est_{it} is the number of establishments in period t. Correspondingly, emp_{it}^{n} is the number of jobs created by new establishments, emp_{it}^{c} is the number of jobs lost due to closing establishments, Δemp_{it} is the change in employment at continuing establishments between years $t-1$ and t, in_{it} is in-migration, out_{it} is out-migration, $emp_{i,t-1}$ is employment in year $t-1$, and emp_{it} is employment in year t.

Results from estimating IV specifications of equation (4) are presented in Table 10. Panel A shows closings have no significant impact on the establishment growth rate and no differential impact on rates of firm entry and exit. Panel B, however, shows closings lead to a 2 percentage point reduction in the employment growth rate. Breaking this out into contributions from entering, existing, and exiting firms reveals that the decline is driven by lower employment growth rates at entering firms. This suggests that, while rates of firm entry are unchanged in exposed tracts relative to controls after a closing, the decline in available credit restricts the size of entering firms. Note that new business owners are likely to be particularly reliant on existing relationships as they lack the track record and data that existing firms can point to. The remaining rows in panel B also show that the decline in employment growth rates is concentrated in capital-intensive industries and, more weakly, amongst small standalones (single-unit establishments with fewer than 20 employees) and private establishments.[29] This is consistent with the effects of closings being most severe on information-intensive borrowers, for whom distance is more important.

V. Conclusion

Does geography still matter in banking? This paper finds that it does. I show that merger-induced branch closings have large effects on credit supply to local small businesses. Annual small business loan originations decline by $453,000 after a closing, off a baseline of $4.7 million. Over the six years following a closing, this amounts to $2.7 million in forgone loans. The impact is especially severe in

[29] Capital-intensive industries are defined as in Rajan and Zingales (1998).

Table 10—IV Estimates of the Effect of Branch Closings on Employment

	All establishments (1)	Subgroups (2)
Panel A. Establishment growth		
Total	−0.009	
	(0.008)	
Entering		−0.001
		(0.004)
Exiting		0.005
		(0.004)
Panel B. Employment growth		
Total	−0.0221	
	(0.0099)	
Entering		−0.0119
		(0.0045)
Existing		−0.0026
		(0.0064)
Exiting		0.0013
		(0.0043)
Capital intensive		
Yes		−0.0321
		(0.0145)
No		−0.0146
		(0.0132)
Small standalone		
Yes		−0.0183
		(0.0097)
No		−0.0177
		(0.0131)
Private		−0.0215
		(0.00991)
Public		−0.0210
		(0.0236)
Observations	44,434	

Notes: This table shows IV estimates of equation (4). In panel A, the dependent variable is annual tract-level establishment growth as defined in Section IVD. In panel B, it is annual employment growth. Column 1 uses growth rates calculated over all establishments. Column 2 uses growth rates calculated over subgroups within each tract. All specifications include the full set of baseline controls interacted with year dummies along with tract and county-by-year fixed effects. Robust standard errors are clustered at the county level and are in parentheses.

Source: NETS, author's own calculations

contractionary periods when lending standards tighten and the value of a relationship is highest. Ultimately, the decline in local credit supply leads to a 2 percentage point reduction in employment growth rates, primarily driven by tighter constraints on the size of entering firms.

The local effects of bank mergers and branch closings are heavily scrutinized, but existing regulation aims at policing closings that have the potential to create banking deserts. This focus on accessibility ignores a key element that separates banking and credit provision from other services: the importance of relationships. In lending, distance matters not only because it improves accessibility, but also because it reduces the costs of transmitting information. Closings can have large effects on local credit supply, even in dense banking markets, if they disrupt lender-specific relationships that are difficult to replace.

It is striking to find that geographic proximity is still important for credit allocation in the United States. The banking sector has undergone a series of vast changes over the last several decades; amongst these are innovations in information technology, credit scoring, and online and mobile banking, all of which have inexorably encroached on the role of local branches. Yet, even in the 2000s, we find that the benefits of those changes have been neither absolute nor evenly distributed: there are some markets and some segments of the population for whom local credit markets still play a crucial role in facilitating access to credit and financial services. These results resonate with the conclusion of Glaeser (1998) that technology is not an adequate substitute for all forms of interaction and show that distance can still constitute a meaningful impediment to the transfer of information.

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