Concentration and Geographic Proximity in Antitrust Policy: Evidence from Bank Mergers

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Antitrust often uses the Herfindahl-Hirschman Index (HHI) for merger screening and review. We argue that HHI-based antitrust policy using predefined markets in the banking industry misses anticompetitive effects that are predicted by the proximity of merging branch networks. Difference-in-differences estimates from thousands of mergers reveal that close-proximity bank acquisitions have harmful effects, including branch closures, even if they fall below the HHI threshold for enforcement. Neither lowering the threshold nor using narrower predefined markets addresses this under-enforcement without introducing significant over- and under-enforcement of other transactions. However, using a proximity threshold to complement the HHI policy could improve bank antitrust.
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Antitrust investigations often use market concentration as evidence that a merger is anticompetitive, typically as measured by thresholds of the Herfindahl-Hirschman Index (HHI). The merits of this practice are debated from both sides. Recent reform proposals call for broader, systematic use of such concentration "screens" in order to enforce a growing volume of transactions (Warren and Jones, 2022). At the same time, competition authorities question whether existing guidelines accurately identify circumstances that warrant enforcement (FTC and DOJ, 2022). Addressing this debate, a growing literature has examined antitrust laxity both theoretically and empirically (Nocke and Whinston, 2022; Bhattacharya, Illanes and Stillerman, 2023; Prager and Schmitt, 2021; Miller and Weinberg, 2017; Scott Morton, 2019). However, there is still little direct evidence on the vulnerabilities of HHI-based enforcement in practice.

This paper contributes empirical analysis of HHI-based policies from bank merger retrospectives. Banking is an important industry with an unusually high merger volume. There were more than 10,000 bank mergers over the past four decades in the U.S., about 250 every year, affecting financial assets and deposits worth trillions of dollars. The impact of bank mergers on financial market structure has significant implications for the broader economy, as well.[1] Every proposed bank merger is reviewed by antitrust authorities, the Department of Justice (DOJ) and the Federal Reserve (Fed). They use a screening policy that relies heavily on HHI thresholds and markets that are predefined (i.e. not merger-specific). Banking is also a data-rich setting, with information on

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[1]For example, bank mergers have been shown to affect monetary policy transmission (Drechsler, Savov and Schnabl, 2017), economic growth (Jayaratne and Strahan, 1996), firm entry and exit (Fraisse, Hombert and Lé, 2018), asset prices (Favara and Imbs, 2015), bankruptcy (Dick and Lehnert, 2010), and crime (Garmaise and Moskowitz, 2006).

other determinants of merger outcomes that may predict antitrust enforcement opportunities missed by the policy-relevant HHIs. These unique features present a laboratory to test the performance of concentration screens across thousands of merger events.

We specifically examine whether HHI-based policy overlooks harm that can be identified with supplemental information about geographic competition between merging banks. Geographic proximity is an important and easily measurable determinant of bank substitutability. In the Survey of Consumer Finances, 40% of respondents cited location as the most important reason they chose their bank, while only 12% of respondents chose their bank based on prices (Benson, Grundl and Windle, 2020). Consumers are therefore more likely to view banks with branches near each other as close substitutes. This reasoning suggests that the proximity of merging branch networks might convey information on the effects of bank mergers beyond what is captured by the HHI.

Using a difference-in-differences (DID) strategy, we find that HHI-based policy with predefined markets neglects important effects that are empirically determined by the proximity of merging branch networks. In sum, close-proximity bank mergers cause branch closures, cause rival banks to offer lower interest rates on deposits, and reduce the growth of deposits of the merging banks. These harms of close-proximity mergers are evident after HHI-based enforcement, and likely have significant effects on welfare. Furthermore, results do not indicate that far-proximity mergers that triggered the concentration screen are benign. Our findings suggest that HHI-based policy in the banking industry was more susceptible to under-enforcement than to over-enforcement. We argue that bank antitrust policy might be improved by complementing concentration-based enforcement with systematic criteria on other observable determinants of merger outcomes like branch proximity.

We present the formal empirical strategy in Section III. Market structure is a determinant of the outcomes we study. A merger results in a discrete change in the structure of overlap markets, where both merging banks operate before the merger, but does not directly change the structure of non-overlap markets.

This naturally leads to a within-merger DID estimator, which identifies the effects of a merger by comparing outcomes between the merging banks' overlap and non-overlap markets. Our regression specification allows for four binned DIDs, conditioned on whether the local proximity of the merging banks is less than 4 miles and on whether the HHI increases more than 200 points, the regulatory threshold for enforcement in the banking industry. Estimating heterogeneous effects in this way enables us to test whether close-proximity mergers have adverse effects, even when the transaction was subject to enforcement based on the HHI. We estimate the heterogeneous effects of mergers on three product attributes: the number of branches, consumer distances to branches, and deposit interest rates (prices). We also estimate effects on deposits growth (quantities) to provide evidence on how price effects and non-price effects impacted consumer welfare on net.

We present the main results in Section IV, beginning with evidence on geographic competition from branch closures. We find that close-proximity mergers cause significantly more branch closures than far-proximity mergers, and that this effect does not vary much with the HHI. After close-proximity transactions, merging banks are 40 percentage points (pp) more likely to close branches compared to non-overlap markets where they operate, and market total branches are 17 pp more likely to decline.

We also find that close- and far-proximity mergers have different effects on the distances between consumers and their banking options. Every bank merger leads to a larger branch network by combining two separate smaller networks, and this convenience synergy reduces many consumers' distances to the merged bank. The convenience synergy of close-proximity mergers is less than half that of far-proximity mergers, on average. Close-proximity mergers also increase the distance to branches for at least 15% of consumers, whereas far-proximity mergers do not. Structural estimates of demand from the literature suggest that these non-price effects significantly impact consumer welfare. The model of Ho and Ishii (2011), for instance, suggests that the value impact of these merger-induced branch closures is equivalent to a 35% price increase for the 15% of consumers who must travel farther to retail banking services.

We find that close-proximity mergers do not significantly affect the deposit interest rates of the acquirer bank, whether or not the HHI screen is triggered. In stark contrast, we find that close-proximity mergers cause rival banks to significantly lower their deposit rates, harming consumers. The effects of mergers on competitors' prices are especially pronounced for close-proximity mergers subjected to HHI-based enforcement. This suggests that even if mergers are closely investigated, enforcement aimed primarily at reducing the HHI potentially neglects anticompetitive effects that could be identified with information on bank proximity.

Finally, we find that close-proximity mergers cause quantity to decline. Within two years of the merger, the merged bank's deposit growth falls over 20 percent relative to growth of the same bank in its non-overlap markets. Based on revealed preference, we interpret our results for deposits quantity as evidence that the price effects and non-price effects of close-proximity mergers harmed consumers, on net, whether or not the merger was subject to HHI-based enforcement.

Section V discusses implications of our results for antitrust policy. To quantify under-enforcement, we tabulate how many transactions evaded HHI-based enforcement that, in retrospect, could be presumed harmful based on geographic proximity. Over 1984-2014, HHI-based policy effectively gave safe harbor to 84% of 4-mile proximity banking acquisitions. We examine alternative policies that are similarly resource- or information-constrained to use predefined markets and HHI thresholds. Neither reducing the threshold nor using narrower market definitions substantially addresses under-enforcement without introducing significant over- and under-enforcement of other transactions. In contrast, we find that supplemental criteria on geographic proximity might improve bank antitrust policy, coming closer to the ideal of information-rich merger-specific analysis. Complementing the concentration threshold with a 1-mile proximity threshold would have increased the volume of likely harmful mergers receiving in-depth investigation by 123%, and may have levied additional remedies (e.g. divestitures) on 54% of mergers that exceeded the HHI threshold.

While our results are specific to the banking industry, there is potentially broader policy relevance for other industries, especially in light of recent reform proposals.[2] For example, complementing the HHI with distance criteria might also improve antitrust enforcement in settings where the proximity of firms' establishments is known to be an important determinant of the competition, such as hospitals (Dafny, 2009), gasoline retailers (Houde, 2012), chain restaurants (Thomadsen, 2005), movie theaters (Davis, 2006), and grocery stores (Ellickson, Grieco and Khvastunov, 2020). More generally, competition authorities might consider criteria on other industry-specific determinants of merger outcomes to complement the HHI thresholds.

Our findings are most closely related to other merger retrospective studies that speak to antitrust laxity, including (Prager and Schmitt, 2021; Miller and Weinberg, 2017), and notably Bhattacharya, Illanes and Stillerman (2023) who examine evidence from a large sample of retail-product mergers. Our analysis is also related to merger retrospectives that highlight non-price effects, including firms' positions in the product space (Atalay et al., forthcoming; Sweeting, 2010) and measurable synergies (Demirer and Karaduman, 2023). A broader industrial organization literature has studied antitrust based on size and the HHI (Nocke and Whinston, 2022; Scott Morton, 2019; Wollmann, 2019, 2021; Nocke and Schutz, 2018; Froeb and Werden, 1998). We contribute causal evidence on the performance of concentration screens in practice, from a data-rich setting where every transaction is subjected to concentration screens, with unique evidence on the potential determinants of

[2]A recent U.S. presidential executive order promoting competition calls the agencies to "review current practices and adopt a plan ... for the revitalization of merger oversight under the Bank Merger Act and the Bank Holding Company Act" (Biden, 2021). The "Prohibiting Anticompetitive Mergers Act" before the U.S. Senate gives antitrust agencies authority to block transactions, without litigation, if they exceed thresholds in the 1992 merger guidelines (Warren and Jones, 2022). Antitrust agencies also recently solicited public comment on whether current guidelines "adequately identify mergers that are presumptively unlawful," "reflect current understanding of the characteristics of mergers that prove to be anticompetitive," and whether there are other "factors that could be applied in such screens" and what "would be their accuracy and predictive power relative to the quantitative factors in the guidelines?" (FTC and DOJ, 2022)

enforcement error should such policies be adopted on a wider scale.

Our paper is also related to literatures that study the effects of bank mergers, especially papers focused on prices (Focarelli and Panetta, 2003; Prager and Hannan, 1998; Liebersohn, 2024; Erel, 2011; Joaquim, van Doornik and Renato Haas Ornelas, 2020; Allen, Clark and Houde, 2014) and branch network outcomes (Nguyen, 2019; Akkus, Cookson and Hortacsu, 2015; Levine, Lin and Wang, 2019). We contribute estimates of heterogeneous effects that depend on pre-merger characteristics like geographic proximity and market concentration, and offer separate results for merging parties as well as their rivals. We also bring analysis on how mergers affect price and non-price outcomes in concert, with a novel finding that that non-price effects of mergers may have bigger impacts on consumer welfare than price effects in the banking industry.

I. Institutional Detail

Although antitrust for banking is overall similar to other industries, banking is unique in a few important ways. To begin, the Fed and the DOJ review every bank merger for potential anticompetitive effects, typically 200 to 300 annual transactions. The banking industry annually accounts for about 10% of all transactions reported to the DOJ or the Federal Trade Commission for premerger review. However, unlike most industries, premerger review of bank acquisitions does not depend on Hart-Scott-Rodino size thresholds. Instead, each bank transaction is reviewed, involving antitrust market definitions and estimates of competitive effects.

Due to judicial standards, as in most other industries, the bank merger review process puts considerable emphasis on market definition and the HHI. However, bank antitrust is unique because judicial precedent also puts considerable emphasis on a particular product market.[3] Litigation has focused on the "cluster" of banking products and services typically supplied at branch locations. Consumers of products in the retail banking cluster are typically households and small businesses, as opposed to larger corporations or wealthy individuals. Geographic markets for the retail banking cluster are therefore narrower than most other relevant product markets, and compared to other potential antitrust markets impacted by a bank merger, the transaction is more likely to have competitive effects within the cluster.

In part to accommodate the large number of annual transactions, the Fed predefines geographic markets ("Fed markets") for the retail banking cluster based on any available data on consumer substitution patterns, including commuting flows and geographic barriers to local economic integration.[4] These policy relevant markets do not change very often, even though they are regularly reviewed. The DOJ in practice often uses similar, if not the very same, markets in their initial competition screen. There are detailed public data on bank deposits for every predefined market, and deposits proxy for a bank's volume across the broader retail product cluster.

For these reasons, the Fed and DOJ prioritize bank mergers for enforcement based on local deposits market shares and HHIs. If the deposits HHI increases less than 200 points or if the pro-forma HHI level is below 1800, and if the pro-forma market share of the merged bank is less than 35 percent, a merger is almost always approved by the Fed without being subject to enforcement action like divestitures. Although the DOJ has no safe harbor for bank mergers and will sometimes decompose the "cluster" into local small business lending and deposits when data are available, its bank merger investigations still place considerable emphasis on HHI thresholds. Transactions that exceed the agencies' thresholds are investigated closely, including analysis of market definitions and scrutiny for competitive effects across the merging banks' lines of business, and are much more

[3]Three Supreme Court cases primarily establish these standards: United States v. Philadelphia National Bank, 374 U.S. 321, 356 (1963); United States v. Connecticut National Bank, 418 U.S. 656 (1974); and United States v. Phillipsburg National Bank & Trust Co., 399 U.S. 350 (1970).

[4]The Fed views the available data through the lens of the hypothetical monopolist test. There were 1,518 Fed markets in 2014, ranging in geographic scope from larger markets comparable to MSAs to smaller markets comparable to counties.

likely to be subject to remedies like divestitures. The HHI factors heavily into enforcement, as in other industries, because the "courts have regularly relied on the thresholds articulated in the 2010 HMG in finding a presumption that a merger is anticompetitive" (Peters and Wilder, 2021).

One might question whether antitrust markets based on local geographic competition still reflect consumer demand today. For example, internet banking could make geographic competition less important. However, survey evidence suggests that many consumers still choose their main banking institution based on location (Benson, Grundl and Windle, 2020; Anenberg et al., 2018; Honka, Hortaçsu and Vitorino, 2017). In the Survey of Consumer Finances, the share of respondents using online banking increased from about 30% in 2004 to 60% in 2016 (Benson, Grundl and Windle, 2020). Over the same period, the share of respondents who cite location as the primary determinant of their bank choice was stable at around 40%, and the share of respondents who made bank choices primarily based on prices (e.g., interest rates and fees) was much lower at around 12%. Structural models of demand for banking in the literature also suggest strong willingness-to-pay either for branches or for reduced distance to branch networks (Ho and Ishii, 2011; Honka, Hortaçsu and Vitorino, 2017; Dick, 2007; Adams, Brevoort and Kiser, 2007; Kuehn, 2018, 2020; Egan, Lewellen and Sunderam, 2021; Xiao et al., 2022). Because of consumer preferences for convenience, geographic competition remains important in the banking industry despite the evolution of competition over the time window we study.

These institutional features have several advantages for studying vulnerabilities of HHI-based policy. The policy-relevant market definitions and market shares are observed for all banks over the entire sample period. Every bank merger over the sample period is observed and, because the agencies' review criteria are nearly algorithmic, we can reliably identify mergers that evaded enforcement based on the HHI. The importance of geographic competition suggests that the proximity of branch networks is likely a determinant of competitive harm from bank mergers, whether or not the policy-relevant HHI triggers enforcement. The same dataset measuring local deposits provides the exact location of every bank branch, so we can measure the evolution of every banks' proximity to rival banks as well as convenience for consumers. Finally, the vast number of bank mergers in our data provides a rich source of variation across markets and merger events in the determinants of policy action (HHI) as well as the policy's vulnerability (proximity).

II. Data and Descriptive Statistics

We use the FDIC's Summary of Deposits (SOD) and the Federal Reserve's National Information Center (NIC) database to take measurements of bank branch networks and deposits and to identify merger events (see Appendix A.5). The SOD is an annual census of U.S. branches of depository institutions, reporting ownership, the amount of deposits, street address, and latitude/longitude coordinates. Prices for deposit products come from RateWatch (S&P Global, 2021). Population and location data are from the 2000 U.S. Census. Our resulting panel spans 1980-2014, in which we study mergers occurring from 1984-2010.

A. *Measuring Bank Proximity*

We construct a measure of bank proximity based on information from the SOD on latitude/longitude coordinates for each branch. Our measure captures geographic substitutability by featuring the distance between a given branch and a rival's closest branch. Formally, let I and J be two branch networks, and let the number of branches in network I be N_I. For each branch $i \in I$, we compute the distance δ_{iJ} from i to the closest branch of J. The average of δ_{iJ} across I's branches measures the distance from network I to network J. For each branch $j \in J$, we compute the analogous distances and average for network J in relation to network I. We call the minimum of these average distances the network distance between I and J.

$$d(I,J) = \min\left\{ \frac{1}{N_I}\sum_{i\in I}\delta_{iJ}, \frac{1}{N_J}\sum_{j\in J}\delta_{jI} \right\} \tag{1}$$

Taking the minimum of the averages ensures that our proximity measure is symmetric $d(I,J) = d(J,I)$, robust to outlier distances, and considers two networks with significant spatial intersection as close proximity.

We classify merger-market observations as being either "close" or "far" based on the network distance between the acquirer and the target the year before their merger. In our main analysis, close-proximity is below-median network distance (approximately 4 miles) across all merger-market pairs.

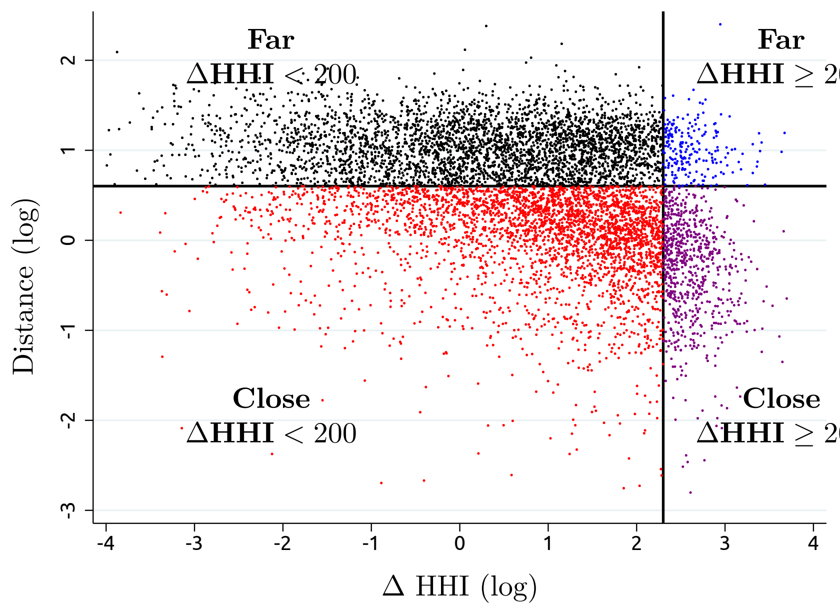


Figure 1: Joint Distribution of ΔHHI and Acquirer-Target Distance

Note: Log (base-10) distance between acquirer and target plotted against log (base-10) change in HHI, both measured the year before the merger. The sample is merger×market pairs from 1984-2014 in which both the acquirer and the target had at least one branch in the year before the merger. Reference lines are for median acquirer-target proximity (about 4 miles) and ΔHHI= 200.

Figure 1 displays the joint distribution between ΔHHI and acquirer-target proximity across merger×market pairs in the year before the merger. The figure shows that there is substantial variation in proximity between the acquirer and target even among mergers with similar changes in HHI. In particular, many close-proximity mergers have ΔHHI< 200. These mergers involve banks that are potentially close substitutes but have safe-harbor under the HHI-based policy.

B. *Summary Statistics*

Table 1 compares sample means of market×merger characteristics across our treatment and control groups. Treatment groups are overlap markets, Fed markets where both the acquirer and the target had at least one branch in the year before the merger. Control groups are composed of the merging parties' non-overlap markets, markets where either the acquirer or the target, but not both, had at least one branch in the year before the merger.

We classify overlap markets into four groups depending on acquirer-target proximity and ΔHHI. The top two rows of Table 1 are the variables by which we group markets, which explains the sharp divergence between their means. The distance between acquirer and target branch networks for far-proximity mergers is over ten times larger than for close-proximity mergers. Similarly, high ΔHHI mergers have an average ΔHHI nearly ten times larger than that of low ΔHHI mergers.

Table 1—: Comparison of Treatment and Control Groups

	(1) Close & ΔHHI≥200	(2) Close & ΔHHI<200	(3) Far & ΔHHI≥200	(4) Far & ΔHHI<200	(5) Non-overlap markets
Acq-Tar Distance (miles)	0.7	1.2	13.3	14.9	.
Δ HHI	594.3	65.0	603.2	48.9	0.0
HHI	1,970.0	1,533.8	2,042.9	1,376.9	2,013.7
Merged Entity Share (%)	36.1	14.4	37.0	11.1	17.0
Market Bank Count	10.5	17.8	9.0	18.3	11.3
Market Branch Count	34.0	77.6	18.7	66.0	33.3
Merged Entity Branches	8.8	9.7	4.9	5.3	3.5
Distance to Merged Entity	4.9	5.5	5.6	9.2	7.7
N. Obs.	405	651	154	731	32,076

There are differences in average market characteristics across groups, as well. For example, because high ΔHHI mergers are more likely to occur in smaller and more concentrated banking markets, we find on average that markets treated by low ΔHHI mergers have nearly twice as many banks and branches as markets treated by high ΔHHI mergers. We also see differences in means between close- and far-proximity groups, particularly for measures of the merged bank.[5] The average number of merged bank branches in close mergers is about twice as large as the average in far mergers, in part because large networks are more likely to have branches located near other banks. We acknowledge these differences, and give further discussion of imbalance and threats to identification in Section III.

C. *Raw Means of Branch Closures*

Among the various merger outcomes, branch creation and destruction are perhaps most naturally impacted by the proximity of merging branch networks. Figure 2 plots raw means for close and far mergers for the year-over-year change in branches.

Figure 2(a) shows raw means for the change in the number of branches in the merged network. For close-proximity markets, the merging banks add about two branches per year before the merger, but remove branches soon after the merger. Raw means for far-proximity markets also decline after the merger, but by a smaller magnitude and are never negative. The average far-proximity overlap market sees slightly more than one branch added per year to the network before the merger, and sees slightly less than one branch added per year after the merger.

Figure 2(b) shows raw means for the year-over-year change in the total number of branches in the market. There is no visible effect on the number of branches in the market for far-proximity overlap markets or for non-overlap markets. In contrast, there is a sizable dip in total branches

[5]Throughout this paper, measures of the merged bank are defined by the counterfactual combination of the acquirer and target before the merger and defined by the actual merged entity after consummation of the acquisition.

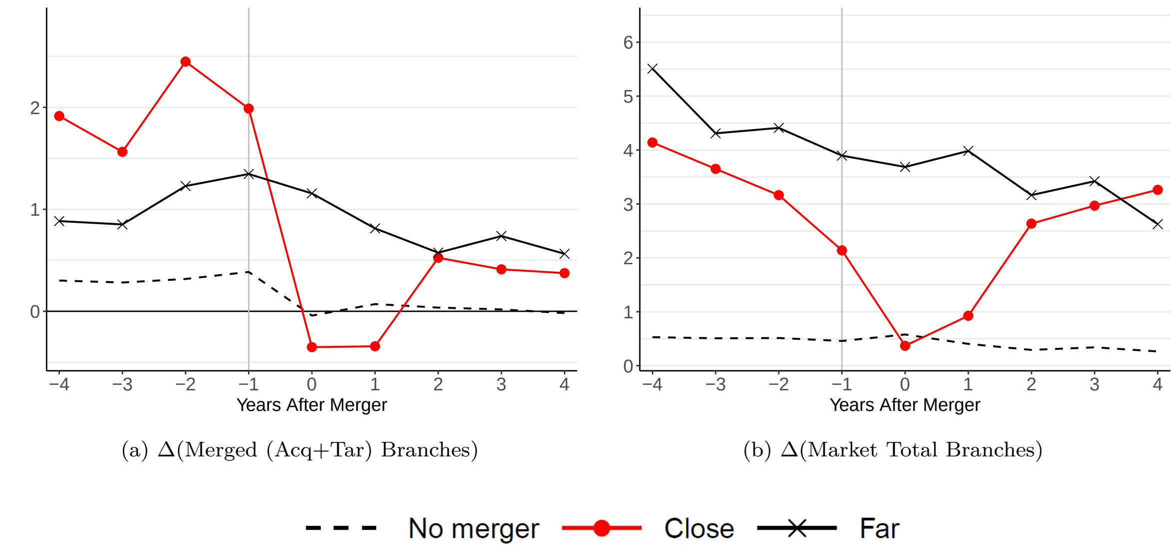


Figure 2: "Raw DID" for Branch Openings and Closures

Note: These graphs show raw means for markets where the acquirer and target overlapped in close-proximity (red - solid line), markets where the acquirer and target overlapped but at far-proximity (black - solid line), and markets where the acquirer and target operated branches but did not overlap (black dashed line). Close/far-proximity is defined by below/above the median distance across all merging parties the year before their merger.

after close mergers. Raw average market total branch creation remains below the pre-merger level for two years after close-proximity mergers but in later years reverts back to its pre-merger level. Since the reduction in market total branches is more muted than branch closures by the merging banks, there is evidence in the raw means that rival banks respond to mergers by opening branches.

These raw means suggest that bank merger effects depend on proximity between branch networks. We examine this more precisely with regressions.

III. Empirical Strategy

We use a DID strategy to identify the effects of mergers, leveraging variation across the vast number of merger×market pairs in our dataset to estimate treatment effects conditional on acquirer-target proximity and ΔHHI. Since the outcomes that we study are measured at the geographic banking market (Fed market) level, our design is based on variation over time between overlap markets and non-overlap markets involved in each merger event. Overlap markets, the treatment group, are Fed markets where both the acquirer and the target operated in the year immediately prior to the merger. Non-overlap markets, the control group, are Fed markets where only one of the merging banks operated a branch.[6]

To implement this identification strategy, we adopt a stacked-event panel regression design.[7] The stacked design allows us to define merger-specific control groups, to study merger-specific outcome variables such as the combined market share of the merging parties, and to leverage variation in

[6]The control group does not include markets unaffiliated with the merger event, where neither the acquirer nor the target operated, because many of our outcomes are only measurable where either the acquirer or target exists.

[7]For each treatment event we build separate balanced panel datasets and append or "stack" them together, as in e.g. (Cengiz et al., 2019; Lafortune, Rothstein and Schanzenbach, 2018; Deshpande and Li, 2019).

DIDs across events. The formal regression specification is

$$
\begin{aligned}
y_{emt} = \sum_{\tau=-4}^{4} \mathbf{1}[t = t_e^* + \tau] \times \Big(& \beta_\tau^{fl} \times Far_{em} \times \mathbf{1}\{\Delta\text{HHI}_{em} < 200\} \\
& + \beta_\tau^{fh} \times Far_{em} \times \mathbf{1}\{\Delta\text{HHI}_{em} \geq 200\} \\
& + \beta_\tau^{cl} \times Close_{em} \times \mathbf{1}\{\Delta\text{HHI}_{em} < 200\} \\
& + \beta_\tau^{ch} \times Close_{em} \times \mathbf{1}\{\Delta\text{HHI}_{em} \geq 200\} \Big) \\
& + \xi_{em} + \xi_{et} + \epsilon_{emt}
\end{aligned}
\tag{2}
$$

Indices (e, m, t, τ) denote merger events e, banking markets m, calendar year t, and τ time relative to the merger year t_e^*. Event-specific two-way fixed effects, ξ_{em} and ξ_{et}, control for time-invariant effects that are specific to every merger-market combination and control for time-varying effects that are specific to every merger.

The regression allows the effect of mergers to depend on proximity and the change in HHI (ΔHHI_{em}). It estimates four different average treatment effects (β_τ^{fl}, β_τ^{fh}, β_τ^{cl}, β_τ^{ch}) for each outcome y_{emt}. The four parameters are separately identified by variation in DIDs between overlap markets having different values of acquirer-target proximity and ΔHHI_{em}. $Close_{em}$ and Far_{em} proximity are dummy variables for whether the distance measure between the acquirer and the target is below or above its median (about 4 miles), respectively. $\mathbf{1}\{\Delta\text{HHI}_{em} \geq 200\}$ is a dummy variable that is equal to one if $\Delta\text{HHI}_{em} \geq 200$, the level that triggers review for antitrust enforcement under the current screen used by the Fed.[8]

Omitted coefficients are the year before the merger, $\tau = -1$. The window for analysis begins four years prior to the merger and ends four years after the merger. All standard errors for equation (2) are clustered at the unit of treatment exposure: the merger-market level.

IDENTIFICATION. — Our research design addresses several threats to identification. One might be concerned about merger selection, i.e. distinguishing the effects of mergers from unobserved heterogeneity of merging banks or unobserved heterogeneity of markets associated with transactions. The banking setting itself addresses certain aspects of selection that arise in merger retrospectives studies, because our data include the universe of bank merger transactions and enforcement action over the study horizon. Nonetheless, the transactions we observe might be endogenous, for example if a bank is more likely to acquire another bank because of demand or cost shocks in markets where it operates. Our design addresses this by drawing identification off within-merger variation. Our treatment/control groups only include markets where the merging parties operated before the transaction. Moreover, in our regression every transaction has its own time trend with the inclusion of merger event×year fixed effects (ξ_{et}).

One might also be concerned about threats to the parallel trends assumption underlying DIDs. Within-merger DIDs identification still requires that residual determinants of the outcome variable in overlap markets evolve similarly ("in parallel") to nonoverlap markets in the counterfactual where the merger did not occur. Indeed, in Table 1 we observe differences between treatment/control markets in average levels of several market characteristics. To address this, we often measure outcome variables using year-over-year changes to ensure that imbalanced levels or trends in levels of the outcomes are removed from the estimator. Supporting our measurement strategy, panel (b)

[8]Although there are also HHI-level and market share criteria, ΔHHI often determines whether enforcement is pursued. Appendix A.3 presents evidence that our results are robust to defining bins based on the complete criteria.

of Table A.1 in Appendix A.1 shows that treatment/control balance improves significantly when considering first-differences instead of levels of the market characteristics. More evidence, nearer to our estimation strategy, is provided in panel (c) of Table A.1, which shows further improvement in balance after merger event-specific fixed effects are removed from the first-differenced market characteristics.

A related concern is that variation in merger treatment effects across degrees of proximity and ΔHHI might be attributable to some other, potentially unobservable, determinant. We include merger event-specific market fixed effects in our design in order to allow each market to have persistent differences from other markets involved in the same merger, including unobserved heterogeneity across types of treated markets. We also test this threat to identification, at least in part, by examining whether regression pre-trends are parallel across proximity-ΔHHI groups. Providing some assurance, we typically cannot reject the parallel trends hypothesis. However, proximity and ΔHHI are correlated with market size, as shown in Table 1. We therefore also examine whether our results robust to an alternative sample selection that only includes relatively small markets in Section IV.F. We find that different market groups are much more comparable to one another with this alternative sample selection, as shown in Table A.2 in Appendix A.1.

Finally are threats arising from implementation using two-way fixed effects regressions (Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021; de Chaisemartin and D'Haultfœuille, 2020; Sun and Abraham, 2021). Because there are thousands of mergers in our data, a traditional market-year panel design invites bias from treatment effect heterogeneity and the presence of repeated and staggered treatments. The event-stacked design helps address these concerns, see Appendix A.6 for further discussion.

Sample Selection. — We refine the main regression sample in two ways. From the treatment and control groups of a given merger event e, (i) we drop markets that the merging parties entered or exited within the nine-year study window, and (ii) we drop markets that were treated by any other merger e' at the same time or the year prior (t_e^* or $t_e^* - 1$). These criteria hold the composition of markets fixed and ensure that the main sample draws on variation from "switching" treatment status.

IV. Results

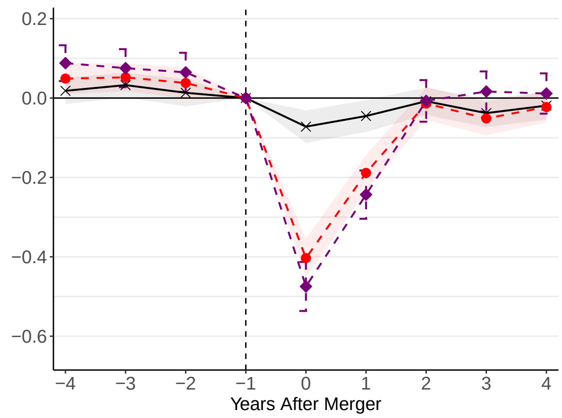
We estimate equation (2) for four sets of outcomes: branch closures, branch network convenience, deposit interest rates (prices), and deposits (quantities). Among many attributes of banks, we focus on the size and convenience of branch networks as well as prices because these are important determinants of consumer utility in banking. By revealed preference, deposits quantity is an indicator of the value a bank provides consumers. We examine deposits quantities to infer how consumers react, on net, to the different price and non-price effects of mergers. For most outcomes, we measure changes or growth instead of levels to ensure parallel trends between treatment and control groups.

We assess under-enforcement under the current HHI-based policy by first comparing mergers that did not increase the HHI enough to trigger enforcement by the antitrust authorities: β_τ^{fl} versus β_τ^{cl}. This comparison provides a baseline effect of a close-proximity merger. Then we determine the importance of the increase in HHI by comparing effects of close-proximity mergers with different changes in HHI: β_τ^{cl} versus β_τ^{ch}. It is important to note that the effects of close-proximity high-HHI mergers, estimated by β_τ^{ch}, account for HHI-based enforcement.

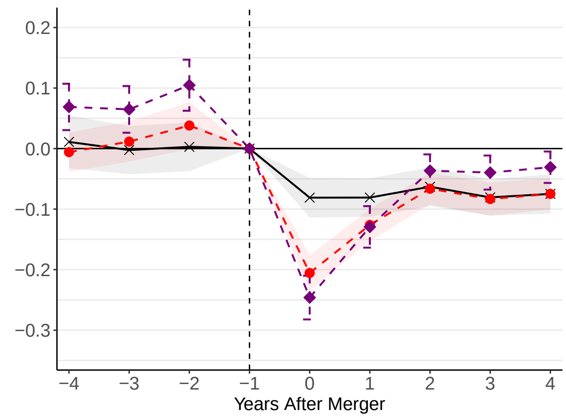
When presenting the main results, we do not discuss estimates for the fourth group (far-proximity mergers with ΔHHI $\geq$ 200) to focus on the exposition on under-enforcement of close-proximity transactions. We discuss estimates for the fourth group in Section IV.E.

A. Post-Merger Branch Closures

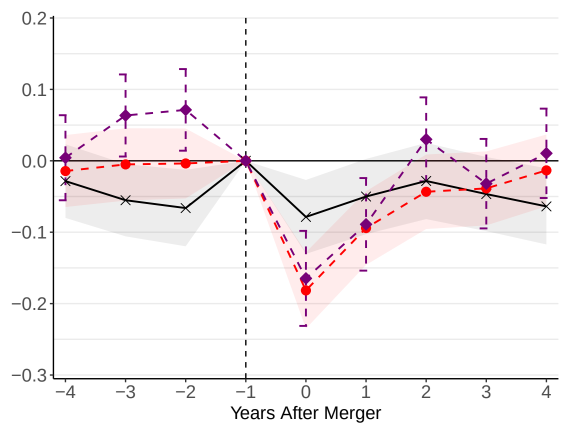
Our first result is that close-proximity mergers are more likely to cause branch closures than far-proximity mergers. Close mergers also cause a decline in growth in the number of branches operated by merging banks, and a decline in growth of the total number of branches in the market. These effects of bank mergers on branches apparently do not depend very much on whether $\Delta\text{HHI} \geq 200$.



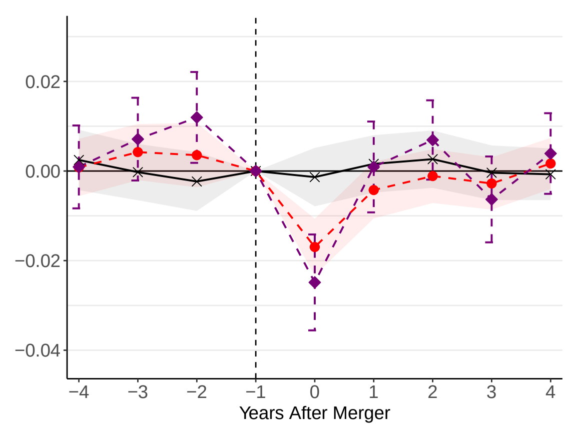
(a) $\mathbb{P}(\Delta\text{Merged Branches} \geq 0)$



(b) $\Delta\log(\text{Merged Entity Branches})$



(c) $\mathbb{P}(\Delta\text{Market Total Branches} \geq 0)$



(d) $\Delta\log(\text{Market Total Branches})$

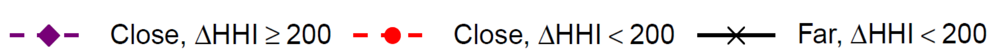


Figure 3: Branch Closures

Note: Difference-in-differences estimates and 95% confidence intervals from equation (2) as a function of time (τ) relative to the merger year; $\hat{\beta}_\tau^{fl}$ in black, $\hat{\beta}_\tau^{cl}$ in red, and $\hat{\beta}_\tau^{ch}$ in purple. All standard errors are clustered at the unit of treatment exposure: the merger-market level.

Figure 3 panels (a) and (b) examine the branch network of the merging banks, defined before the merger as the counterfactual combination of acquirer and target networks. The outcome in panel (a) is an indicator variable for whether the merged bank does not reduce the size of its branch network. Compared to non-overlap markets, after close-proximity acquisitions the merging banks are about 40 pp more likely to reduce the size of their branch network in the year the merger is consummated and about 20 pp more likely to close additional branches the year thereafter. The differential impact of close proximity on the probability of branch closures in the first two years of the merger is economically large and statistically significant.

The outcome in panel (b) is the year-over-year change in the log-number of branches operated by the merged entity, studying the intensive margin of branch closures. Compared to non-overlap markets in the year of the merger, growth in the number of branches operated by the merged bank drops by about 20% in close-proximity markets regardless of the change in HHI, and growth drops by about 10% in far-proximity markets. The differential impact of close proximity—about 10% more branch closures—is economically and statistically significant.

Panels (c) and (d) look at analogous measures for the total number of branches in the market. The change in market-total branches reflects closures as well as any repositioning by the merging banks or their competitors in response to the merger. Close-proximity mergers are about 15 pp more likely to reduce the total number of branches in the market, compared to non-overlap areas, and result in about 2% lower growth in aggregate branches. These estimates for close-proximity mergers are economically and statistically similar whether ΔHHI is large or small. Comparing the estimates for aggregate branches, panels (c) and (d), with estimates for the merged bank's branches, panels (a) and (b), suggests that rival banks react to the merger by creating branches, and that rivals' reactions only partially offset branch closures by the merging banks.

Current HHI-based merger review appears to overlook close-proximity mergers that might adversely impact depositors' welfare through branch closures. Structural models of demand from the literature provide estimates of consumer willingness to pay for the size of a bank's branch network. Viewing our results in light of Xiao et al. (2022), for example, suggests that a 20% decrease in the size of the merged branch network is worth about an 8% lower deposit interest rate (i.e. price increase).

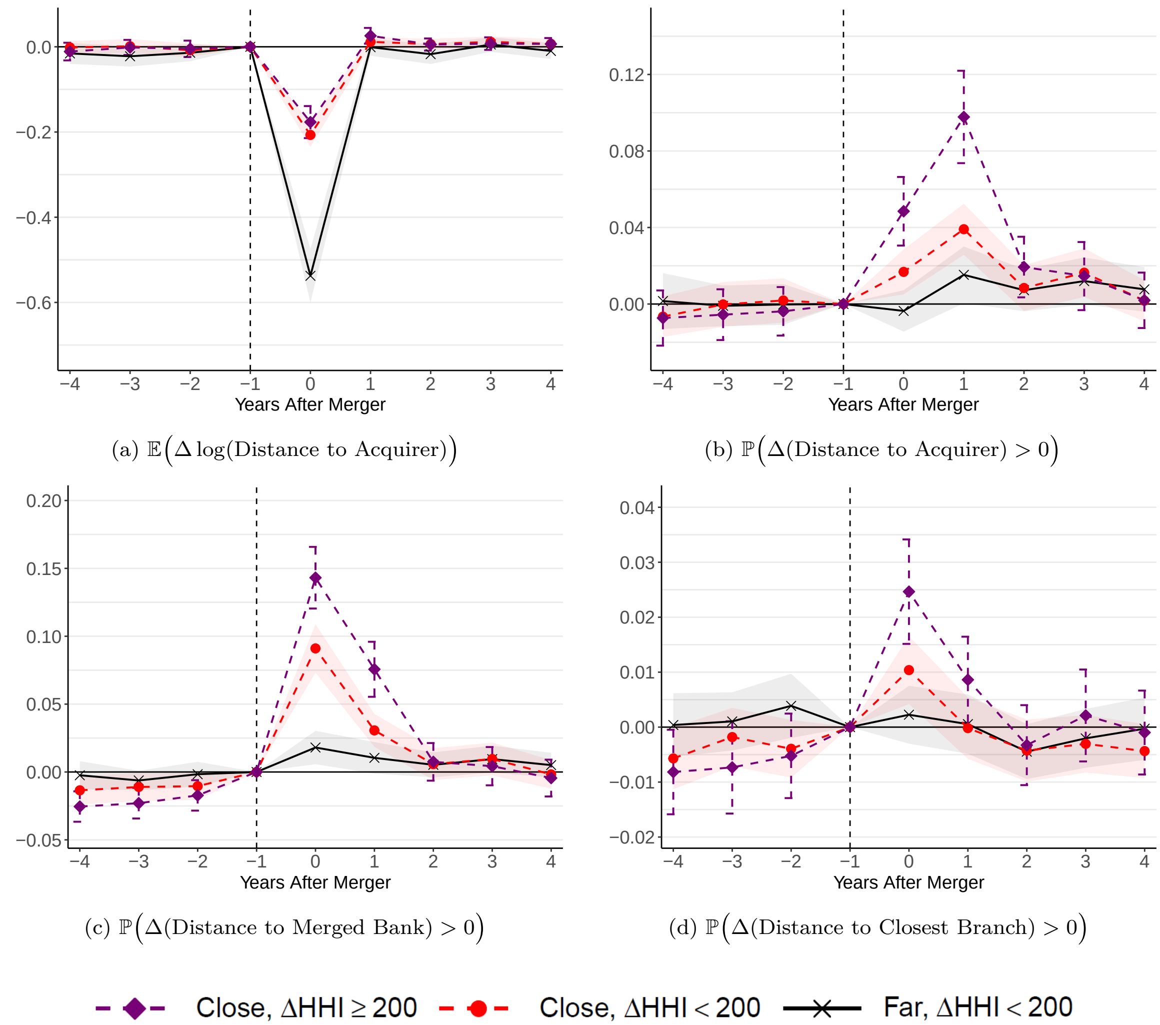
The welfare impact of branch closures likely depends on distances between branches that are closed and other locations in a merged branch network. In an extreme case where an acquirer's branch and a target's branch happen to be right next to each other, closing down one of them should not significantly affect welfare apart from other changes caused by a merger. At the other extreme, if closed branches are strong geographic complements, then the welfare of consumers who have to travel farther for retail banking products might be significantly impacted. This motivates our next analysis on convenience.

B. Branch Network Convenience

The distance between a bank's branch network and its customers, "convenience," is an important determinant of consumer utility. Bank mergers might affect convenience in several ways, and have different impacts on different types of consumers. The synergy from combining the acquirer and target branch networks is an important potential benefit of mergers, one most likely to impact consumers who are located relatively far from one of the merging banks. Mergers could also reduce convenience through branch closures, harming consumers located relatively near one of the merging banks. More broadly, branch repositioning of the merging banks or their rivals in response to the merger might affect the distance between consumers and their most convenient branch of *any* bank. Accordingly, we measure network convenience outcomes that capture the average impact of a merger as well as how mergers advantage some consumers while disadvantaging others.

We begin our analysis by examining average consumer convenience via the year-over-year percent change in distance to a branch network. Formally, let $d(b, I_t)$ denote the distance from the center of census block b to the nearest branch in network I at time t, and let w_b denote the share of market m's population living in b. The average consumer's percent change in distance to branch network I_t in market m at time t is calculated as $\mathbb{E}(\Delta \log(\text{Distance to } I_t)) = \sum_{b\in m} w_b \log\left(d(b, I_t)/d(b, I_{t-1})\right)$.

Figure 4(a) illustrates the effects of mergers on the average percent change in consumer distance to the acquirer's network. After the merger, the acquirer's network is the merged branch network.



(a) $\mathbb{E}\big(\Delta \log(\text{Distance to Acquirer})\big)$

(b) $\mathbb{P}\big(\Delta(\text{Distance to Acquirer}) > 0\big)$

(c) $\mathbb{P}\big(\Delta(\text{Distance to Merged Bank}) > 0\big)$

(d) $\mathbb{P}\big(\Delta(\text{Distance to Closest Branch}) > 0\big)$

Figure 4: Distance Between Consumers and Branches

Note: Difference-in-differences estimates and 95% confidence intervals from equation (2) as a function of time (τ) relative to the merger year; $\hat{\beta}_{\tau}^{fl}$ in black, $\hat{\beta}_{\tau}^{cl}$ in red, and $\hat{\beta}_{\tau}^{ch}$ in purple. All standard errors are clustered at the unit of treatment exposure: the merger-market level.

Thus, effects on this outcome in the year of the merger include the merger's convenience synergy, because consumers' distance to the merged branch network is compared to their distance to the smaller pre-acquisition network.

In the year of the merger, we find a large decline in distance to the acquirer's nearest branch for all merger types. This suggests that synergies from merging branch networks outweigh the effects of branch closures on average across consumer locations. However, far-proximity mergers have significantly larger average convenience synergies than close-proximity mergers, reducing consumers' distance to the nearest branch in the acquirer's network by about 50%. The larger effect of far-proximity mergers reflects the fact that far-proximity acquisitions have greater potential to reduce some consumers' distances to the closest branch controlled by the acquiring bank, as well as the result in Section IV.A that branch closures are less frequent after far-proximity mergers.[9] This

[9]Unreported estimates for the average percent change in distance to the target's network are very similar, with far-proximity mergers having the largest synergies and exhibiting little dependence on the change in HHI.

result suggests that proximity is the primary determinant of a merger's convenience benefits. The consumers who benefit the most from these synergies are those located relatively far from one of the merging banks.

Although mergers improve the acquirer's network convenience on average, mergers could harm consumers located near branches that the acquirer closes as a consequence of the merger. We therefore examine disparate impacts of mergers on convenience by measuring the fraction of consumers whose distance to their nearest branch in a network increased year-over-year. Formally, for branch network I_t in market m we compute $\mathbb{P}(\Delta(\text{Distance to } I_t) > 0) = \sum_{b \in m} w_b 1(d(b, I_t) > d(b, I_{t-1}))$.

Figure 4(b) shows that close-proximity mergers increase the distance to the acquirer's network for a significant fraction of consumers. In the year of consummation, close-proximity mergers increase distance to the acquirer's network for 2% to 5% of consumers, despite the synergy from consolidating two networks. In the year immediately following close-proximity mergers, distance to the acquirer's (now merged) network increases for another 5% to 10% of consumers, with close-proximity mergers with $\Delta\text{HHI} \geq 200$ exhibiting the greatest reduction in convenience. For the first two years after a merger, the differences in our estimates between close-proximity mergers with $\Delta\text{HHI} < 200$ and far-proximity mergers with $\Delta\text{HHI} < 200$ are statistically significant at the 5% level.[10] That convenience declines for a larger fraction of consumers in the first two years after close-proximity mergers is consistent with our result that merging banks tend to close branches during the same period.

Panel (c) illustrates how mergers affect convenience of the merged bank's network. The merged bank's convenience measure is based on consumer distances to the counterfactual combined acquirer and target networks in the years before the merger and on distances to the actual merged network in the years after the merger. Thus, convenience of the merged branch network holds the potential convenience synergy fixed, which allows us to isolate the effects of merger-induced branch openings and closings on consumer distances to the merging banks. We find that in the year of consummation close-proximity mergers increase the distance to the merged network for 10% to 15% of consumers, mergers with $\Delta\text{HHI} \geq 200$ reducing convenience the most. Distances to the merged network also increase in the following year, albeit to a smaller degree. In contrast, far-proximity mergers with $\Delta\text{HHI} < 200$ affect the merged bank's convenience for a much smaller fraction of consumers.[11]

In panel (d), we study the effects of bank mergers on the distance between consumers and the closest branch of any bank, disregarding which banks own which branches. Consumers' distances to the "market network" measures overall convenience of the local banking industry, and captures the effects of branch repositioning both by the merging bank and its rivals. We find that after close-proximity mergers, 1% to 2.5% of consumers experience an increase in distance to their nearest branch, with larger and more persistent adverse effects after close-proximity mergers with $\Delta\text{HHI} \geq 200$. Although close-proximity mergers with $\Delta\text{HHI} < 200$ have slightly smaller effects in the year of consummation, the estimate is statistically larger than the estimate for far-proximity mergers with $\Delta\text{HHI} < 200$.[12] Far-proximity mergers with $\Delta\text{HHI} < 200$ do not significantly affect the convenience of consumers' nearest branch location.

We also examine the convenience of branch networks of competitors of the merging banks. In Appendix Figure A.9 we illustrate results for the acquirer and target's biggest, closest, farthest, and average rivals. We find no evidence that mergers improve the branch network convenience of rival banks.

As judged by the effects on convenience, close-proximity mergers are more likely to harm con-

[10] The p-values for the differences of the estimates for year 0 and 1 between the two types of mergers are 0.013 and 0.018, respectively.

[11] Between far- and close-proximity mergers with $\Delta\text{HHI} < 200$, close-proximity mergers reduce access for about 7% more consumers than far-proximity mergers (p-value 1.121e-10) in the year of consummation.

[12] Between far- and close-proximity mergers with $\Delta\text{HHI} < 200$, close-proximity mergers reduce access for about 0.8% more consumers than far-proximity mergers (p-value 0.048).

sumers and are less likely to convey offsetting benefits. Even though a merger's convenience synergy does benefit the average consumer, the heterogeneous effects of branch closures and repositioning caused by close-proximity mergers harm many consumers. For consumers whose convenience is negatively affected by close-proximity mergers (Panel (b)), we find that their distance to the acquirer's branch network increases by 0.7 miles, on average. And for consumers whose convenience to any bank is negatively affected by close-proximity mergers (Panel (d)), the distance to their nearest branch increases by 0.26 miles, on average. Moreover, a significant fraction of consumers loses access altogether to a branch less than 1 mile away, see Appendix Figure A.10.

To gauge the economic importance of these effects, we use structural demand estimates from the literature to measure the trade-off between distance to branches and deposit interest rates. The estimates from Ho and Ishii (2011) imply that consumers who choose the acquirer bank would be willing to accept a 35% reduction in deposit interest rates to avoid traveling an additional 0.7 miles to their branch network. By the same estimate of willingness-to-pay for convenience, consumers would accept a 13% change in prices to avoid travelling an additional 0.26 miles to access any bank.

These significant harms are determined primarily by close proximity; we find no evidence that far-proximity mergers have such effects. The harm is evident, and in fact most pronounced, for close-proximity transactions subject to HHI-based enforcement. Our results therefore suggest that HHI-based screening not only gave safe harbor to bank mergers with non-price harm, but also that HHI-based enforcement was ineffective at addressing the harm.

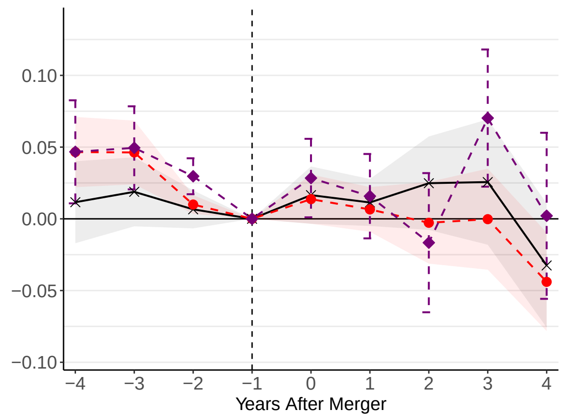
C. Deposit Interest Rates

Next we examine the effects of bank mergers on deposit interest rates. For the merging banks, their proximity impacts the typical trade-off between market power and cost synergies. A close-proximity acquisition eliminates competition from a close substitute and therefore increases market power beyond what might be indicated by the policy relevant HHI. Yet, a merger's cost and quality synergies may also depend on branch network proximity. Whether the merged bank offers lower deposit rates depends on the resolution of these two forces. The proximity of the merging parties can affect their rivals' market power, as well, impacting rivals' prices.

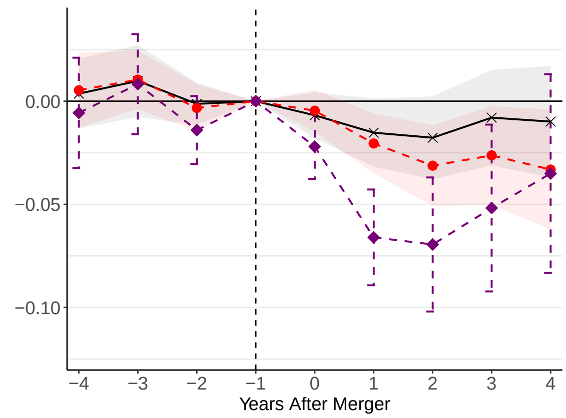
Figure 5 presents our estimates for the effects of mergers on two-year certificate-of-deposit (CD) rates for the acquirer and for the merging banks' average competitor using the RateWatch data. Because deposit rates determine interest that is paid to depositors, consumers prefer a higher price in this setting. Therefore, a lower deposit rate is evidence of harm.

Panel (a) shows that the acquirer's deposit rates in overlap markets do not change very much after close-proximity mergers relative to its rates in non-overlap markets. Our estimates for other measures of deposit prices of the merging banks, illustrated in Appendix Figure A.11, are qualitatively the same. Note that the negative results in panel (a) are not due to imprecise estimation. This may suggest that, even if there are cost efficiencies from close-proximity acquisitions, merging banks do not pass the efficiencies through in the form of better prices. This negative finding could also be explained by uniform pricing. If deposit rates do not vary across geographic banking markets, then our within-merger between-market identification strategy will not detect a merger's effect on prices. Indeed, Granja and Paixao (2023) show that changes in local market power caused by mergers only have moderate impacts on prices due to uniform pricing.

In contrast to the merged bank's prices, panel (b) shows that, relative to non-overlap markets, rival banks decrease their deposit rates about 3% to 6% after close-proximity mergers. Regardless of ΔHHI, our estimates for the effects of close-proximity mergers on rivals' deposit rates are generally statistically different from zero, including our results for other deposit price outcomes illustrated in Appendix Figure A.11. Although the standard errors of our estimates reject the hypothesis that close-proximity mergers have no effect on rivals' prices, it is difficult to statistically distinguish the effects of close- and far-proximity mergers for some post-merger periods. Thus, we cannot precisely



(a) log(2-year CD Rate of Acquirer)



(b) log(2-year CD Rate of Competitors)

Close, ΔHHI $\geq$ 200 Close, ΔHHI < 200 Far, ΔHHI < 200

Figure 5: Deposit Interest Rates

Note: Difference-in-differences estimates and 95% confidence intervals from equation (2) as a function of time (τ) relative to the merger year; $\hat{\beta}_{\tau}^{fl}$ in black, $\hat{\beta}_{\tau}^{cl}$ in red, and $\hat{\beta}_{\tau}^{ch}$ in purple. All standard errors are clustered at the unit of treatment exposure: the merger-market level.

conclude that close-proximity itself leads rivals to decrease deposit rates.

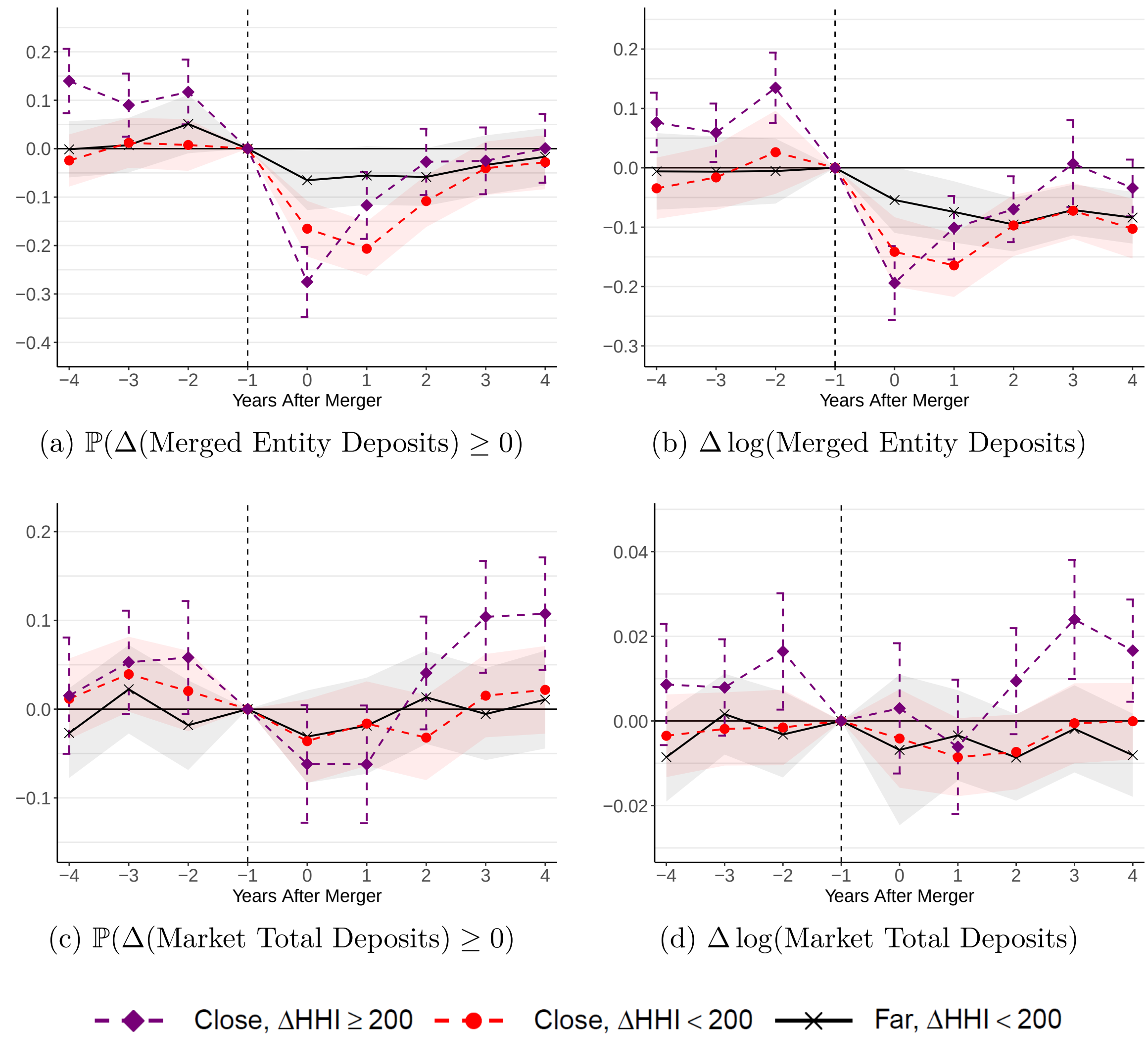
Even so, our results do not suggest that close-proximity mergers cause price responses by merging banks or by rivals that would offset the anticompetitive effects of mergers on branch networks. If anything, close-proximity mergers lower deposit interest rates for consumers who choose rival banks in some periods. That proximity predicts these price effects, even after accounting for HHI-based enforcement, suggests that the current HHI-based policy does not capture some dimension of substitutability that influences the market power of rival banks.

These findings help to further the understanding of the estimated price effects of bank mergers in the literature. For example, Prager and Hannan (1998) found that bank mergers that substantially increase the HHI result in lower deposit rates. Our results suggest that effects on competitor prices might drive their findings and indicate that close-proximity mergers that do not significantly increase the HHI result in worse prices as well. Focarelli and Panetta (2003) also found that bank deposit rates in Italy increased in the long run after mergers, arguably due to cost synergies. In contrast, our results do not suggest that cost synergies significantly improve prices relative to non-overlap markets where the merging firms operated but did not compete. In light of our identification strategy, this may indicate that bank merger cost synergies are primarily firm-level, and not firm-market-level, efficiencies.

D. Deposits Growth

Based on revealed preference, a bank's deposits should increase if consumers altogether value the effects of the merger on its branches, network convenience, interest rates, and any other product characteristics that we do not explicitly study. We study deposits quantities in order to examine how the price and non-price effects of mergers impact consumer choices, on net. Figure 6 illustrates how the effects of bank mergers on deposits growth vary with ΔHHI and acquirer-target proximity.

In panel (a), we measure the effects of mergers on the probability of deposits growth, with a binary outcome that deposits of the merged bank do not decline. Before the transaction occurs,



(a) $\mathbb{P}(\Delta(\text{Merged Entity Deposits}) \geq 0)$ (b) $\Delta \log(\text{Merged Entity Deposits})$

(c) $\mathbb{P}(\Delta(\text{Market Total Deposits}) \geq 0)$ (d) $\Delta \log(\text{Market Total Deposits})$

Figure 6: Deposits Growth

Note: Difference-in-differences estimates and 95% confidence intervals from equation (2) as a function of time (τ) relative to the merger year; $\hat{\beta}^{fl}_{\tau}$ in black, $\hat{\beta}^{cl}_{\tau}$ in red, and $\hat{\beta}^{ch}_{\tau}$ in purple. All standard errors are clustered at the unit of treatment exposure: the merger-market level.

the counterfactual merged quantity is constructed as the combined deposits of the acquirer and target. We find that after far-proximity mergers with $\Delta\text{HHI} < 200$, relative to non-overlap markets, the probability of deposits growth at the merged bank falls only 7 pp and only shortly after consummation. In the year of consummation alone, the probability of deposits growth at the merged bank falls by 28 pp after close-proximity $\Delta\text{HHI} \geq 200$ acquisitions, and declines by 17 pp after close-proximity mergers with $\Delta\text{HHI} < 200$. Comparing close-proximity mergers by ΔHHI, we find that the three-year cumulative effects of $\Delta\text{HHI} < 200$ and $\Delta\text{HHI} \geq 200$ mergers are not significantly different. In contrast, comparing $\Delta\text{HHI} < 200$ acquisitions by close- and far-proximity, we find that cumulative effects within three years of close- versus far-proximity mergers are different by a statistically significant 30 pp.

In panel (b) we illustrate the effects of mergers on the rate of growth of merged deposits. We find that close-proximity mergers cause growth rates of the merged bank to decline by a statistically and economically significant 15% to 20% in the year of the merger. These effects are statistically significantly larger than those of far-proximity mergers with ΔHHI < 200. Moreover, the difference between close-proximity mergers with ΔHHI < 200 and ΔHHI ≥ 200 is not statistically significant.

Panels (a) and (b) together suggests that ΔHHI plays a more limited role than proximity in determining deposits runoff from the merged bank. Because far-proximity mergers do not exhibit a similar runoff, this evidence also rules out the hypothesis that factors common in all mergers, such as destruction of the target's brand, are not main drivers of the effects on quantities.

Our results on deposits growth may be puzzling in light of our finding that acquirers do not lower their deposit interest rates after close-proximity mergers, whereas rivals lower their deposit rates up to 6%. However, recall that structural demand estimates from the literature suggest that consumers have high willingness-to-pay for branches and convenience, e.g. Ho and Ishii (2011). The effects of close-proximity mergers on rivals' deposit rates are smaller than our back-of-the-envelope calculations of the value of closed branches and reduced convenience discussed in Sections IV.A and IV.B. Thus, some consumers of merging banks might switch to rivals after close-proximity acquisitions, even though rivals offer lower deposit interest rates. Also note that reduced convenience is not the only rationale for the deposits runoff. Merger-induced changes in other product attributes that we do not study might also have contributed to the estimated effects on deposits growth.

We complement the analysis of merged bank deposits with a study of market aggregate deposits. Variation in aggregate quantity reflects depositor substitution to outside options, as well as substitution to rival banks. Figure 6 panel (c) examines the effects of mergers on the probability of aggregate deposits growth, while panel (d) examines effects on the rate of aggregate deposits growth. We find no statistically significant evidence that mergers reduce growth in aggregate deposits, at least relative to the merging banks' non-overlap markets.

Figure 6 then suggests that quantity runoff from the merged bank diverts to competitors, and not to outside options. Our results do not indicate that rivals substantively improve their product characteristics in response to the merger. On the contrary, if anything, rival banks seem to offer lower interest rates for similar quality after close-proximity mergers. Furthermore, switching to another bank itself might be costly (Kiser, 2002). We therefore interpret these results as evidence that close-proximity bank mergers cause more consumer harm than far-proximity mergers.

E. *Effects of Far-Proximity $\Delta HHI \geq 200$ Mergers*

Our regression (2) also obtains estimates for far-proximity ΔHHI ≥ 200 mergers. Above, we omit these results in order to focus the exposition on under-enforcement vulnerabilities of the current HHI-based policy. One might be particularly interested in the effects of far-proximity ΔHHI ≥ 200 mergers in order to test the hypothesis of over-enforcement. Appendix A.4 provides the full set of estimates.

In sum, our results do not suggest that far-proximity ΔHHI ≥ 200 mergers are benign. The evidence is more mixed because estimates for this group are typically less precise. However, for some outcome measures there is statistically significant evidence of harm, and for a few outcomes we find harm comparable to close-proximity ΔHHI < 200 transactions. Our evidence therefore suggests that the current HHI-based policy in the banking industry is not susceptible to over-enforcement, but instead is likely vulnerable to under-enforcement of close-proximity acquisitions.

F. *Additional Results and Robustness Checks*

We summarize our results from additional analyses here and provide details in Appendix A.3. We first find that simply lowering the ΔHHI threshold does not address under-enforcement of close-proximity mergers. Even among mergers with ΔHHI < 100, close-proximity mergers still lead to outcomes interpretable as consumer harm. Next, we show evidence that close-proximity mergers with anticompetitive effects are not captured by the Federal Reserve's HHI level criteria, which is a dimension of the current screening policy that we did not consider in the main specification.

We also conducted several robustness analyses. First, we restrict our sample to a more recent period (2000-2014) to see if geographic competition is still an important determinant of the effects of contemporary mergers, despite the evolving nature of bank competition including the rise of internet banking. Second, to see whether our results are sensitive to different definitions of "close" and "far" proximity, we examine defining "close" based on (i) below-median distance across banks operating within the given overlap market, and (ii) having an overlap census tract. Third, we exclude markets that experienced any other bank mergers over the regression horizon from $t_e^* - 4$ to $t_e^* + 4$ in order to minimize the possibility that other mergers in the same market impact the estimates. Lastly, we restrict the sample to markets with eight or fewer banks the year before the merger. The median market has eight banks, and with this subsample of smaller markets the treatment/control groups are much more comparable to one another, as shown in Table A.2 in Appendix A.1. In all robustness analyses, our results remain qualitatively similar to the main results.

V. Policy Implications

Our results suggest that HHI-based antitrust in the banking industry misjudged the anticompetitive effects of close-proximity transactions. How effective would alternative policies, for example alternative thresholds or market definitions, have been at enforcing mergers that our results suggest are likely harmful? What costs might have accompanied the benefits? While a full welfare analysis is beyond the scope of this paper, in this section we provide tabulations that shed light on this question.

To frame a cost/benefit discussion of under-enforcement and over-enforcement with different policies, one must first classify merger×Fed market pairs in the data as "likely harmful" or "likely benign" based on the empirical evidence. With the caveat that no such empirical categorization will be perfect, we proceed using proxy classifications based on the average effects of mergers from our regression results. We let merger×market pairs with ΔHHI ≥ 200 *or* close-proximity (Distance $\leq$ 4 miles) stand for "likely harmful," and let mergers with ΔHHI < 200 *and* far-proximity (Distance $>$ 4 miles) stand for "likely benign." In this framework, transactions in the former group should be enforced, while transactions in the latter should not be enforced. The upper panel of Table 2 shows that the current policy, ΔHHI ≥ 200, under-enforces 3213 likely harmful merger×market pairs with ΔHHI < 200 and close-proximity.

The lower panel of Table 2 summarizes how many likely harmful mergers and likely benign mergers would, or would not, have been subject to enforcement under counterfactual policies. We first examine lowering the concentration threshold to ΔHHI ≥ 100, without changing the market definition, as suggested by Nocke and Whinston (2022). Then we consider narrower geographic market definitions based on counties and census tracts, without changing the ΔHHI ≥ 200 threshold.[13]

The alternative regimes have benefits from identifying additional likely harmful mergers that are missed by the current policy, but also have costs from misclassification and increased resources for investigations. Both lowering the enforcement threshold and narrowing the predetermined market definition risks over-enforcing likely benign far-proximity mergers. Narrower market definitions also pose the risk of under-enforcing likely harmful mergers with high ΔHHI, because some merging banks with significant overlap in the broader Fed market do not operate within the same county

[13]To measure whether a merger × Fed market pair would have passed or failed the concentration screen using the alternative market definition, we compute the maximum ΔHHI across the narrower geographic areas lying within or intersecting the broader geographic Fed market area. Thus, a merger×Fed market pair is classified as "enforce" under the narrower market definition if at least one County Market ΔHHI or Census Tract Market ΔHHI, respectively, within the corresponding Fed market exceeded 200.

or census tract. The lower ΔHHI threshold of 100 would return two correct enforcements per additional misclassification (benefit/cost = 416/208) with an 67% increase in total investigations. This benefit/cost ratio is only 0.8 ($= 307/(209+109+69)$) for the county-based policy with an 36% increase in total investigations, and is 2.5 ($= 1321/(220 + 141 + 176)$) for the census tract-based policy with 132% increase in total investigations.

These results suggest that HHI-based enforcement with predefined markets is vulnerable to misclassification error. An ideal policy might use perfect, case-by-case information to define markets and measure market shares. This richer information set might include proximity of the merging branch networks, among other observable determinants of the banks' substitutability. Under such an ideal, the resulting HHI might more closely reflect competition between merging banks, reducing misclassification error in the policy. In practice, resource constraints or lack of information may prohibit antitrust authorities from implementing the ideal. Policy that relies on predetermined markets and HHI thresholds might be closer to the ideal if another dimension of information about competition between the merging parties, like proximity, is available to reduce misclassification.

Thus we consider a policy with complementary HHI-proximity criteria. The bottom row of Table 2 illustrates a regime that classifies bank mergers for enforcement if the transaction either triggers the current policy threshold $\Delta\text{HHI} \geq 200$, using the current policy market definitions, or independently triggers a conservative 1-mile branch network proximity threshold.[14] This policy would have significant returns as measured by new correct enforcements per additional misclassification. It switches 1147 likely harmful close-proximity mergers from "not-enforce" to "enforce," and levies additional proximity-based remedies on 54% of mergers that exceeded the HHI threshold. By construction, it has less misclassification error because it complements the current policy with the proximity-based threshold. The primary cost of this policy is a 123% increase in total investigations, similar to the census-tract based regime but with a much higher benefit/cost return.

We therefore argue that bank antitrust enforcement could be improved if objective criteria on branch network proximity were used in complement to the HHI-based policy. This approach reduces the HHI's vulnerability to underestimation and overestimation of competitive harm, precisely because complementary criteria based on observable determinants of merger outcomes do not depend on errors in market definitions or market shares. To the extent this lesson speaks to the broader debate about antitrust laxity and concentration-based screens, policy makers should be cautious about the efficacy of turning to stricter versions of the same framework.

[14]For comparison, Appendix Table A.3 also illustrates a policy with a stricter 2-mile threshold.

Table 2—: Retrospective Antitrust Enforcement with Alternative Policies

	Fed Market ΔHHI < 200		Fed Market ΔHHI ≥ 200		
	Distance ≤ 4 miles	Distance > 4 miles	Distance ≤ 4 miles	Distance > 4 miles	Total Enforced[3]
Under current policy:	*Not-Enforce*	*Not-Enforce*	*Enforce*	*Enforce*	
Number of merger×market pairs	3213	3462	716	213	929
	Number of mergers that would have switched from...				
Counterfactual policy:	*Not-Enforce* to *Enforce*[1]		*Enforce* to *Not-Enforce*[2]		
Lower Threshold ΔHHI ≥ 100	416	208	0	0	1553
County Market ΔHHI ≥ 200	307	209	109	69	1267
Census Tract Market ΔHHI ≥ 200	1321	220	141	176	2153
Distance ≤ 1 Mile or ΔHHI ≥ 200	1147	0	0	0	2076

Note: see Appendix A.2 and Table A.3 for further details. Tabulations include all overlap merger-market pairs (N = 7604) for banking acquisitions between 1984 and 2014, based on Federal Reserve banking market definitions ("Fed markets").
[1] The number of mergers classified as "not-enforce" under current policy that would have been reclassified to "enforce" under the counterfactual policy, by 4-mile branch network proximity.
[2] The number of mergers classified as "enforce" under current policy that would have been reclassified to "not-enforce" under the counterfactual policy, by 4-mile branch network proximity.
[3] Total number of merger-market pairs that would have been subject to antitrust enforcement under the respective policy.

VI. Conclusion

Debates over antitrust laxity have raised important questions about the performance and vulnerabilities of HHI-based policies in practice. Our empirical contribution suggest that many close-proximity mergers in the banking industry harmed consumers, and were missed or under-enforced by the HHI. Stricter variants of the same framework – lower thresholds and narrower predefined markets – would not have addressed under-enforcement of close-proximity acquisitions without introducing significant misclassification error. However, basing enforcement on complementary HHI-proximity criteria can address case-by-case misclassification errors due to thresholds and predefined markets, improving bank antitrust.

To our knowledge, such an approach has not been studied in the literature. Recognizing that policy makers may be constrained from implementing ideal, case-by-case, information rich analysis, understanding optimal antitrust policy that incorporates multidimensional enforcement criteria is an interesting direction for future theoretical research. Whether there are measures of substitutability to complement the HHI in other industries is also an empirical question for further research.

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Internet Appendix for:
"Concentration and Geographic Proximity in Antitrust Policy: Evidence from Bank Mergers"

A.1. Treatment/Control Balance

This appendix examines balance between treatment/control groups the year before each merger event. Table A.1 provides conditional means for merger-market observations in the main regression sample. Panel (a) illustrates differences in levels of market characteristics like the HHI or the number of branches in the market across treatment/control groups, showing some apparent imbalance. Panels (b)-(c) and Table A.2 then provide evidence supporting our empirical strategy, which helps addresses this using outcome measurement and regression fixed effects.

Panel (b) speaks to the outcome measurement strategy. Most outcomes we study are based on first differences, e.g. growth in deposits or whether the size of the merged branch network increased year-over-year. Comparing panels (a) and (b) in Table A.1, balance improves significantly when considering first-differences instead of levels of the market characteristics. This suggests that our measurement strategy is helpful.

Next consider the balance implications of our regression strategy, which uses event-specific fixed effects. Allowing each merger to have its own time trend effectively demeans, or within-transforms, an outcome measure merger-by-merger. Comparing panels (b) and (c) in Table A.1, we find even stronger treatment/control balance after merger-specific demeaning the first difference characteristics. This suggests that event-specific time fixed effects in the regression further address imbalance concerns.

Finally, our regressions also include merger event-specific market fixed effects. This allows each market to have persistent differences from other markets involved in the same merger, including unobserved differences that threaten identification. To illustrate, Table A.2 provides means of level characteristics for a subsample of markets with 8 or fewer banks. Focusing on this subsample removes some between-market variation attributable to market size, which is purged by the merger-market fixed effects estimator. In comparison to Table A.1 panel (a), Table A.2 shows stronger balance across treatment/control groups. This suggests that merger event-specific market fixed effects in the regression also help address imbalance concerns.

Table A.1—: Comparison of Treatment and Control Groups in the Main Sample

	(1) Close & ΔHHI≥200	(2) Close & ΔHHI<200	(3) Far & ΔHHI≥200	(4) Far & ΔHHI<200	(5) Non-overlap markets
N merger event × Fed market (e, m) Obs.	405	651	154	731	32,076
(a) Levels year-before-merger x_{em}					
Acq-Tar Distance (miles)	0.7	1.2	13.3	14.9	.
Δ HHI	594.3	65.0	603.2	48.9	0.0
HHI	1,970.0	1,533.8	2,042.9	1,376.9	2,013.7
Merged Entity Share (%)	36.1	14.4	37.0	11.1	17.0
Market Bank Count	10.5	17.8	9.0	18.3	11.3
Market Branch Count	34.0	77.6	18.7	66.0	33.3
Merged Entity Branches	8.8	9.7	4.9	5.3	3.5
Distance to Merged Entity	4.9	5.5	5.6	9.2	7.7
(b) First differences $\dot{x}_{em} = x_{emt} - x_{emt-1}$					
HHI	-56.6	-20.1	-26.2	-2.1	-21.9
Merged Entity Share (%)	-0.0	0.4	0.3	0.3	-0.2
Market Bank Count	0.2	0.4	0.0	0.2	0.2
Market Branch Count	0.5	1.3	0.2	1.0	0.5
Merged Entity Branches	-0.1	0.4	0.1	0.3	-0.0
Distance to Merged Entity	-0.0	-0.1	-0.2	-0.6	-0.0
(c) Merger-demeaned differences $\dot{x}_{em} - \bar{\dot{x}}_e$					
HHI	-26.6	9.6	0.8	7.6	-0.0
Merged Entity Share (%)	0.1	0.5	0.3	0.2	-0.0
Market Bank Count	0.0	0.1	-0.1	0.1	-0.0
Market Branch Count	-0.0	0.6	-0.1	0.4	-0.0
Merged Entity Branches	-0.1	0.3	-0.0	0.1	-0.0
Distance to Merged Entity	0.1	-0.0	-0.0	-0.2	0.0

Table A.2—: Comparison of Treatment and Control Groups for the subsample of markets with 8 or fewer banks

	(1) Close & ΔHHI≥200	(2) Close & ΔHHI<200	(3) Far & ΔHHI≥200	(4) Far & ΔHHI<200	(5) Non-overlap markets
Acq-Tar Distance (miles)	0.6	0.9	11.8	13.7	.
Δ HHI	798.8	93.1	750.3	92.2	0.0
HHI	2,497.0	2,540.9	2,479.2	2,493.9	2,806.6
Merged Entity Share (%)	41.5	19.5	41.7	18.2	23.6
Market Bank Count	6.1	6.8	6.2	7.0	5.8
Market Branch Count	12.3	15.9	10.6	13.6	11.6
Merged Entity Branches	4.2	3.9	3.7	3.3	2.2
Distance to Merged Entity	5.1	5.8	5.6	6.0	7.2
N. Obs.	215	99	99	79	14,261

A.2. Detailed tabulations for policy discussion

This appendix provides more detailed tabulations used in Table 2 in the main paper. The sample includes all overlap merger-market pairs (N = 7604) for banking acquisitions between 1984 and 2014, based on Federal Reserve banking market definitions from 2014 ("Fed markets"). The columns of Table A.3 separate mergers in the data into four categories, identified by headings for whether the Fed market HHI increased more than 200 points and by whether the merging branch networks were within 4 miles of each other as measured by Equation (1). The first row shows the enforcement classification for each type of transaction under current policy and market definitions. The second row of Table A.3 provides the number of mergers for each type of transaction, reproducing the upper-panel of Table 2 in the text.

Subsequent rows tabulate the number of mergers within each respective HHI-proximity group that would or would not have, respectively, met the criteria of alternative screening policies. We examined five alternative policy regimes: lowering the concentration threshold ΔHHI $\geq$ 100 within current policy market definitions, applying the existing ΔHHI $\geq$ 200 threshold but within narrower county-based market definitions, applying the ΔHHI $\geq$ 200 threshold within very narrow census tract-based market definitions, and finally using proximity thresholds (2-mile and 1-mile) to complement the ΔHHI $\geq$ 200 policy within current Fed market definitions.

For tabulations in the "County Market ΔHHI" and "Census Tract Market ΔHHI" rows, we aggregated branch-deposit observations into bank-county and bank-tract level datasets, and then calculated the effects of each merger on the HHI within each county and census tract market. To measure whether a merger $\times$ Fed market observation in our main dataset would have passed or failed the concentration screen using the alternative market definition, we computed the maximum ΔHHI across the narrower geographic areas lying within or intersecting the broader geographic Fed market area. Thus, a merger $\times$ Fed market is classified as "enforce" under the narrower market definition if at least one County Market ΔHHI or Census Tract Market ΔHHI, respectively, within the corresponding Fed market exceeded 200.

To illustrate how these calculations enter into the lower-panel of Table 2 in the text, consider the row labeled "Fed market ΔHHI $\geq$ 100" in Table A.3. Columns 3 and 4 confirm that markets flagged for "enforce" by the current ΔHHI $\geq$ 200 policy would still be classified as "enforce" under the lower concentration threshold. Columns 1 and 2 show the number of mergers that would have been reclassified from "not-enforce" to "enforce." Lowering the threshold to 100 would have flagged 416 likely harmful close-proximity mergers from "not-enforce" to "enforce", but also would have triggered investigation of 208 likely benign far-proximity mergers.

Across the proximity-ΔHHI distribution of mergers, enforcement under the complementary HHI-proximity policy compares favorably to enforcement under a lower HHI threshold. In Figure A.3 below, incremental enforcement gained by lowering the HHI threshold is shown by the hashed rectangle to the left of the Δ200 reference line. The intersection of the hashed rectangle with the red dots in the lower half of the figure show how many likely harmful close-proximity transactions (416) are reclassified into enforcement by the lower HHI threshold. The intersection of the hashed rectangle with the black dots in the upper half of the figure show how many likely benign far-proximity transactions (208) are misclassified as "enforce" under the lower threshold.

Enforcement by the complementary 1-mile proximity $\times\Delta$HHI $\geq$ 200 policy is illustrated in the gray-shaded region of the figure, and tabulated in the bottom-two rows of Table A.3. New enforcement driven by the proximity threshold is given by the shaded area in the lower-left quadrant, flagging 1147 likely harmful close-proximity acquisitions for enforcement. Existing enforcement under the HHI threshold is given by the shaded area to right of the Δ200 vertical line. The lower right quadrant illustrates enforceable transactions (716) under the status quo that might be subject to additional remedies based on acquirer-target proximity. None of the likely benign (black dot) transactions in the upper left quadrant are subject to enforcement under the complementary

Table A.3—: Retrospective Tabulations of Current and Alternative Screening Policies

	(1)	(2)	(3)	(4)
	Fed Market ΔHHI $<$ 200		Fed Market ΔHHI $\geq$ 200	
	Distance $\leq$ 4 miles	Distance $>$ 4 miles	Distance $\leq$ 4 miles	Distance $>$ 4 miles
Under current policy:	*Not-Enforce*	*Not-Enforce*	*Enforce*	*Enforce*
Number of merger $\times$ Fed markets	3213	3462	716	213
and ...				
Fed Market ΔHHI $\geq$ 100	416	208	716	213
Fed Market ΔHHI $<$ 100	2797	3254	0	0
County Market ΔHHI $\geq$ 200	307	209	607	144
County Market ΔHHI $<$ 200	2906	3253	109	69
Census Tract Market ΔHHI $\geq$ 200	1321	220	575	37
Census Tract Market ΔHHI $<$ 200	1892	3242	141	176
Distance $\leq$ 2 Miles	2058	0	631	0
Distance $>$ 2 Miles	1155	3462	85	213
Distance $\leq$ 1 Mile	1147	0	498	0
Distance $>$ 1 Mile	2066	3462	218	213

HHI-proximity policy.

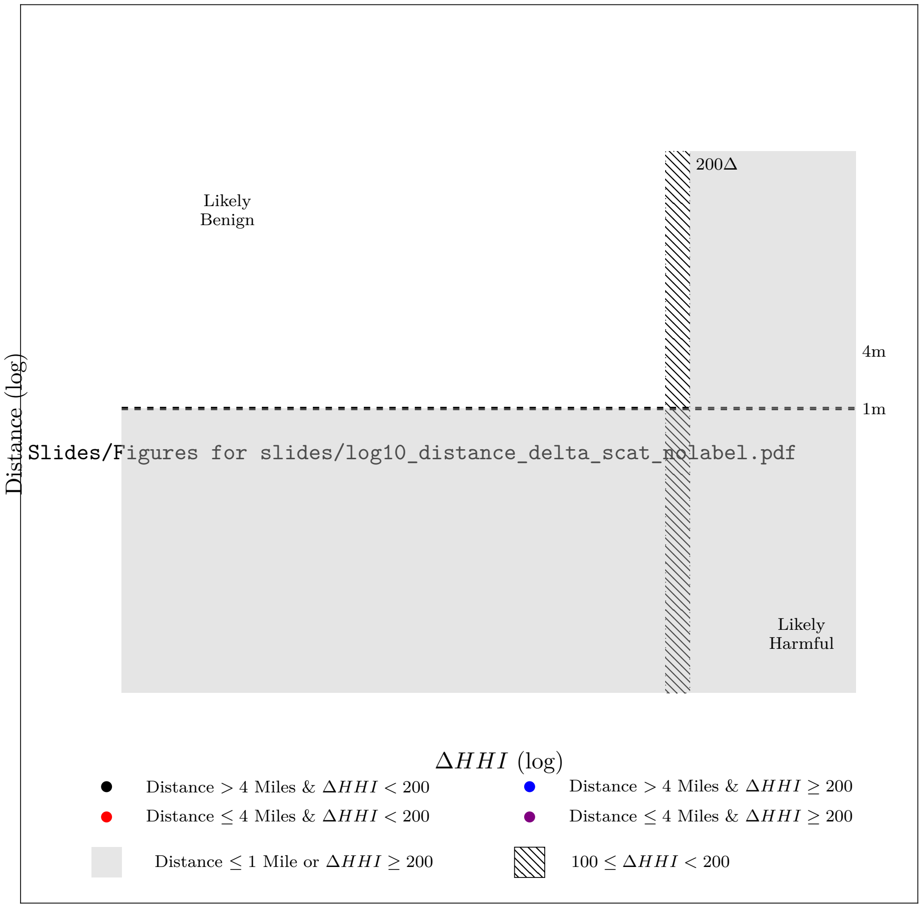


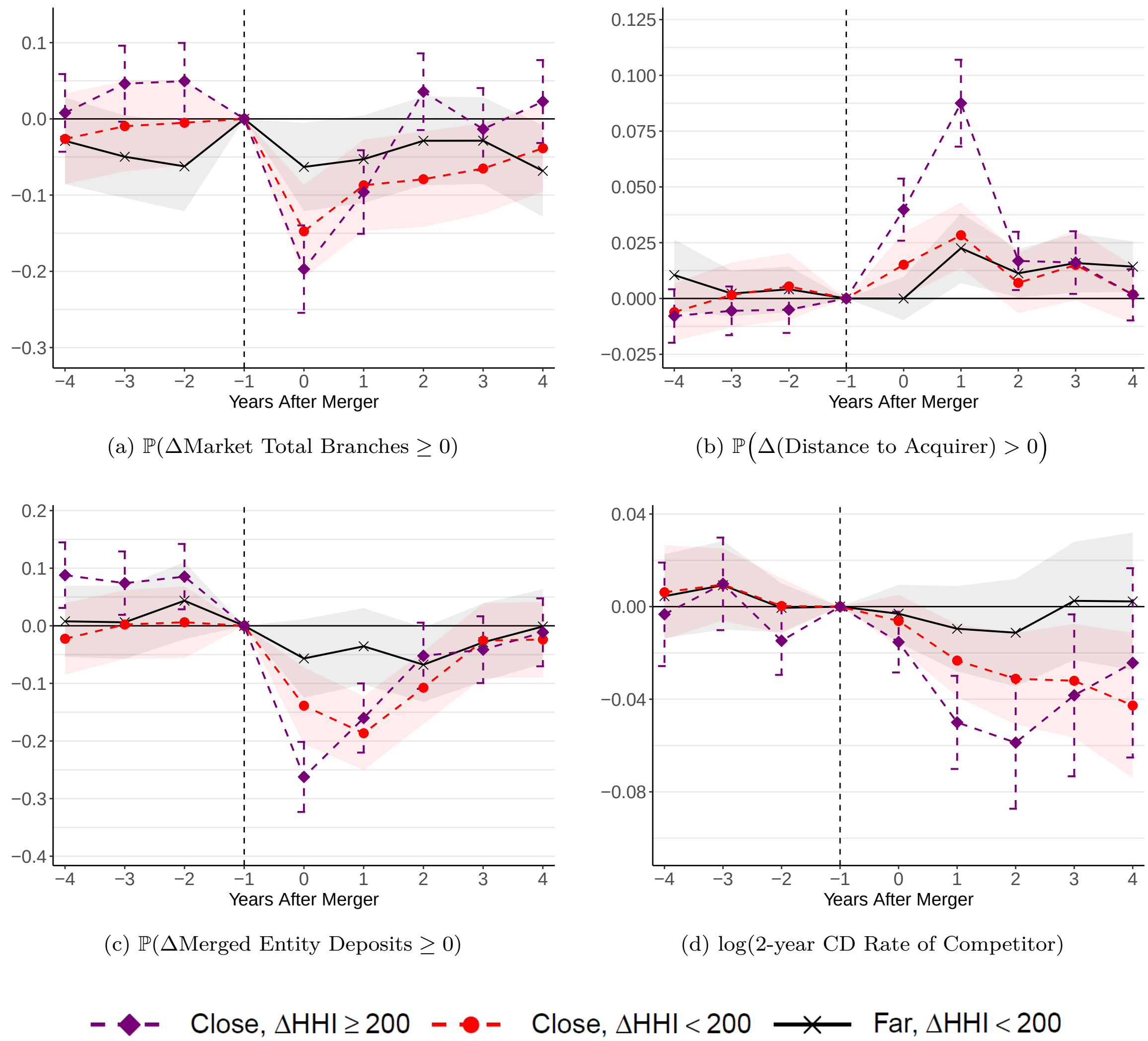
Figure A.1: Counterfactual Enforcement and the Proximity-ΔHHI Distribution

A.3. Robustness analysis and supplemental outcomes

A. Lower ΔHHI Threshold

Our main specification uses a ΔHHI $\geq$ 200 threshold as a simplification of current antitrust screening criteria. However, because of the correlation between proximity and ΔHHI, one might argue that it is possible to adequately screen close-proximity mergers by lowering the ΔHHI threshold. To investigate this possibility, Figure A.2 illustrates DID regression estimates when we define bins using a lower ΔHHI $\geq$ 100 cutoff.

We find that, even among mergers with ΔHHI $<$ 100, close-proximity mergers still lead to outcomes associated with a larger decrease in consumer welfare: more branch closures, more consumers experiencing increases in distances to their closest branch, a decrease in rivals' deposit rates, and a decrease in merged bank's deposits growth. These results altogether show that lowering the threshold for ΔHHI would not effectively screen out close-proximity mergers that potentially have anticompetitive effects.



(a) $\mathbb{P}(\Delta\text{Market Total Branches} \geq 0)$

(b) $\mathbb{P}\big(\Delta(\text{Distance to Acquirer}) > 0\big)$

(c) $\mathbb{P}(\Delta\text{Merged Entity Deposits} \geq 0)$

(d) log(2-year CD Rate of Competitor)

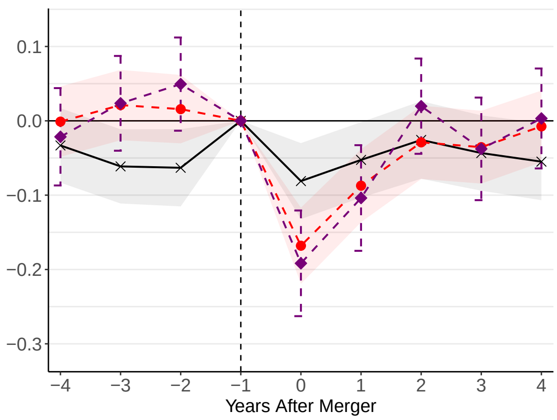
Figure A.2: ΔHHI Threshold of 100

Note: Difference-in-differences estimates and 95 percent confidence intervals from Equation (2) as a function of time (τ) relative to the merger year; $\hat{\beta}^{fl}_{\tau}$ in black, $\hat{\beta}^{cl}_{\tau}$ in red, and $\hat{\beta}^{ch}_{\tau}$ in purple. All standard errors are clustered at the unit of treatment exposure: the merger-market level.

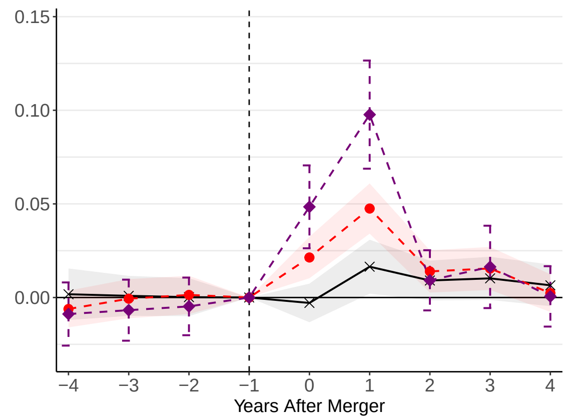
B. *HHI Level Screening Criteria*

As discussed in Section I, the Federal Reserve's merger policy incorporates the HHI level in addition to the change in HHI. Although the change in HHI is the primary investigation trigger, close-proximity mergers with anticompetitive effects might be captured by the other dimensions of the screening policy. To investigate this possibility, instead of classifying mergers based on whether a merger's ΔHHI is above or below 200, we redefine our DID bins using both the change and level criteria: $\Delta\text{HHI} \geq 200$ and $\text{HHI} \geq 1800$.

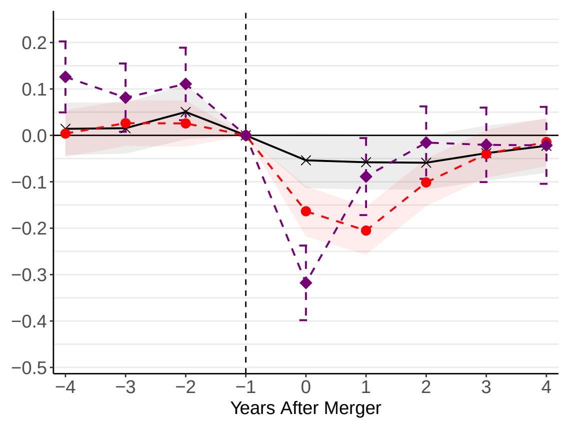
The results from this robustness exercise are illustrated in Figure A.3, and are very similar to our main results. If anything, the results from this robustness exercise suggests that HHI level and market share criteria give safe harbor to even more anticompetitive close-proximity mergers than our main results would indicate.



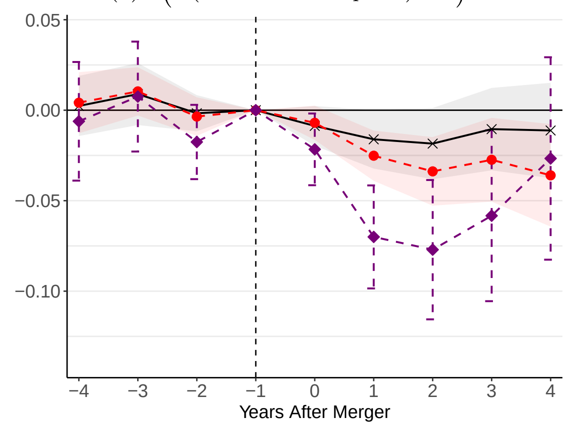
(a) $\mathbb{P}(\Delta\text{Market Total Branches} \geq 0)$



(b) $\mathbb{P}\big(\Delta(\text{Distance to Acquirer}) > 0\big)$



(c) $\mathbb{P}(\Delta\text{Merged Entity Deposits} \geq 0)$



(d) log(2-year CD Rate of Competitor)

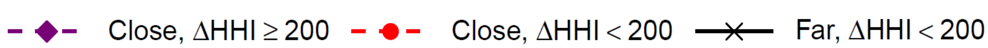
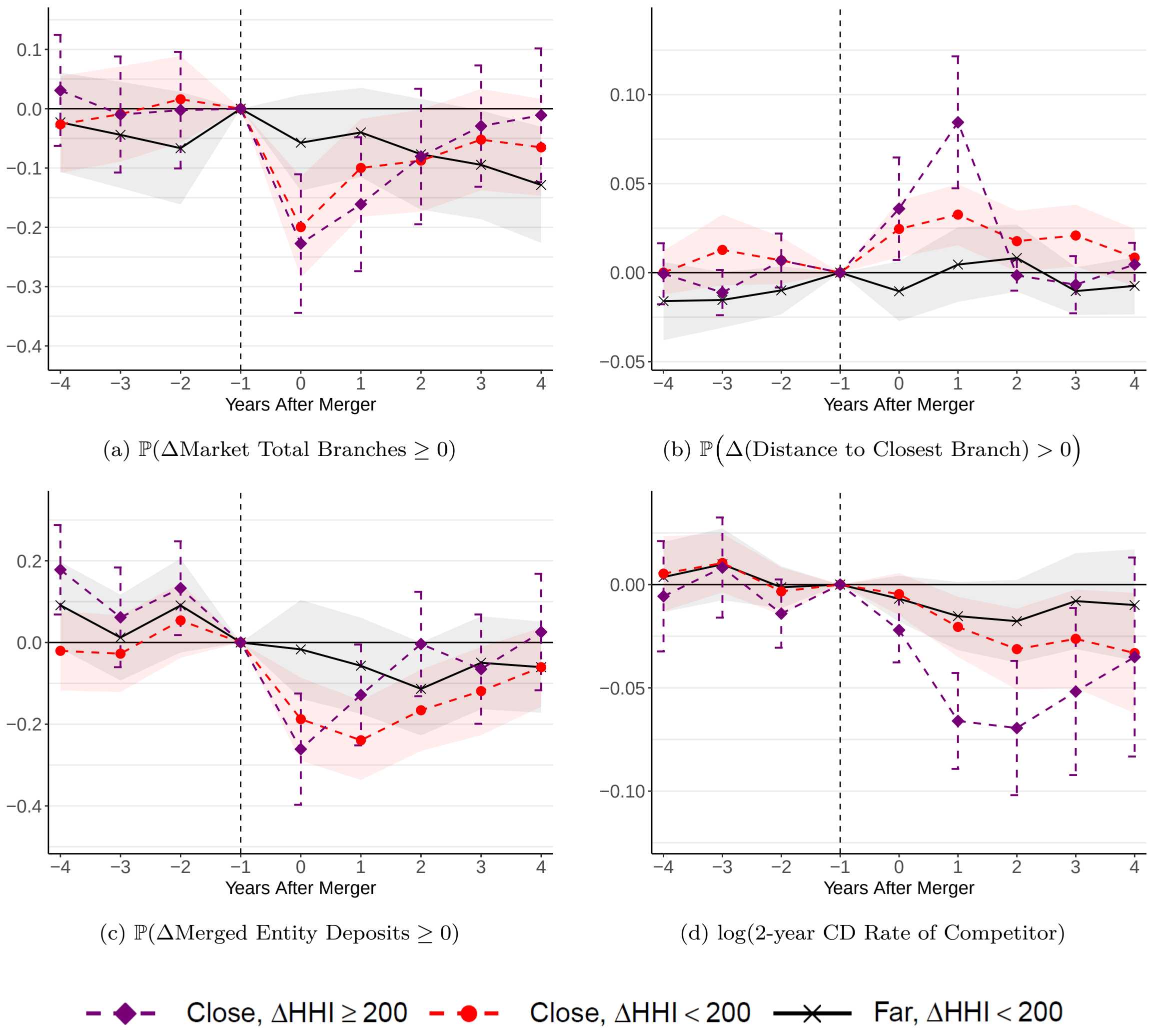


Figure A.3: Federal Reserve Screening Criteria ($\Delta\text{HHI} \geq 200\,\&\,\text{HHI} \geq 1800$)

Note: Difference-in-differences estimates and 95 percent confidence intervals from Equation (2) as a function of time (τ) relative to the merger year; $\hat{\beta}^{fl}_{\tau}$ in black, $\hat{\beta}^{cl}_{\tau}$ in red, and $\hat{\beta}^{ch}_{\tau}$ in purple. All standard errors are clustered at the unit of treatment exposure: the merger-market level.

C. *Evolving Bank Competition - Contemporary Mergers*

Our main sample covers the period 1980 to 2014. Some aspects of bank competition clearly evolved over our sample horizon. For example, due to the rise of internet banking, branch network proximity might play a more limited role in modern bank substitution patterns. To study the robustness of our results to the evolving nature of bank competition, Figure A.4 presents our regression estimates over a later time period, 2000 to 2014. We find that these results are very similar to our main estimates, suggesting that geographic competition is still an important determinant of the effects of mergers in the banking industry.



(a) $\mathbb{P}(\Delta\text{Market Total Branches} \geq 0)$ (b) $\mathbb{P}\Big(\Delta(\text{Distance to Closest Branch}) > 0\Big)$

(c) $\mathbb{P}(\Delta\text{Merged Entity Deposits} \geq 0)$ (d) log(2-year CD Rate of Competitor)

Figure A.4: Sample Period 2000-2014

Note: Difference-in-differences estimates and 95 percent confidence intervals from Equation (2) as a function of time (τ) relative to the merger year; $\hat{\beta}^{fl}_{\tau}$ in black, $\hat{\beta}^{cl}_{\tau}$ in red, and $\hat{\beta}^{ch}_{\tau}$ in purple. All standard errors are clustered at the unit of treatment exposure: the merger-market level.

D. Alternative Measures of "Close" Proximity

If the geographic scope of a market is misspecified, then mergers might be misclassified as high/low ΔHHI. As a result, some of the mergers that are close proximity and low ΔHHI in the data might, under a more accurate market definition, be mergers that are close proximity and high ΔHHI. This potential misclassification does not change our argument in relation to current policy, since antitrust authorities use geographic Fed banking market definitions in merger analysis. However, the broader applicability of our results for other antitrust in industries might be sensitive to misspecified banking market definitions.

Census Tract Proximity: To investigate the role of proximity controlling for potential misclassification of high/low ΔHHI, we first study outcome DIDs between census tracts within overlap Fed banking markets. This strategy allows outcomes to evolve arbitrarily at the local Fed market-year level, fully controlling for any dependence on market-level determinants like the policy relevant ΔHHI, whether or not such determinants are measured with error. We classify three types of census tracts: tracts where both merging banks had operations (close proximity), tracts where only one of the banks operated (far proximity), and tracts where neither bank operated (control group).

The within-market census tract-level DID results, presented in Figure A.5, closely mirror our findings from between-market DIDs. Bank mergers cause more branch destruction in tracts where the merging banks both operated before the merger, even after allowing for arbitrary variation in branches and deposits at the market level. This suggests that the proximity of the merging banks is an important determinant of merger outcomes for any plausible predefined geographic market definition that satisfies the usual hypothetical monopolist test. Therefore, even if the Fed banking markets were more narrowly defined, an HHI-based screen could still be improved by using data on the proximity of the merging banks. Further details on this analysis are provided below.

The census tract analysis focuses on overlap markets in the sample for the main analysis, and estimates the following regression:

$$y_{emct} = \sum_{\tau=-4}^{4} \mathbf{1}[t = t_e^* + \tau] \times (Both_{emc} \times \beta_{\tau-Both} + One_{emc} \times \beta_{\tau-One}) + \xi_{emc} + \xi_{emt} + \epsilon_{emct}. \tag{A.1}$$

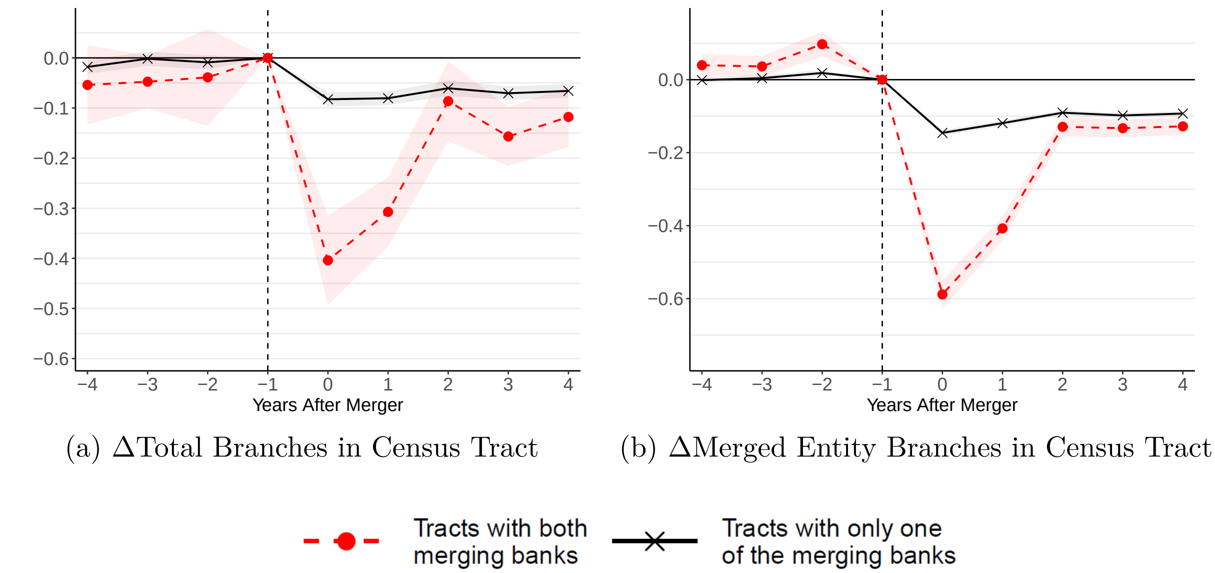
Here, e denotes a merger, m a market, c a census tract, and t a year. There are two treatment dummies. $Both_{emc}$ is a dummy for census tracts where both merging banks operate. One_{emc} is a dummy for census tracts where either the acquirer or the target has a branch presence, but not both. The control group consists of tracts in the same overlap market where neither party operates a branch.

The fixed effects, ξ_{emc} and ξ_{emt} control for time invariant effects that are specific to a combination of merger and a census tract, and for time-varying effects that are specific to a combination of a merger and a market. The standard errors are clustered at the merger-tract level, the unit of exposure.

Figure A.5 plots the estimated coefficient together with the 95% confidence interval. The estimated coefficients for tracts where both merging banks operate ($\beta_{\tau-Both}$) are shown in red, and the coefficients for tracts where only one of them operates are shown in black.

In panel (a), the outcome variable is the year-over-year change in the total number of branches in a tract. In census tracts where both merging banks had branches, the number of branches decreases by 0.4 in the year of the merger, by 0.3 one year after the merger, and by about 0.1 in subsequent years. This graph shows that branch closings are concentrated in census tracts where both merging banks had branches.

Panel (b) shows the effect on the year-over-year change in the number of branches of the merging banks, which evolves in parallel to the estimates in panel (a). This result suggests that the reduction in the number of branches is driven by branch closures of the merged bank. Notice, however, that the number of branches of the merging banks decreases more than the total number of branches. The merged bank decreases the number of branches by 0.6 in the year following the merger and by 0.4 two years after the merger. The differences between panels (a) and (b) suggest that some branch closures of the merging banks are offset by branch openings of rivals that move into the opened territory, which is consistent with the findings in the main estimation specification.

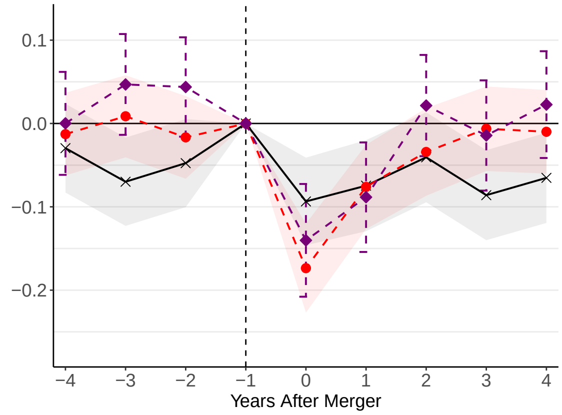


(a) ΔTotal Branches in Census Tract (b) ΔMerged Entity Branches in Census Tract

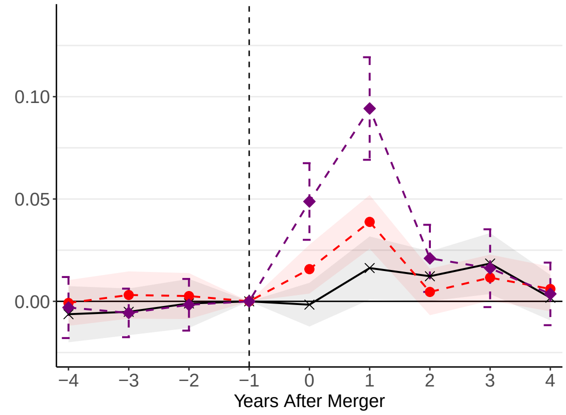
Figure A.5: Within-Market Analysis of Branch Closures, with Close-Proximity Measured by Census Tract Overlap

Note: Difference-in-differences estimates and 95% confidence intervals from Equation (A.1) as a function of time (τ) relative to the merger year; $\beta_{\tau-Both}$ in red and $\beta_{\tau-one}$ in black. All standard errors are clustered at the unit of treatment exposure: the merger-tract level.

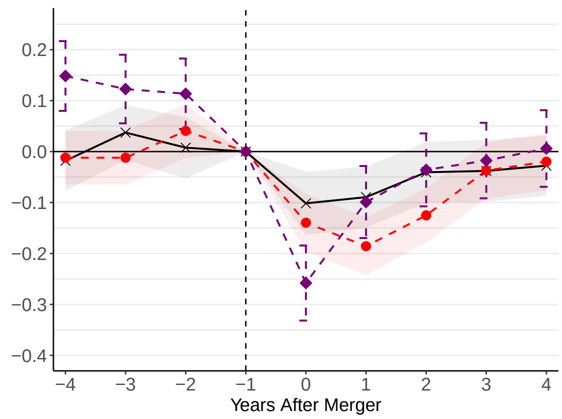
Market-specific Thresholds of Close Proximity: Our main specification distinguishes above from below-median acquirer-target proximity using the distribution of all overlap markets and merger events, resulting in an overall merger-median proximity cutoff just under 4 miles. To further investigate the predictive power of of close-proximity controlling for potential inaccuracy of market definitions, we secondly considered a relative notion of proximity using market-specific distributions of bank distances. For this robustness exercise, close-proximity is defined by distance between merging banks that is less than the median distance across all banks operating within the given overlap market. Figure A.6 shows that mergers that are close in terms of the within-market notion of proximity have similar anti-competitive effects as our main results.



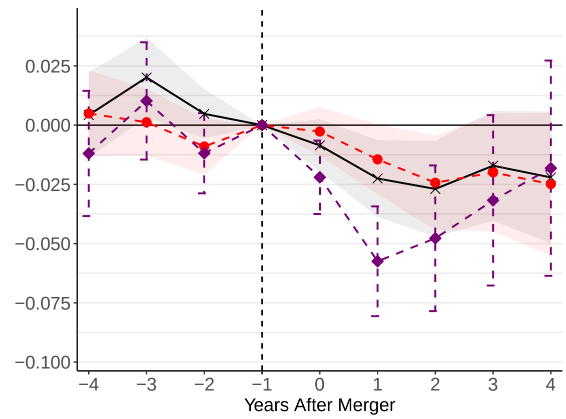
(a) $\mathbb{P}(\Delta\text{Market Total Branches} \geq 0)$



(b) $\mathbb{P}\big(\Delta(\text{Distance to Acquirer}) > 0\big)$



(c) $\mathbb{P}(\Delta\text{Merged Entity Deposits} \geq 0)$



(d) log(2-year CD Rate of Competitor)

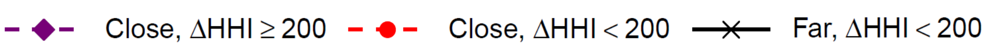


Figure A.6: Within-Market Proximity Measure

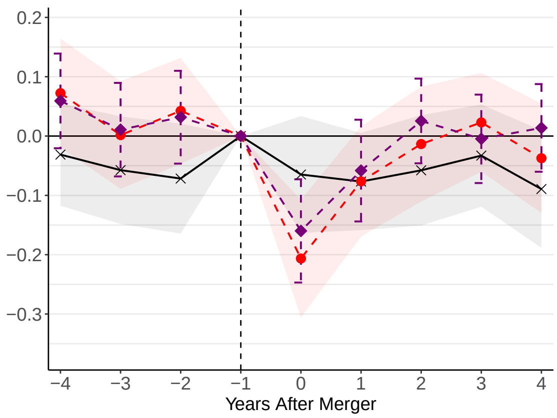
Note: Difference-in-differences estimates and 95 percent confidence intervals from Equation (2) as a function of time (τ) relative to the merger year; $\hat{\beta}^{fl}_{\tau}$ in black, $\hat{\beta}^{cl}_{\tau}$ in red, and $\hat{\beta}^{ch}_{\tau}$ in purple. All standard errors are clustered at the unit of treatment exposure: the merger-market level.

E. Alternative Sample Selection

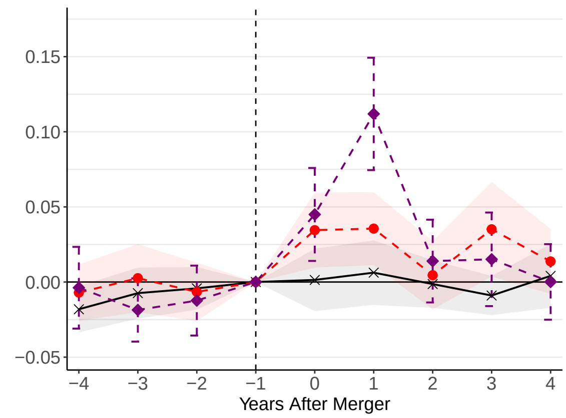
As discussed in Section III, the main sample excludes merger event - market observations (e, m) that experienced other bank mergers in the year of or in the year prior to the focal merger event (t^*_e and $t^*_e - 1$). In a stacked panel design, these are the weakest necessary conditions to focus on identifying variation from observations that "switch" treatment status. We consider alternative sample criteria for robustness.

First, we investigate a stronger counterpart of our main selection criteria, by excluding markets that experienced any other bank mergers over the regression horizon from $t^*_e - 4$ to $t^*_e + 4$. Figure A.7 reports results for this alternative sample, and they are qualitatively unchanged from our main findings.

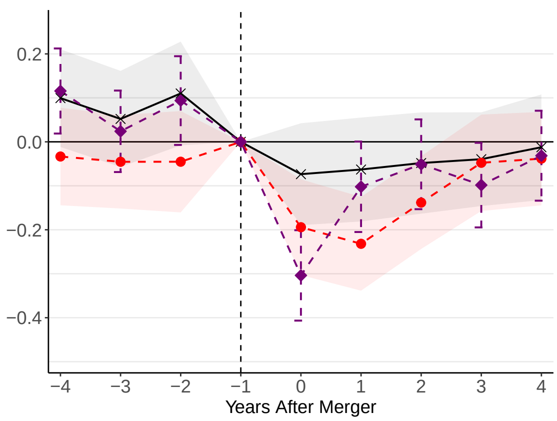
Second, we investigate whether the predictive power of proximity and HHIs can be attributed to other systematic differences between markets. For example, the sample balance Table A.1 shows



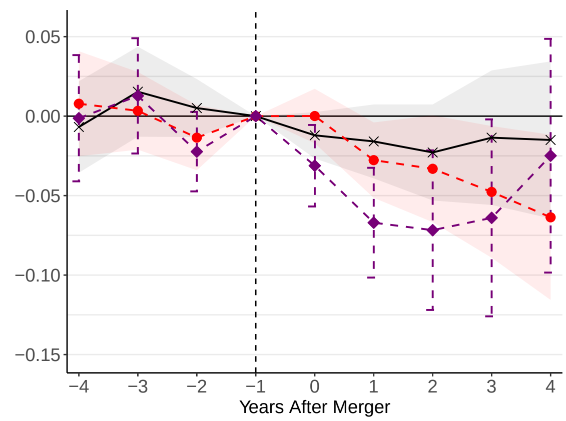
(a) $\mathbb{P}(\Delta\text{Market Total Branches} \geq 0)$



(b) $\mathbb{P}\big(\Delta(\text{Distance to Acquirer}) \geq 0\big)$



(c) $\mathbb{P}(\Delta\text{Merged Entity Deposits} \geq 0)$



(d) log(2-year CD Rate of Competitor)

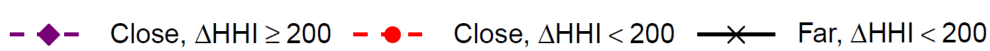
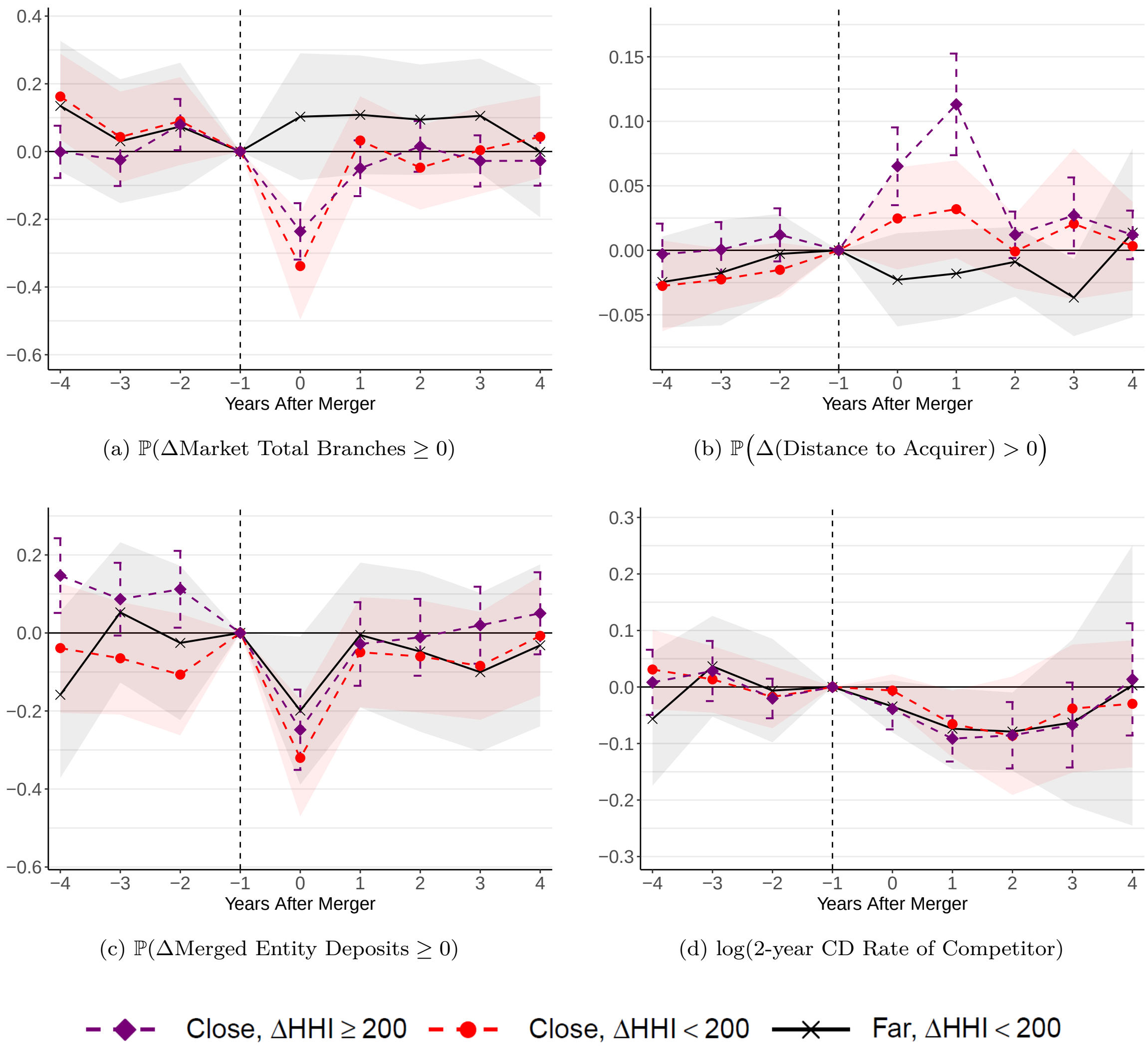


Figure A.7: No Other Mergers in the Sample Window

Note: Difference-in-differences estimates and 95% confidence intervals from Equation (2) as a function of time (τ) relative to the merger year; $\hat{\beta}^{fl}_{\tau}$ in black, $\hat{\beta}^{cl}_{\tau}$ in red, and $\hat{\beta}^{ch}_{\tau}$ in purple. All standard errors are clustered at the unit of treatment exposure: the merger-market level.

that markets with different degrees of proximity and changes in concentration are also different in other dimensions that are mainly related to market size. We study robustness to market size by restricting the regression sample to markets that have eight or fewer banks the year before the merger, the the median market size in our data. Table A.2 shows that different proximity-HHI groups are much more comparable to one another with this alternative sample selection. Figure A.8 reports results from this alternative sample, and they are also qualitatively unchanged from the main analysis.



(a) $\mathbb{P}(\Delta\text{Market Total Branches} \geq 0)$

(b) $\mathbb{P}\big(\Delta(\text{Distance to Acquirer}) > 0\big)$

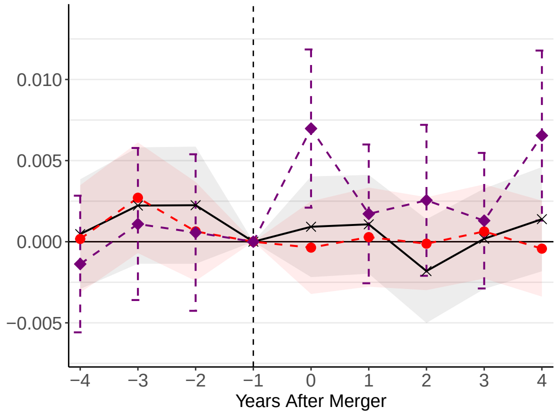
(c) $\mathbb{P}(\Delta\text{Merged Entity Deposits} \geq 0)$

(d) log(2-year CD Rate of Competitor)

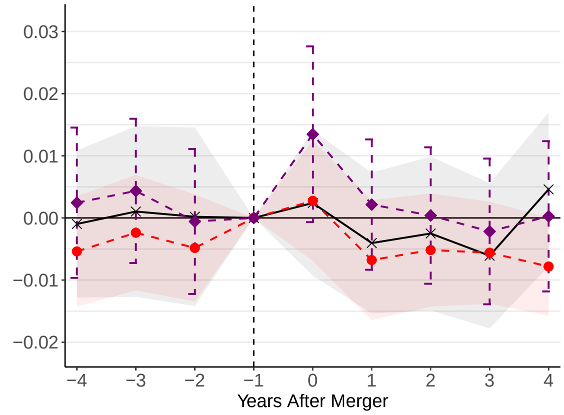
Figure A.8: Markets with 8 or Fewer Banks

Note: Difference-in-differences estimates and 95% confidence intervals from Equation (2) as a function of time (τ) relative to the merger year; $\hat{\beta}^{fl}_{\tau}$ in black, $\hat{\beta}^{cl}_{\tau}$ in red, and $\hat{\beta}^{ch}_{\tau}$ in purple. All standard errors are clustered at the unit of treatment exposure: the merger-market level.

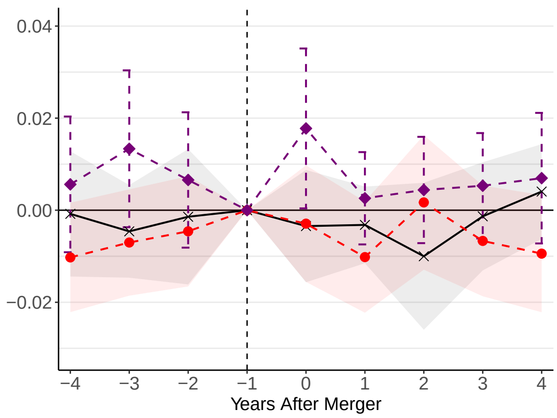
F. *Results for additional outcomes*



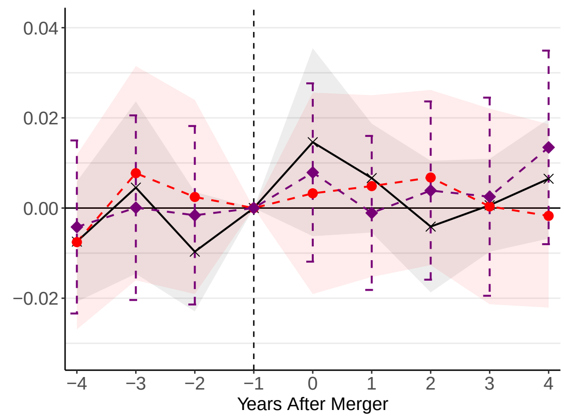
(a) Average Competitor



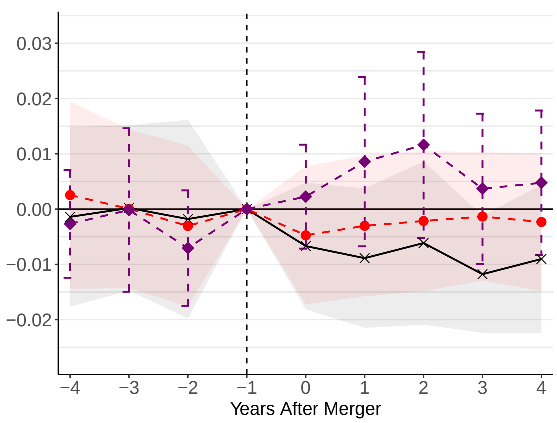
(b) Largest Competitor



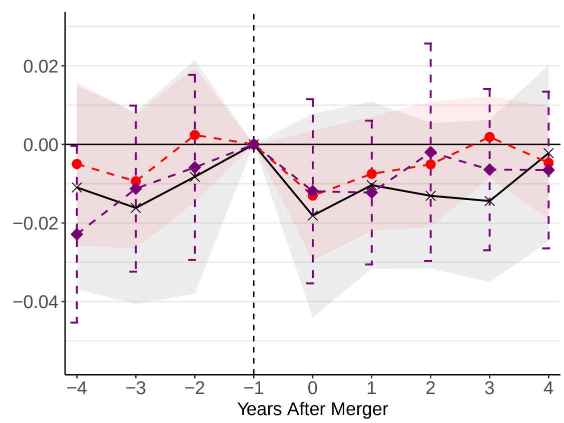
(c) Acquirer's Closest Competitor



(d) Target's Closest Competitor



(e) Acquirer's Farthest Competitor



(f) Target's Farthest Competitor

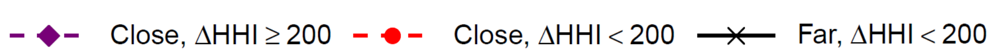


Figure A.9: Increase in Consumer Distance to Competitor Networks: $\mathbb{P}(\Delta(\text{Distance to ...}) \geq 0)$

Note: Difference-in-differences estimates and 95% confidence intervals from Equation (2) as a function of time (τ) relative to the merger year; $\hat{\beta}^{fl}_{\tau}$ in black, $\hat{\beta}^{cl}_{\tau}$ in red, and $\hat{\beta}^{ch}_{\tau}$ in purple. All standard errors are clustered at the unit of treatment exposure: the merger-market level.

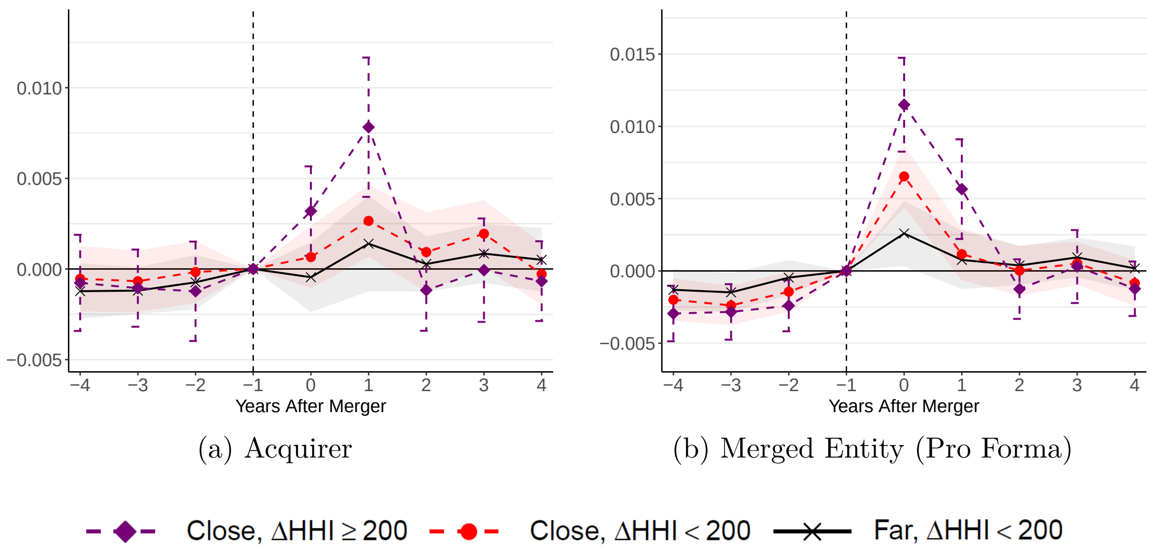
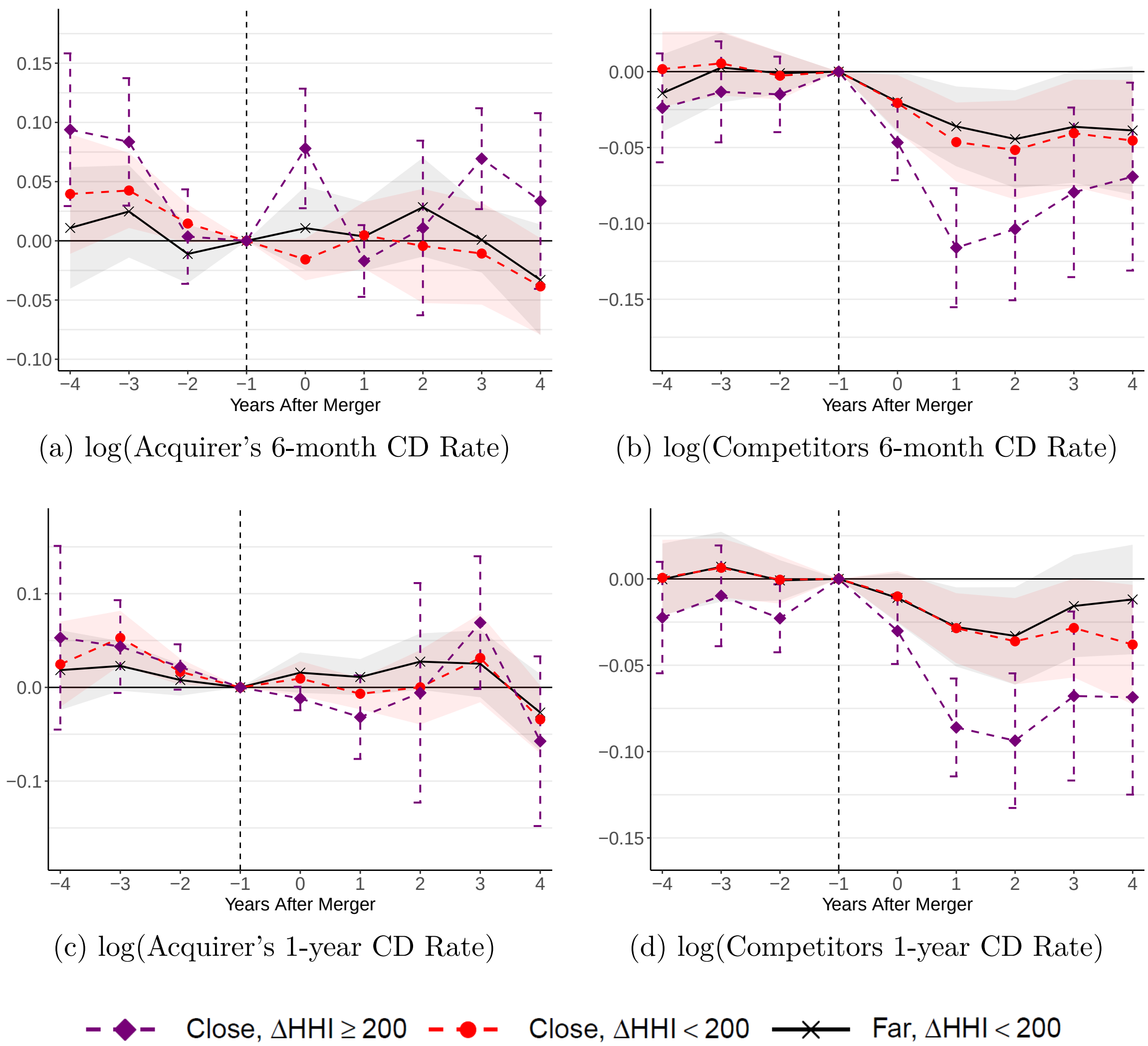


Figure A.10: Fraction Losing 1-mile Access: $\mathbb{P}(\text{Distance}_{t-1} \leq 1 \,\&\, \text{Distance}_t > 1)$

Note: Difference-in-differences estimates and 95% confidence intervals from Equation (2) as a function of time (τ) relative to the merger year; $\hat{\beta}^{fl}_{\tau}$ in black, $\hat{\beta}^{cl}_{\tau}$ in red, and $\hat{\beta}^{ch}_{\tau}$ in purple. All standard errors are clustered at the unit of treatment exposure: the merger-market level.



(a) log(Acquirer's 6-month CD Rate)

(b) log(Competitors 6-month CD Rate)

(c) log(Acquirer's 1-year CD Rate)

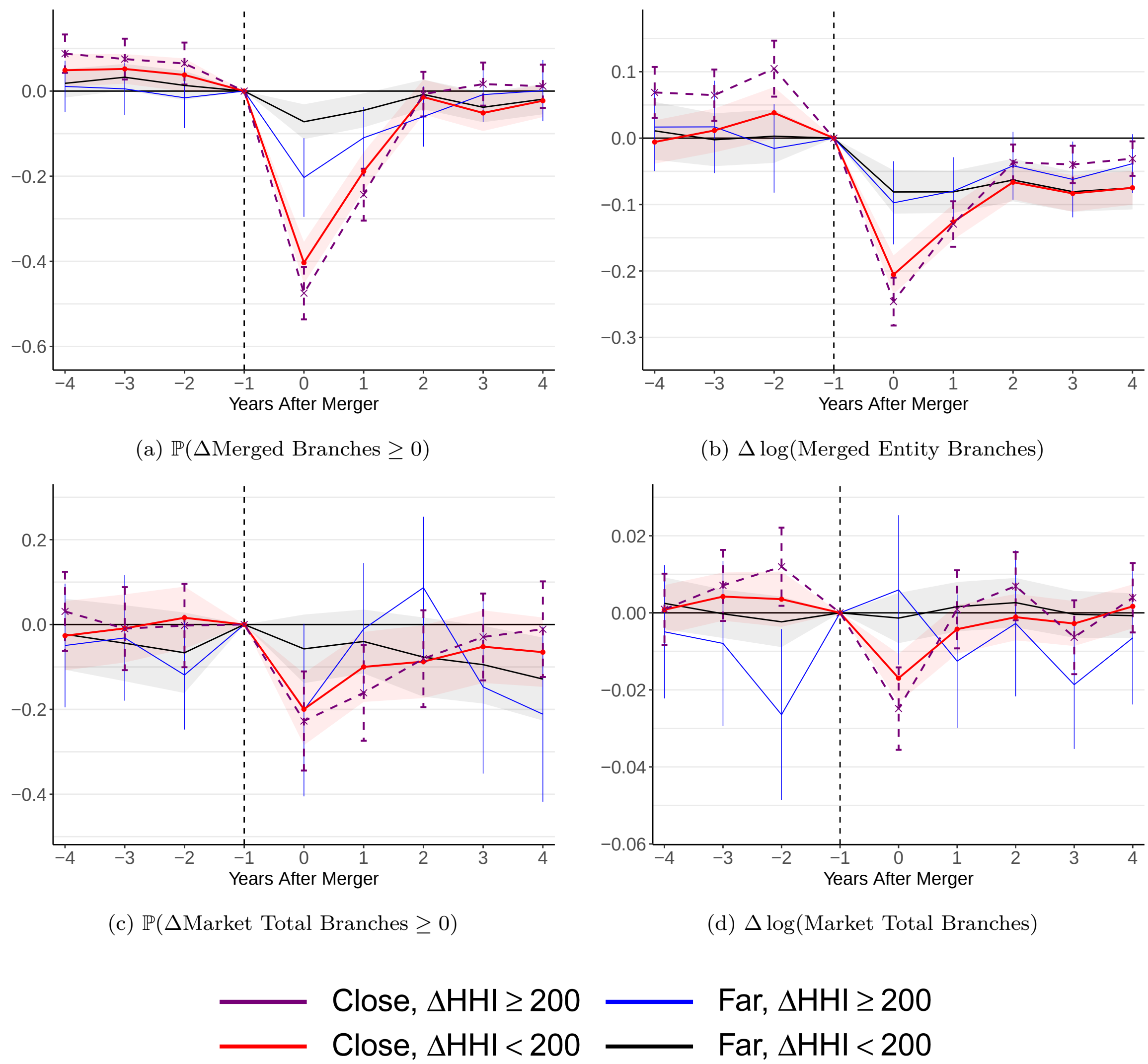
(d) log(Competitors 1-year CD Rate)

Figure A.11: More Deposit Price Outcomes

Note: Difference-in-differences estimates and 95% confidence intervals from Equation (2) as a function of time (τ) relative to the merger year; $\hat{\beta}_{\tau}^{fl}$ in black, $\hat{\beta}_{\tau}^{cl}$ in red, and $\hat{\beta}_{\tau}^{ch}$ in purple. All standard errors are clustered at the unit of treatment exposure: the merger-market level.

A.4. Results for far-proximity ΔHHI ≥ 200 mergers

This appendix presents the full set of regression estimates corresponding to the figures in the main text. We discuss results for far-proximity ΔHHI ≥ 200 mergers, plotted in blue in the figures, because these were omitted from the main text. Altogether, our results do not suggest that far-proximity ΔHHI ≥ 200 mergers are benign. For some outcome measures, there is statistically significant evidence of harm. However, estimates for this group are typically less precise.



(a) $\mathbb{P}(\Delta\text{Merged Branches} \geq 0)$

(b) $\Delta\log(\text{Merged Entity Branches})$

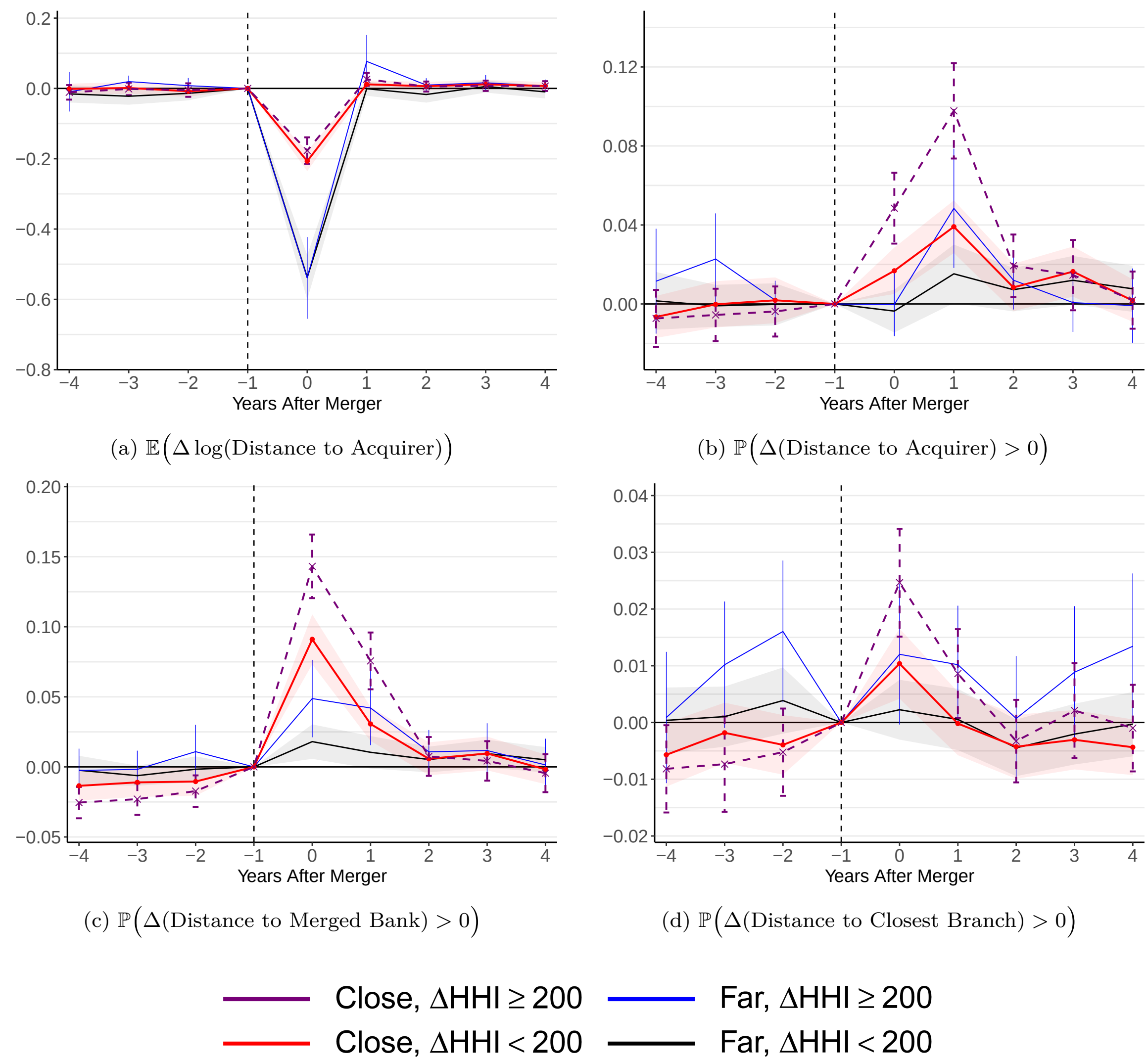
(c) $\mathbb{P}(\Delta\text{Market Total Branches} \geq 0)$

(d) $\Delta\log(\text{Market Total Branches})$

Figure A.12: Branch Closures

Note: Difference-in-differences estimates and 95% confidence intervals from equation (2) as a function of time (τ) relative to the merger year; $\hat{\beta}^{fl}_{\tau}$ in black, $\hat{\beta}^{cl}_{\tau}$ in red, $\hat{\beta}^{fh}_{\tau}$ in blue, and $\hat{\beta}^{ch}_{\tau}$ in purple. All standard errors are clustered at the unit of treatment exposure: the merger-market level.

Figure A.12 presents results for branch closure outcomes. Panel (a) suggests that far-proximity ΔHHI ≥ 200 caused a 20 pp increase in the probability of branch closures in the year of the merger. This effects is about half of the effect of close-proximity mergers, and is statistically different from the effect of far-proximity ΔHHI < 200 transactions. Evidence in panels (b)-(d) of Figure A.12 is more mixed, in part because of standard errors for the far-proximity ΔHHI $\geq$ 200 estimates.



(a) $\mathbb{E}\big(\Delta \log(\text{Distance to Acquirer})\big)$

(b) $\mathbb{P}\big(\Delta(\text{Distance to Acquirer}) > 0\big)$

(c) $\mathbb{P}\big(\Delta(\text{Distance to Merged Bank}) > 0\big)$

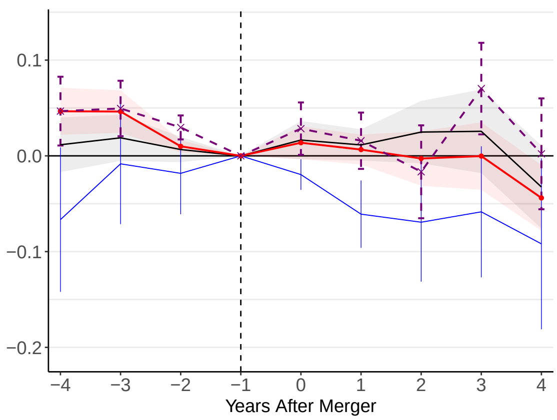
(d) $\mathbb{P}\big(\Delta(\text{Distance to Closest Branch}) > 0\big)$

Figure A.13: Distance Between Consumers and Branches

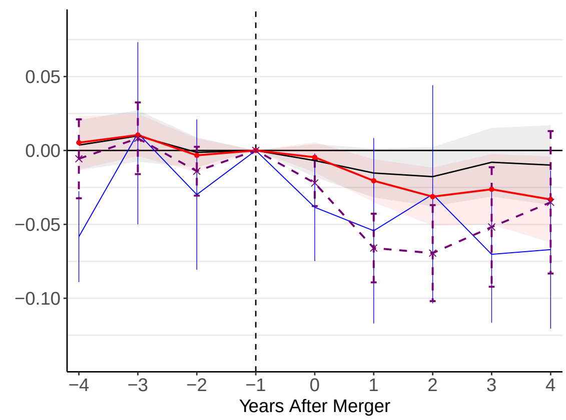
Note: Difference-in-differences estimates and 95% confidence intervals from equation (2) as a function of time (τ) relative to the merger year; $\hat{\beta}^{fl}_{\tau}$ in black, $\hat{\beta}^{cl}_{\tau}$ in red, $\hat{\beta}^{fh}_{\tau}$ in blue, and $\hat{\beta}^{ch}_{\tau}$ in purple. All standard errors are clustered at the unit of treatment exposure: the merger-market level.

Figure A.13 presents the full set of estimate for consumer convenience outcomes. In panel (a), we find that far-proximity mergers have similar convenience synergies whether or not the transaction significantly increased the HHI. This simply reflects the fact that the branches of both merging parties were far apart prior to the merger.

However, effects on outcomes analyzed in panels (b)-(d) suggest that far-proximity ΔHHI $\geq$ 200 transactions do harm some consumers. One year after the merger, in panel (b), distance to the acquirer's branch network increases for 4% of consumers, on average, after far-proximity ΔHHI $\geq$ 200 mergers. This a statistically significant effect is comparable to the harm of close-proximity ΔHHI $<$ 200 transactions. In panel (c), distances to the merged branch network increase for (cumulative) 10% of consumers within 2 years of consummation of far-proximity ΔHHI $\geq$ 200 mergers, significantly more harm than far-proximity mergers that did not increase the HHI more than 200 points. The effects of far-proximity ΔHHI $\geq$ 200 on consumer distances to their closest branch of any bank in the market statistically imprecise.



(a) log(2-year CD Rate of Acquirer)



(b) log(2-year CD Rate of Competitors)

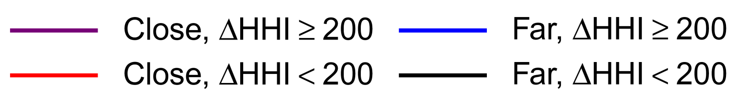
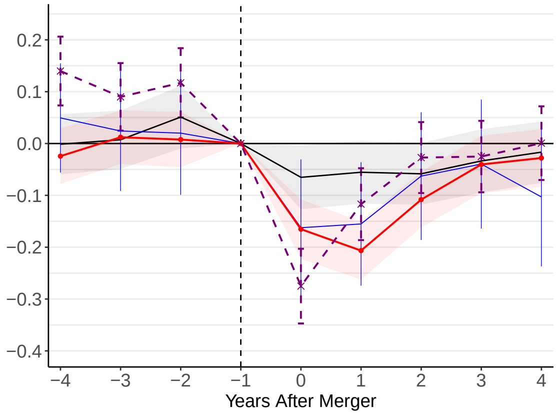


Figure A.14: Deposit Interest Rates

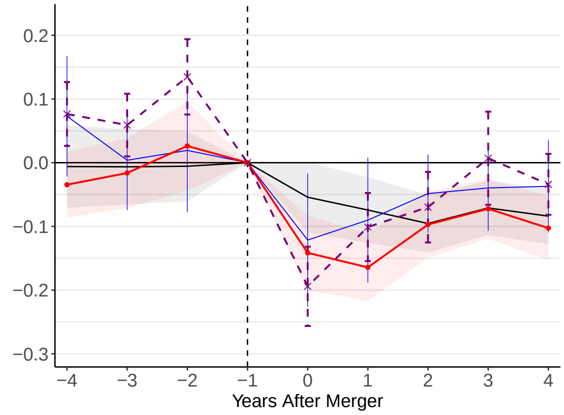
Note: Difference-in-differences estimates and 95% confidence intervals from equation (2) as a function of time (τ) relative to the merger year; $\hat{\beta}_\tau^{fl}$ in black, $\hat{\beta}_\tau^{cl}$ in red, $\hat{\beta}_\tau^{fh}$ in blue, and $\hat{\beta}_\tau^{ch}$ in purple. All standard errors are clustered at the unit of treatment exposure: the merger-market level.

Figure A.14 presents results for 2 year CD interest rates, the price outcome analyzed in the main text. Panel (a) suggests that far-proximity ΔHHI $\geq$ 200 mergers significantly lowered deposit interest rates offered by the acquirer bank, harming consumers. This is in contrast to the results for the other three categories of mergers. In panel (b), although standard errors for far-proximity ΔHHI $\geq$ 200 estimates are larger, several results are statistically different from zero. This suggests that far-proximity ΔHHI $\geq$ 200 also cause competitors to offer lower deposit rates.

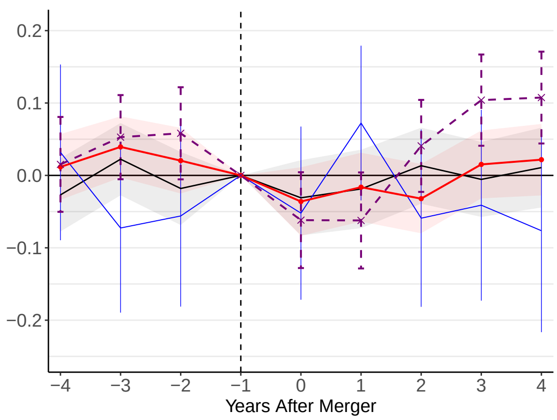
Finally, Figure A.15 presents results for deposits quantities. Panels (a) and (b) show reduced deposits growth for the merged banks. The point estimates suggest that far-proximity ΔHHI $\geq$ 200 mergers have similar, harmful effects on deposits growth as close-proximity ΔHHI $<$ 200 transactions. However, confidence intervals on the estimates do not reject the hypothesis that effects are similar to more benign far-proximity mergers (black line) that did not exceed the HHI threshold for enforcement. Confidence intervals for effects on total market deposits growth, panels (c) and (d), are again too wide to draw conclusions for far-proximity ΔHHI $\geq$ 200.



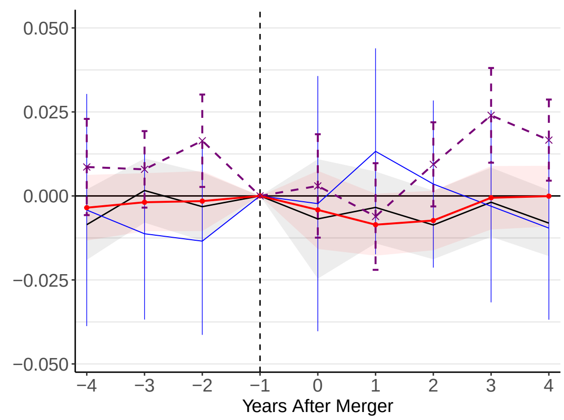
(a) $\mathbb{P}(\Delta(\text{Merged Entity Deposits}) \geq 0)$



(b) $\Delta \log(\text{Merged Entity Deposits})$



(c) $\mathbb{P}(\Delta(\text{Market Total Deposits}) \geq 0)$



(d) $\Delta \log(\text{Market Total Deposits})$

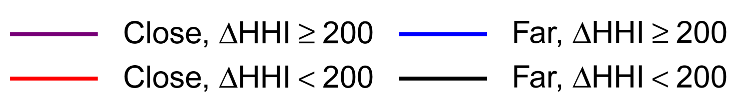


Figure A.15: Deposits Run-Off

Note: Difference-in-differences estimates and 95% confidence intervals from equation (2) as a function of time (τ) relative to the merger year; $\hat{\beta}_{\tau}^{fl}$ in black, $\hat{\beta}_{\tau}^{cl}$ in red, $\hat{\beta}_{\tau}^{fh}$ in blue, and $\hat{\beta}_{\tau}^{ch}$ in purple. All standard errors are clustered at the unit of treatment exposure: the merger-market level.

A.5. Data appendix: identifying mergers

Our main analysis studies mergers that occur between 1984 and 2010. We identify mergers from the SOD dataset. We take advantage of a branch identifier created by the Federal Reserve that identifies branches by their locations. In other words, this identifier is time and ownership invariant and therefore allows us reliably to track how branches change ownership over time. When a bank disappears from the SOD, it is almost always in the event of a merger. Using the branch identifier in the SOD, we define as the acquirer in such events the bank that acquires the plurality of branches from the disappearing bank, and we define as the target the disappearing bank.

During our sample period, the number of banks in the U.S. declined from 16,729 in 1980 to 6,165 in 2014. We identify 11,513 mergers between 1984 and 2014. Of these mergers, 6,182 involve at least one “overlap” market—that is, a market in which both merging parties operate at the time of the merger. Despite the tremendous number of mergers, the average deposit HHI in Fed markets has remained at a level around 2,500 for the entire sample period, which can be seen in Figure A.16.

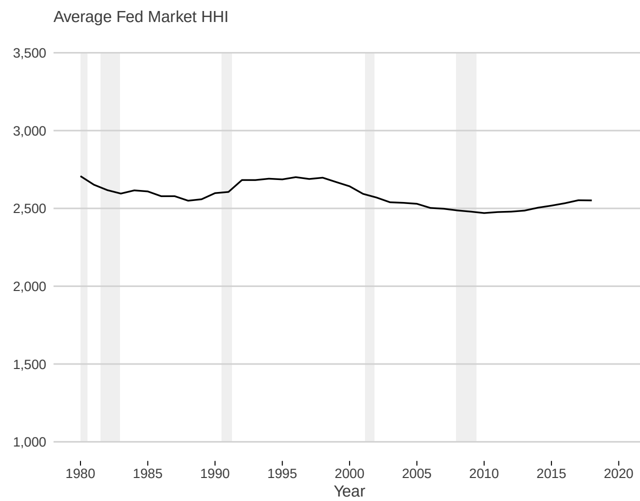


Figure A.16: Market Concentration 1980-2018

Note: The average Herfindahl-Hirschman Index (HHI) in each year across 1,518 Fed markets. The HHI uses deposit market shares derived from the FDIC’s annual SOD data. Vertical bars represent recessions as defined by the National Bureau of Economic Research.

A.6. Stacked design & DIDs with heterogeneity

This appendix discusses the econometric properties of our "stacked" event empirical strategy and regression specification (2). If there is heterogeneity in timing or exposure, a two-way fixed effects regression estimator of the DID can produce biased estimates of the average treatment effect on the treated (ATT) in a regular panel design (de Chaisemartin and D'Haultfœuille, 2020; Goodman-Bacon, 2021).

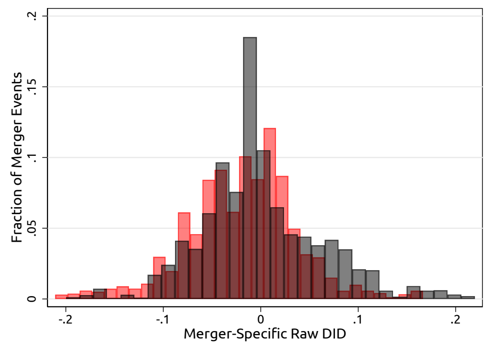
We show that there is observable heterogeneity in merger-specific DID of raw means in our data, as well as variation in treatment timing across merger events in our sample period (Figure A.17). To investigate the performance of the "stacked" design in addressing potential bias from these features of the data, we use results from de Chaisemartin and D'Haultfœuille (2020) to estimate the weights that a two-way fixed effects regression places on observational-level DIDs. The weight estimates confirm the potential bias of an empirical strategy that uses an "un-stacked" traditional panel design. About 20 percent of treated markets have negative weights in the market-year panel regression (Figure A.18). In addition, we find that all weights are positive (and sum to 1) after "stacking" the market-year panels across merger events and using a regression with event-specific two-way fixed effects. We present details below.

Heterogeneity in Raw DIDs. — There are several thousand merger events in our data, each with its own treatment and control markets. For a given outcome, a DID exists for each of these events. We examine the estimator

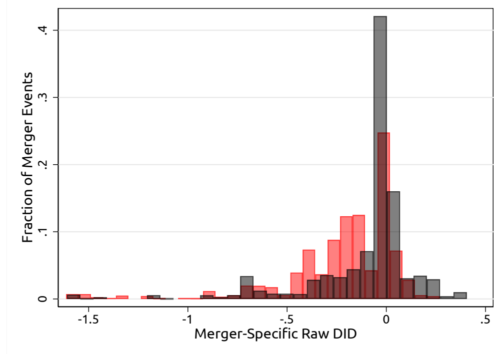
$$DID_{e,\tau} = (\bar{y}_{e,\tau} - \bar{y}_{e,-1}) - (\tilde{y}_{e,\tau} - \tilde{y}_{e,-1})$$

where $\bar{y}_{e,\tau} = \mathbb{E}(y_{emt} | t = t^*_e + \tau, m \in \text{treatment group of } e)$ and $\tilde{y}_{e,\tau} = \mathbb{E}(y_{emt} | t = t^*_e + \tau, m \in \text{control group of } e)$ is a DID of raw means for event e measured τ years after the merger. Sample counterparts of $\bar{y}_{e,\tau}$ and $\tilde{y}_{e,\tau}$ can also be conditioned on covariates like acquirer-target proximity or the merger's effect on the HHI. Hence, one can observe heterogeneity in "raw" event-specific DIDs.

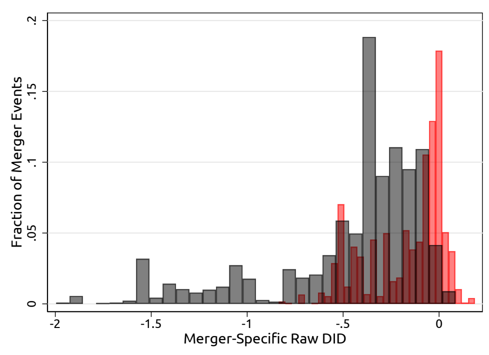
Figure A.17 provides evidence of heterogeneity in DIDs for four outcomes related to those studied in Section IV. We illustrate the empirical densities of merger-year treatment effects $\widehat{DID}_{e,0}$, computed separately by acquirer-target proximity for the following: in panel (a), growth in market total branches; in panel (b), growth in the number of branches operated by the merged entity; in panel (c), the percent change in the distance between consumers and the acquirer's branch network; and in panel (d), growth in the merged entity's market share. The density of the DID for each of these outcomes exhibits significant variation across mergers. Also, the densities of $\widehat{DID}_{e,0}$ for close-proximity markets (red histograms) are starkly different from the corresponding densities of far-proximity markets (grey histograms).



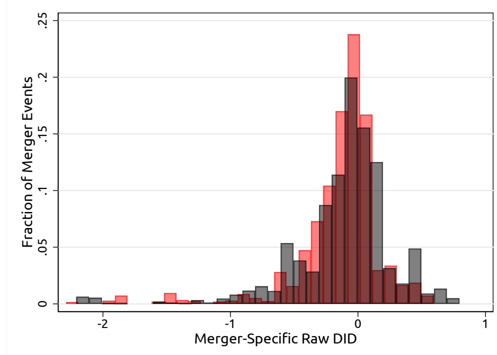
(a) $\Delta \log$(Market Total Branches)



(b) $\Delta \log$(Merged Entity's Branches)



(c) $\Delta \log$(Consumer Distance to Acquirer)



(d) $\Delta \log$(Merged Entity's Market Share)

Figure A.17: Observed Heterogeneity in $DID_{e,0}$ Across Merger Events

Note: In each panel of the figure, we plot the empirical density of merger-year treatment effects.

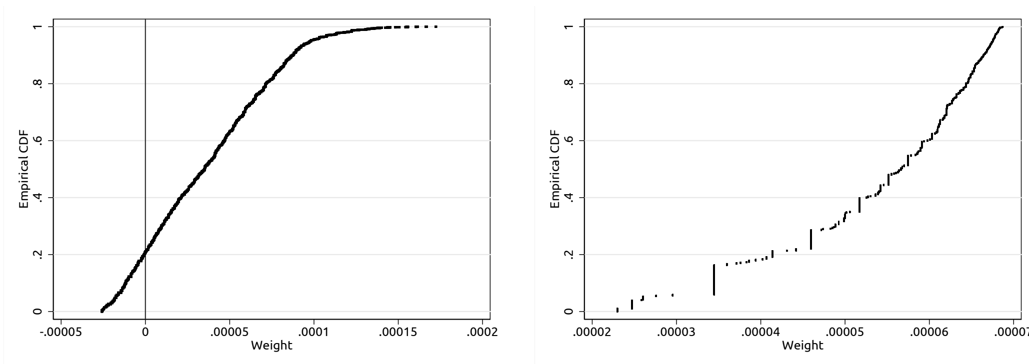
Negative Weights in a Two-way Fixed Effects Regression. — How does this observable heterogeneity impact an empirical strategy for estimating average treatment effects? A two-way fixed effects regression will yield a weighted average of feasible DID comparisons in the data.[1] Variation in exposure within the effective treatment/control groups defined by two-way fixed effects can (i) result in negative weighting and (ii) introduce unwanted but feasible DID comparisons such as the DID between observations treated one year before/after each other. With treatment effect heterogeneity, negative weighting might produce a weighted average that has the opposite sign of all of the underlying treatment effects. If there are feasible DID comparisons in the data that are not properly interpretable as treatment effects, then the weighted average produced by a two-way fixed effects estimator is not interpretable as an average treatment effect.

Several alternative estimators have been proposed to address such concerns. Our strategy is in the spirit of de Chaisemartin and D'Haultfœuille (2020) and Callaway and Sant'Anna (2021). We construct a panel of treatment and control markets specific to each merger event, stack these event-specific panels across treatment events, and employ regressions with event-specific two-way fixed effects. Negative weighting is eliminated mechanically by event-specific market and year fixed effects, because event-specificity controls for variation in treatment timing (now relative to a single reference merger) and intensity (by stacking across merger events). The event-specific two-way fixed effects regression still averages over the underlying merger-specific DIDs, but the weighted

[1] See (de Chaisemartin and D'Haultfœuille, 2020; Sun and Abraham, 2021; Goodman-Bacon, 2021; Athey and Imbens, 2022; Sandler and Sandler, 2014), and the related literature.

average is across events in the stack and is free of negative weighting.

To examine the effectiveness of our strategy, we bring the weighting formula in de Chaisemartin and D'Haultfœuille (2020) to our data. Figure A.18 illustrates the empirical cumulative distribution functions for weights placed on treated observations in a market-year panel regression with market and year fixed effects versus weights in our stacked panel regression with event-specific two-way fixed effects. We find that in a market-year panel design, over 20 percent of treated markets have negative weights (panel (a)). In contrast, in the stacked panel design no treated observations have negative weights (panel (b)). Note that the weights illustrated in Figure A.18 are for the pooled DID regression—i.e., a single coefficient for all post-treatment periods. Estimates of weights for the various τ event-time effect as well as HHI- and proximity-conditioned effects from in the main results in Section IV are qualitatively similar to the weights for the pooled regressions illustrated here.



(a) Market-Year Panel (b) Event-Stacked Panel

Figure A.18: Two-Way Fixed Effects Weight Estimates

Note: Estimates of the weights (de Chaisemartin and D'Haultfœuille, 2020) that two-way fixed effects regressions place on treated observations.

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