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How does competition affect retail banking? Quasi-experimental evidence from bank mergers ☆

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ABSTRACT

This paper studies bank antitrust rules which discontinuously shift bank mergers' competitive impact. The likelihood of mandatory divestiture rises sharply for mergers in markets above a threshold level of concentration, leading to an increase in the number of banks in these markets. Consistent with greater competition, intervention leads to higher deposit rates. Mortgage originations rise by 11%, from both refinancing and purchases. However, small business loan quantities do not change. The effects of intervention do not dissipate over time, and nonbank lenders respond similarly to banks. Overall, antitrust rules can increase bank competition, but relationships protect banks from competitors.

1. Introduction

Over the past thirty years, market concentration has increased substantially in the United States. There has been a robust debate in the economics literature about the causes and consequences of rising concentration, which has been blamed for a variety of problems, including higher prices, less investment, and greater inequality (Benmelech et al., 2018; Grullon et al., 2019; Covarrubias et al., 2020; Dorn et al., 2017).[1] A central question for interpreting these trends is how changes in local concentration affect prices and quantities, and how the effects of competition vary across goods. This paper provides new evidence on these questions in the retail banking market, a setting where the same firms

compete in different product markets that vary in their switching and search costs as well as the importance of relationships.

Understanding how products are affected by local competition has important implications for antitrust policy. There is now substantial interest from policy-makers and economists alike in the effects of antitrust law, which is viewed as a possible remedy for the ills caused by falling competition (Philippon, 2019; Shapiro, 2018). One of the most important antitrust policies is mandatory divestiture, which requires that merging firms sell some of their assets to a third-party competitor. In addition to providing evidence on the effects of competition, this paper provides a new analysis of the effectiveness of such policies in an

industry that has experienced a substantial increase in concentration at both the local and national levels.

I exploit a cutoff rule in the application of bank antitrust policy to study the effects of competition in markets where banks apply to merge. In banking markets where antitrust rules bind, merging banks divest branches, causing lower local bank concentration relative to markets where rules do not bind. Do customers benefit? In retail banking, yes: Antitrust rules cause higher deposit rates, lower mortgage rates and more mortgage refinancing at nearby incumbent banks that are not involved in the merger. However, I estimate no effects of antitrust rules on the small business loan market, where relationships with borrowers "protect" banks from competition. The findings show that antitrust rules can be effective at reducing concentration, but whether they help customers depends on the market structure; in markets where switching costs are high or relationships are important, merger regulation may be ineffective.

The question of how competition affects retail banking is broadly important for credit markets and firms' access to capital. Banking theory makes ambiguous predictions that depend on the precise nature of banking and bank relationships, and the empirical literature has found mixed effects of bank competition. Furthermore, the rise of online and nonbank lenders means that it is no longer clear that physical branches are still important for bank competition, raising questions for banking theory and competition policy that assumes banking is "local." The findings also contribute to a debate about whether bank mergers have negative effects on consumers. Sapienza (2002) finds that bank mergers raise deposit rates in the long run because they lead to greater efficiency. In contrast, I find that the increase in concentration caused by mergers lowers deposit rates, and that this effect does not dissipate over time.[2]

This paper contributes to a growing literature that uses tools from applied microeconomics to understand the effects of mergers. While there is debate about the relative merits of structural versus reduced-form approaches to merger analysis in general (Nevo and Whinston, 2010; Angrist and Pischke, 2010), the setting in this paper is well suited for a quasi-experimental research design. This is because I focus on mergers close to the regulatory cutoff in an industry with many mergers with plausibly-exogenous variation in antitrust enforcement. Nevo and Whinston (2010) argue that these are the criteria that make the "treatment effects" framework I use fruitful.

The source of empirical variation is a quantitative cutoff rule used by U.S. banking authorities to determine the approval conditions of bank mergers which discontinuously changes the probability that regulators intervene in a market.[3] This results in some markets where mergers proceed as planned and others where regulators restrict consolidation. When banks plan to merge, regulators decide whether to intervene based on how the merger would change the Herfindahl Index (HHI) of bank deposits in any market where both the absorbing and acquired bank have branches.[4] Therefore, the same proposed merger might induce intervention in some banking markets but not others. I show that there is no evidence that the rule influences the banks' decision to merge,[5] but it does mitigate the anti-competitive effects of

mergers in the markets where regulators intervene. Regulators require merging banks to divest branches in any market where the HHI would rise by at least 200 points to a level above 1800. If the HHI would rise to a level below 1800, there is no such requirement. I exploit the heterogeneous application of antitrust rules above and below the 1800 HHI cutoff using a difference-in-differences design that compares banking markets whose pre-merger HHI is within a 800-point range of 1800. These effects are robust to using alternative samples and a variety of alternative HHI ranges.

The structure of bank antitrust rules results in an important limitation to the generalizability of the findings. Namely, the results are identified only for markets where bank mergers take place, and where competition is *ex ante* close to the antitrust threshold. Although the findings are not sensitive to the exact choice of markets, it is possible that competition could have different effects in markets without mergers, or in markets where the level of competition is already very high or very low.

The difference-in-differences estimates show that antitrust enforcement has a dramatic effect on the level of concentration, which I then use to study the effect of competition on bank lending and deposits by banks uninvolved in the mergers. I use a dynamic difference-in-differences estimator based on Wooldridge (2021) which calculates the average difference between treated and control markets across mergers and across time. The average effect of intervention is a 183-point decrease in HHI, or a 0.12 log points. For comparison, a 183 point change in HHI would be approximately equivalent to a change from eight to seven equally-sized banks. These effects are long-lasting and are not significantly reduced by later bank entry into more concentrated markets.

Having established that antitrust laws affect the degree of bank concentration, I use this variation to study three different retail markets where local banks play an important role: Deposits, small business loans, and home mortgages. I study mergers taking place from 1994-2015 and study their effects on retail banking markets from 1994-2017, ensuring that I have at least two post-merger years for each market. A good reason to study the effects of antitrust in retail banking, in addition to the possibility of clean identification, is that the same laws affect different product markets where competition works in different ways. The results show that these differences matter for whether antitrust rules are effective. There are three main findings.

First, deposit rates respond substantially to changes in local bank concentration. The 11% relative HHI decrease in treated markets (markets with predicted antitrust intervention) is associated with an increase in 6-month certificate of deposit (CD) rates of 0.08 percentage points and an increase in 12-month CD rates of 0.11 percentage points, both of which are consistent with increased competition and are large compared to sample median rates of 3.41% and 3.76% respectively in treated markets. The increase is roughly uniform across CDs of different maturities. However, I estimate small or statistically insignificant effects on interest-bearing checking and savings accounts. As a result, the effects on deposit quantities are small and as well. These findings are important for two reasons. First, deposits are a relatively uniform product across regions and banks, so banks' deposit rates are a good measure of market prices. The persistent effect on CD rates I estimate implies that antitrust laws affect competition, not just concentration. However, the fact that intervention raise rates only in the CD market and not in other deposit markets means that competition works differently in these markets.[6]

Second, greater competition is associated with a 11% increase in the number of mortgages originated and a 0.10 percentage point reduction

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[2] Covarrubias et al. (2020) argue that concentration during the 1990s — the period under study in Sapienza (2002) — was a result of greater efficiency. By contrast, my paper focuses on the 2000s, when Covarrubias et al. (2020) argue that concentration instead resulted in higher rents.

[3] Note that antitrust rules in the banking sector are, in part, enforced by bank regulators, leading to a different set of rules than in other product markets.

[4] The Herfindahl Index is a measure of market concentration widely used in competition research and policy. Throughout this article, I use the definition of banking market used by bank regulators in the United States. Markets typically correspond to MSAs or non-MSA counties. The HHI for banks is measured using deposits data from the FDIC Summary of Deposits Database.

[5] The reason for this is that merging branks typically operate in many different banking markets, but antitrust intervention nearly always affects only one

or two markets. Therefore, this rule can be inconsequential from the perspective of the merging banks as a whole, even as it does matter for those individual markets.

[6] One possibility is that switching costs are lower in the CD market. Alternatively, banks compete for CD customers on *prices* whereas they compete for

in interest rates on new mortgages. There is some evidence that the effects on refis are bigger, but this depends on the time horizon, since they are affected with a greater lag. Decomposing the competitive response by borrower type, the increase in lending occurs more for lower-income borrowers and borrowers with a lower debt-to-income ratio.

Third, small business lending is hardly affected by changes to bank competition. Neither loan volumes nor the number of loans change in response to antitrust intervention. This finding is not merely due to the local nature of bank competition, as I find similar results when doing the analysis at the level of the entire banking market (typically a city) and as in samples limited to borrowers located in the same zip code as branches of the merging banks. These findings hold for both estimates at the aggregate market level and at the individual bank level.

I also explore the effects of mergers on new entry by online and non-local banks. Even though non-bank mortgage origination is an important part of the mortgage market, I find little evidence that new entry by non-bank lenders ameliorates the anticompetitive effects of bank mergers. Even though online lenders do not have a physical presence, they are still affected by local competition in a similar way to traditional banks. This suggests that the rise of nonbank and FinTech lending has not reduced the need for antitrust rules.

The identifying assumption for these estimates is that banks in markets where regulators intervene are on parallel trends relative to markets where regulators do not intervene. Three facts provide evidence for this. First, I show that there is no bunching of mergers below the 1800 cutoff. One would expect bunching if banks were merging selectively.[7] Second, I show that there are parallel trends in the main outcome variables before intervention and also that intervention and non-intervention markets are *ex ante* indistinguishable on a variety of observables. Third, I do not estimate an effect in a variety of placebo samples, such as one that uses the HHI = 1800 cutoff but restricts mergers to those where the HHI increases by fewer than 200 points and are therefore unaffected by antitrust law. A variety of other tests and control variables provide further support for my identification.

I use theories of bank competition to interpret the empirical results and to explain why different financial products respond differently to antitrust intervention. In models where banks invest in relationships, competition can reduce credit provision — the opposite prediction of traditional models where competition raises rates and reduces lending (Petersen and Rajan, 1995). My findings show that different models apply to different local financial markets (Boot and Thakor, 2000). The results on small business lending are most consistent with models in which long-term relationships insulate banks from competition for commercial loans. However, competition reduces market power in the market for consumer products where relationships matter less.

An influential early literature used inter- and intra-state branch banking deregulation as a shock to competition and found that increased competition in the banking industry led to more lending and a lower cost of capital for firms (Jayaratne and Strahan, 1996, 1998). But in the thirty years since the inter- and intra-state banking deregulation, significant changes have occurred. One major change is an increase in market concentration, similar to changes that have occurred in other industries. Since 1994, the number of commercial banks has declined by fifty percent from about 10,000 to about 5,000. As shown in Fig. 1, this consolidation has led to an increase in local bank concentration as measured by the Herfindahl Index, a change which is more pronounced in large cities.[8] Another change is the entry of new nonbank lenders,
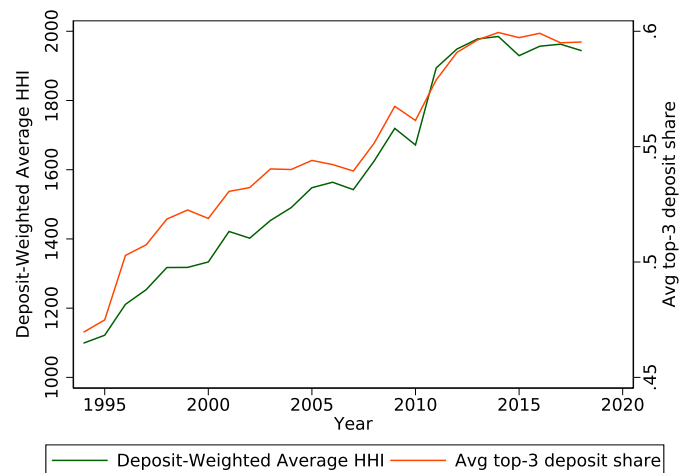
Fig. 1. Average HHI and Top-3 share over time. To create this figure, I calculate the HHI and top-3 bank share by banking market, and take a deposit-weighted average of banking markets in each year. Data source: FDIC Summary of Deposits database.

including FinTech firms. As a result, it is unclear whether competitive forces for retail banking still work the same way.

These two parallel changes — one potentially increasing competition, the other potentially decreasing it — have led to a debate about whether antitrust enforcement in banking is too lax or too strict. On the one hand, banks have argued that old regulations should no longer apply: Online and nonbank lending has nationalized banking markets and increased bank competition, so antitrust rules should be relaxed. For example, a merger application from BB&T Bank argues that "online lending and banking are not reflected in any local deposit-based analysis" and hence its merger "would not have a substantial adverse effect on competition" (BB&T Corporation, 2019). However, some consumer advocates believe that consolidation has reduced the competitiveness of banking markets. Responding to this concern, legislation introduced by Senator Elizabeth Warren would make merger review stricter and require regulators "to examine how the merger would impact market concentration for individual banking products, such as commercial deposits, home mortgage lending, and small business lending rather than just the general availability of banking products in local markets" (Warren, 2019). The findings in this paper speak directly this policy debate.

2. Literature review

Economists have become increasingly interested in competition and competition policy because of evidence that competition has changed in the United States has changed over the past several decades (Gutierrez and Philippon, 2017; De Loecker et al., 2020; Hall, 2018; Syverson, 2019). These changes have led to greater interest in antitrust law as a policy tool to affect competition. An important contribution of this paper is to provide new and direct evidence for the effectiveness of such policies.

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savings customers on services, which are not affected by competition in a single market.

[7] This is not surprising, as bank mergers typically involve a large number of markets, and antitrust action in a handful of markets is unlikely to derail a merger altogether.

[8] Rossi-Hansberg et al. shows that, in many industries, concentration at the national level has increased but concentration at the local level has decreased.

In the case of banking, concentration has increased at both the national and local levels. To create a representative measure of local concentration, the average HHI shown in Fig. 1 is calculated using a population-weighted average across all U.S. markets. Market concentration in low-population and rural markets hardly changed over this time and changes in the average are driven by changes in urban areas.

The existing evidence on the effects of antitrust law is mixed.[9] Although there is substantial research on the effects of mergers, there is little focus on antitrust rules per se, and the research that exists largely uses structural models that estimate the effects of a single merger.[10] In part this is because some "reduced-form" studies of antitrust rules compare the effects of different types of mergers in different industries with potentially different market structures (Nevo and Whinston, 2010). I address these critiques by focusing on a single industry with clearly-defined market definitions, where there exists plausbly-exogenous variation in merger enforcement.

This paper is also related to a large literature on bank competition and the effects of bank mergers.[11] Much of this literature studies the "internal" effects of mergers — the effects on the banks that actually merge — rather than on the effects on the equilibrium provision of credit by other banks in the market. Two classic studies about the deposit market come to different conclusions about the effects of mergers: Prager and Hannan (1998) finds that mergers lead to lower deposit rates, whereas Focarelli and Panetta (2003) finds that the lower deposit rates are only temporary. Other papers have focused on the effects of mergers on bank loans. For commercial lending, Di Patti and Gobbi (2007) finds that mergers reduce loan quantities, whereas Erel (2011) finds that mergers decrease loan rates. Studing the mortgage market, Allen et al. (2014) study the cross-sectional effects of a single large Canadian bank merger on the distribution of mortgage rates.

The methodology I employ is related to a growing literature on bank competition that uses quasi experimental methods to study bank competition, beyond the setting of mergers. Gissler et al. (2020) study how banks and non-bank lenders respond to a rule allowing the expansion of particular types of credit unions. Drechsler et al. (2017) argue that monopoly power in the market for bank deposits plays an important role in explaining monetary policy pass-through. Scharfstein and Sunderam (2016) find that market power matters for monetary policy in the mortgage market.

Theories of bank competition predict that the effects of bank mergers depend on how a particular market functions. In the model described by Petersen and Rajan (1995), monopolists may lend more than banks in competitive markets because monopolists can smooth profits over the course of their lending relationships. Alternatively, the "structure-conduct-performance" paradigm models banks as competing passively for customers' deposits and loans, implying that greater competition should lead to more lending at lower rates (Hannan, 1991). Finally, in the model by Boot and Thakor (2000), an increase to competition would make banks shift from information-intensive loans that rely on competition, to loans that do not rely on relationship-specific information. My finding, that the effects of competition vary by product, most closely match this type of model.

Two recent papers bear special discussion because they use similar data or methods as I do. First, Nguyen (2019) studies banks' voluntary branch closures following mergers to estimate the effects of branch closings on retail banking markets. The paper demonstrates the importance of local bank branches for small business lending by studying branch closings following mergers. Branch closings reduce small business lending but have no effect on mortgage lending, which implies that relationships are important in the small business market but not for mortgages. Both this paper and Nguyen (2019) imply that relationships prevent small businesses from searching for other lenders easily, but "protecting" banks from competition. Second, Williams (2020) studies the effects of bank size on the monetary policy, exploiting bank antitrust rules that create variation in bank size. Instead of studying how competition changes across markets, as I do, this paper looks within the same banking market, comparing branches from different banks. It finds that branches owned by larger banks pass through monetary policy differently than branches owned by smaller banks.

This paper is distinguished from the prior literature on bank competition in several ways. The findings here provide policy-relevant evidence on the effects of antitrust rules; to the best of my knowledge, no other papers have studied the exogenous enforcement of antitrust rules, in banking or in any other industry. Furthermore, I show that the effects of competition vary systematically across different types of financial products offered by the same banks at the same time, which I show using detailed local data rather than data from a single product or from an entire institution. The findings imply that understanding the effects of competition requires distinguishing among industries and even among products within the same industry. In my context, the effects of competition depend on the importance of relationships for each financial product. Finally, instead of focusing on a single event, I use a sample containing a large number of bank mergers, allowing somewhat greater generalizability.

3. Institutional setting

Banks in the United States that plan to merge must apply to the acquiring bank's regulator – a Federal Reserve Bank or the FDIC, for example. Part of the regulator's merger review is a screening for compliance with antitrust law.[12] The screening relies on a formal quantitative analysis of expected changes to the Herfindahl-Hirschman Index (HHI) due to the merger, in each geographic market where both the acquiring and target banks have branches. When banking markets involved in a merger violate the quantitative screening, regulators generally do not block the merger altogether. Rather, they require antitrust remedies to be applied to the violating banking markets. Merging banks often have branches in many of the same banking markets, but only the banking markets violating the quantitative screening require antitrust remedies; the merger can take place as planned in all other banking markets.[13]

The HHI screening has three steps. First, regulators calculate the HHI of bank deposit concentration in each market where both banks have branches.[14] Market shares are measured using branch deposit data from the FDIC Summary of Deposits Database.

Second, regulators calculate the resulting HHI in each market if the merger went through as planned.

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[9] Two reviews, Crandall and Winston (2003) and Baker (2003), come to opposite conclusions about whether antitrust enforcement matters at all. One reason for this is the lack of systematic evidence about the causal effect of antitrust enforcement, a problem that has not changed in the years since these reviews were written.

[10] Exceptions to this include Ashenfelter et al. (2009), Blonigen and Pierce (2016), Kwoka (2014) and Kwoka (2017).

[11] See Boot and Thakor (2000), Rajan (1992) and Petersen and Rajan (1994) for various theories about the effects of bank competition. In addition to local deposit and credit markets, bank competition is thought to affect financial stability, labor markets and other local and national economic variables. Interested readers may refer to Beck (2008) and Degryse and Ongena (2008, 2008) for research on the broader implications of bank competition.

[12] The regulator for the acquiring bank makes the initial merger decisions. Only when the U.S. Department of Justice (DOJ) disagrees with bank regulators, which happens in a minority of cases, do courts get involved. This set of interventions is studied by Williams (2020) to understand how the effects of monetary policy vary by bank size.

[13] This section summarizes the antitrust review process. The Federal Reserve Board provides more detail as well as legal justifications on their web site, available at https://www.federalreserve.gov/bankinforeg/competitive-effects-mergers-acquisitions-faqs.htm.

[14] Regional Federal Reserve Banks define the geographic banking markets. In urban areas, markets often coincide with cities, and in rural areas, they may be single counties. There is widespread discussion in the legal and economic literature about the right definition of a banking "market." In order to exploit regulatory cutoffs, I use the Federal Reserve definitions for the antitrust analysis, just as the regulators themselves do. I use data provide by the Federal Reserve Bank of St. Louis to create correspondence files between Federal Reserve market definitions and other data sources. The procedure for doing this is described in Online Appendix A.

Third, markets where the HHI would increase by at least 200 points ($\Delta HHI > 200$) to a level above $HHI = 1800$ are flagged for further review and antitrust remedies are required in these markets. Discussions with bank regulators indicate that the two parts of the threshold – 200 and 1800 – are not chosen because they are believed to matter in an important and discontinuous way for banking markets. Rather, they are chosen based on their historical use in other, non-bank antitrust settings.[15]

Rather than blocking bank mergers altogether when a banking market fails the HHI screening, regulators require an antitrust remedy: The merging banks must sell some of the branches in the offending markets to a third-party bank with no prior local presence. This is known as branch divestiture. The purpose of branch divestiture is to maintain a high level of competition in banking markets that would otherwise become non-competitive due to a merger. Regulators generally require that the former branches of the target bank be the ones divested. This is to prevent customers from re-opening their accounts at a different branch of their old bank, which would be possible if the branches of the acquirer were divested instead (Pilloff, 2005; Burke, 1998). The number of divested branches is either all the divested branches or however many are enough to bring HHI back to 1800 (Pilloff, 2005). In practice, this nearly always means divesting all the acquired branches in violating markets.[16]

Regulators monitor the divestiture process to ensure that divested branches remain active after they are sold. To keep banks from sabotaging their divested branches, banks must bundle together each customer's complete bank services, including loans, deposits, credit cards, and so on, and all of these must be divested together or kept together. Potential buyers of divested branches must also prove that they are sufficiently large and experienced before they are allowed to buy the branches. Finally, regulators monitor divested branches for several years to ensure that the level of competition stays high in the banking market.[17]

This paper's empirical strategy is to use data from realized bank mergers to predict, on an *ex ante* basis, which mergers required divestiture and which did not. Section 5 will provide evidence that 1) despite regulatory discretion, it is possible to predict reasonably well which markets require divestiture, and 2) comparing markets where I predict intervention and markets where I do not, incumbent banks react differently due to regulatory intervention and not some other difference between these markets. A naive approach would use *realized* rather than *predicted* divestiture to identify changes in competition. The problem with the naive approach is that regulators' actual divestiture decisions might be based on their predictions for the future of the banking market and not the HHI screening alone.

4. Data sources and summary statistics

The sample of banks and markets is determined by the empirical strategy, which studies banking markets in a narrow range of the $HHI = 1800$ cutoff. Specifically, the sample is limited to banking markets where the HHI is predicted to rise by at least 200 points and the pre-merger HHI is within 800 points of 1800, i.e., between 1000 and 2600. The 1000 and 2600 cutoffs are chosen to ensure that the banking markets in the sample are not too far from 1800, but the results are robust to varying the exact cutoffs. Using these cutoffs yields 72 banking markets that are ever non-treated, and 157 banking markets that are ever treated. Differences in geography and years of coverage mean that sample sizes are somewhat smaller when using data sources not available in all regions or years. I will mostly focus on the response of incumbent bank branches to changes in competition. By "incumbents," I mean branches located in the same banking market as merging banks that do not actually take part in the merger themselves either as an acquirer or as the absorbed bank.[18]

Table 1 shows summary statistics for treated and non-treated banking markets in the sample. The summary statistics are reported for the year of the merger. This includes statistics on the main outcome variables (deposit rates and mortgages). It also includes market-level averages of demographic variables in order to give a sense of what kinds places these banking markets are.

Table 1 shows that treated markets have a lower population, lower income and lower deposit rates per capita. In part this is because mergers do not happen at the same time everywhere, which the identification strategy will deal with by comparing treated to non-treated markets with mergers happening *in the same year*. The smaller size is a mechanical result of the fact that smaller banking markets are more concentrated. Importantly, size differences are not a problem for identification. As I will show using formal tests, the two groups of markets show similar *trends* in terms of the main outcome variables, supporting the parallel trends assumption required for identification.[19] The fact they there will be few statistically distinguishable differences in the trends across both types of markets will support this assumption.

Another difference between treated and control markets is that the change in HHI is larger in treated markets. One reason for this follows from how HHI is calculated. Since control markets are concentrated by definition, the same proposed merger will cause a larger change in the HHI in these markets. A second reason is that larger mergers are more likely to result in an HHI above 1800. What does this imply for the identification strategy? One important result of this difference is to underline the fact that the estimates will be average treatment on the treated (ATT). In other words, they will estimate the average effect of mergers on *treated* markets. To understand how treatment effects vary by market size, I will explore differences by market size. Second, this difference raises the question of whether the types of banks entering these markets, or the pre-merger trends in outcome variables, are different between treated and control markets. To understand this, in addition to pre-trends tests, I will test for differences in the types of banks entering each market.

4.1. Data sources

Using various sources of banking data, I construct a panel of market-by-year information for the years 1994-2017. For some data sources, data going back to 1994 is not possible, and I note this in the data description.

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[15] In addition to the "1800/200" rule described here, a merger might also be blocked if it leads to a bank having above 35% market share in any banking market. Discussions with bank regulators also indicate that merger screenings may also consider a Herfindahl Index calculated by weighting all bank branches equally. While these cutoffs may be important in some cases, in estimates, the effects of these rules are quantitatively smaller than the 1800-200 rule that is the primary focus of this paper. There are times when regulators may ignore HHI screening rules, for example if there is "evidence that rapid economic change has resulted in an outdated geographic market," to use an example from an FDIC report. The next section will test and verify that these limits do impact divestiture on average.

[16] In part because *whose* branches are divested may be endogenous to expectations of future performance, my main estimation sample excludes branches from both the acquiring and absorbed banks and studies only the behavior of other banks in the region.

[17] Pilloff (2005) and Burke (1998) provide empirical evidence that the branch divestiture process works well in the sense that divested branches remain competitive.

[18] The estimates are also robust to removing banks that purchase branches of either of the merging banks within two years of the merger.

[19] Similar levels are neither sufficient nor necessary for parallel trends, which is the identifying assumption. However, one might worry that larger and smaller towns are on different trajectories, leading to non-parallel trends.

Table 1

Summary Statistics by Banking Market, Intervention and Non-Intervention mergers. Summary statistics by banking market as of the merger year for mergers with and without predicted intervention. Sample sizes vary depending on data availability. Banking market definitions are provided by the Federal Reserve Bank of St. Louis "CASSIDI" team. Data sources: RateWatch, Community Reinvestment Act, HMDA, FDIC Summary of Deposits, County Business Patterns, IRS Statistics of Income.

	Mean	Std. Dev.	Min.	Max.	Obs.
Panel A: Intervention					
Payrolls Per Worker	25.64	5.45	17.51	40.75	157
APY, 6-Month CD	3.41	1.49	0.07	5.26	95
APY, 12-Month CD	3.76	1.50	0.15	5.58	96
Business Establishments	5,553.06	12,034.53	277.00	84,267.00	157
Deposits Per Branch	31,627.32	10,863.26	9,672.50	88,519.50	171
Purchase Loans Per Capita (th)	11.24	6.49	0.24	31.58	157
Refi Loans Per Capita (th)	12.85	10.63	0.10	71.96	157
Population	228,015.82	479,624.86	6,333.00	2,954,274.00	157
Income per Capita (th)	17.86	4.26	7.79	30.95	157
Households	198,734.75	424,515.94	2,721.00	2,635,986.00	157
Panel B: Non-Intervention					
Payrolls Per Worker (th)	29.07	6.50	16.17	46.13	72
APY, 6-Month CD	2.92	1.60	0.10	5.04	56
APY, 12-Month CD	3.26	1.63	0.10	5.29	56
Business Establishments	14,573.15	26,575.92	694.00	124,110.00	72
Deposits Per Branch	33,666.47	9,784.49	15,861.57	73,271.88	72
Purchase Loans Per Capita (th)	15.65	9.12	0.50	47.41	72
Refi Loans Per Capita (th)	17.02	10.91	0.08	54.91	72
Population	558,033.44	959,185.89	24,734.00	4,644,327.00	72
Income per Capita (th)	21.28	5.23	11.58	39.07	72
Households	481,795.75	824,094.52	21,295.00	3,908,300.00	72

Measuring banking markets   The main analysis uses the same definitions as bank regulators which are provided by the Federal Reserve Bank of St. Louis. Historical definitions from before 2016 are not available. This means that it is not possible to precisely replicate regulators' HHI screening process at every given moment in the past. This limitation, as well as regulators' discretion to waive antitrust rules, means that I cannot perfectly predict when divestiture actually happened and so the estimates will be intent-to-treat (ITT).

For each banking market involved in a merger where both the surviving and purchased bank have branches, I calculate: 1) The HHI immediately before the merger takes place, the *pre-merger HHI;* 2) The HHI that would result if the merger went through as planned and no local branches were divested, the *predicted HHI;* 3) The difference between these, the *predicted change in HHI* due bank mergers.[20] The Federal Reserve Bank of St. Louis also provides banks with the geographic market definitions used in these calculations which I use to reconstruct their calculations. I use the definitions that they provide and match these to the various data sources in order to calculate statistics at the banking market level. I also verify the accuracy of my HHI calculations by showing that they are the same as those calculated by the CASSIDI tool when applied to current branches.

Deposits data   To simulate historical merger screenings and study deposit quantities, I use the FDIC Summary of Deposits (SOD) database. I match SOD data to data on retail deposit rates provided by the firm RateWatch. This dataset includes branch-level retail deposit rates nationally. I use deposit rates data for $10,000 CDs, interest-bearing checking, and savings accounts.

Mortgage data   Mortgage lending data is from the Home Mortgage Disclosure Act (HMDA). The main HMDA variables I consider are total lending and the number of loans for purchase and refinance mortgages.

To measure interest rates, I use data from a national credit registry, the University of California Consumer Credit Panel (UC-CCP). The UC-CCP is a national credit database similar to other credit panels used in recent research, such as the Federal Reserve Bank of New York's Consumer Credit Panel. It contains comprehensive credit information from a 2% sample of U.S. households with credit history. I infer interest rates by calculating the implied interest rate based on the amortization formula and residualizing borrower and lender information. Then I calculate the average interest rate in each zip code and year for mortgages originated that year. See Online Appendix H for more details.

Small business lending   I measure lending using Community Reinvestment Act (CRA) data which is reported in two ways: By lender-county-year and by census tract-year. I study both, although the main sample uses the lender-county-year data because this makes it possible to exclude the merging banks from the sample. The main CRA variables are the number of loans and loan volume, both measured at the bank level. I use CRA data beginning in 1996.

Online Appendix A provides further details of the data and cleaning process.

5. Empirical strategy

This paper's empirical strategy exploits the heterogenous application of antitrust rules in banking markets above and below the 1800 HHI cutoff. As described in Section 3, regulators do not require branch divestiture in banking markets where the predicted HHI rises by at least 200 points to a level below 1800. When the predicted HHI rises by at least 200 points to a level *above* 1800, however, regulators do require branch divestitures, so competition does not fall as much in these markets. This means that mergers in banking markets experiencing similar trends can have very different effects on the level of bank competition, depending on which side of the cutoff the HHI falls. To evaluate the effects of bank competition, I compare the change in competition and bank behavior in markets above and below the 1800 HHI cutoff.[21] A

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[20] The Federal Reserve Bank of St. Louis provides a free online tool, CASSIDI, which performs these calculations for all bank branches that are in place at the time users browse the site. CASSIDI is available at https://cassidi.stlouisfed.org/index. Thanks to the CASSIDI team for their help in understanding the antitrust screening process and for providing data on banking market definitions.

[21] In principal, it is possible for a treated market to have a smaller change in HHI than a similar control market. For example, consider two markets with an

market could be treated either because its ex ante HHI is high or because it has a high predicted change in HHI due to the merger. Because the average change in HHI is small relative to the possible range of ex ante HHIs, most of the selection into treatment comes from the market's ex ante HHI.

I limit my analysis to markets whose pre-merger HHI is within a 800-point range of 1800. The goal is not to ensure that the treated and control markets are identical, but rather to exclude much larger or much smaller markets which might not be on parallel trends.[22] Hence the treatment group is defined as markets involved in mergers, where the predicted HHI increase is at least 200 points, the predicted HHI level is above 1800, and the pre-merger HHI is within 800 points of 1800, i.e. 1800-2600. Likewise, the control group is defined as markets involved in mergers where the predicted HHI increase is at least 200 points, but the predicted post-merger HHI level is 1000-1800.[23]

5.1. Difference-in-differences design

I use a difference-in-differences estimator which compares treated and untreated banking markets before and after mergers occur. Estimates are limited to within a 10-year period of the merger itself. The main estimates use a staggered difference-in-differences estimator. Following the regression-based approach advocated by Wooldridge (2021) and Wooldridge (2022), I allow for treatment effects to vary by merger and by post-treatment year. This methodology addresses several critiques of the dynamic difference-in-differences literature made by Goodman-Bacon (2021). Wooldridge (2021) derives a regression-based method that allows for treatment effect heterogeneity and is optimal under general conditions.[24]

Typical staggered difference-in-differences specifications use non-treated observations (either never-treated or not-yet-treated) as the comparison group for treated observations. Relative to the canonical staggered difference-in-differences assumptions, the setting here uses assumptions that are weaker in one key respect. In a typical staggered difference-in-differences design, units that are treated in the future act as controls for units that are treated at any particular time. But in the setting here, the regression specification only uses mergers from the same cohort but without intervention as a control for mergers with antitrust intervention (i.e., rather than using mergers with intervention that take place in the future). The identifying assumptions are that there are parallel trends and no-anticipation for each treated banking market relative to non-treated banking markets where a merger took place in the same year.

Building on Wooldridge (2021), I use a pooled difference-in-differences estimator that estimates dynamic treatment effects that vary by event-time and by cohort. The specification is:

$$Y_{it} = \sum \delta_{c(i),t} CohortYear_{c(i),t} + \sum \gamma_i Merger_i$$
$$+ \sum_{\{c,t>c(i)\}} \beta_{c(i),t} POST_{c(i),t} \times TREAT_i + \varepsilon_{it} \quad (1)$$

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ex ante HHI of 1500, where the first one has a predicted post-merger HHI of 1700 and the second has a post-merger HHI of 1900. The former might have an HHI of change of 200 points while the latter has a change of only 100 points (because regulators require branches to be divested such that the resulting HHI is 1800). In practice, this situation rarely occurs and treated markets have a more negative change in HHI than control markets. The reason is that most treated markets have an HHI which begins above 1800, hence the change in HHI is roughly zero.

[22] Hertzberg et al. (2011) and Mello (2019) use similar strategies to exploit interventions that are applied with fuzzy cutoff rules.

[23] Figure C.1 in the Online Appendix shows a map of the treatment and control markets; they are evenly distributed throughout the country and tend to be small- to medium-sized cities.

[24] Estimates using this regression-based approach equal estimates from the imputation approach in Borusyak et al. (2021), while being easier to compute using standard statistical packages.

where i indexes mergers taking place in particular banking markets, $c(i)$ is market i's merger cohort defined as the year the merger took place, δ_{ct} is the coefficient on a fixed effect which varies by merger cohort and observation year (cohort-year fixed effects), γ_i is the coefficient on a fixed effect which varies by market-merger pair (merger fixed effects). $TREAT$ is an indicator equal to 1 for treated markets and $POST_{c(i),t}$ is equal to 1 in year t after the merger for markets in cohort $c(i)$.[25] $\beta_{c(i),t}$ is a coefficient on the treatment effect which varies by event-time and merger cohort. Y_{it} is the appropriate outcome variable (e.g., deposit rates or mortgage amounts). I use data from ten years before to ten years after the merger year.[26] Hence a separate treatment effect $\beta_{c(i),t}$ is estimated by treatment cohort and year for each post-treatment year. The overall treatment effect reported in the regression tables is the weighted average over the estimated $\beta_{c(i),t}$s, with weights given by the number of observations used to calculate that coefficient (i.e., the number of observations in that particular year and merger cohort).

I formally test for differential pre-trends between treatment and control markets following the approach proposed by Wooldridge (2021). The pre-trends tests show 1) that there is no statistically significant time pre-trend in treated relative to control markets and 2) that the years immediately before the merger show no evidence of "anticipation" in treated markets. Having verified that the parallel trends assumption holds, I then graph the dynamic effects in post-merger years. The event study plots in this paper are created by calculating the weighted average of β_{ct} for each year relative to the merger year, with the average taken over all cohorts and the weights equal to the number of observations in each cohort-year: $\beta(t) = \frac{1}{W} \sum_{\{c\}} \beta_{ct} w_{ct}$ where w_{ct} is the number of observations and $W = \sum w_{ct}$. Estimates are presented as a graph of $\beta(t)$ for t from 1 to 5 years.[27] While estimates are calculated for later years, the coefficients are noisy since they rely on few observations, making them less informative for understanding the dynamic effects of mergers.

Two additional specifications are used in the paper. First, count variables use Poisson regression instead of ordinary least squares. Second, I use a long differences regression instead of a fixed effects regression. The long-differences specification studies the change in outcome variables from $t = 0$ to $t = 5$ relative to the merger. The advantage of this specification is that it naturally allows for the addition of controls to show how these change the results. These specifications are described in more detail in Online Appendix B.

See Online Appendix G for more detail on the two formal pre-trends tests along with the estimates themselves.

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[25] Note that the non-interacted $TREAT$ effect is fully absorbed by the unit fixed effects and so does not need to be included in the specification. The non-interacted $POST$ variable is absorbed by the cohort-year fixed effects.

[26] An equivalent approach would be to estimate a separate difference-in-differences specification for each merger cohort, comparing the treated merger-markets in each year to the non-treated merger-markets whose merger took place the same year. The regression specification for each specification would be:

$$Y_{it} = \sum \delta_t Year_t + \sum \gamma_i Merger_i + \sum_{\{t>\bar{t}\}} \beta_t PostYear_{it} \times Treat_i + \varepsilon_{it} \quad (2)$$

Specification (2) would be estimated separately for each merger cohort. The overall difference-in-differences estimate from many such specifications would be the average of the coefficients from the non-staggered difference-in-differences estimates. Using this approach would yield the same results as the single-step estimator I use. However, the disadvantage of estimating multiple non-dynamic specifications and taking the average is that the standard errors are not adjusted for the fact that the same markets may be in multiple cohorts.

[27] Wooldridge (2022) and Borusyak et al. (2021) argue against the event-study plots commonly shown in difference-in-differences papers that include both pre- and post-treatment years because they combine tests of identifying assumptions (i.e., pre-trends) with estimates of the coefficients, so I only show post-treatment years.
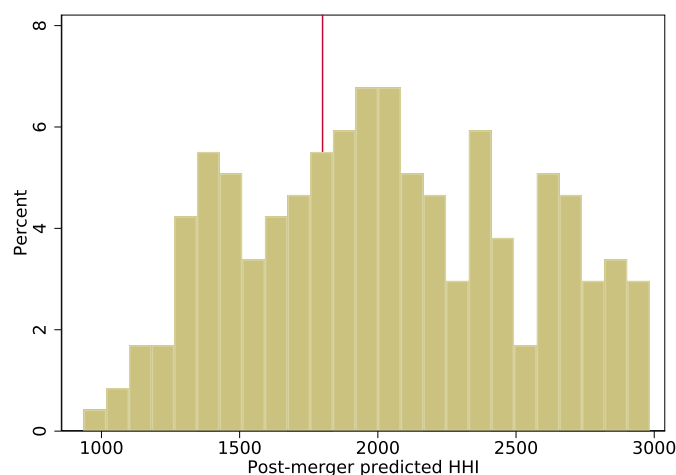
Fig. 2. Histogram of post-merger HHIs. Sample includes each merger with a predicted HHI increase of at least 200 points but where the post-merger HHI varies. Mergers with a predicted post-merger HHI above 1800 are the "treated" group in the regression sample. The lack of bunching below the HHI = 1800 cutoff provides evidence against endogenous merger selection.

Fig. 3. Spinoff likelihood by post-merger HHI. For each merger with a predicted HHI increase above 200, I calculate the fraction of branches sold in the year after the merger. Mergers are binned by their predicted post-merger HHI. Those with a predicted post-merger HHI above 1800 are the "treated" group in the regression samples.

5.2. Identifying assumptions

The identifying assumption is that incumbent banks' response to a merger in their market would have been the same in treatment and control markets if regulators had not intervened in the treated markets. As antitrust intervention is determined by a regulatory cutoff that is not chosen with the banking industry in mind, this identifying assumption is plausible. Alongside the pre-trends tests, key support for this assumption will come from placebo tests showing no effect when mergers occur but regulations do not bind.

I also provide evidence that sample selection coming from strategic bank behavior is not a major concern for the empirical design. One might worry that banks strategically forego some otherwise-profitable mergers when the mergers are likely to require branch divestiture. If this were the case, markets with mergers might be unobservably different than markets without mergers. In reality, bank mergers typically involve multiple banking markets and antitrust remedies are generally required in no more than a few of these. Therefore, as long as violating bank markets are not pivotal for banks' merger decisions at a national level, it is plausible that the cutoffs do not lead to strategic merger behavior around the 1800 cutoff.[28]

A histogram showing the density of mergers at different HHI thresholds provides direct evidence against strategic bank selection. Fig. 2 shows the density of predicted HHIs in banking markets where mergers occur, limited to a sample where the HHI increase is at least 200 points. If mergers were being avoided because of anticipated antitrust violation, one would expect to see fewer mergers just above the 1800 threshold than below.[29] Tables D.1 and D.2 in the Appendix show that banks in treatment and control markets are statistically indistinguishable, providing further evidence against strategic manipulation.

6. Effects of intervention on market structure

The empirical design relies on correctly predicting the application of antitrust rules. Section 3 discussed two reasons that predicting antitrust rule application could be challenging – regulatory discretion and
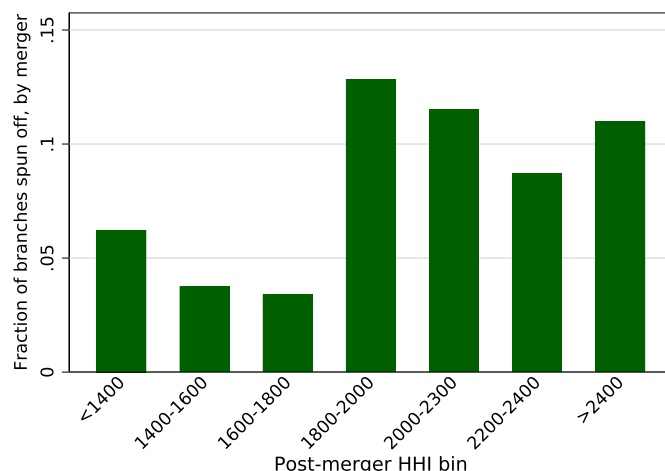
mismeasurement of banking markets. However, this section provides evidence that antitrust rules do indeed affect bank competition in a measurable way. The first piece of evidence is that, as required by regulation, merging banks sell branches above the 1800 cutoff. Second, these branch sales successfully lower the banking market HHI.

6.1. Branch sales following mergers

The immediate effect of antitrust enforcement is an increase in branch sales. Fig. 3 shows the fraction of bank branches sold by merging banks in markets both above and below the 1800 HHI cutoff. The figure measures branches sold to competitors by merging banks in the year after bank mergers. In markets where the predicted HHI is below 1800, about 3% of branches are sold to competitors, whereas in markets above the 1800 cutoff, 13% are. Why are only 13% of branches sold and not more? The reason is that only enough branches must be sold to maintain an HHI below 1800. Furthermore, one would never expect *all* branches owned by the merging banks to be sold, since in most cases only the branches of the bank being purchased – and not the acquiring bank – would generally be divested.[30] Confirming that these sales are a result of antitrust rules, Online Appendix Figure C.2 shows that a spike in sales that happen in the year of mergers or in the following year.

6.2. Effect of intervention on HHI

This subsection provides evidence that divestiture affects the banking market HHI. Relative to non-intervention, intervention is intended to lower the HHI in the short run. If long-run differences in HHI persist, it means that there is not sufficient entry by new banks into less-competitive markets to equalize concentration.

Table 3 presents the main estimates of antitrust intervention on the HHI. Column 1 shows the average effect of antitrust rules on the HHI and column 2 shows the effect on the log of HHI, both estimated using Equation (1). The estimated effect of intervention is about 0.12 log points. The sample in this study consists of banking markets with an HHI near the cutoff of 1800. A natural question is how antitrust

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[28] Further, the main regression results are very similar in a sample of bank mergers where the absorbing and acquired banks both have branches in multiple banking markets.

[29] Of course, it could be that banks avoid mergers which would cause more extreme changes in the HHI, but this does not matter for my identification.

[30] Online Appendix Figure C.3 replicates Fig. 3, limited to markets where the predicted change in HHI is below 200 points – a placebo test. This is limited to mergers with a predicted ex post HHI below 2000 because there are very few with a predicted HHI level above this where the HHI increase is below 200. There is no discontinuity at the 1800 cutoff for the placebo sample.
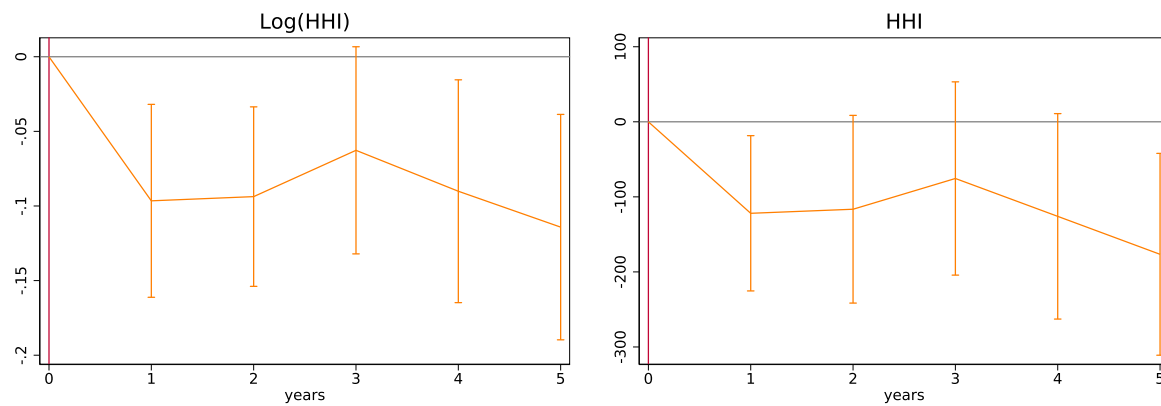
Fig. 4. Herfindahl Index Event Study. Event study graph showing the path of the HHI and the natural log of HHI in treated/divestiture and non-treated/no-divestiture banking markets relative to merger year. Event study calculation is described in the text. HHI calculations give thrifts half weight as per regulatory guidelines. Calculations are as described in the text and are calculated from the FDIC Summary of Deposits Database.

Table 2
Pre-Trend Tests. This table tests for statistically significant differences in the main outcome variables between the treated and control markets in the three years prior to the merger, following Wooldridge (2022). The estimates in this table come from specifications in which the independent variables are treatment indicators interacted with pre-merger event-time indicators for the three years prior to the merger year. Columns 1, 2, 9 and 10 are estimated using ordinary least squares, and Columns 3, 4, 5, 6, 7 and 8 are estimated using Poisson regression. The sample is the same as the main estimates but omits post-merger years in treated markets. Standard errors clustered by banking market. See Online Appendix B for more details on the empirical specification and Online Appendix G for further placebo tests.

	(1) 6m CD	(2) 12m CD	(3) Purchase Ln	(4) Refi Ln	(5) CRA Vol	(6) CRA Lns	(7) Branches	(8) Deposits	(9) Log(HHI)	(10) HHI
$I_{t-1} \times TREAT$	0.0366	0.0424	0.102*	0.0702	-0.0173	-0.0330	0.106	0.0521	-0.0443	-106.7
	(0.0710)	(0.0689)	(0.0587)	(0.0505)	(0.0986)	(0.0968)	(0.0917)	(0.0808)	(0.0383)	(70.80)
$I_{t-2} \times TREAT$	-0.0207	-0.105*	0.0825*	0.0691	0.0145	0.00247	0.0728	0.0205	-0.0104	-84.09
	(0.0623)	(0.0623)	(0.0496)	(0.0550)	(0.0631)	(0.0780)	(0.0805)	(0.0749)	(0.0335)	(66.46)
$I_{t-3} \times TREAT$	0.0472	0.0157	0.0401	0.0427	-0.0163	-0.0287	0.0718	0.0289	-0.0149	-82.08
	(0.0495)	(0.0554)	(0.0380)	(0.0587)	(0.0469)	(0.0564)	(0.0667)	(0.0611)	(0.0286)	(59.48)
N	1459	1456	2325	2315	2135	2135	2585	2612	2693	2693
Cohort-Year FE	X	X	X	X	X	X	X	X	X	X
Merger FE	X	X	X	X	X	X	X	X	X	X
Chi2-Statistic	.	.	5.752	3.665	2.120	6.165	4.491	4.539	.	.
F-Statistic	0.760	2.724	.	.	.	.	.	.	1.074	0.848
P-Value	0.519	0.0475	0.124	0.300	0.548	0.104	0.213	0.209	0.361	0.469

rules affect markets of other sizes, either smaller or larger. How do estimates vary by market size? To shed light on this question, the estimates shown in columns 3 and 4 are restricted to a sample of markets with above- and below sample median population respectively. Note that while each contains the same number of banking markets, the number of observations is nearly twice as large in column 4 because these mergers take place in the middle of the sample so more years of data are available. For small markets, the effect of antitrust intervention on HHI is above 128 points and for large markets it is 163 points. Both estimates are statistically significant. This finding show that antitrust intervention has substantial effects in both types of markets, suggesting that there is scope for antitrust rules in markets of widely varying sizes. To understand the dynamic effects of treatment, Fig. 4 shows an event study graph showing HHI changes in treatment and control markets relative to merger years. Following mergers, the HHI falls in the treatment group relative to the control group. Most of the effect of the merger on the HHI is immediately in the year following the merger, with a small long-run change (the difference in coefficients between early and late years is not statistically significant at conventional levels). This follows naturally from the fact that most branch sales occur in the year after mergers. Pre-treatment trends are also not statistically distinguishable between treated and control markets, as shown in Table 2 and G.1 of the Online Appendix.

Since the treatment causes a lower level of market concentration in treated relative to control markets, it would be natural to expect greater entry in non-treated markets in the years after the merger. This would lead to treatment effects that dissipate over time. Yet this is not what the results show. Instead, the treatment effects both for concentration and for interest rates appear to be constant in the years following the merger. The main reason for this lack of convergence may lie in the way that banks decide to engage in mergers. Acquiring banks typically purchase all the branches of a target bank, so the level of concentration in a single market may not matter much for a bank's entry decision. In addition, the change in concentration due to the treatment may not be large enough for banks to pay close attention to what happens in that market. For both these reasons, there may not be greater entry in non-treated markets despite higher concentration.

6.2.1. Robustness and placebo checks

Online Appendix E shows a variety of and placebo checks for the effect of intervention on HHI. First, I show that there is no estimated effect in samples where antitrust rules do not apply. Second, I use long differences specifications to successively control for variables that are *ex ante* different between treatment and control markets. The results do not change substantially. Third, I show that similar results hold in estimates using alternative geographies, such as ZIP codes. Fourth, I show that the estimates are similar using alternative HHI and ΔHHI

Table 3

Deposit Market Herfindahl Index. Difference-in-differences estimates from an ordinary least squares regression show the effect of predicted antitrust intervention on local bank concentration. These estimates come from a specification which estimates treatment effects for each cohort that mergers take place and for each year in the ten years following the merger. Coefficients shown in the table are the average treatment effect across cohorts and across years, weighted by the number of observations in the sample. See section 5 for more details on the empirical strategy. Columns 1-2 show the effect on the HHI (Herfindahl Index) and the log of HHI as measured using FDIC Summary of Deposits data. Columns 3-4 show HHI estimates limited to markets with above sample median and below sample median population respectively. Standard errors are clustered by banking market.

	(1) HHI	(2) Log HHI	(3) HHI (small markets)	(4) HHI (large markets)
Post X Treat	-182.9***	-0.119***	-127.5**	-163.4***
	(58.44)	(0.0336)	(63.02)	(59.07)
N	4356	4356	2103	4127
Cohort-Year FE	X	X	X	X
Merger FE	X	X	X	X

cutoffs. Fifth, I show that similar results hold when using alternative estimation strategies, such as a sample with no overlap between years and markets.

7. Effect of competition on retail and small business banking

7.1. Retail banking

The small business market is a quintessential setting where relationships are likely to matter. By contrast, many retail banking markets, such as mortgage and deposits are often purely "transactional" (Boot and Thakor, 2000). At the same time, the degree of search and switching costs and market power may vary by particular product — consumers may find it more difficult to move their deposit accounts than to find a new bank for their CDs or mortgages. This section explores how these differences result in differences in the effects of competition.

7.1.1. Deposit rates and quantities

The market for bank deposits is a natural setting to study the importance of antitrust rules on competition in the banking market. Deposit insurance means that deposits are a relatively uniform product across banks and across regions, relative to products in most other industries. Also, in specifications with time fixed effects, estimates of the effect of competition on deposit rates are equivalent to estimates on deposit spreads, which are a good measure of competition. Finally, the deposits market shows similar dynamics to other parts of banking where there is competition between physical locations and new online entrants. For all these reasons, it is a natural starting point for understanding whether antitrust intervention matters for local competition and whether "local" branches still matter.

The first finding is that antitrust intervention has a large effect on rates for certificates of deposit, but not for other deposit products. Table 4 shows estimates of antitrust intervention on certificate of deposit (CD) rates, interest-bearing checking rates and savings account rates at incumbent bank branches.[31] The estimates in this table are again estimated using the specification shown in Equation (1). Columns 1 and 2 of the table show that antitrust intervention, and the accompanying

increase in competition, is associated with a 0.08 percentage point relative increase in 6-month deposit rates and a 0.11 percentage point relative increase in 12-month deposit rates. The estimates are statistically significant at the 5% level. The estimates in columns 3 and 4 show that the effects on rates for interest-bearing checking are negative and not statistically distinguishable from zero.[32] Pre-trends tests, shown in columns 1-2 of Tables 2 and G.1 of the Online Appendix, support the identifying assumption of parallel trends between treated and control markets.

To explore dynamic effects, Fig. 5 shows two series, the evolution of 12-month deposit rates and the evolution of savings rates following the merger. Overall the dynamic effects are somewhat noisy, but the effect on 12-month CDs is statistically significant and does not follow any particular trend. There is evidence that the effect on rates is lagged by a year, consistent with the effects of market power (as in Neumark and Sharpe 1992). The effects of antitrust intervention on HHI do not diminish over time, so it is natural that the effects on deposit rates will not diminish either. In contrast, the effect on savings rates is consistently close to zero.

What explains the difference in effects between CD rates and savings accounts? One reason may be that the benefits of a savings account are not just the interest rate on the deposits. A deposit account might give access to an ATM network, an online banking platform, and in-person personalized customer service. Bundled services could increase switching costs for customers and make search more difficult (Sharpe, 1997).

The average effect of competition on the rates of all deposits can be calculated by considering the fraction of bank deposits in each category. Call reports data shows that, for the sample of banks studied in the regressions, approximately 45% of deposits are held as CDs, 24% are checking and 31% are savings. I approximate the average effect of antitrust intervention on deposit rates (i.e., combining all three categories) by considering the share-weighted average of the coefficients in Table 4. The overall average effect on deposit rates is about 0.023.[33]

Table 5 estimates the effect of antitrust intervention on deposit quantities and the number of bank branches. The overall effect on deposit quantities is 0.025 and the effect on the number of branches is 0.057. Given that the average effect on deposit rates is 0.023, the im-

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[31] Repeating the estimates in this section using maturity-matched spreads over Treasury rates as the main dependent variable, rather than the spreads themselves, yields very similar results to what I show. One should expect similar results because the year fixed effects used in the main specification control for much of the time variation in Treasury rates. In addition, Table D.3 estimates long-differences specifications that include successive control variables, similar to Table E.2.

[32] Nakane et al. (2006) find, in the context of Brazil, that the interest rate elasticity of deposit supply is substantially higher for time deposits than for other types of deposits. These findings are consistent with the idea that switching costs are lower for time deposits, resulting in greater elasticities and a higher responsiveness to competition.

[33] Assuming CDs respond as the average of the 6-month and 12-month effects.

Table 4

Deposit Rates. Difference-in-differences estimates from an ordinary least squares regression show the effect of predicted antitrust intervention on rates an local bank branches. Regression estimates come from a specification which estimates treatment effects for each cohort that mergers take place and for each year in the ten years following the merger. Coefficients shown in the table are the average treatment effect across cohorts and across years, weighted by the number of observations in the sample. See section 5 for more details on the empirical strategy. Interest rates are a market-level average by product type, calculated using data from Ratewatch. Standard errors are clustered by banking market.

	(1) 6-Month CD	(2) 12-Month CD	(3) Interest-Bearing Checking	(4) Savings
Post X Treat	0.0813** (0.0410)	0.109*** (0.0378)	-0.0635 (0.0536)	-0.0145 (0.0389)
N	2421	2417	2256	2281
Cohort-Year FE	X	X	X	X
Merger FE	X	X	X	X
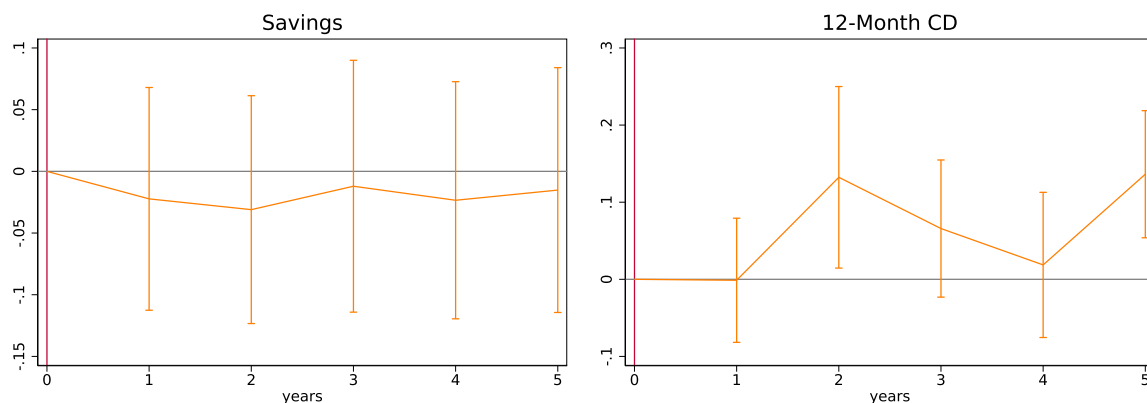
Fig. 5. Deposit rates event study. Event study graph showing average deposit rates for 6-month CDs and savings accounts in treated/divestiture banking markets relative to non-treated/divestiture banking markets from the same merger cohort. Event study calculation is described in the text. Interest rate data is calculated using data from RateWatch.

Table 5

Deposit Quantities. Difference-in-differences estimates from Poisson regressions show the effect of predicted antitrust intervention on rates an local bank branches. Regression estimates come from a specification which estimates treatment effects for each cohort that mergers take place and for each year in the ten years following the merger. Coefficients shown in the table are the average treatment effect across cohorts and across years, weighted by the number of observations in the sample. Deposit and branch quantities are measured from FDIC Summary of Deposits data. See section 5 for more details on the empirical strategy. Standard errors are clustered by banking market.

	(1) Deposits	(2) Branches
Post X Treat	0.0249 (0.0314)	0.0573** (0.0288)
N	3928	3846
Cohort-Year FE	X	X
Merger FE	X	X

plied elasticity of deposit demand is about 1. This is a bit lower than the estimates in Dick (2008), which vary from 1.77 to 2.99. It is higher than the estimate in Egan et al. (2016) of 0.56. The estimated effect on the log number of bank branches is 0.56. This indicates that antitrust intervention is associated with a greater number of bank branches among incumbent banks, suggesting that incumbents open new branches to accommodate the increase in deposit supply. Dynamic effects on deposit quantities are shown in Fig. 6. There is some evidence of a slow-moving or lagged effect in this figure, consistent with the lagged effect on deposit rates shown in Table 5 and with evidence for switching costs in the deposit market (Sharpe, 1997). However, the difference in effects from t = 1 to t = 5 is not statistically significant, so I cannot reject that the effects are constant and that the time variation is due to noise.

7.1.2. Bank characteristics and deposit competition

Previous research has found that bank competition is affected by both the number of banks in a market and also by how effectively they compete. If bank characteristics are more important than the number of banks for local competition, then aggregate increases in the HHI over time could be consistent with rising competition, if the types of local banks are changing. This could also mean that antitrust rules are wrong to target the HHI but should instead depend on other characteristics of the merging banks.[34]

Requiring a bank to divest branches changes both the number of banks present in a market and, potentially, which types of banks are present. For example, the banks purchasing divested branches could

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[34] Inter-state banking liberalization provides an important example of how bank characteristics matter for competition. Perhaps surprisingly, legalizing inter-state banking did not change the concentration of U.S. banking markets (Strahan, 2003). Rather, inter-state banking reform made banking markets more competitive by allowing well-run, efficient banks to purchase smaller and less-efficient ones (Jayaratne and Strahan, 1998).
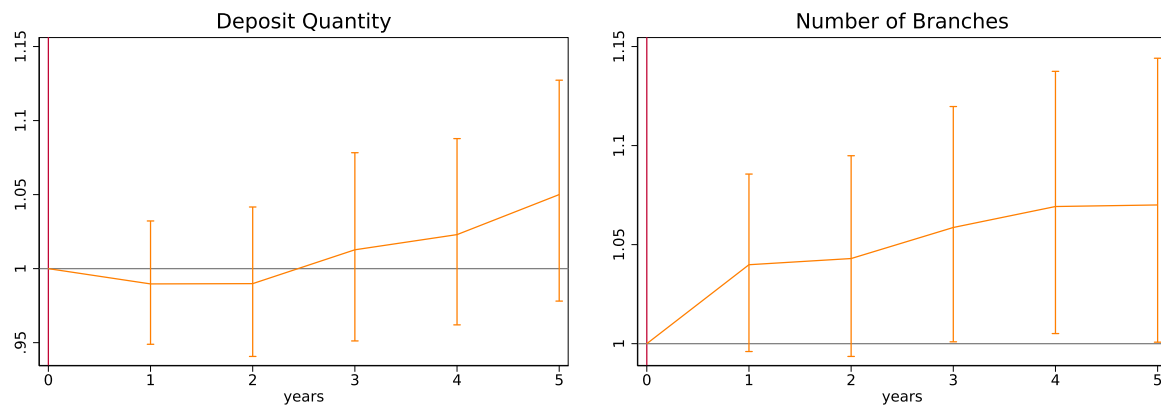
Fig. 6. Divestiture and CD Rates and Quantities. Event study graph showing average deposit quantity and branch count in treated/divestiture and non-treated/no-divestiture banking markets relative to merger year. Event study calculation is described in the text. Deposits data is calculated from the FDIC Summary of Deposits database.

Table 6

Deposit Rates on Certificates of Deposit, Controlling for Bank Characteristics. Difference-in-differences estimates from an ordinary least squares regression show the effect of predicted antitrust intervention on rates and local bank branches. Estimates come from a specification which estimates treatment effects for each cohort that mergers take place and for each year in the ten years following the merger. Coefficients shown in the table are the average treatment effect across cohorts and across years, weighted by the number of observations in the sample. See section 5 for more details on the empirical strategy. Controls are market averages of bank-level variables for banks which acquire branches through merger or divestiture. Risk controls are loan loss reserves divided by total loans abd non-performing loans divided by total loans. Size control is the log of total bank assets. Efficiency controls are the ratio of noninterest expenses to total assets and the ratio of salary expenses to total assets. Standard errors are clustered by banking market.

	(1) 12-Month CD	(2) 12-Month CD	(3) 12-Month CD	(4) 12-Month CD
Post X Treat	0.104***	0.110***	0.100***	0.0926***
	(0.0390)	(0.0390)	(0.0370)	(0.0350)
N	2127	2136	2136	2127
Cohort-Year FE	X	X	X	X
Merger FE	X	X	X	X
Risk Ctrl	X			X
Size Ctrl		X		X
Efficiency Ctrl			X	X

be larger or more efficient than the banks acquiring branches through a merger. Therefore, branch divestiture potentially affects bank competition by increasing bank concentration or by introducing different types of banks to the market. Both channels may be consistent with antitrust rules being "effective," although so far this paper has focused on the effects of bank concentration. To understand whether the bank-characteristics channel is relevant as well, this subsection will study the characteristics of the merging banks.

To investigate how merging bank characteristics affect competition, I estimate the main regression specification, but add control variables for the characteristics of the newly-entered banks interacted with the post-treatment dummy variable. If the characteristics of these banks explain why divestiture effects competition following mergers, then adding this interaction term should decrease the coefficient on the POST x TREAT dummy variable and make it no longer statistically significant.

I focus on three possible ways bank characteristics could be changing, drawing on the findings in Jayaratne and Strahan (1998). First, newly entered banks could manage risk in a different way, which I measure using loan loss reserves and the non-performing loan ratio. Second, newly entered banks could be larger, hence I control for the log of bank total assets. Finally, newly entered banks could be more efficient, which I measure using operating costs per dollar value of assets and salary ex-

pense per dollar value of assets. Each variable uses data on the new owner of the branches that once belonged to the target bank as of the merger year.

Table 6 shows estimates for the effect of intervention on 12-month CD rates, controlling for bank characteristics. Columns 1-3 show results with added controls for risk controls (loan loss reserves and non-performing loans divided by loans), size (log total assets) and expenses (operating costs divided by total assets and salary divided by total assets) respectively. Column 4 adds controls for all four variables. Across all five specifications, the coefficient on POST x TREAT hardly changes. Overall, there is no evidence that changing bank characteristics explain why divestitures lower the deposit rates offered by incumbent banks.

7.1.3. Home mortgage origination

The mortgage market is one of the most important consumer financial product markets. Getting a mortgage is the most important type of financial transaction a typical household participates in, and mortgages have broader economic importance, like for the transmission of monetary policy. For all these reasons, understanding the effects of competition in the mortgage market — and whether antitrust rules have the potential to change competition — is important. Furthermore, mortgages will provide an interesting point of comparison with the market

Table 7

Mortgage Lending. Difference-in-differences estimates from a Poisson regression show the effect of predicted antitrust intervention on total mortgage lending volumes and rates. Estimates come from a specification which estimates treatment effects for each cohort that mergers take place and for each year in the ten years following the merger. Coefficients shown in the table are the average treatment effect across cohorts and across years, weighted by the number of observations in the sample. See section 5 for more details on the empirical strategy. Loan quantities measured using HMDA data. In Column 1, the dependent variable is the total number of loans. In Columns 2 and 3, it is the refinancing and purchase loans respectively. In Columns 4 and 5, it is total loans from nonbank and local lenders respectively. In Column 6, it is the residualized interest rate calculated from credit registry data. Standard errors are clustered by banking market.

	(1) Total	(2) Refinance	(3) Purchase	(4) Non-Bank	(5) Local	(6) Rates
Post X Treat	0.106***	0.111**	0.0860**	0.119***	0.109**	-.0997**
	(0.0364)	(0.0442)	(0.0353)	(0.0427)	(0.0495)	(0.0479)
N	3965	3963	3972	3984	3905	1399
Cohort-Year FE	X	X	X	X	X	X
Merger FE	X	X	X	X	X	X

for small business loans. While small business loans rely heavily on relationships, mortgages are relatively standardized across banks and across regions, and do not generally rely on relationship-specific information. Another question I address is whether competition in the mortgage market is "local," and whether non-bank mortgage lenders can substitute for banks when bank competition changes.

I start by studying show how intervention affects total mortgage origination volumes. Estimates are shown in Table 7. Column 1 shows that antitrust intervention is associated with about 11% more loans overall. However, the average effect on lending hides differences between the refinancing and the purchase market. Columns 2 and 3 consider these markets separately. The estimated effect is positive for both markets but is somewhat larger in the refinancing market (11%) compared to the purchase market (9%). While the long-run effects on refis mortgages are a bit larger, event study plots will show that this is not true in the short run. The effects on refis depend on overall economic conditions and do not necessarily occur right away, whereas the effect on purchase mortgages is more immediate.

The rise of non-bank and FinTech lenders raises the question of whether antitrust rules based on local market definitions still make sense given that much of lending is now online. Columns 4 and 5 estimate the effect of competition on loan quantities for these types of lenders. The estimates show that non-bank lenders are affected by greater competition to a similar extent as local lenders, which are both similar to the overall average. This suggests that even non-bank lenders compete to a substantial extent with physical, local bank branches.

The estimates in Column 6 show the effect on interest rates. The number of observations available is less than half, since I use interest rate data from after 2000 and it is not available for all markets. I estimate that antitrust intervention reduces interest rates by about 0.10 percentage points, or about ten basis points. The implied elasticity lies between previous estimates. The most closely comparable previous estimates of the elasticity of mortgage demand come from Lo (2017), who estimate that a 25 basis point cut results in a 50 percent increase in the likelihood of obtaining a purchase mortgage. Compared to that estimate, the elasticity estimated here is about half as large. However, it is larger than the estimate from Bhutta and Keys (2016), who find that a one percentage point change in the interest rate leads to a 27% increase in the number of households extracting home equity from their homes.

Does greater competition bring in new types of borrowers to the mortgage market? Table 8 explores this question by estimating the effects of mergers on different groups of borrowers. Column 1 shows the effect of intervention on loan applications (rather than actual originations). The effect on applications is about one percentage point larger than the effect on originations, suggesting that new applications are accepted at slightly lower rates. Columns 2 and 3 split originations by

Table 8

Mortgage Lending, Composition. Difference-in-differences estimates from a Poisson regression show the effect of predicted antitrust intervention on mortgage lending volumes by mortgage and customer type. Estimates come from a specification which estimates treatment effects for each cohort that mergers take place and for each year in the ten years following the merger. Loan quantities measured using HMDA data. Column 1 estimates the effects of predicted antitrust enforcement on the number of loan applications using a Poisson regression. Columns 2-5 estimate the effect of predicted enforcement on the number of loans for different samples of loans and individuals, as noted by column. "Post" is an indicator equal to 1 following mergers, "Treat" is an indicator equal to 1 in markets which I calculate should have antitrust intervention, and "Period" is an indicator equal to 1 in 2010 or later. "Low Inc" refers to individuals who report an income of below $100,000 and "High Inc" have an income above this. "Low DTI" and "High DTI" refer to loans above and below the median DTI. Standard errors clustered by banking market.

	(1) Applications	(2) Low Inc	(3) High Inc	(4) Low DTI	(5) High DTI
Post X Treat	0.113***	0.120***	0.0887**	0.117***	0.0751
	(0.0412)	(0.0440)	(0.0435)	(0.0403)	(0.0520)
N	3963	3961	3939	3967	3962
Cohort-Year FE	X	X	X	X	X
Merger FE	X	X	X	X	X

income groups, based on whether borrowers have an income of above or below $100,000. Overall, the effect is larger on low income borrowers (a coefficient of 0.12) than on high income borrowers (a coefficient of 0.09). Similarly, columns 4 and 5 split the borrowers based on whether their debt-to-income ratio is below or above the sample median. The effects are larger for low-DTI borrowers than high-DTI borrowers and the latter is not statistically significant. The findings in this table show that lower-DTI borrowers are drawn into the mortgage market due to antitrust intervention relative to higher-income borrowers. A natural explanation is that the interest elasticity of mortgage demand is more elastic for low-income borrowers than for high-income borrowers.

The dynamic effects of refinancing are shown in Fig. 7. The event study plot shows effects which increase over time (although not statistically significantly so). Why should an increase in competition cause effects over time, rather than a one-time refinancing wave? Online Appendix F explores this question in greater detail by studying the effects of mergers taking place at different times. The evidence in this Appendix suggests that competition can have a lagged effect on refinancing, since refinancing is only worthwhile when interest rates are generally low. Mergers taking place during periods of rising rates may not affect refinancing until years later, when interest rates fall and make refinancing a worthwhile option. Online Appendix Table D.4 presents similar re-
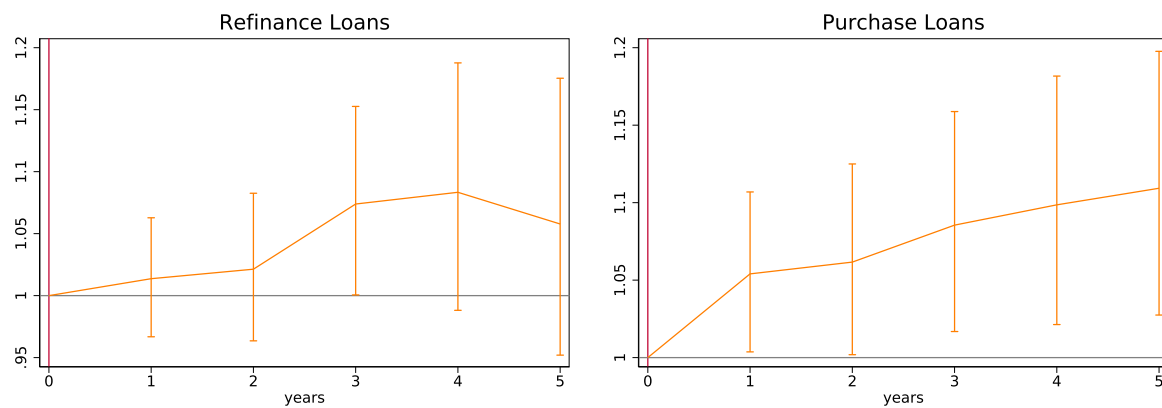
Fig. 7. Number of Refinancing Mortgages (left) and Purchase Mortgages (right). Both are event study graphs showing average mortgage origination by bank in treated/divestiture and non-treated/no-divestiture banking markets relative to merger year. Event study is estimated using a Poisson regression as described in the text. Mortgage data is calculated using raw mortgage data from HMDA.

sults to Table 7, but instead of showing treatment effects that are the average across all years, I calculate treatment effects pre-2010 and post-2010 and estimate the difference between the two. The differences are not statistically significant except for non-bank lenders, who have a larger effect in recent years, underlining the importance of these types of lenders more recently. This finding also underscores the point that non-bank lenders are affected by local antitrust intervention even in recent years. The effect on refinancing is also larger post-2010, which may be because of lower interest rates in this period.

These findings have three implications. First, antitrust rules have large effects in the mortgage market. The marginal antitrust enforcement decision does benefit consumers. Second, entry by nonbank lenders is not substantial enough to undo the effects of antitrust intervention, and indeed nonbank lenders respond similarly to intervention as banks, meaning that the mortgage market is still mostly "local." Third, refinancing is more sensitive to antitrust intervention than purchase mortgages.

7.2. Small business lending

Bank antitrust regulations assume that retail banking is a single "market" that is the same for all financial products. Therefore, the same merger policy affects small business loans as well as the consumer loans studied in the previous section. This provides an opportunity to study how the same merger rules affect different types of product markets. The view that retail banking is all a single market is inconsistent with theories of competition in the small business market which emphasize the importance of relationships. The centrality of relationships for small businesses makes it different from commoditized consumer markets like mortgages and deposits which do not require relationships in the same way. But do these differences matter for the effects of antitrust law?

Theories of small business lending make varying predictions. One set of theories emphasizes that small business lending requires banks to invest in learning about new borrowers' expected likelihood of repayment. Investing is costly for banks initially, but can result in *ex post* rents for lenders who have private information about good borrowers (Rajan, 1992; Petersen and Rajan, 1994). If the cost of learning about borrowers is high, then changes in the competitive environment might not affect borrowers who are already part of an existing relationship because switching lenders is expensive. However, lower competition might actually *increase* small business lending for new customers, since banks expect higher rents later in the relationship. Empirically, Nguyen (2019) shows that it is difficult for small businesses to find a new bank when their existing branch is closed.

On the other hand, some types of small business lending, such as small business credit cards, do not rely as heavily on relationships. Likewise, commercial mortgages can have valuable collateral and may not require banks to know borrowers as well. If these types of loans are similar to residential mortgages, competition could increase the overall amount of small business lending.

I estimate the effect of competition on small business lending using the Poisson regression difference-in-differences specification. I find that greater competition does not lead to a change in small business lending that is distinguishable from zero either in the short run or in the long run. Estimates in Table 9 show the effect of competition on total small business lending, measured at the county level. The estimated effect of competition on both categories of small business lending is not statistically distinguishable from zero. In columns 2 and 4 show that the same is true using the total dollar amount of small business lending. Columns 3 and 4 use ZIP rather than market-level data as the unit of observation.

Further evidence comes from an event study graph is shown in Fig. 8. Panel A shows the long-term effect on the number of loans and Panel B shows the effect on the loan volume. The average effect is negative but not statistically significant. There is statistically significant effect on loan counts (but not volume) two years-post merger, but the net average effect is neither statistically significant nor economically large. Overall, the graphical evidence in this figure suggests little effect of competition on the amount of small business lending even at longer time horizons. This long-run finding provides evidence against the view that competition affects small business lending even for either existing or new borrowers.

The general finding is that changes in local bank competition have no detectable effect on the amount of small business lending. This finding is consistent with the theoretical intuition that relationships limit small businesses' ability to "shop around" for a better rate even when the market structure changes (Nguyen, 2019). Boot and Thakor (2000) argue that greater competition can cause banks to shift their focus from relationship-intensive forms of lending. The finding that competition increases mortgage lending and has no effect on small business lending is consistent with this theory.

In addition to providing new evidence about how the small business market functions, this finding acts as a placebo check for the results from the consumer retail market. If the findings for the retail banking market were driven by an omitted variable, one would expect to find an effect in the small business market as well.

An important open question beyond the scope of this paper is how competition interacts with nonbank lending in the small business market. Specifically, do non-bank and Fintech lenders compete directly, as I have shown they do for mortgages, or do they mostly serve different markets? This question is particularly important given recent research documenting that these types of lenders increasingly substitute for traditional banks (Gopal and Schnabl, 2022; Erel and Liebersohn, 2022). Due to the lack of non-bank data in the CRA loan database, this ques-

Table 9

Small Business Lending. Difference-in-differences estimates from a Poisson regression show the effect of predicted antitrust intervention on rates an local bank branches. Estimates come from a specification which estimates treatment effects for each cohort that mergers take place and for each year in the ten years following the merger. Coefficients shown in the table are the average treatment effect across cohorts and across years, weighted by the number of observations in the sample. See section 5 for more details on the empirical strategy. Small business lending is measured using Community Reinvestment Act (CRA) data. In Columns 1-2 the unit of observation is a bank marking, and in Columns 3-4 it is a ZIP code. The dependent variable in columns 1 and 3 is the number of loans and in columns 2 and 4 it is the loan dollar amount. Standard errors are clustered by banking market.

	(1) Number of Loans	(2) Loan Volume	(3) Number of Loans	(4) Loan Volume
Post X Treat	-0.00776	-0.00497	0.0105	0.00399
	(0.0423)	(0.0417)	(0.0401)	(0.0444)
N	3881	3881	16288	16288
Cohort-Year FE	X	X	X	X
Merger FE	X	X	X	X
Geography	Market	Market	ZIP	ZIP
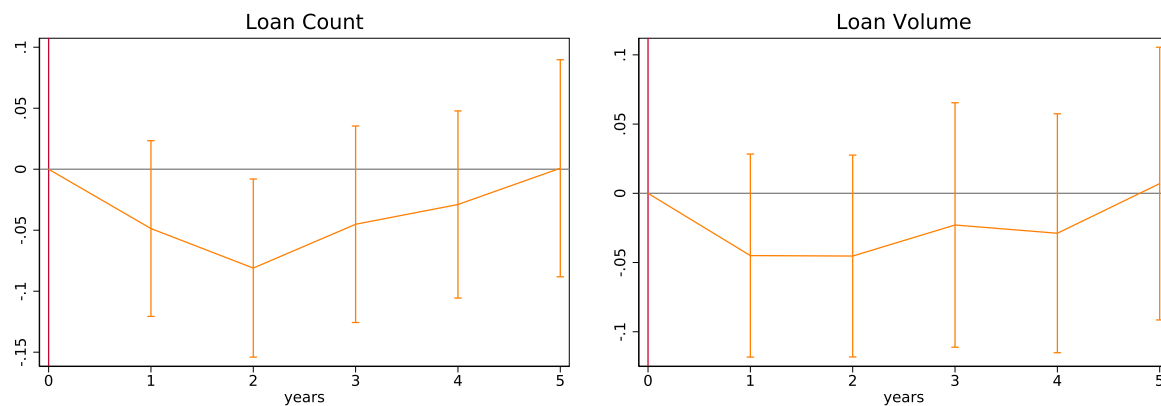
Fig. 8. Small Business Lending. Event study graph showing the number of small business loans to businesses in treated/divestiture and non-treated/no-divestiture banking markets, relative to merger year. Event study point estimates come from a Poisson regression which estimates treatment effects for each cohort that mergers take place and for each year in the ten years following the merger. Coefficients shown in the table are the average treatment effect across cohorts within each year relative to merger year, weighted by the number of observations in the sample. See section 5 for more details. Data is from the Community Reinvestment Act database.

tion is beyond the scope of this paper, but is a promising area for future research.

8. Conclusion

This paper investigates the effects of antitrust intervention on market structure, deposit markets, home mortgages, and small business lending. It uses a new source of exogenous variation in the competitive impact of bank mergers. The estimates show that intervention leads to a permanent decline in banking market concentration. Even many years after mergers take place, differences in HHI persist.

Clearly antitrust policy is effective for reducing banking market concentration in the banking sector. But the more important question is whether customers benefit. Here, the evidence is mixed and varies by market. In the mortgage market, greater refinancing quantities suggest that competition helps consumers. Likewise, consumers benefit from higher CD rates. On the other hand, I do not estimate any effect of antitrust intervention on the amount of small business lending. Sharpe (1997) argues that switching costs can reduce the competitiveness of the banking market, for example, because consumers may find it bothersome to change where their paychecks are deposited or because banks make switching inconvenient on purpose. Consistent with this, the estimated effect on deposit rates for checking and savings accounts is also very small compared to the effect on CDs.

These results support a view of banking in which different models apply to different markets. The "structure-conduct-performance" paradigm would appear to apply best to the relatively-commoditized home mortgage and deposit markets, whereas relationship lending models might apply better to the market for small business loans.

The increase in bank concentration since the passage of the 1994 Riegle-Neal Act has mirrored a rise in concentration across all U.S. industries. Contrary to some claims in the banking industry – such as statements made in BB&T Bank's merger application – the findings here show that local bank competition remains important. In other words, retail banking markets are still substantially "local". And, despite the rise of online and nonbank lending, there is not much substitution between these types of lenders and traditional banks when competition changes. For these reasons, antitrust intervention has substantial effects in markets for retail consumer financial products. On the other hand, policy-makers who want to regulate changes in competition for products where relationships are important will have to find other tools.

CRediT authorship contribution statement

Jack Liebersohn: Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Software, Resources, Project administration, Methodology, Investigation, Funding acquisition, Formal analysis, Data curation, Conceptualization.

Declaration of competing interest

I have no relevant disclosures to make. There are also no conflicts of interest for this submission.

Data availability

I have attached a link under the "Research Data".

Supplementary data for "How Does Competition Affect Retail Banking? Quasi-Experimental Evidence From Bank Mergers" (Original Data) (Mendeley Data)

Appendix A. Supplementary material

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.jfineco.2024.103797.

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