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Did technology contribute to the housing boom? Evidence from MERS[☆]

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ABSTRACT

We examine the effects of the Mortgage Electronic Registration System, or MERS, on mortgage origination volumes and foreclosure rates prior to the Great Recession. MERS was introduced in the late 1990s and significantly reduced the cost and time associated with secondary mortgage sales. Using novel data from the Massachusetts Registry of Deeds, we show that the introduction of MERS led to an expansion in mortgage credit supply that was primarily fueled by nonbank lenders originating mortgages to low-income borrowers. We also find that foreclosure rates were higher on these mortgages. Our paper provides a new explanation for the credit supply increases observed prior to the 2008 financial crisis and for the disproportionate supply increase observed in low-income areas.

1. Introduction

Why did the supply of residential mortgage credit expand so dramatically in the early 2000s? Starting with Mian and Sufi (2009), a large literature has found that credit supply increased significantly prior to the 2008 financial crisis. However, the origins of this credit supply increase remain relatively unexplored. For example, why did credit supply increase so dramatically in the early 2000s

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instead of at some other point in time? Why was much of the increase in credit supply fueled by nonbank lenders such as mortgage brokers and mortgage bankers (see, e.g., Berndt et al., 2016)? Finally, why were so many new mortgages originated to lower-income borrowers who were often of questionable credit quality (see, e.g., Keys et al., 2010)?

In this paper, we argue that financial innovation can help to explain this collection of events. In particular, we provide evidence that the introduction of the Mortgage Electronic Registration System (MERS) in the late 1990s played a meaningful role in boosting credit supply prior to the 2008 financial crisis.[1] MERS is a private electronic mortgage registry that allows member institutions to buy and sell mortgages without having to file and audit legal documents at local land record offices. More than two-thirds of all mortgages in the United States were registered through MERS by the end of 2007, less than a decade after its introduction (Ketcham, 2012). As such, MERS represented a major innovation in the secondary market for mortgages.

Our central argument is that MERS indirectly helped to fuel the boom in mortgage credit supply prior to the 2008 financial crisis. By reducing the time and costs associated with secondary mortgage market transactions, we argue that the introduction of MERS increased the demand for purchased mortgages. This extra demand in turn led to higher mortgage origination volumes. Furthermore, since MERS speeds up the mortgage sale process, capital-constrained institutions could free up capital to make additional loans more quickly. Hence, we argue that the MERS technology *itself* led to increases in the supply of mortgage credit.

We test this argument using a novel database from the Massachusetts Registry of Deeds containing all land records filed with county clerks in the state of Massachusetts from 1990 to 2018. To our knowledge, these land records contain the only publicly available loan-level data on MERS registration. The land records also contain detailed information about the originating lender, whether the mortgage was sold, the identity (if any) of the mortgage purchaser, and whether the mortgage was ever foreclosed upon. As such, our data set provides a comprehensive picture of the primary and secondary market for mortgages within six counties in Massachusetts. In some tests, we also combine lender-purchaser relationships identified from the Massachusetts Registry of Deeds with nationwide origination data from the Home Mortgage Disclosure Act (HMDA) loan application register.

Our results suggest that the use of the MERS technology is associated with significant increases in the supply of mortgage credit. We find that mortgage origination volumes increase by approximately 10% and mortgage approval rates increase by about 4% each year at MERS member lenders, relative to non-MERS lenders. In aggregate, we estimate that total credit supply increases by 3.4% per year as a result of MERS. While our aggregate measures are extrapolated from one state (Massachusetts), our results suggest these credit supply effects represent approximately 20% of the total credit supply increase observed by Mian and Sufi (2009) and are of similar magnitude to the credit supply increases caused by the removal of anti-predatory lending laws (Di Maggio and Kermani, 2017).

We next explore the cross-sectional consequences of the MERS technology. First, nonbank lenders should arguably be best placed to meet the increase in demand for purchased mortgages, since these lenders originate all mortgages with the intent to sell them. Similarly, the impact of the MERS technology should be largest for mortgages sold multiple times, such as those destined for private label securitization (PLS) pools. Hence, we argue that increased origination volumes are likely to be largest for mortgages originated by nonbank lenders that are sold into PLS pools. In addition, the marginal new mortgage origination in this setting would likely be a poorly screened subprime mortgage, since prime borrowers had no trouble obtaining credit (Bhutta and Keys, 2016; Akey et al., 2021) and lenders face reduced incentives to screen mortgages they intend to sell (Calomiris and Kahn, 1991; Aghion et al., 2004; Keys et al., 2010).

We present evidence consistent with these arguments. First, we find that the credit supply increases we observe are driven almost entirely by loans originated by nonbank lenders that are ultimately sold into PLS pools. We also find that credit supply increases are greater in areas populated by lower income borrowers: MERS member nonbank lenders tend to originate mortgages in census tracts with 1.6% lower average income than the tracts served by MERS member banks. Finally, long-term foreclosure rates are higher for mortgages originated by MERS member nonbank lenders than for those originated by other types of institutions. Hence, while the introduction of MERS represented a significant financial innovation for the mortgage industry, this innovation appears to have also contributed to the origination of a significant quantity of low-quality mortgages during the housing boom.

Our empirical design exploits the bilateral nature of the MERS system: to make use of the MERS technology, the buyer and seller of a mortgage must *both* belong to MERS. Our primary tests compare mortgage origination volumes for MERS member lenders relative to non–MERS member lenders in the periods before and after *a common purchasing partner* joins MERS. That is, suppose lender A (existing MERS member) and lender B (not a MERS member) both operate in a Zip code, and both lenders sell mortgages to purchaser C. Our tests compare changes in origination volumes at lender A with changes in origination volumes at lender B after *purchaser C* joins MERS. This design ensures that any changes in origination volumes are not a function of the lenders themselves joining MERS, but are rather a function of one of their purchasing partners joining MERS at a later time.

Our main regressions also include Zip code × year, purchaser × year, and purchaser × lender (i.e., relationship) fixed effects. Zip code × year fixed effects absorb any time-varying demand shocks within a Zip code and help to ensure that our results are not driven by changes in consumers' demand for mortgages (see, e.g, Barberis et al., 2018). Purchaser × year fixed effects absorb any time-

[1] MERS is a registered trademark of MERSCORP Holdings Inc.

varying shocks to the demand for purchased mortgages by a given institution and ensure that our results are not driven by increased investor demand for mortgages or mortgage-backed securities (see, e.g., Chernenko et al., 2014). Relationship fixed effects help to ensure that our results are not driven by the formation of new lender-purchaser relationships after a purchaser joins MERS. Parallel trends tests, placebo tests, and a variety of other robustness checks help to assuage remaining concerns about the endogenous take-up of MERS membership, correlated (omitted) demand and supply shocks, and any endogenous switching of relationships between lenders and purchasers that are not absorbed by our fixed effects. Our results are also robust to a variety of empirical specification choices.

Our paper makes four primary contributions to the literature. First, a large literature has examined the increase in mortgage credit in the run-up to the financial crisis [see, e.g., Mian and Sufi, 2009; Mian and Sufi, 2011, Adelino et al., 2016, and Di Maggio and Kermani, 2017, among others]. We contribute to this literature by identifying a new factor, MERS, that contributed to the increase in aggregate credit supply prior to the crisis. In particular, we find higher origination volumes *particularly* by nonbank lenders, particularly for low credit–quality borrowers following the introduction of MERS. Despite a wealth of evidence on the role of subprime mortgages in the financial crisis (see, e.g., Ashcraft and Schuermann, 2008; Mian and Sufi, 2009; Demyanyk and Hemert, 2011; Purnanandam, 2011; and Dell'Ariccia et al., 2012), the literature has not yet identified why nonbank lenders in particular were responsible for the rise in mortgage originations to low credit–quality borrowers prior to the crisis. Our paper also provides additional evidence supporting the credit supply view of the financial crisis, which asserts that the financial sector played an active role in the boom and bust, as opposed to the passive view expressed in Foote et al. (2012) and Adelino et al. (2016).

Second, our paper contributes to the literature on secondary mortgage sales and securitization. For example, Keys et al. (2010) show that both the quality of initial screening and subsequent loan performance are worse for mortgages that are originated with an intent to sell. Piskorski et al. (2010) show that the foreclosure rates on securitized mortgages are higher than portfolio-owned delinquent mortgages and Agarwal et al. (2011) show that securitization reduces the likelihood of mortgage renegotiation. These findings are particularly relevant in our setting given that institutions only benefit from MERS if they sell mortgages. We contribute to this literature by showing that the MERS technology itself (as opposed to, say, increased investor demand for securitized mortgages) is associated with higher subsequent foreclosure rates, particularly for mortgages originated by nonbank lenders.

Third, our paper adds to the growing literature on the “plumbing” of the mortgage market and the institutional details associated with mortgage securitization. Hunt et al. (2012) first observed the pervasive use of MERS within the U.S. mortgage market. Stanton et al. (2014) provide a detailed analysis of the industrial organization of the U.S. residential mortgage market and highlight the interrelationships among originators, funding sources, and purchasers. Our paper complements these studies by specifically analyzing the effects of MERS on mortgage credit supply.

Fourth, our results contribute to the literature on FinTech (Buchak et al., 2018; Fuster et al., 2019) and the efficiency of innovations within the finance industry (Philippon, 2015; Bai et al., 2016). We show that the MERS technology created cost and time savings that led to the expansion of mortgage credit, thereby providing new evidence that FinTech innovations can benefit the real economy. However, we also find that the increased credit supply effects of MERS are linked to higher foreclosure rates, suggesting that even successful financial innovations like MERS could be associated with unintended consequences that can have important impacts on the health of the financial sector and the real economy.

Finally, we stress that many of our results are based on data from one state, Massachusetts, rather than a nationwide sample. We attempt to overcome this limitation by merging our Massachusetts lender/purchaser relationship data with nationwide origination data from HMDA. However, these tests are limited to lenders and purchasers that operate in multiple states and are therefore not representative of the mortgage market as a whole. We are also unable to account for general equilibrium effects.[2] Nonetheless, while imperfect, our evidence strongly suggests that the introduction of MERS had a sustained effect on mortgage credit supply during the housing boom.

The rest of this paper is organized as follows. Section 2 describes the institutional details of MERS and explains why the introduction of MERS could cause credit supply to increase. Section 3 describes the data and provides summary statistics. Section 4 describes our main findings and contains a number of robustness checks and other analyses. Section 5 concludes.

2. Institutional background

2.1. MERS

The Mortgage Electronic Registration System is a privately owned mortgage registry that was developed by Fannie Mae and Freddie Mac, incorporated in 1997, and subsequently sold to a small consortium of large mortgage market participants now doing business as MERSCORP Holdings. MERS was created in response to concerns that the process of registering mortgage transactions with local land record offices was impeding the sale of mortgages on the secondary market (Cocheo, 1996).

When a homeowner takes out a mortgage, they are required to sign a mortgage document that provides the lender with legal claim to the property in the event of a default. This document is registered at the county land records office. If the mortgage is later sold, an “assignment document” is also filed with the county land records office, formally transferring the legal claim to the property to the

[2] For example, if MERS causes increases in credit supply, this can cause house prices to increase, which in turn can increase the demand for mortgage credit, thereby causing more lenders to consider joining MERS.

new owner. By auditing the history of assignment documents for a given mortgage, interested participants such as owners, financial institutions, and lawyers can determine which parties have claim to the title of the property. The smooth functioning of the mortgage market relies heavily on accurate mortgage assignment data since no institution would originate or purchase a mortgage without certainty that they could take possession of the property upon default.

The rapid increase in securitization activity during the 1990s placed great strains on local land records offices. As part of the securitization process, mortgages are often sold multiple times; for example, a mortgage in a private label securitization is typically sold four or five times before it reaches investors.[3] Consequently, increased securitization activity resulted in a dramatic increase in the number of assignments required to be filed, which overwhelmed county land records offices and created severe backlogs. Prior to the introduction of MERS, the filing, preparation, and audit of assignment documents for an average mortgage pool could take up to six months to complete (Arnold, 2010), and cleaning up assignment problems alone could cost as much as $250,000 (Cocheo, 1996; Hansen, 2010).

MERS was designed to remove these costly, time-consuming impediments to the mortgage sale process by eliminating the need to file assignment documents with county land records offices. When a MERS lender originates a mortgage, it still registers the mortgage document in the county land records, but it lists both the lender *and MERS* as beneficial owners of the mortgage.[4] Legally, since MERS is a beneficial owner of the mortgage, the mortgage can be transferred between MERS members without having to file assignment documents since "ownership" of the mortgage has not changed. Mortgage sales are instead tracked within the private MERS registry.

There are three primary benefits to using MERS. First, MERS eliminates the direct dollar costs associated with filing assignment documents at county land record offices, which are roughly $35 per assignment. For a typical private label securitization in which mortgages are sold five times (Peterson, 2010; Levitin, 2013), the direct cash savings from using MERS would thus be approximately $160, or roughly 2% of the average total costs of originating a mortgage.[5]

Second, MERS reduces the time and effort required to audit a mortgage's assignment history. Since ownership changes are tracked through the MERS registry, assignment histories can be audited without having to perform numerous records requests in county land offices.[6] While it is difficult to quantify the dollar value of savings associated with auditing, Hansen (2010) conservatively estimates an additional direct savings of roughly $100 per mortgage.

Finally, since it speeds up the mortgage sale process, capital-constrained institutions can use MERS to free up capital to make additional loans or loan purchases more quickly. Collectively, MERS benefits members by reducing both the time and costs associated with selling a mortgage, and these savings are likely to be largest for mortgages that are sold multiple times during the securitization process.

Despite these benefits, there are two limitations to the usefulness of MERS. First, if a mortgage owner never intends to sell a mortgage, then registering the mortgage with MERS would yield no benefits. Second, the buyer and seller of a mortgage must *both* be MERS members in order for MERS to be useful. If a non-MERS member originates a mortgage and sells it to a MERS member, the MERS system cannot be used and an assignment document must still be filed with the county land office. The same is true if a MERS member sells a mortgage to a non-MERS member. Hence, the benefits associated with MERS are only obtained if a mortgage is originated (or purchased) with the intent of being sold and if both parties, the seller and purchaser, are already members of MERS.

2.2. MERS and credit supply

We argue that the MERS technology impacted mortgage origination volumes through two distinct channels. First, by reducing the time and costs associated with mortgage sales, the use of MERS should cause the supply curves of securitization sponsors and other mortgage purchasers to shift outward, leading to an increased demand for purchased mortgages and hence an increase in origination volumes. Second, capital-constrained lenders should be able to sell mortgages faster, thereby freeing up more capital that can be used to increase origination volumes.

These channels also yield cross-sectional implications. For example, the benefits of MERS should be greater for nonbank lenders than banks. Nonbank lenders such as mortgage brokers, finance companies, and so-called mortgage banks typically sell 100% of the mortgages that they originate, are more likely to be capital-constrained, and are more likely to originate the mortgages that end up in PLS pools.[7] In contrast, commercial banks often keep

[3] In most PLS transactions, the originator sells the mortgage to an aggregator, which in turn sells it to a sponsor, which pools the mortgage with other mortgages and sells the pool to a depositor, which in turn sells the pool to a Real Estate Mortgage Investment Conduit (REMIC) trust or a trustee to be held for the benefit of the trust. The trust then issues securities. See, e.g., Peterson (2010) and Levitin (2013) for more details about the legal requirements of residential mortgage securitization and the ABA Section of Litigation Annual Conference 2013 for related legal anecdotes.

[4] A "beneficial owner" is a legal term conveying specific property rights ("use and title") to a person even though legal title of the property belongs to another person. MERS refers to this process as "MERS as Original Mortgagee," or "MOM."

[5] Typical county land office registration fees are $35 and MERS registration is $11.95, so total savings are $\$35 \times 5 - \$11.95 = \$163.05$. The average cost of originating a mortgage is roughly $8,500 according to the Mortgage Bankers Association. Source: https://www.mba.org/mba-newslinks/2018/march/mba-newslink-tuesday-3-27-18/.

[6] In addition, MERS-registered mortgages are assumed to be "clean" and hence do not undergo the same detailed audit as non-MERS mortgages. See the white paper "Understanding Current Assignment Verification Practices," by Nationwide Title Clearing (http://info.nwtc.com/wp-understanding-current-assign-thank-you-page), for more details on assignment audit and validation requirements and mortgageorb.com for anecdotal evidence on assignment validation.

[7] According to Congressional testimony from the Mortgage Bankers Association, nonbank lenders are more likely to sell their mortgages to

sizable portfolios of mortgages on their balance sheets (Buchak et al., 2018), and are therefore less likely than nonbanks to benefit from the introduction of MERS.

In addition, the marginal new mortgage originations that result from MERS would likely be lower quality subprime mortgages. Prime borrowers are only rarely denied credit (Bhutta and Keys, 2016; Akey et al., 2021) and lenders face lower screening incentives for mortgages that are later sold (Calomiris and Kahn, 1991; Aghion et al., 2004; Keys et al., 2010). Given an increase in the demand for purchased mortgages, nonbank lenders should thus naturally turn to the subprime market to find new borrowers.[8] Hence, we argue that MERS caused an increase in credit supply that was predominantly fueled by nonbank lenders originating mortgages to lower quality and subprime borrowers.

3. Data

3.1. Massachusetts land records data

We obtain county land records data from 1990–2018 in bulk format from the Registry of Deeds Division of the Secretary of the Commonwealth of Massachusetts. This data, which is also available to the public at http://masslandrecords.com, contains every property-related document filed with county clerks in each of the state's 14 counties.

3.1.1. Mortgage and assignment documents

Our data set is primarily constructed using mortgage documents and assignment documents. For each property loan, a mortgage document is filed with the county clerk. The mortgage document contains the address of the property, the name of the property buyer (the mortgage grantor), the institution making the loan to fund the purchase (the mortgage grantee), and the total consideration paid by the buyer to the seller. The mortgage document also lists MERS as a mortgage grantee alongside the lender if the lender has registered the mortgage with MERS. We are aware of no other public data sources that allow us to track MERS activity.

Assignment documents state the name of the prior lender (the assignment grantor) and the name of the new lender (the assignment grantee), among other items. We link each assignment document with its corresponding mortgage through a linking file provided by the Registry of Deeds, and only keep those assignment documents for which the seller of the mortgage is the same institution that originated the mortgage.[9] This allows us to identify relationships between mortgage originators and institutions that purchase mortgages on the secondary market. The land records data also contain foreclosure documents, which we link to mortgage documents via the linking file provided by the Registry of Deeds.

One significant caveat to our data is that once a lender joins MERS, any subsequent mortgage sales by that lender may not appear in our data set. This is precisely because of MERS: if the original lender lists MERS as a mortgage grantee, then no assignment document needs to be filed with the county clerk when a mortgage is sold to another MERS member, and hence, the sale will not appear in our data. We therefore infer relationships between a lender and a purchaser based on their relationship histories prior to both parties joining MERS.[10]

3.1.2. Data set construction

We construct an unbalanced loan-level panel data set spanning the sample period 1990–2018. We first combine our data so that each mortgage (plus subsequent assignments) corresponds to one row in our final data set. We manually determine whether each unique lender and purchaser in our data set is an individual or institution and discard all data from individuals or trusts controlled by individuals. All information in the land records is input by hand, and hence, there are numerous ways of recording the same institution. To ensure that each lender is coded accurately in our data set, we conduct a fuzzy matching exercise supplemented by manual verification to ensure that, for example, "JP Morgan Chase," "J.P. MorganChase," and "JPMChase" are all matched to the same institution. In total, our sample contains roughly 50,000 unique institution names corresponding to approximately 6000 unique institutions operating in the state of Massachusetts.

We then manually match each institution to the HMDA data set by name and obtain the institution's lender code, which can be zero (commercial banks), one (subsidiaries of banks), two (subsidiaries of bank holding companies), three (nonbank lenders), or five (affiliates of banks).[11] We manually confirm HMDA lender type information using Google searches. To ensure completeness, we manually look up institutions that do not have an HMDA match via Google searches to identify whether they are a lending institution (and if so, what type).[12] We also hand-collect data such as bank regulatory identification numbers (RSSD IDs) and M&A activity for each depository institution from the

loan aggregators (also known as "correspondent lenders"), and these aggregators tend to focus on mortgages that are not conventional, conforming mortgages and hence do not meet the requirements to be sold to government-sponsored entities like Fannie Mae or Freddie Mac (Stanton et al., 2014). Hence, mortgages originated by nonbank institutions are more likely to end up in private label securitizations.

[8] In fact, most subprime mortgages were originated by nonbank lenders during the housing boom (Berndt et al., 2016)

[9] We incorporate this restriction because there are instances in which an assignment could be filed even if a mortgage is not sold. For example, if one bank acquires another bank and decides to sell part of its acquired loan portfolio, assignments will be filed but the assignment grantor will be the acquiring bank and the mortgage grantee will be the acquired bank.

[10] For our main empirical tests outlined in Section 4, we only need to infer that the relationship lasts for at least one year after a purchaser becomes a MERS member.

[11] To ensure that our classifications are correct, we manually checked each lender and purchaser using the Federal Reserves NIC website, HUD websites, and systematic Google searches. For example, a Google search of "Long Beach Mortgage" clearly shows that the company is a subsidiary of Washington Mutual Bank, and hence the lender code of three (independent mortgage company) provided by HMDA would be replaced with a lender code of one (subsidiary of a depository institution) in our sample.

[12] Lenders originating less than $25m per year are not required to provide HMDA disclosures. Hence, this manual step ensures that our data set is representative of all lenders, even very small ones.

National Information Center.[13] Our final data set consists of approximately 1.6 million mortgages originated between 1990 and 2018.

We identify a mortgage as being a MERS mortgage if the Mortgage Electronic Registration System (or some variant of this spelling) is listed as a mortgage grantee when the mortgage is originated. We infer the date that each institution joined MERS as the first date for which MERS appears alongside the lender as a mortgage grantee. For example, if bank A and MERS are both listed as mortgage grantees on July 1, 2004, and MERS never appeared as a mortgage grantee on bank A's previous mortgages, then we would infer that bank A became a MERS member in July 2004 and define the MERS start year as 2004 for that bank. We identify MERS start dates for both lenders and mortgage purchasers, thereby allowing us to determine, for a given lender-purchaser pair, whether one or both institutions are MERS members at a given point in time.

Our final data set contains data from 6 of the 14 counties in Massachusetts: Berkshire, Franklin, Hampshire, Middlesex, Suffolk, and Worcester counties. Collectively, these counties account for more than 52% of the state's population and contain four of the five largest cities in the state (Boston, Worcester, Lowell, and Cambridge). The six counties in our final sample are also spread out across the entire state. We exclude data from the other eight Massachusetts counties because the data from these counties are either incomplete or are not available in a research-friendly electronic format.

One concern is that the six counties we include in our sample may not be representative of either Massachusetts or the United States as a whole during our sample period. To address these concerns, we compare real GDP growth, employment growth, and house price appreciation in the six counties we study versus Massachusetts and the United States as a whole, both before and during our sample period. In untabulated results, we find that the six counties in our sample are similar to the excluded counties (and the United States as a whole) in terms of demographics, homeownership, home values, and economic trends. We also combine our relationship data from Massachusetts with nationwide mortgage origination data to provide a rough assessment of the broader impact of MERS on mortgage origination volumes. Nevertheless, given that all of our results depend at least partially on data from six counties in Massachusetts, it is possible that our results may not be representative of the full effect of MERS during our sample period.

3.2. Other data sources

We supplement our Massachusetts land records data with data from HMDA. This data set contains information on virtually all residential mortgage applications in the United States over the entirety of our sample period. We are able to match HMDA data to our Massachusetts data by manually matching lender names from the land records data with lender names in HMDA, which we then link to each institution's numeric HMDA identifier. If the lender is a bank, we also obtain accounting and financial information from quarterly bank-level FFIEC 031/041 reports (commonly known as the Call Reports). Finally, we obtain census tract information (including demographics) from the U.S. Census Bureau and tract-level house price indices from Bogin et al. (2019).

3.3. Summary statistics

Panel A of Table 1 presents summary statistics on the percentage of lenders and mortgage originations in each census tract/year that are MERS members or are registered with MERS. The table shows that MERS membership grew extremely rapidly following the introduction of MERS in the late 1990s. By 2001, roughly 20% of all mortgages were registered with MERS, and roughly 12% of all lenders were utilizing the MERS system. By the end of 2007, over 50% of all mortgages were registered with MERS and nearly half of all lenders were MERS members.

Panel B of Table 1 provides summary statistics on the number of mortgages assigned to MERS during our sample period. The table shows that approximately 379,000 (or around 23%) of the mortgages originated during our sample period listed MERS alongside the lender. Panel B also shows that most of the mortgages listing MERS alongside the lender (more than 248,000) were originated by nonbank institutions rather than banks and their subsidiaries and affiliates (collectively referred to as "banks"). Conditional on originating a mortgage, nonbanks were also more likely to register the mortgage with MERS: Panel B shows that more than 55% of all mortgages originated by nonbanks listed MERS alongside the lender, whereas only 11% of mortgages originated by banks listed MERS alongside the lender.

Table 2 provides summary statistics on many of our key variables of interest. Panel A shows that, on average, lenders within a census tract in our sample originate 110 mortgages per year with a face value of approximately $34 million. The average foreclosure rate on these mortgages is 1.2%. Panel B breaks out these statistics by lender type. On average, nonbanks in total originate 35 mortgages worth about $8 million per census tract per year, while banks originate in total 81 mortgages worth approximately $27 million per census tract per year. The average foreclosure rate for mortgages made in these census tracts is significantly higher for nonbanks (2.1%) than for banks (0.9%). Panel B also examines statistics specifically for MERS lenders. MERS lenders on average originate 44 mortgages in total worth about $10.6 million per census tract per year, and the foreclosure rate for MERS mortgages is similar to that for nonbanks at 2.2%.

Fig. 1 plots the time series fraction of all mortgages within the sample that have assignment documents filed at some point during their lives. Despite the fact that our sample period includes the housing boom of the early 2000s, the figure shows that the fraction of mortgages being assigned (i.e., sold) has actually *fallen* significantly over time in the land records data. This is precisely due to the introduction of MERS: since registering a mortgage with

[13] Specifically, we use the National Information Center's "institution search" web page available at https://www.ffiec.gov/nicpubweb/nicweb/searchform.aspx.

Table 1
Summary statistics: MERS adoption.
This table contains summary statistics using data obtained from the Massachusetts Registry of Deeds. Panel A shows the proportion of all mortgages that are registered with the Mortgage Electronic Registration System (MERS) and the proportion of all lenders that are MERS members by year and averaged across census tracts. Panel B shows the total number of mortgages originated in the sample, by lender type. Bank-originated mortgages are defined as mortgages originated by institutions with HMDA lender code 0, 1, 2, or 5, which are banks, subsidiaries of banks, subsidiaries of bank holding companies, and affiliates of banks respectively. Nonbank-originated mortgages are defined as mortgages originated by institutions with HMDA lender code 3, which are stand-alone institutions not related to banks either as a subsidiary or as an affiliate. HMDA lender codes are obtained through a fuzzy matching process by institution name, and a manual search of any nonmatched names. The table also shows the number of mortgages by lender type and in total that are registered with the Mortgage Electronic Registration System at origination.

Panel A

Year	Census tracts	MERS-registered mortgages	MERS-member lenders
1998	551	0.0%	0.0%
1999	552	0.9%	0.4%
2000	547	6.7%	4.2%
2001	541	19.0%	12.1%
2002	556	24.6%	17.2%
2003	556	37.6%	30.2%
2004	548	39.7%	32.3%
2005	558	50.2%	40.4%
2006	547	55.7%	46.6%
2007	549	51.7%	44.4%
2008	531	43.0%	39.2%
2009	540	54.9%	50.7%
2010	547	57.0%	53.3%
2011	546	53.5%	50.2%
2012	549	58.5%	53.5%
2013	544	55.3%	50.2%
2014	547	50.3%	44.5%
2015	540	56.3%	49.9%
2016	545	58.7%	51.9%
2017	547	54.8%	47.5%
2018	548	56.4%	45.8%

Panel B

	Mortgages	MERS-registered mortgages	% MERS
Bank originated	1,162,762	130,738	11%
Nonbank originated	451,259	248,563	55%
Total	1,623,199	379,301	23%

MERS removes the need for the buyer and seller to file an assignment document, mortgages can be sold to securitization trusts (or to other lenders) without a subsequent assignment document having to be filed.

4. Results

4.1. OLS results

We begin by running a series of simple OLS regressions using data from the Massachusetts land records to measure simple correlations between MERS membership and mortgage origination volumes. We collapse our loan-level panel into an institution by census tract by year data set and use this data set to assess whether credit supply increases after lenders join MERS.

The dependent variables for our tests are the (log) dollar origination volume and (log) number of mortgages originated by a given lender within a given census tract and year. We define a dummy variable, *Post*, that takes the value of one if a lender is a MERS member in any given year, and takes the value of zero otherwise. We then regress mortgage origination volumes on this dummy variable. Columns (1) and (2) of Table 3, which contain only lender fixed effects, show that, on average, mortgage originations are higher by 52% (dollar volume) and 8% (count) for lenders that are MERS members. These results suggest that MERS membership is strongly positively correlated with increased mortgage origination.

4.1.1. Potential sources of bias

While these simple OLS tests are instructive, they are subject to some potential biases. For example, omitted factors such as demand shocks could be correlated with a lender's decision to join MERS. This bias would likely inflate our coefficient estimates since (for example) positive mortgage demand shocks could cause lending volumes to increase and could also cause lenders to join MERS. In addition, using the *lender* joining MERS as the treatment event could result in measurement error because MERS

Table 2
Summary statistics: Massachusetts land records.
This table contains summary statistics using data obtained from the Massachusetts Registry of Deeds. Panel A shows total mortgage origination by census tract-year for the full sample. Panel B shows mortgage origination by census tract-year-lender type where nonbanks are identified as institutions with an HMDA lender code of 3, or manually identified when no HMDA code exists. Banks are identified as institutions with an HMDA lender code of 0, 1, 2, or 5, or manually identified when no HMDA code exists. Panel B also shows mortgage origination by MERS lenders.

	Panel A: Full sample					
	Average	Min	25	50	75	Max
All census tracts						
No. mortgages per census tract-year	110	1	14	57	150	1512
Total origination per census tract-year	33,500,000	20,000	2,438,247	10,500,000	29,400,000,000	13,100,000,000
Foreclosure rate	1.2%	0.0%	0.0%	0.0%	0.9%	100%
	Panel B: By lender type					
	Average	Min	25	50	75	Max
Nonbanks						
No. mortgages per census tract-year	35	1	6	18	44	567
Total origination per census tract-year	7,950,030	29,500	935,559	3,195,377	8,831,999	2,410,000,000
Foreclosure rate	2.1%	0%	0%	0%	0%	100%
Banks						
No. mortgages per census tract-year	81	1	11	42	111	991
Total origination per census tract-year	27,100,000	20,000	1,890,415	7,531,018	21,000,000	13,100,000,000
Foreclosure rate	0.9%	0%	0%	0%	0.4%	100%
MERS lenders						
No. mortgages per census tract-year	44	1	7	24	62	424
Total origination per census tract-year	10,600,000	99,950	1,667,820	5,518,243	13,900,000	2,420,000,000
Foreclosure rate	2.2%	0%	0%	0%	1.4%	100%

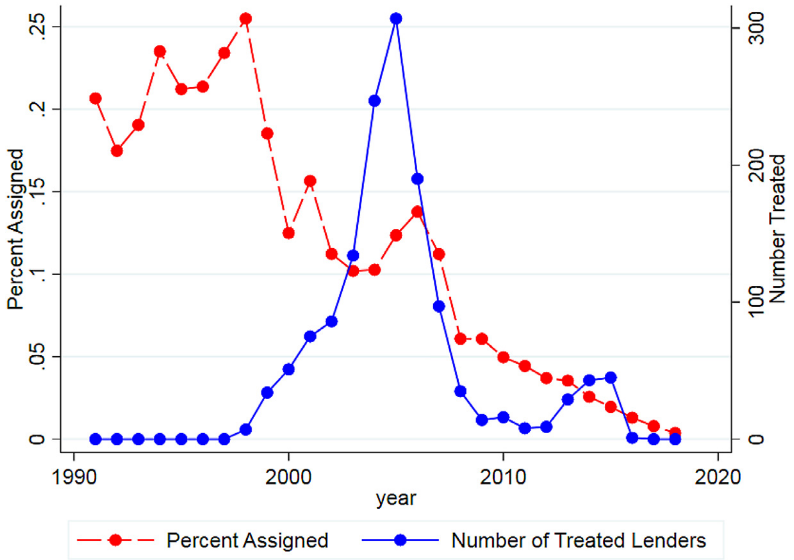

Fig. 1. Fraction of all mortgages with assignment documents filed immediately after origination (left-hand scale) and number of treated lenders per year (right-hand scale).

lenders do not benefit until one or more purchasers also join MERS. This bias would lead to attenuation of the estimated coefficient because the misclassification of a nonevent as an event (and vice versa) would induce a negative correlation between the error term and the true value (Aigner, 1973).

We account for correlated omitted variables by adding Zip code by year fixed effects to our tests. These fixed effects account for the presence of time-varying correlated omitted variables in a given Zip code. Columns (3) and (4) show that the correlation between MERS and credit supply drops significantly once we include Zip code by year fixed effects; while statistically significant, the effects of MERS on the dollar volume and count of originations are economically smaller.

In columns (5) and (6) of Table 3, we address the second source of potential bias by changing our definition of the treatment event. Our new treatment definition, which we employ in the remainder of the paper, designates a lender as being treated if it is already a MERS member and

Table 3
Credit supply effects of MERS: Simple OLS.
This table contains results of lender-year-census tract regressions. The dependent variable is either the log of the total dollar amount of mortgages, volume, or the log of the total number of mortgages, Num. Loans, originated per lender-year-census tract. In columns (1) to (4), *Post* is a dummy variable that takes a value of one for every year after which the lender becomes a MERS member and a value of zero otherwise. In columns (5) and (6), *Post* is a dummy variable that takes a value of one for the year of and year after the year a purchaser with which the lender has a relationship becomes a MERS member, and a zero for the year prior. Various levels of fixed effects are noted in each column and standard errors are clustered by Zip code.

Dependent variable	(1) Log (Volume)	(2) Log(Num. Loans)	(3) Log (Volume)	(4) Log(Num. Loans)	(5) Log (Volume)	(6) Log(Num. Loans)
Post	0.5231***	0.0806***	0.0740***	0.0698***	0.150***	0.162***
	(0.0106)	(0.0123)	(0.0096)	(0.0065)	(0.0076)	(0.0070)
Zip x Year Fixed Effects	N	N	Y	Y	Y	Y
Lender Fixed Effects	Y	Y	Y	Y	Y	Y
Observations	393,047	394,641	392,528	394,118	95,473	95,579
R-squared	0.508	0.572	0.564	0.623	0.386	0.454

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

defines the treatment event as the year in which a *purchaser* it has a relationship with becomes a MERS member. We also restrict our sample to only include the three-year period (years −1, 0, and 1) around each purchaser's MERS membership date. These changes allow us to isolate cases where the lender previously could not use MERS with a purchasing partner, but now *can* use MERS, thus representing an unambiguous increase in the lender's potential benefit from MERS membership.

Our revised treatment definition should reduce the effects of measurement error and should also help to address concerns about endogenous treatment timing since we measure changes in *lenders*' origination volumes even though the treatment event is when a *purchaser* partner joins MERS. Indeed, columns (5) and (6) show that after correctly defining the treatment event, we observe a significant jump in the economic magnitude and the statistical significance of our point estimates: mortgage volumes are 15% higher and mortgage counts are 16.2% higher for MERS members relative to non-MERS members after a common purchaser joins MERS. Hence, simple OLS tests suggest that the use of the MERS technology is strongly correlated with increases in mortgage origination.

4.2. Main empirical specification

Our main empirical tests build on the tests in columns (5) and (6) of Table 3 by adding purchaser by year and relationship (lender by purchaser) fixed effects and by carefully defining the control group for our tests. These changes help to account for any remaining omitted factors that are correlated with both purchasers' decisions to join MERS and with credit supply volumes. Specifically, we run a difference-in-differences regression of the form:

$$\begin{aligned} \ln Y_{ijczt} = {} & \alpha + \beta Post_{jt} + \gamma MERS_{it} \\ & + \delta Post_{jt} \times MERS_{it} \\ & + \xi_{zt} + \phi_{jt} + \theta_{ij} + \varepsilon_{ijczt} \end{aligned} \tag{1}$$

where i indexes the original mortgage lender, j indexes a purchaser that i has previously sold mortgages to that became a MERS member in year t, z indexes Zip code, and c indexes census tract.[14] The unit of observation is a purchaser-lender-census tract-year. Our specification also includes Zip code × year, purchaser × year, and relationship fixed effects.

A lender becomes treated when a purchasing institution that the lender has previously sold mortgages to becomes a MERS member. The control group for our tests consists of non-MERS lenders active in the same Zip codes as treated lenders that have also sold mortgages to the same purchaser that is joining MERS. We measure outcomes in a three-year window surrounding each treatment event, with year −1 designated as the pre-event period and years 0 and 1 designated as the post-event period (where year 0 is the year that the purchaser joins MERS). We require control lenders to not be a MERS member at any time during the three years surrounding the date that a common purchaser joins MERS. Since the land records do not contain data on mortgage sales by MERS member lenders to MERS member purchasers, an implicit assumption in our empirical strategy is that each lender-purchaser relationship continues for at least one year after the purchaser becomes a MERS member. We provide a variety of anecdotal, theoretical, and empirical evidence in Section 4.5.1 to support this assumption.[15]

Fig. 2 displays our identification strategy graphically. In effect, we are comparing lending outcomes across two institutions, one a MERS member and one not a MERS member, in a given Zip code, in a given year, before and after a common purchaser joins MERS. The inclusion of Zip code × year fixed effects should account for any specific factors such as shocks to housing demand or local economic conditions that could cause lending to rise or fall within a given Zip code at a given point in time. The inclusion of purchaser × year fixed effects should account for any specific factors that might cause a purchaser to join MERS, or that might cause a purchaser to increase or decrease his mortgage purchase activity. For example, if investor demand for mortgage-backed securities increases, this might

[14] Each census tract c is mapped to a single Zip code z.

[15] Furthermore, to construct the panel, we aggregate the data by lender-census tract-year, and append a purchaser in the pre-event and post-event periods if the lender had a relationship with that purchaser in the pre-event period.

Table 4
Credit supply effects of MERS.
This table contains results of purchaser-lender-year-census tract regressions. The dependent variable is either the log of the total dollar amount of mortgages, volume, or the log of the total number of mortgages, Num. Loans, originated per year-census tract-lender. We append a purchaser in the pre-event and post-event periods if the lender had a relationship with that purchaser in the pre-event period in each census tract-year. *MERS* is a dummy variable taking a value of one if the lender is a MERS member, and a value of zero if the lender is not a MERS member in the pre and post period. *Post* is a dummy variable taking a value of one for the year of and year after the year the purchaser with which the lender has a relationship becomes a MERS member, and a value of zero for the year prior to the year the purchaser with which the lender has a relationship becomes a MERS member. Nonbank is a dummy variable that takes a value of one if the institution has an HMDA lender code 3 (i.e. is a nonbank or a manually verified institution of this type), and a value of zero if the institution has an HMDA lender code 0, 1, 2, or 5 (i.e., is a bank, or a subsidiary or affiliate of a bank or a manually verified institution of this type). Zip code × year, purchaser × year and relationship fixed effects are included. *Relationship* is the purchaser-lender relationship. Standard errors are clustered by Zip code.

	Total		Total	
	Log(Volume)	Log(Num. Loans)	Log(Volume)	Log(Num. Loans)
Post × MERS	0.101***	0.0922***	−0.175***	−0.124***
	(0.0366)	(0.0290)	(0.0518)	(0.0444)
Post × MERS × Nonbank			0.401***	0.364***
			(0.0608)	(0.0530)
Zip × Year Fixed Effects	Y	Y	Y	Y
Purchaser × Year Fixed Effects	Y	Y	Y	Y
Relationship Fixed Effects	Y	Y	Y	Y
Observations	37,816	37,911	37,816	37,911
R-squared	0.513	0.529	0.514	0.531

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

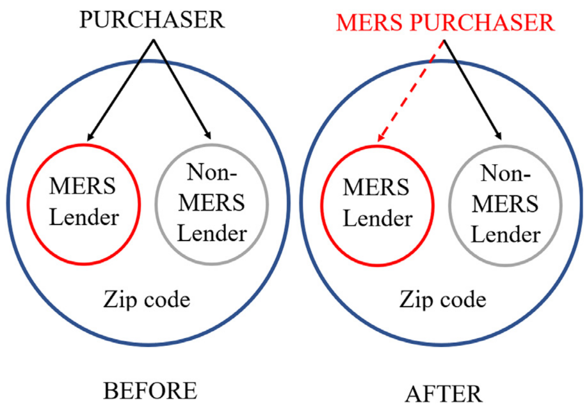

Fig. 2. Illustration of difference-in-differences methodology.

cause purchasers to join MERS and might also cause purchasers to increase their demand for purchased mortgages. By restricting the analysis to within a purchaser-year, our specification should largely account for such factors. Finally, the inclusion of relationship fixed effects should help to capture any specific factors that might cause certain lenders to sell higher or lower volumes of mortgages to certain purchasers. Collectively, these fixed effects should help us to isolate the credit supply effects of the MERS technology itself rather than capturing shocks to housing demand, investor demand for mortgages, and any other local economic factors that correlate with the supply of mortgages.

4.3. Credit supply effects of MERS

4.3.1. Baseline estimates

Table 4 reports our baseline estimates of Eq. (1). Consistent with our main hypothesis, columns (1) and (2) of Table 4 show that total mortgage origination volumes increase by 10.1% at MERS members relative to non-MERS members when a common purchaser joins MERS. Fig. 3 presents visual confirmation of parallel trends in origination volumes between MERS members and non-MERS members prior to their common trading partner joining MERS.[16] Hence, we find that MERS has a positive and economically significant overall effect on the supply of mortgage credit. Fig. 3 also confirms that the growth in MERS adoption parallels the growth in housing markets: we find that the number of annual treatment events increased dramatically from the introduction of MERS until the end of the housing boom, and then fell significantly.

Since approximately one-third of all lenders per census tract are MERS members during our sample period, a back-of-the-envelope calculation suggests that overall lending in Massachusetts increases by approximately $10.1\% \times 1/3 = 3.4\%$ per census tract per year as a direct result of the MERS technology. A second, more detailed calculation produces a nearly identical estimate.[17] Extrapolating these estimates across the entire United States is nontrivial given that we only have data from one state. We return to this

[16] We confirm additional parallel trends in Figs. A.1 and A.2 that show annual origination at the census tract level. Fig. A.1 shows lending in census tracts with at least one lender with a purchaser who recently joined MERS, and Fig. A.2 shows lending within those census tracts for MERS lenders relative to non-MERS lenders.

[17] This calculation starts by examining annual changes in credit supply at the census tract level. Table A.1 of the Internet Appendix shows that aggregate origination volumes increase by an average of 9.1% in census tracts with "treated" lenders (i.e., one of their purchasers recently joined MERS) relative to census tracts with no treated lenders. The table also shows that about 60% of this increase came from MERS lenders. Since only one-third of all lenders are MERS lenders, we estimate that overall lending also increased by 3.1% per census tract per year due to the MERS technology.

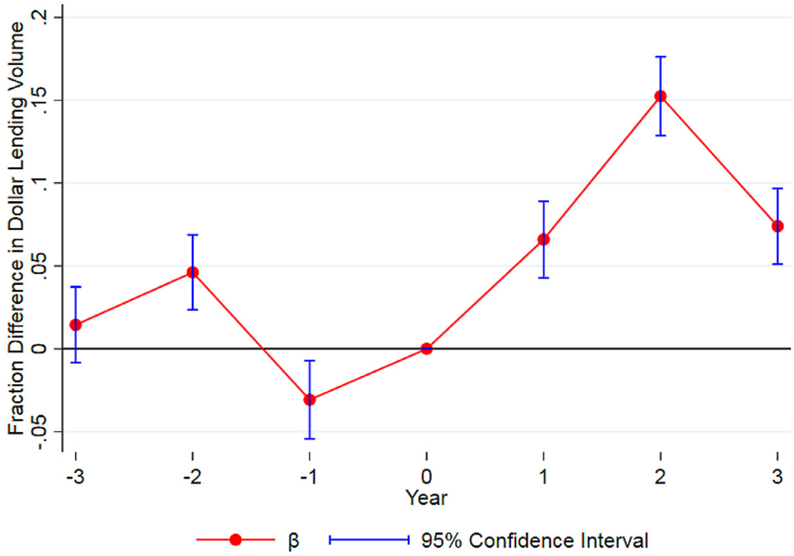

Fig. 3. Annual difference-in-differences coefficients relative to treatment year (year zero).

issue below in the section on robustness. Nonetheless, our estimates suggest that the MERS technology resulted in a 3.1–3.4% increase in aggregate credit supply per year.

To place this finding into context, Mian and Sufi (2009) find that 15% of annual mortgage originations during the housing boom can be traced to increases in credit supply. Our results suggest that approximately 20% of this effect (3.4% divided by 15%) is due to the introduction of MERS. Similarly, Di Maggio and Kermani (2017) show that national banks increased mortgage lending by 10% per year between 2003 and 2006 due to the removal of antipredatory lending laws (APLs). Using bank regulatory data, we estimate that a 10% increase in lending by national banks translates to an overall increase of roughly 3% in total mortgage origination volume per year as a result of the removal of APLs. Hence, the introduction of MERS appears to account for a similar share of the total increase in credit supply to the removal of APLs.

4.3.2. Nonbank lenders

Nonbank lenders intend to sell 100% of the mortgages that they originate and should therefore be better placed to meet the increase in demand for purchased mortgages since banks often originate and hold mortgages on their balance sheets. Furthermore, nonbanks are more likely to benefit from selling mortgages faster since this frees up scarce capital to make new loans. Hence, we hypothesize that origination volumes will increase more rapidly for nonbank lenders relative to banks after a common purchaser joins MERS. To test this hypothesis, we construct a dummy variable, *Nonbank*, that takes the value of one if the lender has an HMDA lender type code of three (nonbank lender), and is zero otherwise. We then interact *Nonbank* with all of the other variables in Eq. (1). Consistent with our hypothesis, columns (3) and (4) of Table 4 show that origination counts and volumes are higher for nonbank lenders than for other lender types after a trading partner joins MERS. In fact, columns (3) and (4) show that the entire increase in lending from MERS member lenders is coming from nonbanks, as the coefficients on *Post* × *MERS* are negative and statistically significant.[18]

These estimates also allow us to quantify how MERS could have contributed to the rising share of non bank mortgage lending during the housing boom. Using data from the land records, we calculate that the share of nonbank lending in Massachusetts rose from approximately 24% in 1999 to approximately 34% in 2007, while total nonbank origination volume rose from $1.3 billion to $4.2 billion during the same time period. The results in Table 4 suggest that nonbank lending increased by approximately 3.7% per census tract per year. Hence, starting from a baseline nonbank origination volume of $1.3 billion in 1999, the introduction of MERS led to approximately $400 million in incremental nonbank origination volume in 2007 relative to 1999. We can attribute approximately 14% of the total increase in nonbank lending from 1999 to 2007 to the use of MERS technology.[19]

4.3.3. Application denial rates

To provide further evidence that the increases we observe represent increases in credit supply, we merge our lender-purchaser relationship data from Massachusetts with the nationwide HMDA data set. Since HMDA contains data on all mortgage loan applications, not just mortgages that were originated, we can determine whether lenders increased mortgage approval rates once a trading partner

[18] One explanation for the negative coefficient on *Post MERS* is provided by Buchak et al. (2018), who find that small changes in marginal costs can have a very large impact on lenders' origination versus purchasing behavior. Since it reduces the marginal costs of purchasing mortgages, MERS could induce banks to shift towards purchasing more mortgages and originating fewer mortgages. Consistent with this explanation, Table A.2 shows that of the loans originated by nonbanks, the share sold to banks increases after a purchaser (most likely the bank) joins MERS.

[19] 14% ≈ $400 million / ($4.2 billion −$1.3 billion).

Table 5
Mortgage application denial rates.
This table contains results of purchaser-lender-year-census tract regressions using nationwide mortgage origination data from HMDA. The dependent variable is the fraction of new mortgage applications that were denied by lenders. We append a purchaser in the pre-event and post-event periods if the lender had a relationship with that purchaser in the pre-event period in each census tract-year. *Post* is a dummy variable that takes a value of one for the year of and year after the purchaser with which a lender has a relationship becomes a MERS member, and zero for the year prior. *MERS* is a dummy variable taking a value of one if the lender is a MERS member, and a value of zero if the lender is not a MERS member in the pre and post period. Nonbank is a dummy variable that takes a value of one if the institution has an HMDA lender code 3 (i.e., is a nonbank or a manually verified institution of this type), and a value of zero if the institution has an HMDA lender code 0, 1, 2, or 5 (i.e., is a bank, or a subsidiary or affiliate of a bank or a manually verified institution of this type). Zip code × year, purchaser × year, and relationship fixed effects are included. *Relationship* is the purchaser/lender relationship. All purchaser and lender variables are based solely on data from the Massachusetts land records. Standard errors are clustered by zip code.

Dependent variable	Denial fraction	
Post × MERS	−0.0395***	−0.0063
	(0.0044)	(0.0060)
Post × MERS × Nonbank		−0.0496***
		(0.0067)
Post × Nonbank		0.0264***
		(0.0064)
Purchaser × Year Fixed Effects	Y	Y
Relationship Fixed Effects	Y	Y
Zip × Year Fixed Effects	Y	Y
Observations	25,936,044	25,936,044
R-squared	0.383	0.383

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

joined MERS. While our tests only include lenders with a presence in Massachusetts, the nationwide nature of our tests also helps to reduce concerns about the external validity of our previous results.

Table 5 presents the results of our application denial rate tests. Column (1) shows that after a purchasing partner joins MERS, MERS lenders' denial rates on new mortgage applications fall by approximately 4% relative to non-MERS members. Consistent with previous tests, column (2) of Table 5 also shows that this effect is completely driven by nonbank lenders. It is important to note that Table 5 only contains results for lenders that have a presence in Massachusetts and hence may not be fully representative of the national mortgage market. Despite this caveat, our results suggest that the mortgage origination increases that we are capturing represent increases in the actual supply of credit granted to homeowners during our sample period.

4.3.4. Low income borrowers

We next assess whether nonbank lenders specifically increase credit supply to lower income populations after a common purchaser joins MERS. Table 6 shows the results of an institution-by-year regression in which the dependent variables are (log) number of census tracts a lender operates in and the median (log) income of residents in those census tracts. Columns (1) and (2) show that after a purchaser joins MERS, MERS lenders increase the number of census tracts they operate in relative to non-MERS members. However, this expansion only occurs for nonbanks, as column (1) shows that the coefficient on the *Post* × *MERS* term is insignificant. Indeed, column (2) shows that once broken out by lender type, nonbank lenders that are MERS members significantly expand their geographic footprints relative to MERS member banks and institutions that have not joined MERS.

Columns (3) and (4) report the results of similar regressions in which the dependent variable is now the average median income per census tract. Column (4) shows that this expansion by nonbank MERS members seems to be concentrated within lower-income areas, whereas MERS members that are banks do not seem to be similarly expanding into low-income areas. Hence, while we cannot directly observe borrowers' credit scores, our results are consistent with nonbank lenders increasing origination volumes to subprime borrowers.[20] Collectively, the results in Tables 4–6 are consistent with our main hypothesis: they show that the introduction of MERS caused credit supply to expand, particularly at nonbank lenders, and that this increase was likely directed to low-income consumers.

4.3.5. Private label securitization

Since the mortgages destined for PLS are sold numerous times as part of the securitization process, the cost and time savings associated with MERS are likely to be larger for PLS mortgages relative to mortgages that are placed into agency securitizations. We examine this hypothesis in Table 7 using the sample of nationwide mortgage applications from HMDA. In particular, the *PURTYPE* variable in HMDA describes which type of institution purchases a given mortgage. One such category (*PURTYPE* = 3) is PLS. We code a dummy variable named *PLS* that equals one if a mortgage was sold into a PLS, and equals zero otherwise. We then examine whether nonbanks are more likely to originate mortgages that ended up in PLS deals after a purchaser joined MERS. Columns (1) and (2) show that nearly the entire credit supply increase associated with MERS can be attributed to mortgages that were ultimately sold into a PLS deal. Columns (3)–(6) report results after splitting the sample by banks and nonbank lenders. MERS member banks did not sell more mortgages into PLS deals after a purchaser joined MERS (columns (3) and (4)). However, columns (5) and (6) show that the entire credit supply increase associated with MERS can be attributed to nonbank MERS members that originated mortgages that were ultimately sold into PLS deals. Hence, it appears that nonbanks were able to expand credit supply in low-income areas in part because of increased demand for these mortgages as part of private label securitizations.

[20] Subprime borrowers tend to be concentrated in lower income areas; see, e.g., the white paper "Unequal Burden: Income and Racial Disparities in Subprime Lending in America" by the U.S. Department of Housing and Urban Development (https://www.huduser.gov/Publications/pdf/unequal_full.pdf).

Table 6

Credit supply effects in lower income areas.

This table contains results of lender-year regressions. The dependent variable is the log of the total number of census tracts that the lender operates in a year, or the log of the average median income per census tract averaged over all census tracts in which the lender operates. *MERS* is a dummy variable taking a value of one if the lender is a MERS member, and a value of zero if the lender is not a MERS member in the pre and post period. *Post* is a dummy variable that takes a value of one for the year of and year after the purchaser with which a lender has a relationship becomes a MERS member, and zero for the year prior. *Nonbank* is a dummy variable that takes a value of one if the institution has an HMDA lender code 3 (i.e., is a nonbank or a manually verified institution of this type), and a value of zero if the institution has an HMDA lender code 0, 1, 2, or 5 (i.e., is a bank, or a subsidiary or affiliate of a bank or a manually verified institution of this type). Institution and year fixed effects are included. Standard errors are clustered by county.

Dependent variable	Log (No. of Census Tracts)		Log (Median Income)	
Post × MERS	−0.0724	−0.438	−0.00210	0.00576
	(0.196)	(0.254)	(0.00591)	(0.00769)
Post × MERS × Nonbank		0.575**		−0.0160**
		(0.216)		(0.00737)
Post × Nonbank		−0.252		0.0117**
		(0.196)		(0.00449)
Year Fixed Effects	Y	Y	Y	Y
Lender Fixed Effects	Y	Y	Y	Y
Observations	2,236	2,236	2,236	2,236
R-squared	0.671	0.672	0.199	0.199

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 7

Private label securitization.

This table contains results of purchaser-lender-year-census tract-mortgage sale type regressions using nationwide mortgage origination data from HMDA. The dependent variable is either the log of the total dollar amount of mortgages, volume, or the log of the total number of mortgages, Num. Loans, originated per lender-year-census tract-sale type. We append a purchaser in the pre-event and post-event periods if the lender had a relationship with that purchaser in the pre-event period in each census tract/year. *Post* is a dummy variable that takes a value of one for the year of and year after the purchaser with witch a lender has a relationship becomes a MERS member, and zero for the year prior. *MERS* is a dummy variable that takes a value of one if the lender is a MERS member and zero otherwise. *PLS* is a dummy variable that equals one if a mortgage was sold into a PLS, and equals zero otherwise. Columns (1) and (2) contain results for all lenders, columns (3) and (4) contain results for banks, and columns (5) and (6) contain results for nonbanks. Nonbanks are institutions with an HMDA lender code 3 (i.e., is a nonbank or a manually verified institution of this type), and banks are institutions with an HMDA lender code 0, 1, 2, or 5 (i.e., is a bank, or a subsidiary or affiliate of a bank or a manually verified institution of this type). Zip code × year, purchaser × year and relationship fixed effects are included. *Relationship* is the purchaser-lender relationship. All purchaser and lender variables are based solely on data from the Massachusetts land records. Standard errors are clustered by Zip code.

	(1)	(2)	(3)	(4)	(5)	(6)
	All lenders		Banks		Nonbanks	
Dependent variable	Log(Volume)	Log(Num. Loans)	Log(Volume)	Log(Num. Loans)	Log(Volume)	Log(Num. Loans)
Post × MERS	−0.00548	0.0120*	0.0007	0.0170*	−0.0449***	0.0038
	(0.00983)	(0.00635)	(0.0248)	(0.0096)	(0.0086)	(0.0080)
Post × MERS × PLS	0.151***	0.0982***	−0.0410	−0.0568	0.2653***	0.0862***
	(0.0339)	(0.0278)	(0.0747)	(0.0476)	(0.0367)	
Zip × Year Fixed Effects	Y	Y	Y	Y	Y	Y
Purchaser × Year Fixed Effects	Y	Y	Y	Y	Y	Y
Relationship Fixed Effects	Y	Y	Y	Y	Y	Y
Observations	17,880,177	17,880,177	7,732,854	7,732,854	10,100,006	10,100,006
R-squared	0.317	0.232	0.325	0.278	0.332	0.220

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

4.3.6. Foreclosures

Finally, we examine the effects of MERS membership on foreclosures. We do so by running a conditional logit regression at the mortgage level in which mortgages are grouped by census tract. Table 8 shows that foreclosures are slightly less likely for mortgages originated by MERS members relative to nonmembers after a common trading partner joins MERS (column (1)). However, consistent with our proposed channel and the results in Tables 4 and 6, column (2) of Table 8 confirms that the probability of foreclosure is significantly higher for MERS member nonbanks than for non-MERS nonbanks when a common trading partner joins MERS. Similarly, the probability of foreclosure is significantly higher for MERS member nonbanks than for MERS member banks when a common trading partner joins MERS. Hence, while MERS caused a significant increase in credit supply, it appears that many of the

Table 8
Foreclosures.
This table contains results of lender-year-census tract conditional logit regressions grouped by census tract. The dependent variable, *Foreclosed*, is a dummy variable taking a value of one if the proportion of mortgages that were subsequently foreclosed on originated by a lender in that census tract-year is greater than zero and a value of zero otherwise. *MERS* is a dummy variable taking a value of one if the lender is a MERS member, and a value of zero if the lender is not a MERS member in the pre and post period. *Post* is a dummy variable that takes a value of one for the year of and year after the purchaser with which a lender has a relationship becomes a MERS member, and zero for the year prior. *Nonbank* is a dummy variable that takes a value of one if the institution has an HMDA lender code 3 (i.e., is a nonbank or a manually verified institution of this type), and a value of zero if the institution has an HMDA lender code 0, 1, 2, or 5 (i.e., is a bank, or a subsidiary or affiliate of a bank or a manually verified institution of this type). Standard errors are clustered by census tract.

	Proportion Foreclosed> 0	
Post × MERS	-0.0646	-0.578***
	(0.109)	(0.158)
Post × MERS × Nonbank		1.027***
		(0.243)
Post	0.432***	0.661***
	(0.107)	(0.137)
MERS	1.228***	1.087***
	(0.0842)	(0.141)
Nonbank		−0.191
		(0.160)
MERS × Nonbank		0.226
		(0.180)
Post × Nonbank		−0.650***
		(0.223)
Group	Census Tract	Census Tract
Observations	38,150	38,150

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

"extra" loans that were made as a result of the MERS technology were of a lower quality.

4.4. Other effects

4.4.1. Entry into new geographic areas

Our main tests capture credit supply increases along both the intensive margin (i.e., to the same borrower within a census tract) and, to some extent, the extensive margin (all other borrowers within that census tract).[21] However, to be in our main sample, lenders must have originated mortgages in the same census tract both before and after a purchaser joins MERS. If lenders expand their lending footprint after a purchaser joins MERS, those loans will not be captured in our main tests.

To better capture the full effect of MERS on lenders' geographic footprints, we define a dummy dependent variable that equals one in the first year a lender originates mortgages in a given census tract, and equals zero in the years immediately prior to and immediately following the lender's entry. This variable is designed to be a pure measure of a lender's entry into a new census tract. We then run a within-census tract logit regression, using the same independent variables as in our main specification, to assess whether MERS impacted the likelihood of a lender entering into new census tracts.

Table A.3 shows that the likelihood of entering a given census tract is higher for MERS lenders relative to non-MERS lenders after a mutual purchaser becomes a MERS member. In addition, these effects are concentrated at nonbank lenders. Hence, consistent with the results in Table 6, it appears that nonbank lenders began to actively originate mortgages in new census tracts after a common purchaser joined MERS.

4.4.2. House price appreciation

Finally, we examine whether the increased credit supply associated with MERS can help to explain the rapid increases in housing prices that occurred during our sample period. Following Bernstein et al. (2021), we use the house price index constructed by Bogin et al. (2019) as our outcome variable. The index we use is defined at the census tract-year level. We define MERS-active census tracts as census tracts in which at least one lender active in that tract has a relationship with a purchaser who recently joined MERS. We then examine whether tract-level house prices increase once a census tract becomes MERS-active. In these tests, we include Zip code × year and census tract fixed effects. We run this test using both the Massachusetts land records data and our nationwide HMDA sample. The results are reported in Table A.4. In both tests, we find that the use of MERS is significantly associated with an increase in house prices. The magnitudes are sizable: for example, the first column of Table A.4 shows that house prices rise by approximately 3.1% per year after census tracts become MERS-active relative to other census tracts in the same Zip code that are not MERS-active. Hence, the credit supply increases caused by MERS appear to be correlated with increases in house prices that occurred during our sample period.

4.5. Robustness

4.5.1. Relationships between lenders and purchasers

Our main tests make use of relationships between lenders and purchasers to identify the credit supply increases associated with MERS. In particular, our tests assume that relationships are relatively stable and that MERS itself does not cause lenders to shift their relationships with other trading partners. If MERS were to cause systematic shifts in lending relationships, this type of endogenous switching could raise questions about the robustness of our findings.

However, a variety of existing anecdotal, empirical, and theoretical evidence suggests that lender-purchaser relationships are likely to be quite stable. Theoretically, Diamond (1991) shows that repeated interactions among financial intermediaries reduce adverse selection concerns, and thus, frequent relationship changes would be very costly for lenders. Indeed, Adelino et al. (2019) find evidence consistent with this theory in the market for purchased mortgages. Anecdotal evidence also suggests that lender-purchaser relationships are likely to be stable; for

[21] The literature has also defined the extensive margin as reflecting lending to completely new borrowers (Mian and Sufi, 2009; Jiménez et al., 2012; Ramcharan et al., 2016; Célérier et al., 2017; Di Maggio et al., 2017) and lending in new geographic areas (Adelino et al., 2016; García, 2019).

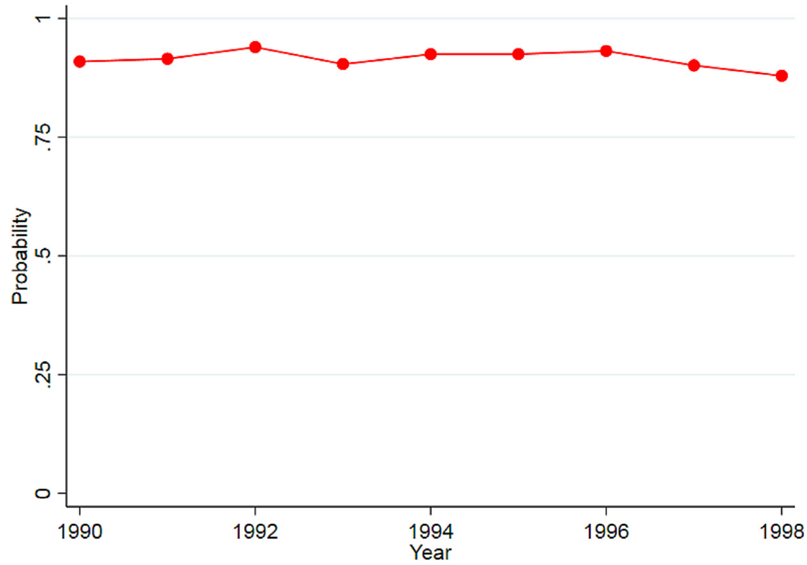

Fig. 4. Probability that year-t lender-purchaser relationship exists in year $t+1$ (limited to non-MERS institutions).

example, it is costly and time-consuming for lenders to work out pricing agreements and forward sales commitments with purchasers (Parker et al., 2016).

Consistent with this evidence, we find that lender-purchaser relationships appear to be fairly stable in our sample. First, Fig. 4 plots the probability of a relationship that exists in year t persisting in year $t+1$ prior to the introduction of MERS.[22] The figure shows that relationships are highly persistent: more than 90% of relationships that exist in year t continue to exist in year $t+1$. Second, Table A.5 shows that the total number of observable relationships for a given non-MERS lender does not materially change after a purchaser joins MERS.[23] Third, Tables A.6 and A.7 show that a lender's MERS status is not predictive of the existence or persistence of a relationship. Finally,Table A.8 shows that purchasers joining MERS do not form new relationships with lenders that have recently become MERS members.

Since relationships between MERS lenders and MERS purchasers are not visible in the land records (due to MERS), we cannot observe the factors that affect the existence or persistence of relationships between MERS institutions. Collectively, however, these results, combined with anecdotal and theoretical evidence, suggest that endogenous relationship switching is unlikely to materially affect our results.

4.5.2. Consumer demand shocks

Another possibility is that we are simply capturing an outward shift in the demand for residential mortgages by borrowers (see, e.g., Barberis et al., 2018) rather than an increase in credit supply. This explanation does not seem consistent with our results. First, we include granular geography $\times$ time fixed effects, which should absorb any time-varying demand for mortgages at a very local level. Notwithstanding these fixed effects, for consumer demand effects to be driving our results, consumers would have to be aware of when the institution *purchasing* mortgages from their lender becomes a MERS member, and there would then have to be heterogeneous demand at that specific point in time for mortgages originated specifically by nonbank MERS members. This explanation is highly unlikely to be true, as MERS membership is private, and there is no evidence that consumers were aware of MERS in large numbers until after the collapse of the housing bubble. In addition, the housing demand hypothesis cannot explain increased origination volume by nonbanks particularly in low-income areas, and cannot explain the reductions in application denial rates shown previously.

4.5.3. Common investor demand shocks

A third potential concern is that demand shocks from investors could explain our results. For example, investors could have increased their general demand for mortgage-backed securities, leading securitization trusts to respond by increasing their demand for purchased mortgages. This would in turn allow lenders to expand their supply of mortgages to consumers. Hence, this chain of events would also cause mortgage origination volumes to increase. If these demand shocks are also correlated with the mortgage purchaser joining MERS, then these types of demand shocks could explain our results.

Our main specifications include purchaser $\times$ time fixed effects, which should absorb any purchaser-specific changes in the demand for purchased mortgages. Nonetheless, to ensure that the use of MERS itself is driving any subsequent changes in purchase volumes, we take steps to show that purchasers are not demanding additional pur-

[22] Since MERS came into existence, it is no longer possible to accurately measure relationships in the land records data because no assignment documents are filed. Because of this limitation, we restrict the sample period of the figure to end in 1997, the year MERS was introduced.

[23] Here, we restrict our analysis to non-MERS lenders since we cannot observe relationships between a MERS lender and a MERS purchaser.

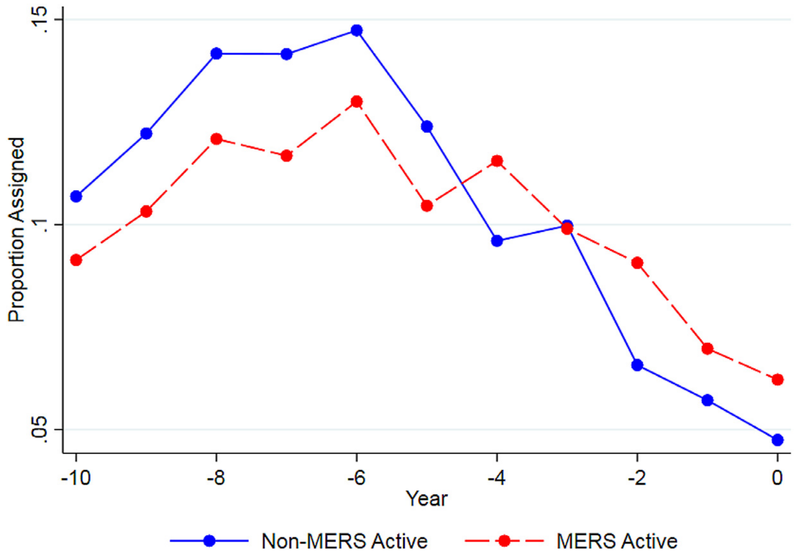

Fig. 5. Proportion of purchased mortgages from MERS member and non–MERS member lenders prior to purchaser joining MERS in year zero.

chased mortgages from MERS lenders for reasons *other than MERS itself*. We run a number of robustness tests to rule out this channel.

First, we want to ensure that MERS members (or more specifically, nonbank MERS members) do not have flatter supply curves than other types of lenders. For example, to the extent that MERS lenders have flatter supply schedules than non-MERS lenders (or MERS nonbanks have flatter supply schedules than MERS banks), a demand shock from a common mortgage purchaser would result in a larger quantity increase for the MERS lenders relative to the non-MERS lenders. This could explain our finding that quantity increases are larger for MERS members relative to non-MERS members, or MERS nonbanks relative to MERS banks.

However, this channel does not seem consistent with our collection of results. If MERS lenders are systematically different from non-MERS lenders, say, larger lenders with flatter supply schedules, we would expect to see MERS members constituting a larger proportion of all mortgages purchased over time. Fig. 5 shows the average percentage of all mortgages purchased from each MERS member and non-MERS member in the ten years prior to the purchaser becoming a MERS member. Fig. 5 shows that these ex-ante relationships seem to be very similar, and follow parallel trends.

We also run a series of robustness tests presented in Table 9. In Panel A, we restrict our sample to only include "small" institutions, which are defined as nonbanks with less than 50 employees or banks with less than $1 billion in assets. Intuitively, small institutions are more likely to be homogeneous in nature and, hence, are more likely to have similar supply schedules. Panel A of Table 9 shows that our main results continue to hold after this sample restriction. In Panel B, we run placebo tests in which we restrict the sample to institutions that are *not* MERS members and check to see if there is any differential in lending response for large versus small institutions. We find no results, indicating that we are not simply capturing increased lending by institutions with flatter supply schedules (as proxied for by the size of the lending institution), as opposed to capturing increased lending due to institutions' adoption of MERS.

4.5.4. External validity

We also re-run our main tests (Table 4) using HMDA data, which includes mortgage originations from all over the country. Panel C of Table 9 presents the results of these tests and shows that our main results continue to hold at the national level. While the point estimates are somewhat smaller in magnitude relative to our baseline tests in Table 4, the HMDA tests are also limited to lenders that are active in both Massachusetts and other states, which tilts the sample towards larger lenders.[24] Hence, using lender-purchaser relationship data from Massachusetts, we find that the introduction of MERS is associated with credit supply increases across the entire United States.

4.5.5. Specification choices

Finally, we confirm that our results are not driven by our selection of empirical specifications. In Table A.9, we confirm that our results continue to hold after extending the pre-event and post-event windows from one year to three years. Similarly, Table A.10 shows that our main results continue to hold and are of similar magnitude after restricting the sample to only include the very first time that a lender is treated, thereby removing multiple and potentially overlapping treatment events for the same lender. Table A.10 also helps to assuage concerns that changes in

[24] In untabulated results, we also re-run our baseline tests using HMDA data from only the state of Massachusetts. We find very similar magnitudes and statistical significance to the results reported in Table 4, suggesting that the six counties that comprise our main sample are representative of the state of Massachusetts as a whole.

Table 9
Robustness.
This table reports results from a series of purchaser-lender-year-census tract regressions. In all panels, the dependent variable is either the log of the total dollar amount of mortgages, volume, or the log of the total number of mortgages, Num. Loans, originated per year-census tract-institution. We append a purchaser in the pre-event and post-event periods if the lender had a relationship with that purchaser in the pre-event period in each census tract-year. In Panel A, the sample is restricted to mortgages in which the mortgage is originated by a "small" institution. An institution is defined as small if it has smaller than the median total assets (if the institution is a bank or subsidiary of a bank) or number of employees (if the institution is a nonbank). In Panel B, the sample is restricted to non-MERS lenders. Panel C replaces mortgage origination data from the Massachusetts land records with nationwide data on loan originations from the Home Mortgage Disclosure Act (HMDA). Despite using loan originations from HMDA, data on buyer-seller relationships and MERS membership are still sourced from the Massachusetts land record data in this panel. The variables *MERS, Nonbank*, and *Post* are defined in Table 4. All panels include Zip code × year, purchaser × year, and relationship fixed effects. *Relationship* is the purchaser-lender relationship. All purchaser and lender variables are based solely on data from the Massachusetts land records. Standard errors are clustered by Zip code.

Independent variable	log(Volume)	log(Num. Loans)	*N*	*R*-squared
Panel A: Sample limited to "small" institutions (supply curve tests)				
Post × MERS	0.084*	0.094**	28,112	0.498
	(0.0492)	(0.0393)		
Post × MERS × Nonbank	0.294***	0.364***	28,112	0.512
	(0.0778)	(0.0694)		
Panel B: Large vs. small institutions (placebo tests)				
Post × Large	0.099	0.021	5,385	0.551
	(0.123)	(0.099)		
Post × Large × Nonbank	−0.053	−0.0585	5,411	0.552
	(0.245)	(0.183)		
Panel C: HMDA data (nationwide tests)				
Post × MERS	0.0588***	0.0747***	17,880,117	0.336
	(0.0112)	(0.0082)		
Post × MERS × Nonbank	0.0643**	0.0985***	17,880,117	0.336
	(0.0252)	(0.0137)		

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

the composition of the control group that naturally occur in our setting as more lenders become MERS members (and hence can no longer be control lenders) are driving our results (Goodman-Bacon, 2019).

5. Conclusion

This paper argues that the introduction of the Mortgage Electronic Registration System in the late 1990s and early 2000s contributed significantly to the expansion in mortgage credit supply that occurred during the run-up to the 2007–2009 financial crisis. By removing the need for lenders to file and audit documents in the local land records every time a mortgage was sold, MERS significantly reduced the time and costs associated with secondary mortgage sales.

We use detailed data from the Massachusetts land records and the bilateral nature of MERS membership coupled with stringent fixed effects to present four primary results. First, MERS member institutions increased mortgage origination volumes after a trading partner joined MERS relative to institutions that were not MERS members but had relationships with the same partner. Second, nonbank lenders were primarily responsible for the overall increase in mortgage origination volumes. Third, these "extra" mortgage loans made by nonbank lenders were disproportionately made to borrowers residing in lower income areas. Finally, long-term foreclosure rates were higher for mortgages originated by MERS member nonbank lenders than for mortgages originated by other types of institutions.

To our knowledge, our paper is the first in the literature to examine the effect of MERS on mortgage originations and explain why credit supply increased more dramatically at nonbank lenders prior to the onset of the crisis. Our results also contribute to the debate over the nature of mortgage credit expansion and the beneficiaries of increased mortgage credit supply prior to the 2007–2009 financial crisis. Finally, our results contribute to the literature on FinTech (Buchak et al., 2018; Fuster et al., 2019) and the efficiency of innovations within the finance industry (Philippon, 2015; Bai et al., 2016) by showing that even a very successful financial innovation like MERS could be associated with unintended consequences that can have an important impact on the health of the housing market.

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