Did Technology Contribute to the Housing Boom? Evidence from MERS

February 9, 2020

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 loan sales. Using novel data from the Massachusetts Registry of Deeds, we show that the introduction of MERS led to an expansion in credit supply that was primarily fueled by non-bank lenders originating loans to low-income borrowers. We also find that foreclosure rates were higher on these loans. Our paper provides a new explanation for why credit supply increased prior to the 2008 financial crisis and why supply increases were larger 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 documented that credit supply increased prior to the 2008 financial crisis, which in turn affected numerous variables such as house price appreciation, mortgage defaults, and the real economy. However, the origins of this increase in credit supply remain relatively unexplored. For example, why did credit supply increase so dramatically in the early 2000s instead of at some other point in time? Why was much of the increase in credit supply fueled by non-bank lenders such as mortgage brokers and mortgage bankers (see, e.g., Berndt, Hollifield, and Sandas (2016))? Finally, why were so many new loans originated to lower-income borrowers who were often of questionable credit quality (see, e.g., Keys, Mukherjee, Seru, and Vig (2010))? To date, there is no single, consistent answer to these questions in the literature.

In this paper, we argue that financial innovation played a significant role in boosting credit supply prior to the 2008 financial crisis. In particular, we focus on the introduction of the Mortgage Electronic Registration System (MERS) in the late 1990s.[1] In simple terms, MERS allows a financial institution to sell a mortgage without having to legally update the mortgage's owner of record, thereby significantly reducing the time and costs associated with secondary loan sales. As such, the introduction of MERS represented a major innovation in the secondary market for mortgages.[2]

Our central argument is that by reducing the time and costs associated with selling mortgages, MERS may have indirectly helped to fuel the boom in residential mortgage credit supply prior to the 2008 financial crisis. First, the introduction of MERS increased the efficiency of the mortgage sale process through direct dollar cost and time savings, and hence reduced the marginal cost of loan sales. Second, these time and cost savings were largest for loans sold multiple times, such as mortgages destined for private-label securitization (PLS) pools. Third, the increased efficiency of the securitization process resulted in increased

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

[2] According to Ketcham (2012), more than two-thirds of all home loans in the United States were registered through MERS by the end of 2007, less than a decade after its introduction.Ketchum, Christopher (2012), "Stop Payment! A Homeowners' Revolt Against the Banks." *Harper's Magazine*, January 2012. Accessed at https://harpers.org/archive/2012/01/stop-payment-a-homeowners-revolt-against-the-banks/.

mortgage demand from securitization sponsors and other purchasers, which in turn led to increased mortgage origination volumes. Fourth, the increase in origination volumes were larger for non-bank lenders, since these lenders sell virtually all loans they originate and were better positioned to meet this demand. Non-bank lenders were also able to sell loans faster, thus freeing up scarce capital to make new loans. Finally, the marginal new loan originations in this setting would likely involve sub-prime borrowers, since prime borrowers had no trouble obtaining credit (Bhutta and Keys, 2016; Akey, Heimer, and Lewellen, 2020), and lenders faced reduced incentives to screen mortgages that were later sold (Calomiris and Kahn, 1991; Aghion, Bolton, and Tirole, 2004; Keys, Mukherjee, Seru, and Vig, 2010).

Collectively, our argument predicts higher origination volumes *particularly by non-bank 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, Igan, and Laeven (2012)), we are not aware of any other papers that attempt to show why non-bank lenders in particular were responsible for the rise in mortgage originations to low credit-quality borrowers prior to the crisis.

Consistent with our main hypotheses, we find that mortgage origination volumes increase significantly following an institution's adoption of MERS. These effects are concentrated within non-bank lenders and are stronger in areas populated by lower-income borrowers. The economic magnitudes of the increase in credit supply are large: we estimate that total credit supply rises by approximately 10% for MERS members relative to non-members each year. Using data from the Home Mortgage Disclosure Act (HMDA) loan application register, we also find that MERS lenders increase their mortgage approval rates relative to non-MERS lenders, confirming that the increase we observe is capturing an increase in supply. We also find that foreclosure rates are higher for mortgages originated by MERS-active non-bank lenders than for bank-originated loans or loans originated by institutions that are not MERS members. Hence, while the introduction of MERS represented a significant financial innovation for the mortgage industry, its unintended consequences may have led to the origination of a significant quantity of low-quality loans prior to the 2007-2009 financial crisis.

To place the magnitudes of our results in context, Mian and Sufi (2009) find that aggregate mortgage credit supply increased by approximately 15% per year during the housing boom. We estimate that MERS caused a 3.4% increase in aggregate credit supply per year, which represents approximately 20% of the total credit supply increases documented by Mian and Sufi (2009). This magnitude is similar to the magnitudes associated with the removal of anti-predatory lending laws (Di Maggio and Kermani, 2017). Hence, while many factors contributed to the total increase in credit supply documented by Mian and Sufi (2009), the introduction of MERS appears to have played a significant role in the expansion of mortgage credit supply prior to the 2007-2009 financial crisis.

To test our hypotheses, we utilize a novel database from the Massachusetts Registry of Deeds that contains all land records filed with county clerks in the state of Massachusetts from 1990-2018. These land records indicate whether a loan was registered with MERS at any point along the ownership chain of a mortgage.[3] We then hand-match the names of the lenders from the land records with various Federal Financial Institution Examination Council (FFIEC) databases to obtain data on each institution's history and entity type (i.e. mortgage broker, commercial bank, bank subsidiary, bank affiliate, or other). As such, our data set provides a comprehensive picture of the secondary market for purchased mortgage loans within six counties in the state of Massachusetts.[4]

Our empirical design combines the bilateral nature of the MERS system (i.e. in a MERS-registered transaction, the buyer and seller of a loan must both belong to MERS) with stringent fixed effects. In particular, our primary tests compare loan origination volumes, foreclosure rates, and other outcome variables for MERS-active lenders relative to non-MERS active lenders in the periods before and after *another transaction partner* joins MERS. That is, suppose lender A (existing MERS member) and lender B (not a MERS member) both operate in a census tract, and both lenders sell loans 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. In this way, any changes in origination volumes are not a

[3]One caveat of our data is that, because MERS alleviates the need to register mortgage sales with the county clerk, once a loan is registered with MERS, we do not know whether the loan was subsequently purchased or the identity of any future purchasers.

[4]Eight counties' data are either incomplete or are not in a research-usable format. We discuss the implications of this sample restriction in section 2.

function of the lenders themselves joining MERS, but are rather a function of one of their business partners joining MERS. In support of this assumption, we verify that parallel trends exist for MERS members and other lenders in the period before a mutual trading partner joins MERS.

Our main regressions also include zip code × year, purchaser × year, and buyer × seller (i.e. relationship) fixed effects. Zip code × year fixed effects absorb any time-varying demand shocks within a zip code. These fixed effects help to ensure that our results are not driven by changes in consumers' demand for mortgages (see, e.g, Barberis, Greenwood, Jin, and Shleifer (2018)). Purchaser × year fixed effects absorb any time-varying shocks to the demand for purchased mortgages by a given institution. These fixed effects help to ensure that our results are not driven by increased investor demand for mortgages or mortgage backed securities (see, e.g., Chernenko, Hanson, and Sunderam (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.

We also run a number of placebo tests and robustness checks to confirm that the effects we are capturing are related to the MERS technology itself rather than other, unrelated factors. For example, we verify that relationships between lenders and purchasers are stable over time, reducing concerns that our results capture substitution across lenders rather than a true increase in credit supply. Using the HMDA data set, we also verify (based on lender-purchaser relationships from Massachusetts) that the credit supply effects of MERS hold across the entire U.S. mortgage market. Finally, since different institutions adopted MERS at different times, our setting benefits from staggered treatment adoption within very narrowly-defined geographic areas, making it unlikely that a common economic shock is responsible for driving our results.

Our paper makes four primary contributions to the literature. First, a large literature has examined the supply of mortgage credit in the run up to the financial crisis (see, e.g., Mian and Sufi (2009, 2011), Adelino, Schoar, and Severino (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. 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, Gerardi, and Willen (2012) and Adelino, Schoar, and Severino (2016)).

Our paper is also related to a literature that documents the impact that secondary mortgage sales (including securitization) had on the mortgage market more generally. For example, Keys, Mukherjee, Seru, and Vig (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, Seru, and Vig (2010) show that the foreclosure rates on securitized loans are higher than portfolio-owned delinquent loans and Agarwal, Amromin, Ben-David, Chomsisengphet, and Evanoff (2011) show that securitization reduces the likelihood of loan renegotiation. These findings are particularly relevant in our setting given that institutions only benefit from MERS if they sell mortgage loans. We contribute to this literature by showing that the MERS technology itself (as opposed to, say, increased investor demand for securitized mortgages) contributed to higher subsequent foreclosure rates, particularly for loans originated by non-bank lenders.

Third, our paper adds to the growing literature on the "plumbing" of the mortgage market and the mortgage securitization process. Hunt, Stanton, and Wallace (2012) first documented the pervasive use of MERS within the U.S. mortgage market. Stanton, Walden, and Wallace (2014) provide a detailed analysis of the industrial organization of the US 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 credit supply, lender-purchaser relationships, and lending outcomes.

Finally, our results contribute to the literature on FinTech (Buchak, Matvos, Piskorski, and Seru, 2018; Fuster, Plosser, Schnabl, and Vickery, 2019) and the efficiency of innovations within the finance industry (Philippon, 2015; Bai, Philippon, and Savov, 2016). We show that MERS led to increased credit supply, thereby providing new evidence that FinTech innovations can benefit the real economy. Furthermore, by showing that the increased credit supply effects of MERS are also linked to higher foreclosure rates, our results suggest that even successful financial innovations like MERS may be associated with unintended consequences that can affect the health of the financial sector and the real economy.

2 Institutional Background

2.1 MERS

The Mortgage Electronic Registration System (MERS) is a privately-owned mortgage registry which 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 burdens associated with registering mortgage transactions with local land record offices was impeding the sale of mortgages on the secondary market (Cocheo, 1996).

Since the early days of property ownership in the United States, each county has maintained records documenting all changes in property ownership, including mortgages. Changes in ownership are indexed through the names of "grantors" (sellers) and "grantees" (buyers). Even if a property is not sold, changes in the ownership of a *mortgage* still constitute changes in who has legal claim to the property, so the transaction must still be registered with the county land office. In particular, each time a mortgage transfers from one party to another, an "assignment document" must be filed with the county land office listing the mortgage's grantors and grantees.[5] Hence, by examining the history of assignment documents for a given mortgage (or a given property), owners, bankers, and other interested participants can determine who has claim to the title of the property. The smooth functioning of the mortgage market relies on having accurate information regarding a mortgage's title. Incorrect title information can impact the validity of foreclosures and can affect the clarity of who actually owns mortgage liens at a given point in time.

As securitization activity rapidly increased during the 1990s, county land offices became overwhelmed with the sheer number of assignments that were required to be filed to facilitate the creation of mortgage backed securities (MBS). This is because the construction of a mortgage-backed security often requires a loan to be sold multiple times before it can be placed into a securitization trust (which is the entity issuing the MBS). For example, in

[5] The term mortgage colloquially refers to two distinct documents: a promissory note and a security instrument. The promissory note creates the debt obligation whereas the security instrument links the property or the collateral to the note. In this setting we refer to a mortgage as the security instrument. The security instrument is vital in determining which entity or person has legal claim to the property.

private-label securitizations, loans are often sold five or more times before reaching investors.[6] The process of tracking and validating the claim to title of the property became arduous as volumes increased, to the point where a typical assignment validation for an average pool size could take up to six months to complete (Arnold, 2010). By one industry estimate, it could cost as much as $250,000 to clean up assignment problems relating to a single block of 2,500 loans, highlighting the monetary costs involved in the assignment validation process (Hansen, 2010). The overarching goal of MERS was (and is) to make the secondary market for mortgages as efficient as possible by removing these costly, time-consuming impediments to the mortgage sale process.

MERS offers two primary benefits to its members. First, MERS eliminates the dollar costs associated with filing assignment documents with county land offices (roughly $35 per assignment). When a MERS member originates a mortgage, it still registers the mortgage in the county land records (it also registers the mortgage with MERS at a one-time cost of $11.95). However, MERS is listed alongside the originator in the land records as a beneficial owner of the mortgage (MERS calls this process "MERS as Original Mortgagee", or "MOM"). By registering the mortgage in the name of MERS, the originator can then sell or transfer the mortgage to another MERS member without having to file assignment documents in the land records. As such, all of the fees associated with filing assignment documents are eliminated if the originator and purchaser are both MERS members.

Second, MERS can significantly reduce the amount of time needed to complete the assignment validation process. Assignment validation refers to the process of auditing a property's entire chain of assignments to determine which parties possess valid liens on the property. This process is critical when constructing securitizations. Registering a mortgage with MERS upon origination removes the need to perform subsequent assignment validation via county land offices because all of the mortgage's ownership changes will be tracked directly through the MERS system itself.[7] Hence, MERS can also greatly reduce the time needed to perform

[6]In a PLS transaction, the loan originator typically sells the loan to an aggregator, which in turn sells it to a sponsor, which pools the loan with other loans and sells the pool to a depositor, which in turn sells the pool to a REMIC (real estate mortgage investment conduit) 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 more legal anecdotes.

[7]See the white paper "Understanding Current Assignment Verification Practices" by Nationwide Title

assignment validation, which can in turn speed up the entire loan sale and securitization process.[8]

Two conditions must be met in order for MERS to be useful to mortgage market participants. First, the originator (or purchaser) of a loan must intend to eventually sell the mortgage or mortgage servicing rights in the secondary marketplace. If the owner of a mortgage intends to hold the mortgage to maturity, 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 loan and sells the loan 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 loan to a non-MERS member. Hence, the benefits associated with MERS are only obtained if a mortgage is originated with the intent to be sold and if both parties to the loan sale are already members of MERS.

2.2 MERS and credit supply

By reducing both the dollar and time costs associated with loan sales, the introduction of MERS should arguably make the secondary mortgage market more efficient. Naturally, loans that are sold repeatedly – such as the mortgages in PLS pools[9] – should realize the largest efficiency gains. As a result of the increased efficiency in secondary mortgage market, the supply curve for securitization sponsors and other mortgage purchasers should shift outward, leading to an increased demand for purchased mortgages, and in particular for mortgages destined for private-label securitizations such as non-conforming mortgages.

We argue that this increased demand for purchased mortgages would be best met by non-bank lenders. Non-bank lenders typically sell 100% of the mortgages that they originate, thereby allowing such lenders to benefit from MERS every time they originate a loan.[10]

Clearing (http://info.nwtc.com/wp-understanding-current-assign-thank-you-page) for more details on assignment validation requirements and mortgageorb.com for anecdotal evidence on assignment validation.

[8] Validating assignments in the land records is not required for MERS-registered mortgages because all assignments will be recorded in MERS.

[9] As noted by Peterson (2010), in a typical private-label securitization, a mortgage is passed from the lender to a warehouse lender, to a sponsor, to a REMIC trust, and finally to investors. Hence, in a typical private-label transaction, loans might be assigned five or more times

[10] In contrast, most commercial banks keep sizable portfolios of mortgages on their balance sheets (Buchak,

Furthermore, anecdotal evidence suggests that non-bank lenders are unconditionally more likely to originate loans that are later sold into PLS pools.[11] At the same time, MERS reduced the time that non-banks would have to hold loans on their balance sheets, thereby freeing up scarce capital that in turn allowed these lenders to originate more loans. As such, the benefits of MERS would potentially be greater for non-banks than banks for all loans (even loans not destined for PLS pools), thus leading to higher origination volumes at non-banks relative to banks.

Finally, the marginal new loan originations that result from MERS would likely involve sub-prime borrowers, since prime borrowers are only rarely denied credit (Bhutta and Keys, 2016; Akey, Heimer, and Lewellen, 2020) and lenders faced lower screening incentives for mortgages that were later sold (Calomiris and Kahn, 1991; Aghion, Bolton, and Tirole, 2004; Keys, Mukherjee, Seru, and Vig, 2010).[12] Hence, we argue that MERS caused an increase in credit supply that was predominantly fueled by non-bank lenders originating loans to sub-prime borrowers.

3 Data

3.1 Massachusetts Registry of Deeds Data

We obtain data from the Massachusetts land records 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.

Matvos, Piskorski, and Seru, 2018).

[11] According to Congressional testimony from the Mortgage Bankers Association, non-bank lenders are more likely to sell their mortgages to loan aggregators (also known as "correspondent lenders") relative to banks, and these correspondent lenders tend to focus on loans that are not conventional, conforming mortgages and hence do not meet the guidelines to be sold to government-sponsored entities like Fannie Mae or Freddie Mac (Stanton, Walden, and Wallace, 2014). Hence, loans originated by non-bank institutions are more likely to end up in private-label securitizations.

[12] In fact most sub-prime loans were originated by non-bank lenders in the run up to the financial crisis (Berndt, Hollifield, and Sandas, 2016)

3.1.1 Mortgage 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 names of the buyers (the mortgage grantors), the institutions funding the purchase (the mortgage grantees), and the total consideration paid by the buyers to the sellers. The mortgage document also lists MERS as a mortgage grantee alongside the lender if the lender is a MERS member. We are aware of no other public data sources that allow us to track MERS activity.

If the original mortgage lender decides to sell the loan to another institution, they are generally required to file an assignment document with the county clerk. Among other data items, the assignment document states the names of the prior lenders (the assignment grantors) and the names of the new lenders (the assignment grantees). 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 where the seller of the loan is the same institution that originated the mortgage.[13] If a loan is sold from one MERS member to another MERS member, and if the original lender listed MERS as a mortgage grantee, then no assignment document needs to be filed with the county clerk when a loan is sold. All subsequent ownership changes are tracked within the private MERS system. The Registry of Deeds also contains foreclosure documents, which we link to mortgage documents via the linking file provided by the Registry of Deeds.

3.1.2 Dataset Construction

Our primary dataset is a loan-level panel spanning the sample period 1990-2018. For each mortgage document filed, we record the property address, purchase date, mortgage amount, lender name, and borrower name (that is, grantee and grantor information). We match mortgage documents to assignment documents through the linking file and flag a mortgage if it was subsequently assigned. If the loan was assigned, we also record the assignment date and the buyer and seller of the loan. The data on assignments is then combined with

[13] We incorporate this restriction because there are instances where 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.

data from the mortgage documents so that each mortgage corresponds to one row in our final dataset. Matching mortgage documents to assignment documents allows us to identify relationships between lenders and who they sell mortgages to after origination.

For each unique grantee or grantor in our dataset, we then manually determine whether the entity is an individual or institution. We discard the names of all individuals and trusts that appear to be controlled by individuals. All information in land records is input by hand, and hence, there are various ways of spelling or abbreviating institution names. To ensure that each lender is coded accurately in our dataset, we conduct a fuzzy matching exercise supplemented by manual verification to ensure that, for example, "JP Morgan Chase", "J.P. MorganChase", "J P M Chase", and "JPMCahse" are all matched to the same institution. In total, our sample contains roughly 50,000 unique institution names corresponding to approximately 6,000 unique institutions operating in the state of Massachusetts between 1990 and 2018.

We then manually match each institution to the Home Mortgage Disclosure Act (HMDA) dataset 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 (non-bank lenders), or five (affiliates of banks)[14]. We also manually confirm HMDA lender type information using Google searches. We then define a dummy variable, *Non-Bank*, that equals one if the institution has a HMDA lender code of three and equals zero otherwise. We manually look up institutions that do not have a HMDA match via Google searches to identify whether they are a lending institution and if so, what type.[15]

We also hand-collect data such as bank regulatory identification numbers (RSSD IDs) and M&A activity for each depository institution from the National Information Center.[16] Our final dataset consists of approximately 1.6 million mortgages originated between 1990

[14] To ensure that our classifications are correct, we manually checked each lender and purchaser using the Federal Reserve's NIC website, HUD websites, and through 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 3 (independent mortgage company) provided by HMDA would be replaced with a lender code of 1 (subsidiary of a depository institution) in our sample

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

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

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 that lender appears alongside MERS 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 is a MERS member at a given point in time.

Our final dataset contains data from six 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 U.S. 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 U.S. 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 U.S. as a whole) in terms of demographics, home ownership, home values, and economic trends.

3.2 Other data sources

We supplement our data from the Massachusetts land records with data from the Home Mortgage Disclosure Act loan application register, commonly known as 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. We also obtain bank accounting and financial information from quarterly bank-level FFIEC 031/041 reports (commonly known as the Call Reports), census tract definitions and tract-level demographic information from the U.S. Census Bureau, and tract-level house price indices from Bogin, Doerner, and Larson (2019).

3.3 Summary Statistics

Panel A of Table 1 shows the average of the fraction of mortgages and lenders in each census tract/Year that are MERS mortgages or MERS lenders respectively. The table shows that MERS membership grew extremely fast following the introduction of MERS in the late 1990's. 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 as a grantee. However, most of the loans listing MERS as a grantee (more than 248,000) were originated by non-banks rather than banks, bank subsidiaries, or bank affiliates (collectively referred to as "banks"). Indeed, more than 55% of all loans originated by non-banks listed MERS as a grantee, whereas only 11% of loans originated by banks listed MERS as a grantee. Hence, most of the "MERS loans" during our sample period were originated by non-banks.

Panel A of Table 2 provides summary statistics on many of our key variables of interest. In total, lenders within a census tract originate 122 mortgages per year with a face value of approximately $34 million. The average foreclosure rate on these mortgages is 1.2%.

Panel B of Table 2 examines similar statistics for loans originated by non-banks versus banks. In total, non-banks originate 35 loans worth about $8 million per census tract per year and banks originate 81 loans worth approximately $27 million per census tract per year within the census tracts in our sample. The average foreclosure rate for loans made in these census tracts is significantly higher for non-banks (2.1%) than for banks (0.9%). Panel B also examines statistics specifically for MERS lenders. MERS lenders in total originate 44 loans worth about \$10.6 million per census tract per year, and the foreclosure rate for MERS loans is similar to that for non-banks at 2.1%.

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

4 Results

4.1 OLS tests

We begin by running a series of simple OLS regressions to better understand the correlations between MERS membership and credit supply. Using data from the Massachusetts land records, we construct an institution by census tract by year panel dataset. 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 the originating institution is a MERS member in a given year, and takes the value of zero for non-MERS members. Standard errors in all tests are clustered by year.

Table 3 reports the results from these regressions. Columns (1) – (4) show that MERS members' origination volume are significantly higher than origination volume by non-MERS members. The estimates in columns (1) and (2) (with no fixed effects) show that dollar volumes increase by 69% and mortgage counts increase by 7% for new MERS members relative to non-MERS members active in the same census tract at the same point in time.

Both estimates are statistically significant at the 1% level. Column (3) and (4) add zip code by year fixed effects, which should capture any time-varying trends in housing demand or local economic conditions across the zip codes in our sample. We find that MERS members originate 34% higher dollar volumes and 8% higher mortgage counts relative to non-MERS members, both of which are statistically significant at the 5% level or better. Hence, simple OLS tests suggest that the introduction of MERS is strongly correlated with increased credit supply.

4.1.1 Potential sources of bias in simple OLS tests

While these tests are instructive, there are at least two potential sources of bias that may cause the estimates in columns (1) – (4) to understate or overstate the true causal effect of MERS on credit supply volumes. The first potential bias relates to the existence of omitted factors such as supply shocks or demand shocks that could be correlated with lenders' decisions to join MERS. While the existence of correlated omitted variables could cause our coefficient estimates to be biased in either direction, it is reasonable to think that our estimates would be biased upwards, since (for example) an increase in the demand for mortgages or an increase in the demand for mortgage-backed securities could be associated with both higher lending volumes and with lenders' decisions to join MERS. In this case, the effects we document would not only contain the causal effects associated with the use of MERS, but would also capture additional effects due to the existence of non-MERS demand or supply shocks.

The second potential bias relates to the nature of the treatment event. In order for MERS to be useful for a lender, it must have a relationship with a purchaser that is also a MERS member. If no purchasers are MERS members, a lender joining MERS would not benefit from the MERS technology, because any loan sales would still have to be registered with the county land records office. Thus, using the *lender* joining MERS as the treatment event (as was done in columns (1) – (4)) could result in significant measurement error. While this issue could again theoretically bias our point estimates in either direction, it would most likely bias estimates downward, since we would potentially count non-events as events and could potentially miss many actual events.

Columns (5) and (6) of Table 3 attempt to correct for the first bias (correlated omitted variables) by making two changes. First, we limit the sample to only include three years of data after a lender joins MERS (years -1, 0, and 1, where year 0 is the year the lender joins MERS). Second we add both zip code by year and lender fixed effects to account for the presence of time-varying correlated omitted variables in a given census tract, and time-invariant differences across lenders. In these columns, the treatment definition is still the lender joining MERS, so the second bias (measurement error) is still present. Columns (5) and (6) show that the effects of MERS on credit supply drop enormously once we attempt to account for correlated omitted variables – both effects are economically small and neither effect is statistically significant from zero.

In the final two columns of Table 3, we keep the same regression specification used in columns (5) and (6), but we now change the treatment event as follows: we assign a lender as treated if the lender is already a MERS member and one of the lender's *purchasers* joins MERS. Intuitively, MERS only benefits a lender if it sells loans to a purchaser that is also a MERS member. Hence, by defining the treatment event as a *purchaser* joining MERS, we can isolate cases where the lender previously could not use MERS, but now *can* use MERS, when it sells loans to that purchaser. This revised treatment definition should reduce the effects of measurement error, since the treatment event now signifies relationships being "switched on" such that lenders could actually benefit from their MERS membership. This revised treatment definition should also help to address concerns about endogenous treatment timing, since we measure changes in *lenders*' lending even though the treatment event is the *purchaser* joining MERS. As such, these specification changes should get us closer to the "true" causal effect of MERS adoption on lending outcomes. Columns (7) and (8) show that after correctly defining the treatment event, we indeed see a significant jump in both the magnitudes and the statistical significance of our point estimates: mortgage volumes are 13% higher and mortgage counts are 16% higher for MERS members relative to non-MERS members after a common purchaser joins MERS.[17]

Collectively, these tests suggest that the introduction of MERS is strongly correlated with

[17]The number of observations increases slightly in columns (7) and (8) because a given lender may have multiple purchasers join MERS, and hence, may be treated multiple times.

increases in credit supply. However, there could still be omitted factors that are correlated with both purchasers' decisions to join MERS and with credit supply. Hence, we augment our OLS tests with a number of additional fixed effects and add a carefully-defined control group in order to better estimate the causal effects of the MERS technology on credit supply.

4.2 Main empirical specification

Our main tests take the form:

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

where i indexes the original mortgage lender, j indexes a purchaser that i has previously sold loans to that became a MERS member in year t, z indexes the zip code, and c indexes the census tract.[18] The unit of observation is a lender-census tract-year combination. Zip code × year, purchaser × year, and "relationship" (i.e. lender × purchaser) fixed effects are also included. We use a three-year window for each "event", with year -1 designated as the pre-event period and year 1 designated as the post-event period. As before, our treatment event is defined as the year in which a purchasing institution becomes a MERS member. We require control lenders to not be a MERS member at any time during the three years surrounding the date that a purchaser joins MERS.

Figure 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 census tract, 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 their loan purchase activity. For example, if investor demand for mortgage-backed securities increases,

[18]Each census tract c is mapped to a single zip code z.

this could cause purchasers to join MERS and could 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 loans 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 Main results

4.3.1 Credit supply effects

Table 4 reports our estimates of equation (1). Consistent with our main hypothesis, columns (1) and (2) of Table 4 show that total mortgage origination volumes increase by 10% at MERS members relative to non-MERS members when an institution they both have a relationship with joins MERS. Figure 3 shows that parallel trends exist for the origination volumes of MERS members and non-MERS members prior to their common trading partner joining MERS. However, following the trading partner joining MERS, origination volumes increase far more rapidly at MERS member institution. Intuitively, since MERS requires bilateral relationships, the purchaser will only benefit from MERS if they purchase loans from MERS lenders rather than non-MERS lenders.

Our central hypothesis is that following the introduction of MERS, origination volumes should increase more for non-banks relative to other types of lenders. If MERS is more beneficial to non-banks, we should see larger responses in origination volumes for non-banks relative to banks when a common purchaser joins MERS. To examine this hypothesis, we construct a dummy variable, *Non-bank*, that takes the value of one if the lender is not a bank, a bank subsidiary, or a bank affiliate, and is zero otherwise. We then interact *Non-bank* with all of the other variables in equation (1). Consistent with our hypothesis, columns (3) and (4) show that both origination counts and origination volumes are higher for non-bank 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 active lenders is coming through non-banks, as the coefficients on *Post* × *MERS Active* are negative and statistically significant.[19]

4.3.2 Non-bank lending to low-income borrowers

We next assess whether non-banks specifically increase credit supply to lower-income populations once they become MERS members. Table 5 documents the results of an institution by year regression where the dependent variables are (log) number of census tracts a lender operates in and the median (log) income of residents in those census tracts. The treatment event is again the year in which a purchaser (not the lending institution itself) became a MERS member. Columns (1) and (2) show that after a purchaser joins MERS, MERS lenders, who can now make use of the technology adopted by their trading partner, increase the number of census tracts they operate in. However, this expansion only occurs for non-banks, as column (1) shows the coefficient on the *Post* × *MERS Active* term is insignificant. Indeed, column (2) shows that once broken out by lender type, non-banks are rapidly expanding the areas in which they lend.

Columns (3) and (4) report the results of similar regressions where the dependent variable is now the average median income per census tract. Column (4) shows that this expansion by non-bank 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 non-bank lenders increasing origination volumes to sub-prime borrowers.[20] In combination, Tables 4 and 5 show that the introduction of MERS allowed non-banks to expand mortgage origination, particularly within lower-income areas.

[19] One explanation for the negative coefficient on *Post* × *MERS Active* is that banks may have switched from originating mortgages to purchasing mortgages (and then selling them into securitizations). Since the benefits of MERS are relatively larger for non-banks, a purchaser joining MERS may change MERS banks' optimization problems such that it becomes more profitable for them to begin purchasing some mortgages rather than originating them. Indeed, Buchak et al. (2018) show that small changes in marginal costs can have a very large impact on lenders' origination vs purchasing behavior.

[20] Sub-prime 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).

4.3.3 Foreclosures

To examine foreclosures, we run a conditional logit regression at the mortgage level, where mortgages are grouped by census tract. Table 6 shows that foreclosures are slightly less likely for loans originated by MERS-active members relative to non-members after a common trading partner joins MERS (column (1)). However, consistent with our proposed channel and the results in Table 4 and Table 5, column (2) of Table 6 confirms that the likelihood of foreclosure is in fact 61% higher for MERS-active non-banks than for MERS-active banks when a common trading partner joins MERS. When combined with results in Table 5, the results in Table 6 are consistent with non-banks expanding credit supply to lower-quality borrowers in low-income areas.

4.3.4 Application denial rates

The results in Table 4 suggest that the introduction of MERS led to a substantial increase in credit supply. However, one drawback of the Massachusetts land records data is that mortgage documents are only filed with county clerks if a mortgage is ultimately originated. To formally show that the increases we document represent increases in credit supply (rather than, say, shocks to credit demand), we would ideally like to show that lenders approved a greater fraction of loan applications following the introduction of MERS.

To perform such a test, we merge our relationship data from Massachusetts with the nationwide HMDA dataset. Since HMDA contains data on all loan applications (not just mortgages that were ultimately originated), we can use HMDA data to determine whether lenders (and particularly non-bank lenders) reduced the rates at which they denied new mortgage applications once a trading partner joined MERS. The HMDA dataset is also national in scope, reducing concerns about the external validity of our previous results. However, one caveat with our HMDA tests is that they likely understate the true effects of MERS because some lenders in our control group may have been treated via having relationships with purchasers who recently joined MERS but do not operate in Massachusetts (recall that we can only observe lender-purchaser relationships in Massachusetts).

Table 7 contains the results of these tests. Column (1) shows that after a purchaser joins

MERS, MERS lenders' denial rates on new mortgage applications fall by approximately 4% relative to non-MERS members, which is an economically large effect. Consistent with previous tests, column (2) of Table 7 also shows that this effect is completely driven by non-bank lenders. These results suggest that the effects we are capturing represent increases in the actual supply of credit granted to homeowners during our sample period.

4.3.5 Private-label securitization

The benefits of MERS would arguably be largest for loans that were ultimately destined for private-label securitizations. PLS mortgages are routinely sold multiple times before reaching investors, and sponsors must validate the mortgage title chain for each loan in a securitization pool. Hence, when a securitization sponsor joins MERS, this should lead to an increase in the sponsor's demand for purchased mortgages. In addition, many PLS loans are originated by non-banks that originate loans and immediately sell them to correspondent lenders or aggregators, who in turn often bundle lower-quality loans that do not conform to GSE standards (Stanton, Walden, and Wallace, 2014). Hence, it is possible that the increase in credit supply we document is largely fueled by loans that were originated by non-bank lenders and then purchased by PLS sponsors.

We examine this question in Table 8 using the sample of nationwide mortgage applications from HMDA. In particular, the *PURTYPE* variable in HMDA describes which type of institution purchases a given loan. One such category ($PURTYPE = 3$) is PLS. We code a dummy variable named *PLS* that equals one if a loan was sold into a PLS, and equals zero otherwise. We then examine whether non-banks are more likely to originate loans 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 loans that were ultimately sold into a PLS deal. Columns (3) – (6) report results after splitting the sample into banks and non-banks. MERS member banks did not sell more loans 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 non-bank MERS members that originated loans that were ultimately sold into PLS deals. Hence, it appears that non-banks were able to expand credit supply in low-income areas in part because of increased demand for these loans as part of private-label securitizations.

4.3.6 Magnitudes

Our main results in column (1) of Table 4 show that lending increased by 10.1% per lender-census tract-year for MERS lenders relative to non-MERS lenders, which we attribute to the use of the MERS technology. On average, approximately one-third of all lenders per census tract are MERS members (meaning that two-thirds of lenders are not MERS members in our sample period). Hence, we find that overall lending increases by approximately $10.1\% \times 1/3 = 3.4\%$ per census tract per year as a direct result of the MERS technology. We also perform a second, more detailed calculation that produces the same estimated magnitude.[21] Hence, using two different approaches, we find that MERS resulted in a 3-4% increase in aggregate credit supply per year.

We next compare our estimated magnitudes with the credit supply magnitudes reported in other papers on the housing boom. Mian and Sufi (2009) document 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 anti-predatory 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 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 as the removal of APLs.

We can also use our estimates to quantify how MERS may have contributed to the rising share of non-bank mortgage lending during the housing boom. In the Massachusetts land records, the share of non-bank lending rose from approximately 24% in 1999 to approximately

[21] Our second calculation starts by examining annual changes in credit supply at the census tract level. Table A.1 of the appendix shows that aggregate origination volumes increase by an average of 17% 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 3.4% per census tract per year due to the MERS technology.

34% in 2007, while total non-bank origination volume rose from \$1.3 billion to \$4.2 billion during the same time period. Using the results in Table 4, we calculate that non-bank lending increased by approximately 3.7% per census tract per year. Hence, the introduction of MERS led to approximately \$400 million in incremental non-bank origination volume in 2007 relative to 1999. As such, we can attribute approximately 14% of the total increase in non-bank lending from 1999 to 2007 to the use of MERS technology.[22]

We also attempt to quantify the loan-level cost savings associated with MERS. For a typical private-label securitization in which loans are sold five times (Peterson, 2010; Levitin, 2013), the cash savings from using MERS would be approximately \$160, or roughly 2% of the average total costs of originating a mortgage.[23] While it is more difficult to estimate the dollar value of the time savings associated with MERS, anecdotal evidence suggests that MERS could reduce the time required to file assignment documents and perform assignment validation by as much as six months (Arnold, 2010), which would most likely yield even larger savings than the cash savings associated with MERS.

Finally, it is worth noting that our magnitude estimates are subject to numerous caveats. For example, the results in Table 4 are based on data from one state (Massachusetts) and may not extrapolate across the entire United States. In addition, due to the staggered nature of MERS adoption, there may be general equilibrium effects caused by early MERS adopters that our empirical specifications fail to adequately capture. In particular, MERS adoption may increase loan volumes, which in turn may increase house prices, which in turn may increase demand for mortgages and further increase loan volumes, and so on. While we attempt to address these points in the section on robustness (section 4.4), we nonetheless acknowledge that our estimated magnitudes could be measured with significant error.

4.3.7 Extensive versus intensive margin effects

Our main tests measure credit supply at the lender-census-tract-year level. As such, they capture loans made along both the intensive margin (i.e. to the same borrower, for the same

[22] 14% $\approx$ \$400 million / (\$4.2 billion - \$1.3 billion).

[23] 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 \$8,500 according to the Mortgage Bankers Association. Source: https://www.mba.org/mba-newslinks/2018/march/mba-newslink-tuesday-3-27-18/.

property) and the extensive margin (all other loans). While there are multiple definitions of the extensive margin in the credit supply literature, our results capture extensive margin effects according to all such definitions. First, within the same census tract, lenders may supply credit to borrowers whose applications would have previously been rejected (Mian and Sufi (2009), Jiménez, Ongena, Peydró, and Saurina (2012), Ramcharan, Verani, and van den Heuvel (2016), Célérier, Kick, and Ongena (2017), and Di Maggio, Kermani, Ramcharan, and Yu (2017), among others). Consistent with this definition of the extensive margin, we show in Table 7 that HMDA application denial rates fall by 4% for MERS lenders relative to non-MERS lenders after a common purchaser joins MERS. Second, lenders may expand into new geographic areas (see, e.g., Adelino, Schoar, and Severino (2016) and GarcÍa (2019)). Indeed, Tables 5 and A.2 show that MERS lenders begin lending in new census tracts after a common purchaser joins MERS. Finally, the total quantity of lending may simply go up in a census tract. Consistent with this definition, we show higher overall lending volumes in Table 4 for MERS members. Table A.1 also shows that aggregate census tract-level origination volumes increase after lenders in that tract join MERS.

One caveat with these extensive margin calculations is that, to be in our sample, lenders must originate mortgages in a census tract during the years before and after a purchaser joins MERS. If lenders add or drop census tracts around the time a purchaser joins MERS, our main tests may not capture the full extensive margin effects associated with MERS. To better understand the importance of this sample restriction, we augment our baseline panel to add zeros in places where a lender could have plausibly lent in given census tract, but did not.[24] Because the natural log of zero is not well-defined, we use an inverse hyperbolic sine (IHS) transformation to measure lending outcomes in these tests.[25] We first re-run our main specification from Table 4 on our entire augmented sample (with zeros added). We then restrict the augmented sample to only include lender-census tract-event groupings that contain zeros, which allows us to examine lending volumes specifically in places where a lender added or dropped census tracts around the time that a purchaser joined MERS.

[24]Specifically, we add zeros in the year prior to the first year a lender makes a loan in a given census tract, and we add zeros in cases where the lender made a loan in the census tract in one year but did not lend in that tract the following year.

[25]We thank the editor and referee for this helpful suggestion.

Table A.2 reports the results of our tests. All four columns of the table show economically large and statistically significant extensive margin effects. Hence, consistent with the results in Table 5, it appears that lenders began to actively originate mortgages in new census tracts as a direct result of the MERS technology.[26]

4.3.8 Spillover effects: House price appreciation

We also examine whether the increased credit supply associated with MERS can help to explain the rapid increases in housing prices prior to the 2007-2009 financial crisis. Following Bernstein, McQuade, and Townsend (2019), we use the house price index constructed by Bogin, Doerner, and Larson (2019), Winsorized at the 5% level, as our outcome variable. We run three specific tests. First, using the Massachusetts land records data, we define MERS-active census tracts as census tracts in which at least one purchaser with a relationship with a lender in that census tract has recently become a MERS member. We then examine whether tract-level house prices increase once a census tract becomes MERS-active. Since house prices are defined at the census tract-by-year level, we cannot include the same set of fixed effects that we used in our other tests. Instead, we include zip code $\times$ year and census tract fixed effects. Second, we define a lender as MERS-active if they are a MERS member and examine what happens to house prices in areas with more MERS active lenders. We also split lenders into banks and non-banks to examine whether the effects are concentrated among non-banks (as we find in our main results). Finally, in our third test, we repeat the same specification using the nationwide HMDA sample. The results are reported in Table A.3. In all three tests, we find that the use of MERS is significantly associated with an increase in house prices, particularly in areas where non-bank lenders supplied credit. The magnitudes, while modest, are still economically relevant; for example, the first column of Table A.3 shows that house prices rise by approximately 1.5% after census tracts become MERS-active relative to other census tracts in the same zip code that are not MERS-active.

[26]Given the arbitrariness of where to add zeros to our data and the difficulty of interpreting aggregate magnitudes from this augmented sample, we view the results in Table A.2 as being largely suggestive in nature.

4.4 Robustness

4.4.1 Relationship formation and dissolution

Our main tests rely on relationships between lenders and purchasers to identify the credit supply increases associated with MERS. However, one concern is that MERS itself may cause lenders to form or dissolve relationships with other trading partners. For example, once a lender joins MERS, that lender may be more likely to form relationships with other purchasers that are MERS members. Similarly, MERS purchasers may be more likely to dissolve relationships with non-MERS lenders once the purchaser joins MERS. This type of endogenous switching of relationships could bias our estimates in either direction, which would raise questions about the robustness of our findings.

However, existing 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, Gerardi, and Hartman-Glaser (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 example, it is costly and time-consuming for lenders to work out pricing agreements and forward sales commitments with purchasers (Lederman and Lasota, 2016).

Consistent with the existing evidence, we find that lender-purchaser relationships appear to be stable in our sample. First, Figure 4 plots the probability of a relationship that exists in year t persisting in year $t+1$. 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.4 shows that the total number of relationships for given lender does not materially change after a purchaser joins MERS. Third, Tables A.5 and A.6 in the appendix show that an lender's MERS status is not predictive of either relationship formation or relationship persistence. Finally, Table A.7 in the appendix shows that purchasers joining MERS do not form new relationships with lenders that have recently become MERS members. Collectively, these results suggest that the endogenous switching of relationships due to MERS membership changes is unlikely to materially affect our results.

4.4.2 Consumer demand shocks

Another possibility is that we are simply capturing an outward shift in the demand for residential mortgages (see, e.g, Barberis, Greenwood, Jin, and Shleifer (2018)) rather than an increase in credit supply. This explanation does not seem consistent with our results. First, we include granular geographic × time fixed effects, which should absorb any time-varying demand for mortgages at a very local level. For consumer demand effects to be driving our results, it would have to be the case that consumers are 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 loans *specifically* from MERS members, *specifically* from non-banks. 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 non-banks particularly in low-income areas, and cannot explain the reductions in application denial rates documented previously.

4.4.3 Common investor demand shocks

A third potential concern is that demand shocks from investors may explain our results. For example, investors may have increased their general demand for mortgage-backed securities, leading securitization trusts to respond by increasing their demand for purchased loans. This would in turn allow lenders to expand their supply of mortgages to consumers. Hence, this chain of events would also cause mortgage rates to fall and 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 × time fixed effects, which will absorb any purchaser-specific changes in the demand for purchased loans. Nonetheless, to ensure that the use of MERS itself is driving any subsequent changes in purchase volumes, we need to show that the purchaser is not demanding additional purchased loans 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, non-bank MERS members) do not have flatter supply curves than other types of lenders. For example, to the extent that non-MERS lenders have flatter supply schedules than non-MERS lenders (or MERS non-banks 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 non-banks relative to MERS banks.

However, this channel does not seem consistent with our collection of results. First, we analyze the extent of the relationship between the purchaser and the MERS active lender relative to the non-MERS active lender in the run up to the purchaser becoming a MERS member. 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. Figure 5 documents the average percentage of all mortgages purchased from each MERS member and non-MERS member, in the 10 years prior to the purchaser becoming a MERS member. Figure 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 non-banks 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 where 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.

4.4.4 External validity

We also re-run our main tests (Table 4) using HMDA data, which includes mortgage applications 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, this is understandable given that the control group includes many lenders who were likely treated (via their purchasers joining MERS) in states other than Massachusetts. Hence, using relationships between mortgage lenders and mortgage purchasers identified only in Massachusetts, we find that MERS lenders expand lending relative to non-MERS lenders across the entire United States after a purchaser that they both have a relationship with becomes a MERS member.[27] We also confirm in untabulated results that the baseline coefficients in Table 4 remain economically and statistically similar during the early (1998-2002) and latter (2002-2006) phases of the housing boom.

4.4.5 Specification choices

Finally, we confirm that our results are not driven by our selection of empirical specifications. In Table A.8, 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.9 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; see, e.g., Goodman-Bacon (2019)).

5 Conclusion

This paper studies the introduction of the Mortgage Electronic Registration System (MERS) in the late 1990s and early 2000s and documents the contribution of MERS to the significant expansion in mortgage credit supply that occurred during the run-up to the 2007-2009

[27] 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.

financial crisis. By removing the need for lenders to update county courthouse records every time a loan was sold, and by removing the requirement to validate mortgage title chains during the securitzation process, 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 show three primary results. First, MERS-active institutions increased their mortgage origination volumes after a trading partner adopted MERS, relative to institutions that were not MERS members but also had relationships with the same trading partner. Second, non-bank lenders were primarily responsible for the overall increase in mortgage origination volumes. Finally, these "extra" loans made by non-bank lenders were disproportionately made to borrowers residing in lower-income areas and were subsequently more likely to be foreclosed upon. Hence, our results suggest that the introduction of MERS led to an expansion in lenders' credit supply, with the bulk of the expansion coming from non-bank lenders expanding credit access to lower-income borrowers.

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

References

Adelino, M., Gerardi, K., Hartman-Glaser, B., 2019. Are lemons sold first? Dynamic signaling in the mortgage market. Journal of Financial Economics 132, 1–25.

Adelino, M., Schoar, A., Severino, F., 2016. Loan originations and defaults in the mortgage crisis: The role of the middle class. Review of Financial Studies 29, 1635–1670.

Agarwal, S., Amromin, G., Ben-David, I., Chomsisengphet, S., Evanoff, D., 2011. The role of securitization in mortgage renegotiation. Journal of Financial Economics 102, 559–578.

Aghion, P., Bolton, P., Tirole, J., 2004. Exit options in corporate finance: Liquidity versus incentives. Review of Finance 8, 327–353.

Akey, P., Heimer, R. Z., Lewellen, S., 2020. Politicizing consumer credit. Journal of Financial Economics forthcoming.

Arnold, R. K., 2010. Comments on "Problems in mortgage servicing from modification to foreclosure". Remarks of R.K. Arnold, President and CEO of MERSCORP, Inc., before the Senate Committee on Banking, Housing and Urban Affairs in the 111th Congress.

Ashcraft, A., Schuermann, T., 2008. Understanding the securitization of subprime mortgage credit. Foundations and Trends in Finance 2, 191–309.

Bai, J., Philippon, T., Savov, A., 2016. Have financial markets become more informative? Journal of Financial Economics 122, 625–654.

Barberis, N., Greenwood, R., Jin, L., Shleifer, A., 2018. Extrapolation and bubbles. Journal of Financial Economics 129, 203–227.

Berndt, A., Hollifield, B., Sandas, P., 2016. How subprime borrowers and mortgage brokers shared the pie. Real Estate Economics 44, 87–154.

Bernstein, S., McQuade, T., Townsend, R., 2019. Do household wealth shocks affect productivity? Evidence from innovative workers during the great recession. Journal of Finance forthcoming.

Bhutta, N., Keys, B. J., 2016. Interest rates and equity extraction during the housing boom. American Economic Review 106, 1742–1774.

Bogin, A. N., Doerner, W. M., Larson, W. D., 2019. Local house price dynamics: New indices and stylized facts. Real Estate Economics 47, 365–398.

Buchak, G., Matvos, G., Piskorski, T., Seru, A., 2018. Fintech, regulatory arbitrage, and the rise of shadow banks. Journal of Financial Economics 130, 453–483.

Calomiris, C. W., Kahn, C. M., 1991. The role of demandable debt in structuring oprimal banking arrangements. American Economic Review 81, 497–513.

Célérier, C., Kick, T., Ongena, S., 2017. Changes in the cost of bank equity and the supply of bank credit, Working Paper, University of Toronto.

Chernenko, S., Hanson, S. G., Sunderam, A., 2014. The rise and fall of the demand for securitization, Working Paper, Harvard Business School.

Cocheo, S., 1996. Moving from paper to blips (the proposed Mortgage Electronic Registration System). ABA Banking Journal 88.

Dell'Ariccia, G., Igan, D., Laeven, L., 2012. Credit booms and lending standards: Evidence from the subprime mortgage market. Journal of Money, Credit, and Banking 44, 367–384.

Demyanyk, Y., Hemert, O. V., 2011. Understanding the subprime mortgage crisis. Review of Financial Studies 24, 1848–1880.

Di Maggio, M., Kermani, A., 2017. Credit-induced boom and bust. Review of Financial Studies 30, 3711–3758.

Di Maggio, M., Kermani, A., Ramcharan, R., Yu, E., 2017. Household credit and local economic uncertainty, Working Paper, Harvard University.

Diamond, D. W., 1991. Monitoring and reputation: The choice between bank loans and directly placed debt. Journal of Political Economy 99, 689–721.

Foote, C., Gerardi, K., Willen, P., 2012. Why did so many people make so many ex post bad decisions? The causes of the foreclosure crisis. FRB Atlanta Research Department Public Policy Discussion Papers 12-2.

Fuster, A., Plosser, M., Schnabl, P., Vickery, J., 2019. The role of technology in mortgage lending. Review of Financial Studies 32, 1854–1899.

GarcÍa, D., 2019. Employment in the great recession: How important were household credit supply shocks? Journal of Money, Credit, and Banking 52, 165–203.

Goodman-Bacon, A., 2019. Differences-in-differences with variation in treatment timing, Working Paper, Vanderbilt University.

Hansen, C. J., 2010. Property: Innovations to historic legal traditions–Jackson v. Mortgage Electronic Registration Systems, Inc. William Mitchell Law Review 37, 355–389.

Hunt, J., Stanton, R., Wallace, N., 2012. All in one basket: The bankruptcy risk of a national agent-based mortgage recording system. UC Davis Law Review 46.

Jiménez, G., Ongena, S., Peydró, J.-L., Saurina, J., 2012. Credit supply and monetary policy: Identifying the bank balance-sheet channel with loan applications. American Economic Review 102, 2301–26.

Keys, B. J., Mukherjee, T., Seru, A., Vig, V., 2010. Did securitization lead to lax screening? Evidence from subprime loans. Quarterly Journal of Economics 125, 307–362.

Lederman, J., Lasota, T., 2016. The Mortgage Professional's Handbook, CreateSpace Independent Publishing Platform, chap. 7.

Levitin, A. J., 2013. The paper chase: Securitization, foreclosure, and the uncertainty of mortgage title. Duke Law Journal 63, 637–734.

Mian, A., Sufi, A., 2009. The consequences of mortgage credit expansion: Evidence from the U.S. mortgage default crisis. Quarterly Journal of Economics 124, 1449–1496.

Mian, A., Sufi, A., 2011. House prices, home equity-based borrowing, and the U.S. household leverage crisis. American Economic Review pp. 2132–56.

Peterson, C. L., 2010. Foreclosure, subprime mortgage lending, and the Mortgage Electronic Registration System. University of Cincinnati Law Review 78, 1359–1407.

Philippon, T., 2015. Has the US finance industry become less efficient? On the theory and measurement of financial intermediation. American Economic Review 105, 1408–38.

Piskorski, T., Seru, A., Vig, V., 2010. Securitization and distressed loan renegotiation: Evidence from the subprime mortgage crisis. Journal of Financial Economics 97, 369–397.

Purnanandam, A., 2011. Originate-to-distribute model and the subprime mortgage crisis. Review of Financial Studies 24, 1881–1915.

Ramcharan, R., Verani, S., van den Heuvel, S. J., 2016. From Wall Street to main street: The impact of the financial crisis on consumer credit supply. Journal of Finance 71, 1323–1356.

Stanton, R., Walden, J., Wallace, N., 2014. The industrial organization of the US residential mortgage market. Annual Review of Financial Economics 6, 259–288.

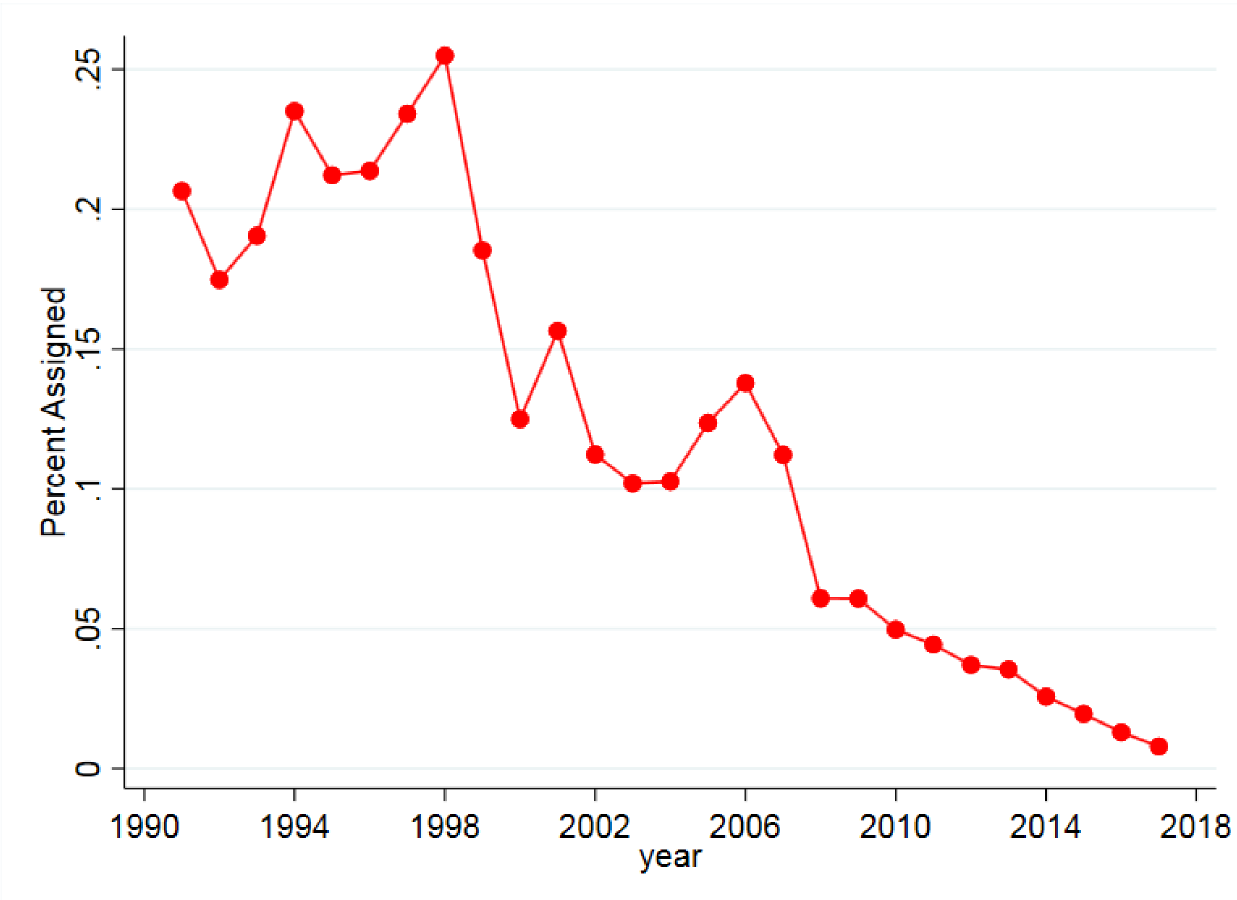

Figure 1: This figure shows the fraction of all mortgages with assignment documents filed immediately after origination.

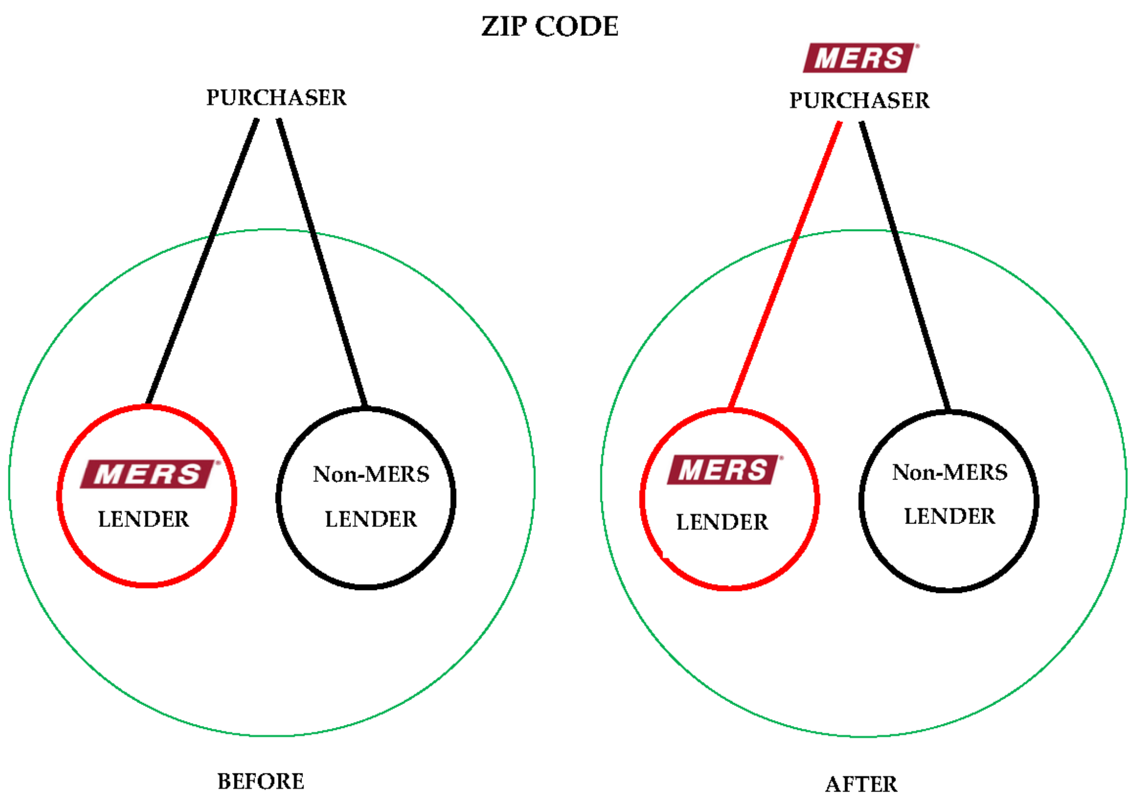

Figure 2: This figure illustrates the differences in differences methodology used.

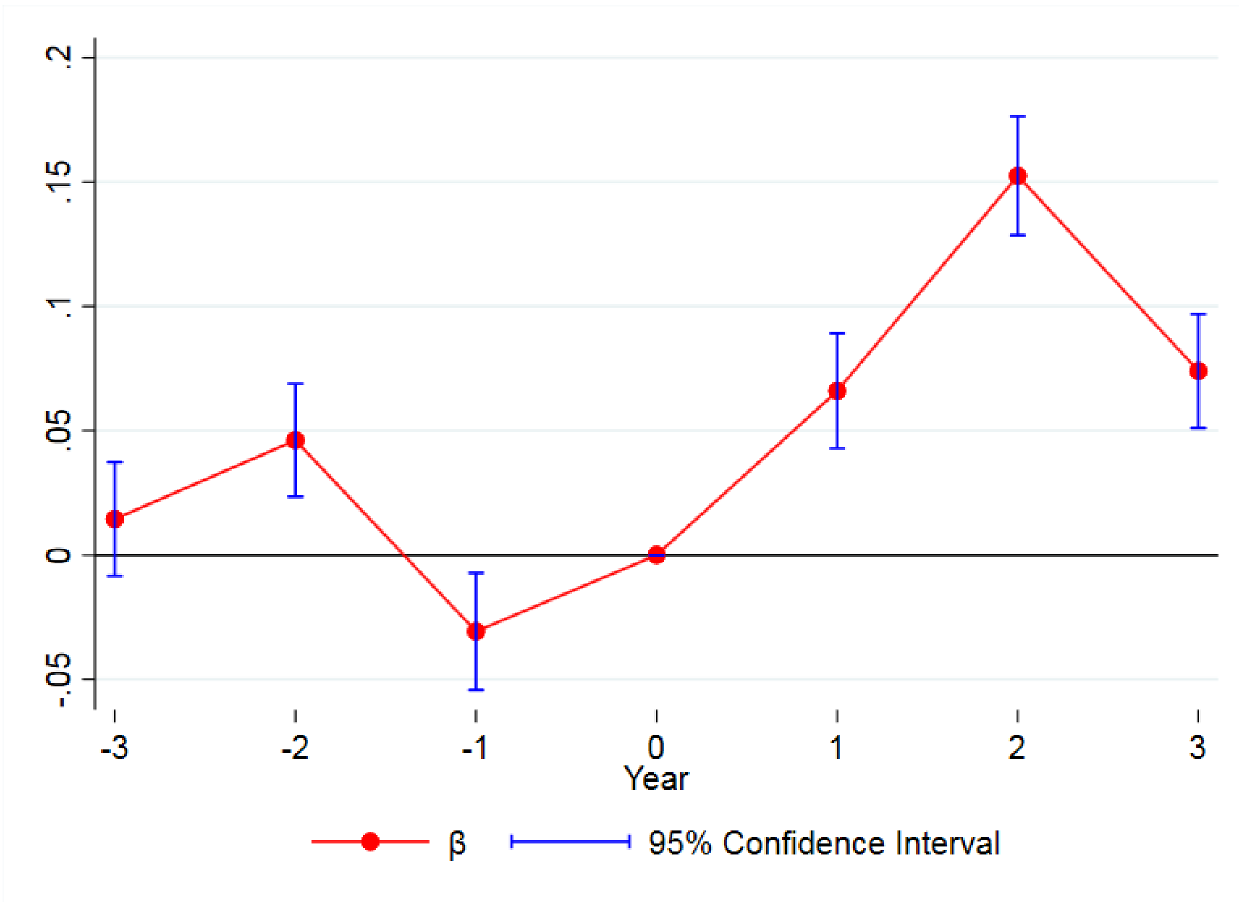

Figure 3: This figure plots the annual coefficients of differences in differences regression, coefficients are relative to treatment year=0.

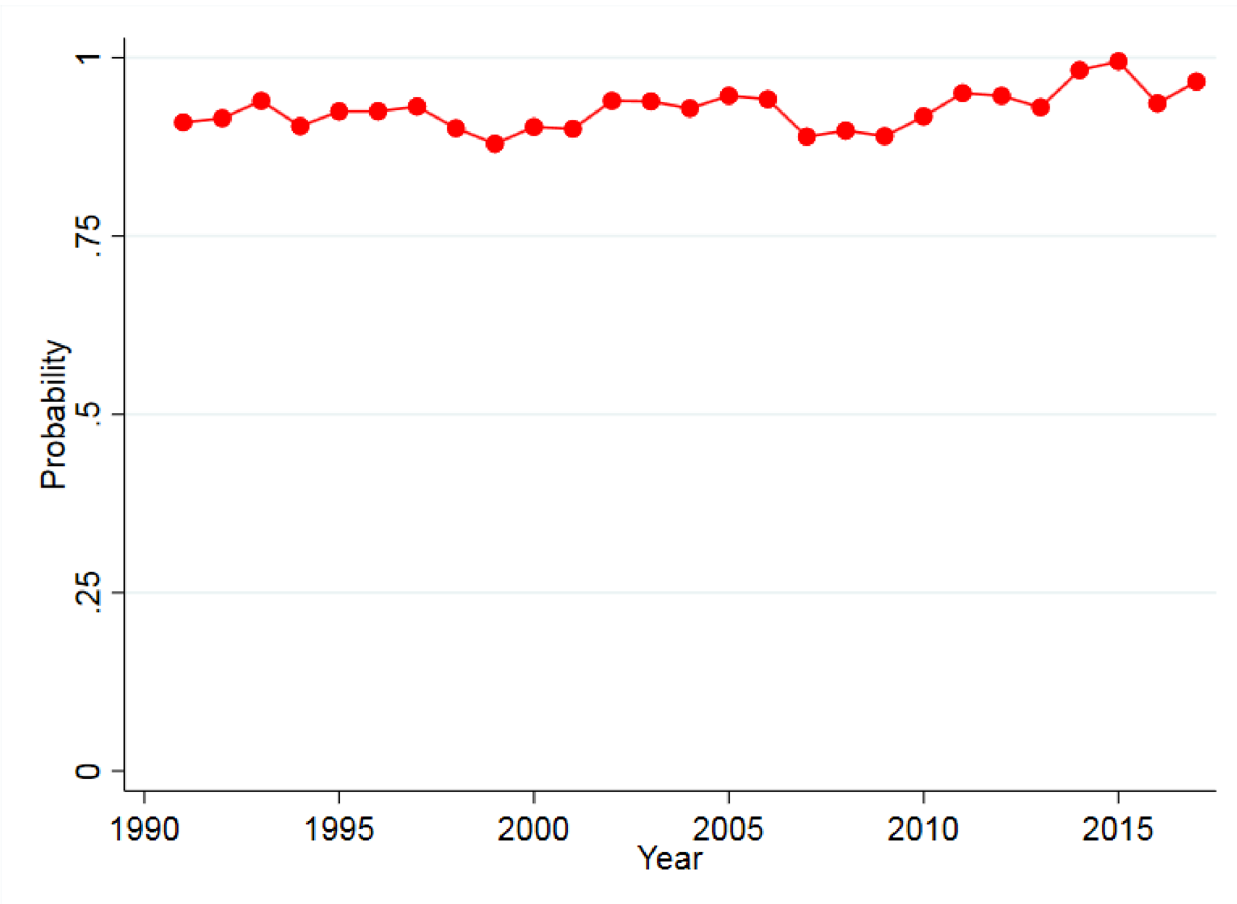

Figure 4: Probability relationship in each year persists the following year

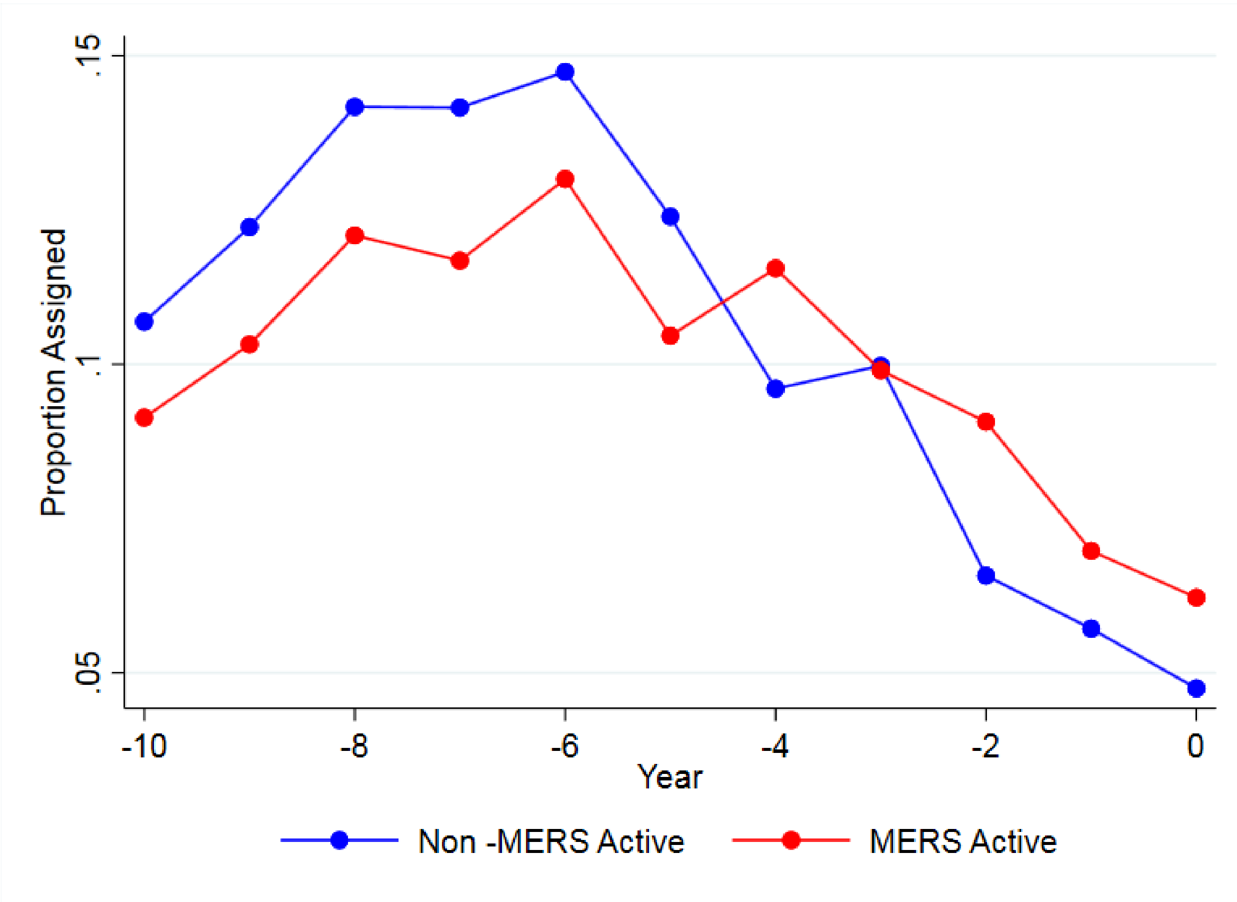

Figure 5: This figure documents the proportion of purchased mortgages from MERS Active and non-MERS active lenders the purchaser has a relationship with in years -10 to 0 where year 0 is the year the purchaser becomes a MERS member.

Table 1: **Summary Statistics: MERS Adoption**

This table contains summary statistics using data obtained from the Massachusetts Registry of Deeds. Panel A documents the proportion of all mortgages that are registered with the Mortgage Electronic Registration System (MERS) and the proportion of all lenders that are MERS active by year and averaged across census tracts. Panel B documents 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. Non-Bank 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 non-matched names. The table also documents 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-active 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%
Non-Bank Originated	451,259	248,563	55%
Total	1,623,199	379,301	23%

Table 2: **Summary Statistics: Massachusetts Land Records**

This table contains summary statistics using data obtained from the Massachusetts Registry of Deeds. Panel A documents total mortgage origination by census tract/year for the full sample. Panel B documents mortgage origination by census tract/year/lender type where non-banks are identified as institutions with a HMDA lender code of 3, or manually identified when no HMDA code exists. Banks are identified as institutions with a HMDA lender code of 0,1,2, or 5, or manually identified when no HMDA code exists. Panel B also documents 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	1,512
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
Non-Banks						
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%

Table 3: **Credit Supply Effects of MERS - Simple OLS**

This table contains results of institution/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/institution/census tract. In columns (1) to (4) Post is a dummy variable that takes a value of 1 for every year after which the lender becomes a MERS member and a value of 0 otherwise. In columns (5) to (8) Post is a dummy variable that takes a value of 1 for the year of and year after which a purchaser who the lender has a relationship with becomes a MERS member and a 0 for the year prior. Various levels of fixed effects are noted in each column and standard errors are clustered by year.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	Log (Volume)	Log (Num. Loans)	Log (Volume)	Log (Num. Loans)	Log (Volume)	Log (Num. Loans)	Log (Volume)	Log (Num. Loans)
Post	0.689***	0.0732***	0.335***	0.0833**	0.0486	-0.0142	0.133***	0.162***
	(0.0688)	(0.0236)	(0.0405)	(0.0388)	(0.0568)	(0.0654)	(0.0182)	(0.0295)
Fixed effects	None	None	Zip × year	Zip × year	Zip × year Lender	Zip × year Lender	Zip × year Lender	Zip × year Lender
Clustering	Year	Year	Year	Year	Year	Year	Year	Year
Observations	495,667	498,848	492,130	495,301	87,227	87,475	95,473	95,579
R-squared	0.102	0.002	0.198	0.064	0.131	0.097	0.376	0.454

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

Table 4: **Credit Supply Effects of MERS**

This table contains results of institution by year by 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/institution. MERS Active is a dummy variable taking a value of 1 if the lender is MERS active, and a value of 0 if the lender is not MERS active in the pre and post period. Post is a dummy variable taking a value of 1 for the year of and year after the year the purchaser the lender has a relationship with becomes MERS active, and a value of 0 for the year prior to the year the purchaser the lender has a relationship with becomes MERS active. Non-Bank is a dummy variable that takes a value of 1 if the institution has a HMDA lender code 3 (i.e. is a non-bank or a manually verified institution of this type), and a value of 0 if the institution has a 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 year.

	Total		Total	
	Log(Volume)	Log(Num. Loans)	Log(Volume)	Log(Num. Loans)
Post × MERS	0.101**	0.0922**	-0.175**	-0.124**
	(0.0426)	(0.0392)	(0.0724)	(0.0552)
Post × MERS × Non-Bank			0.401***	0.364***
			(0.100)	(0.0864)
Fixed effects	Zip code × year Purchaser × year Relationship	Zip code × year Purchaser × year Relationship	Zip code × year Purchaser × year Relationship	Zip code × year Purchaser × year Relationship
Clustering	Year	Year	Year	Year
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

Table 5: **Credit Supply Effects in Lower-Income Areas**

This table contains results of a lender by year regression. 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 the lender operates in. MERS Active is a dummy variable taking a value of 1 if the lender is MERS active, and a value of 0 if the lender is not MERS active in the pre and post period. Post is a dummy variable that takes a value of 1 for the year of and year after the purchaser a lender has a relationship becomes a MERS member, and 0 for the year prior. Non-Bank is a dummy variable that takes a value of 1 if the institution has a HMDA lender code 3 (i.e. is a non-bank or a manually verified institution of this type), and a value of 0 if the institution has a 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 year.

Dependent Variable	Log (No. of Census Tracts)		Log (Median Income)	
Post × MERS Active	-0.0724	-0.438	-0.00210	0.00576
	(0.196)	(0.254)	(0.00591)	(0.00769)
Post × MERS Active × Non-Bank		0.575**		-0.0160**
		(0.216)		(0.00737)
Post × Non-Bank		-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 6: **Foreclosures**

This table contains results of a lender/census tract/year level conditional logit regression grouped by census tract. The dependent variable - Foreclosed - is a dummy variable taking a value of 1 if the proportion of mortgages that were subsequently foreclosed on originated in that census tract/year is greater than 0 and a value of 0 otherwise. MERS Active is a dummy variable taking a value of 1 if the lender is MERS active, and a value of 0 if the lender is not MERS active in the pre and post period. Post is a dummy variable that takes a value of 1 for the year of and year after the purchaser a lender has a relationship becomes a MERS member, and 0 for the year prior. Non-Bank is a dummy variable that takes a value of 1 if the institution has a HMDA lender code 3 (i.e. is a non-bank or a manually verified institution of this type), and a value of 0 if the institution has a 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 x MERS	-0.0646	-0.578***
	(0.109)	(0.158)
Post x MERS x Non-Bank		1.027***
		(0.243)
Post	0.432***	0.661***
	(0.107)	(0.137)
MERS	1.228***	1.087***
	(0.0842)	(0.141)
Non-Bank		-0.191
		(0.160)
MERS x Non-Bank		0.226
		(0.180)
Post x Non-Bank		-0.650***
		(0.223)
Group	Census Tract	Census Tract
Clustering	Year	Year
Observations	38,150	38,150

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

Table 7: **Mortgage Application Denial Rates**

This table contains results of an institution by year by census tract regression using nationwide mortgage origination data from HMDA. The dependent variable is the fraction of new mortgage applications that were denied by lenders. Post is a dummy variable that takes a value of 1 for the year of and year after the purchaser a lender has a relationship becomes a MERS member, and 0 for the year prior. Non-Bank is a dummy variable that takes a value of 1 if the institution has a HMDA lender code 3 (i.e. is a non-bank or a manually verified institution of this type), and a value of 0 if the institution has a 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 year.

Dependent Variable	Denial Fraction	
Post × MERS Active	-0.0395***	-0.0063
	(0.0044)	(0.0060)
Post × MERS Active × Non-Bank		-0.0496***
		(0.0067)
Post × Non-Bank		0.0264***
		(0.0064)
Assignee x Year Fixed Effects	Y	Y
Relationship Fixed Effects	Y	Y
Zip Code x 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

Table 8: **Private Label Securitization**

This table contains results of an institution by year by census tract regression 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 year/census tract/institution. Post is a dummy variable that takes a value of 1 for the year of and year after the purchaser a lender has a relationship becomes a MERS member, and 0 for the year prior. MERS Active is a dummy variable that takes a value of 1 if the lender is a MERS lender and 0 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 non-banks. Non-Banks are institutions with a HMDA lender code 3 (i.e. is a non-bank or a manually verified institution of this type), and banks are institutions with a 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 year.

	(1)	(2)	(3)	(4)	(5)	(6)
	All Lenders		Banks		Non-Banks	
Dependent Variable	Log(Volume)	Log(Num. Loans)	Log(Volume)	Log(Num. Loans)	Log(Volume)	Log(Num. Loans)
Post × MERS Active	-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 Active × PLS	0.151***	0.0982***	-0.0410	-0.0568	0.2653***	0.0862***
	(0.0339)	(0.0278)	(0.0747)	(0.0476)	(0.0367)	
Fixed Effects	Zip × year Purchaser x year Relationship	Zip × year Purchaser x year Relationship	Zip × year Purchaser x year Relationship	Zip × year Purchaser x year Relationship	Zip × year Purchaser x year Relationship	Zip × year Purchaser x year Relationship
Clustering	Year	Year	Year	Year	Year	Year
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

Table 9: **Robustness**

This table reports results from a series of institution by year by 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. In Panel A, the sample is restricted to mortgages where 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 non-bank). 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 Active, Non-Bank, 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 year.

Independent Variable	ln(Volume)	ln(Num. Loans)	N	R-Squared
Panel A: Sample limited to "small" institutions (supply curve tests)				
Post × MERS Active	0.084**	0.094***	28,112	0.498
	(0.0378)	(0.0302)		
Post × MERS Active × Non-Bank	0.294***	0.364***	28,112	0.512
	(0.0985)	(0.0623)		
Panel B: Large vs. small institutions (placebo tests)				
Post × Large	0.095	-0.039	8,904	0.557
	(0.160)	(0.170)		
Post × Large × Non-Bank	-0.025	-0.0585	8,904	0.591
	(0.0674)	(0.496)		
Panel C: HMDA data (nationwide tests)				
Post × MERS Active	0.0588***	0.0747***	17,880,117	0.336
	(0.0112)	(0.0082)		
Post × MERS Active × Non-Bank	0.0643**	0.0985***	17,880,117	0.336
	(0.0252)	(0.0137)		

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

Appendix to:

Did Technology Contribute to the Housing Boom? Evidence from MERS

Stefan Lewellen and Emily Williams

January 2020

Table A.1: **Credit Supply Effects of MERS - Tract-level Evidence**

This table contains results of census tract by year regressions. The first two regressions capture the effects of a census tract becoming "MERS-active" on lending outcomes. We define a census tract as "MERS-active" if at least one lender that makes a loan in that tract has a relationship with a purchaser who has just joined MERS. To avoid the issue of overlapping treatment events, we drop all census tracts that simultaneously appear in the pre-event and post-event groups (where events are defined as in Table 4). As in our other tests, the sample is limited to the three years surrounding purchasers' decisions to join MERS. The control group for these tests are census tracts in the same zip code for which no purchasers joined MERS. For brevity, we report only the interaction term $PostxMERS\ Active$. Standard errors are clustered by year.

	All tracts		Treated tracts only	
	ln(Loan volume)	Ln(Num. Loans)	Ln(Loan Volume)	Ln(Num. Loans)
Post × MERS Active	0.171***	0.110***	0.610***	0.642***
	(0.053)	(0.043)	(0.225)	(0.200)
Fixed effects	Zip code × year	Zip code × year	Zip code × year	Zip code × year
	Tract	Tract	Tract	Tract
Clustering	Year	Year	Year	Year
Observations	6,664	6,670	3,902	3,905
R-squared	0.825	0.904	0.820	0.966

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

Table A.2: **Credit Supply Effects of MERS - Adding Zeros**

This table contains results of institution by census tract by year regressions. We augment the baseline data by adding zeros in the year prior to the first year a lender makes a loan in a given census tract, and in the years following the last year a lender lends in a census tract. The dependent variable is either the inverse-hyperbolic sine of the total dollar amount of mortgages – volume – or the inverse hyperbolic sine of the total number of mortgages – num Loans – originated per year/census tract/institution. MERS Active is a dummy variable taking a value of 1 if the lender is MERS active, and a value of 0 if the lender is not MERS active in the pre and post period. Post is a dummy variable taking a value of 1 for the year of and year after the year the purchaser the lender has a relationship with becomes MERS active, and a value of 0 for the year prior to the year the purchaser the lender has a relationship with becomes MERS active. Non-Bank is a dummy variable that takes a value of 1 if the institution has a HMDA lender code 3 (i.e. is a non-bank or a manually verified institution of this type), and a value of 0 if the institution has a 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). All tracts contain the baseline data and the added zeros, Only tracts with entry and exit contain census tract/relationship instances with zeros in either the pre or post period. Zip code × year, purchaser × year and relationship fixed effects are included. Relationship is the purchaser/lender relationship. Standard errors are clustered by year.

	All Tracts		Only Tracts with Entry or Exit	
	Volume	Num. Loans	Volume	Num. Loans
Post × MERS Active	1.342***	0.240***	2.666***	0.300***
	(0.317)	(0.036)	(0.661)	(0.070)
Fixed effects	Zip code × year Purchaser × year Relationship	Zip code × year Purchaser × year Relationship	Zip code × year Purchaser × year Relationship	Zip code × year Purchaser × year Relationship
Clustering	Year	Year	Year	Year
Observations	43,257	43,257	10,609	10,609
R-squared	0.710	0.598	0.763	0.697

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

Table A.3: **MERS and House Price Appreciation**

This table presents the results of census tract by year regressions of the Bogin, Doerner, and Larson (2019) house price index on the variables *Post* × *MERS Active* and *Post* × *MERS Active* × *Non-Bank*, which are defined in Table 4. Columns (1) and (2) use data from the Massachusetts land records, while column (3) uses the full nationwide HMDA dataset. In column (1), the unit of observation is a census tract-year. In columns (2) and (3), the unit of observation is a lender-census tract-year. Standard errors are clustered by year.

	Land records		HMDA
	Zip code-year	Zip code-year-lender	Zip code-year-lender
Post × MERS Active	2.893***	-0.033	-1.708***
	(1.319)	(0.061)	(0.501)
Post × MERS Active × Non-Bank		0.225***	1.112*
		(0.051)	(0.667)
Fixed effects	Zip code × year	Zip code × year	Zip code × year
	Tract	Tract	Tract
Clustering	Year	Year	Year
Observations	1,983	53,269	390,313
R-squared	0.988	0.989	0.858

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

Table A.4: **Relationship Dynamics**

This table shows the average number of relationships for non-MERS lenders in the years prior to, during, and after a purchaser joins MERS. A relationship is defined as a lender selling at least one loan to a purchaser in a given year. The data source is the Massachusetts land records and the sample period is 1990-2018. Note that once a lender joins MERS, we cannot observe the lender's loan sales (and hence, relationships) in the Massachusetts land records; therefore, the table focuses on the number of relationships that *non*-MERS lenders have with purchasers during the period when a purchaser joins MERS.

	Year		
	$t-1$	t	$t+1$
Mean	6.55	6.11	6.67
10th percentile	1	1	1
50th percentile	5	4	5
90th percentile	14	13	14
t-statistic for difference in means relative to year $t-1$		-0.36	0.12
Observations	283	283	283

Table A.5: **Relationship Persistence**

This table reports the results of a logit regression that examines the factors associated with whether a relationship in one year between a lender and a purchaser persists to the following year. A relationship is defined as a lender selling at least one loan to a purchaser in a given year. The dependent variable takes the value of one if a relationship that exists in year t also exists in year $t+1$ and takes the value of zero otherwise. The variable $MERS$ takes the value of one if a lender is a MERS member in year t and zero otherwise. The variables $ln(Volume)$ and $ln(Num.Loans)$ represent the natural logs of the lender's dollar origination volume and the number of mortgages the lender originated in that year. The variable $Non-bank$ takes the value of one if a lender has a HMDA lender code 3 (i.e. is a non-bank or a manually verified institution of this type) and a value of zero if the institution has a 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). The data source is the Massachusetts land records and the sample period is 1990-2018.

	Relationship persists in year $t+1$
MERS	-0.0102
	(0.0903)
ln(Volume)	-0.175***
	(0.0523)
ln(Num. Loans)	-0.0350***
	(0.0514)
Non-bank	0.160***
	(0.0542)
Constant	0.557
	(0.660)
Observations	18,978

Table A.6: **Predicting Relationship Formation: Lenders**

This table reports the results of a logit regression that examines whether lenders' MERS status predicts relationship formation with purchasers who subsequently join MERS. We first restrict the sample to only include purchasers who join MERS. For each lender-purchaser pair, we then code a variable that equals one if a relationship exists between the two parties in the year before the purchaser joins MERS (year $t-1$ in our main regressions) and 0 otherwise. We then determine whether various lender characteristics are correlated with relationship formation. The variable $MERS$ takes the value of one if a lender is a MERS member in year t and zero otherwise. The variables $ln(Volume)$ and $ln(Num.Loans)$ represent the natural logs of the lender's dollar origination volume and the number of mortgages the lender originated in that year. The variable $Non-bank$ takes the value of one if a lender has a HMDA lender code 3 (i.e. is a non-bank or a manually verified institution of this type) and a value of zero if the institution has a 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). The data source is the Massachusetts land records and the sample period is 1990-2018.

	Relationship exists the following year
MERS	-0.212
	(0.239)
ln(Volume)	-0.0472
	(0.0717)
ln(Num. Loans)	0.385***
	(0.066)
Non-bank	1.151***
	(0.124)
Constant	-5.435***
	(1.034)
Observations	116,291

Table A.7: **Predicting Relationship Formation: Purchasers**

This table reports the results of a logit regression that examines whether purchasers form relationships with lenders who become a MERS member in the year before the purchaser joins MERS. We first restrict the sample to only include lenders who join MERS the year before a purchaser joins MERS. For each lender-purchaser pair, we then code code a variable that equals one if a relationship exists between the two parties in the year before a purchaser joins MERS (year $t-1$ in our main regressions) and 0 otherwise. We then determine whether the purchaser's MERS status is correlated with relationship formation. The variable $MERS$ takes the value of one if a purchaser is a MERS member and zero otherwise. The data source is the Massachusetts land records and the sample period is 1990-2018.

	Relationship exists the following year
MERS	-0.128
	(0.224)
Constant	-4.303***
	(0.105)
Observations	144,095

Table A.8: **Credit Supply Effects of MERS: Extended Event Window**

This table contains results of an institution by year by census tract regression. We rerun the tests reported in the first two columns of Table 4 after extending the pre-event and post-event windows to be three years each instead of one year each. All other variables are defined as in Table 4. Standard errors are clustered by year.

	Log(Volume)	Log(Num. Loans)
Post × MERS	0.114**	0.0749*
	(0.0426)	(0.0392)
Fixed effects	Zip code × year Purchaser × year Relationship	Zip code × year Purchaser × year Relationship
Clustering	Year	Year
Observations	112,556	112,890
R-squared	0.416	0.429

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

Table A.9: **Credit Supply Effects of MERS: First Treatment Event per Lender**

This table contains results of an institution by year by census tract regression. We rerun the tests reported in the first two columns of Table 4 after restricting the sample to only include the first time a lender is treated (via a purchaser joining MERS). All subsequent treatments for the lender (via other purchasers joining MERS) are excluded from the sample. All other variables are defined as in Table 4. Standard errors are clustered by year.

	Log(Volume)	Log(Num. Loans)
Post × MERS	0.0911**	0.0593**
	(0.0387)	(0.0298)
Fixed effects	Zip code × year Purchaser × year Relationship	Zip code × year Purchaser × year Relationship
Clustering	Year	Year
Observations	18,459	18,541
R-squared	0.517	0.528

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