Mortgage Securitization and Shadow Bank Lending*

Pedro Gete
IE Business School

Michael Reher
Rady School of Management,
University of California, San Diego

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Abstract

We show how securitization affects the size of the nonbank lending sector through a novel price-based channel. We identify the channel using a regulatory spillover shock to the cross-section of mortgage-backed security prices: the U.S. liquidity coverage ratio. The shock increases secondary market prices for FHA-insured loans by granting them favorable regulatory status once securitized. Higher prices lower nonbanks' funding costs, prompting them to loosen lending standards and originate more FHA-insured loans. This channel accounts for 22% of nonbanks' growth in overall mortgage market share over 2013–2015. While the shock creates risks for financial stability, homeownership also increases. (*JEL* G12, G18, G21, G23, E32, E44)

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Send correspondence to Michael Reher, mreher@ucsd.edu.

A critical function of securitization is to ensure borrowers' access to capital markets by transforming illiquid loans into liquid asset-backed securities (e.g., Strahan 2012). This process of liquidity transformation generated intense policy debate in the wake of the 2008 Financial Crisis (e.g., Willen 2014), with allegations that it destabilized the financial system by channeling credit to risky *borrowers*. We provide evidence that securitization also affects financial stability by channeling market share to risky *lenders*. This lender-oriented view is particularly relevant given the recent expansion of the nonbank lending sector, often called the shadow banking system. In the mortgage space, nonbanks originate around 80% of loans insured by the Federal Housing Administration (FHA) and more than 50% of all mortgages. This trend concerns policymakers, who fear that nonbanks introduce excessive credit risk into the financial system, and a credit-induced bust could spark a financial crisis because of nonbanks' fragile funding model (e.g., Pinto and Oliner 2015; Wallace 2016; Di Maggio and Kermani 2017).

We document spillover effects from liquidity regulation, and, in so doing, we show how securitization increases the size of the shadow banking system through a novel price-based channel. The underlying theory that we test begins with variation in how lenders fund mortgage originations. Unlike banks, nonbanks lack access to stable deposit funding, and so they fund originations through short-term, warehouse debt that is collateralized by the originated mortgage and repaid once the mortgage has been securitized (e.g., Kim et al. 2018). An increase in mortgage-backed security (MBS) prices raises nonbanks' revenue per mortgage originated, and it lowers their funding costs by improving the value of their collateral. Both forces incentivize nonbanks to extend more credit in the primary market, which they accomplish by relaxing lending standards. Consequently, higher MBS prices increase nonbanks' market share.

Testing this hypothesis with a naive regression of nonbanks' market share on MBS prices would suffer two econometric issues. The first issue is omitted variables bias: unobserved factors (e.g., expectations about the housing market) affect both primary market lending and secondary market prices. To overcome this challenge, we develop a novel empirical strategy based on the cross-section of MBS returns. Broadly speaking, the U.S. MBS market is segmented into two categories: securities insured by Ginnie Mae (GNMA); and securities insured by the government-sponsored enterprises (GSEs), namely, Fannie Mae (FNMA) or Freddie Mac (FHLMC).[1] This market segmentation allows us to difference out common shocks to MBS submarkets and study the relative supply of credit across their corresponding primary markets. In particular, only loans to borrowers satisfying specific requirements stipulated by the Federal Housing Administration (FHA) can be securitized into GNMA MBS. Thus, according to our theory, an increase in the price of GNMA MBS relative to, say, FNMA MBS should increase the relative supply of credit by nonbank lenders in the FHA market.

The second econometric issue is reverse causality: changes in the relative supply of FHA credit affect the relative price of GNMA MBS, whereas we are interested in the reverse effect. We address this challenge by appealing to a natural experiment: the introduction of the U.S. liquidity coverage ratio (LCR). Proposed in October 2013, the LCR is intended to ensure that sufficiently large financial institutions have enough liquidity-weighted assets to survive a 30-day stress period. However, by assigning a preferential regulatory weight to GNMA MBS, this policy also stimulated GNMA demand and consequently increased the market price of GNMA MBS relative to other securities. Using an event study, we find that the introduction of the

[1] A third category, the private label market, evaporated in the years following the 2008 Financial Crisis, so we focus on GNMA- and GSE-backed MBS.

LCR indeed increased GNMA prices and lowered the required return on GNMA MBS by 22% (55 basis points, bps). Since the LCR announcement was largely unexpected and unrelated to contemporaneous trends in the U.S. housing market, it provides exogenous variation in the cross-section of MBS prices. We use this variation to identify the effect of MBS prices on the relative supply of nonbank credit.

Our first exercise is a loan-level, triple difference-in-differences research design. We obtain identification from the triple difference between: banks versus nonbanks (i.e., "treated lenders"); conventional versus FHA loans (i.e., "treated loan types"); and the GNMA premium from before versus after the LCR shock (i.e., the "treatment"). This strategy allows us to include lender-year, metropolitan statistical area (MSA)-year, and MSA-lender fixed effects along with borrower controls that might otherwise affect origination decisions. Consequently, we rely on a very weak identification assumption. We find that nonbanks respond to the increase in GNMA prices by relaxing their lending standards and denying 15% (2.1 pp) fewer FHA loan applicants, relative to banks.

We assess the implications of our loan-level results for nonbanks' overall mortgage market share using a ZIP-code-level regression, where "treated ZIP codes" are those with greater reliance on both FHA credit and nonbanks in 2013. Per our loan-level results, such ZIP codes are more exposed to the LCR-induced increase in nonbanks' credit supply. We find that more-exposed ZIP codes experience higher growth in nonbanks' market share over 2013–2015 as well as higher growth in overall mortgage volume, suggesting that nonbanks drive an increase in credit supply rather than merely substituting for banks. Using our ZIP-code-level estimates, we perform an aggregation exercise in the spirit of Chodorow-Reich (2014). Accordingly, we find that the shock accounts for 22% (1.2 pp) of nonbanks' observed growth in market share over 2013–2015. As expected, the effect is substantially stronger within the FHA market, where the shock accounts for 48% of nonbanks' growth.

The increase in nonbanks' market share affects financial stability through two channels: credit risk and funding fragility. First, nonbanks' relaxation of lending standards and willingness to approve larger loans imply an increase in credit risk, and especially so in our setting because FHA loans are intended for riskier borrowers (e.g., Urban Institute 2017). Indeed, we find that ZIP codes more exposed to the LCR-induced increase in nonbank lending see higher mortgage default rates. Second, a larger nonbank lending sector increases average funding fragility because nonbanks' short-term funding model makes them more prone to run-like withdrawals (e.g., Kim et al. 2018).[2]

Yet, despite these negative implications for financial stability, the spillovers from LCR regulation have an ambiguous effect on overall welfare. For example, we also find that the increase in nonbanks' credit supply raises ZIP-code-level homeownership, which may increase welfare by giving lower-income households access to mortgage credit.

We pursue several extensions to evaluate the theory by which higher secondary market prices increase nonbanks' share of the primary market. First, to confirm that the relevant mechanism is variation in funding models, we obtain similar results when defining "treated lenders" as those with less historical reliance on deposit funding or greater historical reliance on securitization. In fact, the results are almost the same when dropping nonbanks from the sample, consistent with heterogeneity in bank funding models (e.g., Loutskina 2011; Cornett et al. 2011; Dagher

[2]Quoting the financial press, "many nonbanks remain highly reliant on short-term funding ..., so if wholesale markets froze again, many Americans would quickly lose access to mortgage finance" (*Economist* 2020).

and Kazimov 2015). Second, we find that nonbanks do not tighten their lending standards among non-FHA loans to compensate for their looser lending in the FHA market, and this lack of reallocation supports the validity of our large estimated effect on nonbanks' overall market share. However, consistent with a new literature pioneered by Chakraborty, Goldstein, and MacKinlay (2018, 2020), we find that capital-constrained banks who rely on securitization indeed reallocate loanable funds across mortgage types, as they loosen standards among FHA borrowers but tighten them among non-FHA borrowers.

Our findings are also robust to a variety of other tests meant to assess internal validity. For example, the results are similar when restricting the sample to small lenders, studying other measures of credit supply (e.g., loan volume, interest rates), and controlling for regulatory arbitrage incentives, as measured by capital ratios or stress testing requirements. To ensure the validity of our ZIP-code-level results, we conduct a placebo test over the 2011–2013 period and find no effect. Furthermore, our measure of a ZIP code's exposure is uncorrelated with other drivers of nonbanks' market share, such as household demographics or the strength of local banks' balance sheets, further supporting the results' validity. Indeed, based on a wide variety of robustness tests, we find no evidence that our findings are driven by: increased litigation risk associated with the False Claims Act; the Fed's quantitative easing program; a pre-trend in nonbank denial rates; or spurious correlation between the introduction of the LCR and the increase in the GNMA premium.

Our paper makes three contributions to the literature. First, a large number of papers have studied how securitization affects the quantity and quality of credit in primary lending markets (e.g., Loutskina and Strahan 2009; Keys, Mukherjee, Seru, and Vig 2010; Keys, Seru, and Vig 2012; Benmelech, Dlugosz, and Ivashina 2012; Nadauld and Sherlund 2013). These papers focus on how securitization affects the distribution across types of loans that are originated in the primary market. By contrast, we study how securitization affects the distribution across types of lenders who intermediate those loans, which has implications for financial stability.

Second, we contribute to a growing number of papers on the consequences and causes of recent growth in the nonbank lending sector. In terms of consequences, several papers highlight the systemic risks associated with greater reliance on nonbanks (e.g., Kim et al. 2018; Drechsler, Savov, and Schnabl 2019; D'Avernas, Vandeweyer, and Pariès 2020; Hanson et al. 2015). In terms of causes, the existing literature has found that nonbanks' market share depends on regulatory arbitrage (e.g., Buchak et al. 2018), technological innovation (e.g., Fuster et al. 2019), bank capitalization (e.g., Irani et al. 2019; Chernenko, Erel, and Prilmeier 2019), and creditor protection in the warehouse lending market (e.g., Ganduri 2019). Our paper shows how secondary market prices are another force—among the aforementioned ones—that affects nonbanks' market share. In addition, we document potentially welfare-improving consequences of a larger nonbank lending sector, such as access to homeownership.

Third, researchers and policy makers have been increasingly interested in how financial regulations introduced in the wake of the 2008 Financial Crisis affect housing markets. To date, papers have documented important effects related to stress tests (e.g., Calem, Correa, and Lee 2019; Gete and Reher 2018), qualified-mortgage requirements (e.g., De Fusco, Johnson, and Mondragon 2020), litigation risk (e.g., D'Acunto and Rossi 2019; Gissler, Oldfather, and Ruffino 2016), and capital requirements (e.g., Reher 2020). In Europe, Van Bekkum, Gabarro, and Irani (2017) show how MBS rating requirements affect credit supply. We show how liquidity regulation (i.e., the LCR) also affects the housing market in meaningful ways, such as raising nonbanks' share of mortgage lending, increasing credit risk, and bolstering homeownership. These effects are an unintended consequence of the LCR, and they must be weighed against the intended effect of increasing banks' liquid asset holdings (e.g., Roberts, Sarkar, and Shachar 2018).

1 Theory

Our empirical analysis is grounded in a theory of mortgage markets where lenders vary in how they fund mortgage originations. The theory has several steps, which we describe here and complement with a simple framework in Internet Appendix B.

1. *Difference in Funding Model*: Nonbanks fund mortgage originations using short-term warehouse credit because they do not have access to deposits. Consequently, nonbanks have an originate-to-securitize funding model as shown in panel A of Figure 1: they fund a mortgage using short-term debt that is borrowed from a separate warehouse lender and collateralized by the mortgage; then, they securitize and sell the mortgage in the secondary market at a price P_S; lastly, using the proceeds from this sale, they repay the warehouse lender at the effective gross interest rate R_W (e.g., Kim et al. 2018; Echeverry, Stanton, and Wallace 2016). Depository institutions (i.e., banks), on the other hand, can fund originations using deposits, as shown in panel B.

   We summarize the two funding models shown in Figure 1 as follows: the originate-to-securitize model is a loan-by-loan funding model because warehouse lenders have a claim on the loans originated. By contrast, depositors have a claim on the collective value of the lender's assets.

2. *Nonbanks' Funding Costs*: An increase in MBS prices improves the value of nonbanks' collateral and lowers their funding costs, since warehouse lenders can expect to break even at a lower promised interest rate.[3] In our empirical setting, MBS prices increase because a regulatory shock incentivizes large financial institutions to buy more MBS. Indeed, the shock we study comes with a reduction in nonbanks' cost of warehouse credit, as we will show in Figure 4.

3. *Credit Supply*: An increase in MBS prices not only lowers nonbanks' funding costs, as mentioned in Step 2, but also raises nonbanks' revenue per dollar of mortgage credit originated. Both channels incentivize nonbanks to originate more loans in the primary mortgage market. Nonbanks originate more loans by denying fewer borrowers, as we will show in Table 2. They also originate larger loans, as in Table 9.

4. *Market Share*: More relaxed lending standards increase the relative supply of credit by nonbanks because, as described in the next point, banks do not respond to higher MBS prices in the same way as nonbanks. Therefore, nonbanks' mortgage market share increases, as Tables 3 and 4 will show.[4] This effect stems from differences between banks and nonbanks' funding models, as distinct from other differences like risk aversion.[5]

[3] We implicitly assume that warehouse lenders are competitive and that the reduction in interest rates is for a given level of borrower credit risk.

[4] This step implicitly assumes a lack of crowding-out effects across different types of mortgages. We do not find any evidence of crowding out by nonbanks, as discussed in Section 7.4.

[5] If nonbanks are less risk averse than banks, then their steady-state market share among riskier loan types will be higher, but their response to a change in MBS prices will be the same. We formalize this argument in Internet Appendix B and substantiate it empirically in Table 8 and Internet Appendix Table A7.

5. *Choice of Funding Model*: In principle, banks can adopt an originate-to-securitize model, but, empirically, banks securitize a smaller share of their loans than nonbanks, as shown in Internet Appendix Table A1. This observation is consistent with a literature on the advantages that deposit funding confers on banks. A partial list of such advantages includes: the ability to exert market power over depositors (e.g., Drechsler, Savov, and Schnabl 2017); extract a money premium from the fact that deposits are government insured (e.g., Hanson et al. 2015); and the option to cross-sell other products, such as insurance, to depositors.[6] Consistent with these advantages, Internet Appendix Table A2 shows how banks with more market power over depositors choose to securitize fewer of their loans.[7]

2 Identification

The theory described in Section 1 predicts that higher MBS prices will lower nonbanks' lending standards and increase the overall share of mortgage credit that they intermediate. We test this hypothesis in the U.S. market using a novel methodology that has two key features: (a) we obtain identification through the cross-sectional distribution of MBS prices, and (b) we utilize an exogenous, regulatory shock to this cross-sectional distribution.

First, we address the challenge of omitted variables bias by turning to the cross-section of MBS prices, or, to be precise, MBS expected returns. Specifically, we focus on the price of

[6]Molyneux, Reghezza, and Xie (2019) show how banks complement higher deposit rates with higher fees.

[7]Market power is proxied by taking a bank's average deposit market share across U.S. counties in which the bank has a branch. We control for various bank risk factors studied in Cornett et al. (2011) as well as the bank's average mortgage market share, which ensures that the negative relationship between securitization activity and deposit market share is not driven by the scale of the bank's mortgage lending. The only variable that is consistently significant across specifications is the bank's deposit market share.

Ginnie Mae (GNMA) MBS *relative* to either Fannie Mae (FNMA) or Freddie Mac (FHLMC) MBS. This technique differences out common shocks to the MBS market, such as expected housing demand or the Fed's quantitative easing program, which also affect outcomes in the primary mortgage market. Correspondingly, in our main analysis we study how increases in the relative price of GNMA MBS—or, equivalently, reductions in expected return—affect nonbanks' market share among borrowers whose loans are eligible for securitization as GNMA MBS, namely, Federal Housing Administration (FHA) loans.

Second, we address the question of reverse causality by turning to a natural experiment: the introduction of the U.S. LCR. Exogenous changes in nonbanks' FHA lending standards can affect GNMA prices, that is, the reverse of the causal relationship we are interested in estimating. Thus, we perform our analysis over a period during which there was an exogenous shift in the GNMA premium due to the introduction of the LCR, which we will describe next.

2.1 The liquidity coverage ratio

The U.S. LCR was introduced as part of the post-crisis regulatory overhaul, and it was intended to ensure that sufficiently large financial institutions have enough liquid assets to survive a 30-day period of cash outflows. The policy assigned different liquidity weights to assets, where a higher weight implies more favorable regulatory treatment.[8] In particular, the rule favored GNMA MBS with a weight of one, as opposed to 0.85 for FNMA and FHLMC MBS. This distinction reflects the explicit government guarantee associated with GNMA MBS, versus

[8]Explicitly, a bank's LCR is defined as the sum of liquidity-weighted assets divided by 30-day cash outflows. This ratio is required to exceed one for affected banks. See the report by the Basel Committee on Bank Supervision (2013) or Diamond and Kashyap (2016) for discussion of additional institutional details and the policy's motivation.

the implicit guarantee associated with FNMA and FHLMC MBS because they are not officially government entities and, under normal circumstances, would be publicly listed companies. The regulation was proposed on October 24, 2013, and finalized in September 2014, with few changes relative to the initial proposal. Before this proposal, there was uncertainty over the institutional details of the LCR, since Federal Reserve Governor Daniel Tarullo had raised the possibility that the U.S. LCR implementation might differ from international standards, but he did not indicate how it would differ.[9] We therefore refer to the introduction of the LCR on October 24, 2013, as the "LCR shock," and we define the "shock year" as 2014, the first full year after this introduction.

Given these details, one might expect the introduction of the LCR to affect MBS prices through: (a) an increase in affected institutions' demand for GNMA MBS; and (b) consequently, an endogenous increase in GNMA market liquidity, which would increase nonaffected institutions' GNMA demand. Both channels imply that GNMA prices should rise—and expected returns should fall—because of an increase in demand. Importantly, banks affected by the LCR must purchase GNMA MBS on the secondary market to satisfy the regulatory requirement: they cannot satisfy the requirement by simply originating more FHA loans and holding them on their balance sheets.[10]

Beginning with quantities, in Figure 2 we examine the direct effect of the LCR shock (i.e., channel (a) from the previous paragraph) by plotting the GNMA portfolio holdings of banks subject to the LCR rule. The figure shows how affected banks substantially increase their

[9]See Governor Daniel K. Tarullo's (2011) speech and Getter (2014).

[10]In principle, banks could originate more FHA loans, sell them as GNMA MBS, and immediately repurchase a securitized loan. However, based on conversations with industry practitioners, this would be an unprofitable strategy relative to simply buying GNMA MBS directly, because originating new loans entails additional operating costs. We thank an anonymous referee for encouraging us to investigate this possibility.

holdings of GNMA MBS after the LCR shock. In particular, the increase equals 7 pp of the holdings by affected plus nonaffected banks. Internet Appendix Figure A1 suggests the supply of GNMA MBS increased to meet this demand, showing that the share of FHA loans sold on the secondary market increases relative to non-FHA loans after the introduction of the LCR. This supply response comes with a substantial reduction in the cost of warehouse credit used to produce GNMA MBS, as discussed in the context of Figure 4. These large direct effects of the LCR shock help reconcile the large spillover effects that we estimate in Sections 3 and 4.

Turning to prices, in Figure 3 we plot the 12-month-ahead GNMA total gross return (i.e., expected return) relative to FNMA MBS. The expected return on GNMA and FNMA MBS track each other closely in the months leading up to the LCR shock, after which the return on GNMA is lower because its price is higher (i.e., high expected returns correspond to low prices, and vice versa). Phrased differently, investors who purchase GNMA MBS on or after the announcement of LCR regulation would be willing to receive a lower return relative to holding FNMA MBS. By contrast, this differential was absent in the preannouncement period. To get a sense of magnitude, the change in the expected return to GNMA MBS relative to FNMA MBS in the three months after the LCR shock is 37% larger than the analogous change after Fannie Mae was delisted in June 2010.

The previous results provide evidence that the introduction of the LCR increased the demand for and the price of GNMA MBS, both in absolute terms and relative to non-GNMA MBS. We provide more rigorous evidence by conducting an event study which estimates the GNMA premium generated by the introduction of the LCR. To keep the paper focused, we defer details on this exercise to the Internet Appendix. Briefly, our central estimate in Internet Appendix Table A15 suggests that the introduction of the LCR lowered the expected total return to GNMA MBS relative to FNMA MBS by 55 bps, which we call the "LCR premium."[11] This premium is equal to 22% of the average real total return to GNMA MBS over 2000–2015 and 0.9 standard deviations of the FNMA-GNMA spread. We obtain similar results when studying the option-adjusted spread (OAS) as opposed to total return, which implies that the results are not driven by changes in prepayment risk.

2.2 Graphical evidence

Before conducting our main analysis, we begin with some preliminary evidence which situates the LCR shock within our theory. As described in Section 1, an exogenous increase in GNMA MBS prices should incentivize nonbanks to originate more FHA loans, in part because higher MBS prices lower their funding costs in the warehouse credit market. This is exactly what we find in Figure 4. Based on novel data on nonbanks' income statements from the Mortgage Bankers' Association (2014), the introduction of the LCR comes with a 0.9-pp decline in the average nonbank's cost of warehouse credit, measured by the ratio of its interest expense to the value of its credit lines.

By stimulating originations, higher MBS prices should also increase nonbanks' share of the mortgage market. Figure 5 provides evidence in favor of this hypothesis which strongly supports the parallel trends assumption made later in the paper. Using the already-established data set

[11] The "control securities" in our setting are FNMA or FHLMC MBS, since, like GNMA MBS, these also comprise mortgages and carry a government guarantee against credit risk. This research design allows us to credibly identify the GNMA premium by differencing out common shocks to the mortgage market, but it makes our results conservative because FNMA and FHLMC MBS also received favorable regulatory status compared to, say, certain types of corporate bonds. Following Diep, Eisfeldt, and Richardson (2017), we focus on MBS total returns measured using the Bloomberg-Barclays Total Return index, since total returns are less model dependent than an option-adjusted spread (OAS).

from Buchak et al. (2018), we plot the dynamics of nonbanks' FHA mortgage market share.[12] The figure reveals a secular trend in nonbanks' FHA market share leading up to the policy's introduction, which is also present in the non-FHA (i.e., conventional) market. This secular trend has been well-documented by the literature on nonbanks referenced in the introduction. However, the introduction of the LCR comes with a sharp break from trend, and nonbanks' FHA market share in 2015 is 17 pp higher than what one would have predicted based on its pre-LCR growth rate. By contrast, there is no break from trend within the non-FHA market. Internet Appendix Figure A2 shows how the increase in nonbanks' FHA market share drives an increase in their overall market share, which in 2015 was 8 pp higher than the value implied by its pre-LCR trend.

Collectively, these preliminary results support the idea that the LCR-induced increase in MBS prices raises the supply of nonbank credit and contributes to nonbanks' growth in overall market share. The rest of the paper rigorously evaluates this hypothesis, studying credit supply (i.e., lending standards) in Section 3 and market share in Section 4. To be clear, our research hypothesis is not that the LCR-induced increase in MBS prices is the only driver of nonbanks' market share, but rather that it is a first-order contributor among the others which have been documented in the literature (e.g., regulatory arbitrage, technology).

3 Effect on Lending Standards

Our parameter of interest is the effect of an increase in the GNMA premium on the supply of nonbank credit for FHA-eligible borrowers, recalling that only FHA loans can be securitized

[12]We thank Greg Buchak for sharing these data.

as GNMA MBS. We measure credit supply using loan denial rates, which allows us to use microdata and include multiple fixed effects to absorb confounding factors. Moreover, studying denial rates allows us to focus on the extensive margin of credit, so that our estimates have the interpretation of an effect on lending standards. We study other outcomes in Section 7 (e.g., loan size, interest rates).

3.1 Data

Our core data set is a merge of the Home Mortgage Disclosure Act (HMDA) mortgage application registry with bank FRY-9C Call Reports. In the interest of space, we defer a detailed description of all our data sets to Internet Appendix A. Briefly, HMDA data contain information on the borrower and outcome of almost all mortgage applications in the United States. We retain FHA and conventional loan applications for the purchase of owner-occupied, single-family dwellings. We use the term "conventional" to describe non-FHA loans whose value is below the associated conforming loan limit (i.e., nonjumbo loans). These restrictions give a sample of 396 lenders over the 2010–2015 period, 123 of which are nondepository institutions, which we call "nonbanks." The upper panels of Table 1 summarize the resultant data set. For computational convenience, we perform our loan-level analysis on a 25% random sample of the full data.

3.2 Loan-level specification

We perform a triple difference-in-differences analysis across lenders, years, and loan types, and our baseline regression equation is

$$\begin{aligned} Denial_{i,l,s,t} = {} & \beta\left(Nonbank_l \times Premium_t \times FHA_s\right) + \gamma X_{i,t} + \alpha_{l,t} + \alpha_{m(i),t} + \alpha_{m(i),l} + ... \\ & ... + \alpha_{s,t} + \alpha_{s,l} + u_{i,l,s,t}, \end{aligned} \tag{1}$$

where i, l, s, and t index borrower (i.e., loan applicant), lender, loan type, and year, respectively; $Denial_{i,l,s,t}$ indicates if the application was denied; $Nonbank_l$ indicates if the lender is a nonbank; and the set of loan types are FHA or conventional. In words, "treated lenders" are nonbanks, "treated loan types" are FHA loans, and the "treatment," $Premium_t$, is a measure of the relative price of GNMA MBS and thus nonbanks' incentive to originate FHA loans.

Our first measure of $Premium_t$ is an indicator for whether LCR regulation is in place. Specifically, we use an indicator for whether $t \geq 2014$, the first full year after the LCR announcement in October 2013. More directly, we also measure $Premium_t$ using the spread in the 1-year-ahead total return between FNMA and GNMA MBS.[13] For purely interpretive purposes, we normalize the FNMA-GNMA spread by 55 bps, which is the estimated effect of LCR regulation discussed in Section 1 and estimated in the Internet Appendix.

From the standpoint of identification, the most powerful feature of our triple difference-in-differences research design is the lender-year fixed effect, $\alpha_{l,t}$, which absorbs shocks to a lender's

[13] We take the average 12-month-ahead total return among months in year t, where total returns are measured using the Bloomberg Barclays MBS Total Return indices. Based on the law of iterated expectations, the realized 12-month-ahead return in a given month equals the expected return in that month, on average.

overall level of credit supply. Thus, any confounding factor coinciding with $Premium_t$ would not only need to disproportionately affect nonbanks but also need to affect nonbanks' willingness to approve FHA over conventional loans. The type-year fixed effects $\alpha_{s,t}$ absorb time variation in lending standards for FHA loan applications due to, say, greater litigation risk. In addition, the type-lender fixed effect $\alpha_{s,l}$ accounts for the effect of lenders' sorting into FHA or conventional loan markets.

The remaining fixed effects ensure robustness to geographic variation. The MSA-year fixed effect $\alpha_{m(i),t}$ captures contemporaneous shocks to local demand in borrower i's MSA of residence, $m(i)$. These contemporaneous demand shocks might otherwise bias the estimate to the extent that they also affect a borrower's propensity of being denied (e.g., expected income growth). We also restrict variation to the same geographic lending relationship by including an MSA-lender fixed effect, $\alpha_{m(i),l}$. This fixed effect rules out the possibility that nonbanks sort into markets where their applicant pool is of better credit quality. Finally, the borrower controls $X_{i,t}$ account for time variation in the observable credit quality of bank versus nonbank applicants.[14]

The identification assumption implicit in Equation (1) is

$$0 = \mathbb{E}\left[Nonbank_l \times Premium_t \times FHA_s \times u_{i,l,s,t} \middle| \alpha_{l,t}, \alpha_{m(i),t}, \alpha_{m(i),l}, \alpha_{s,t}, \alpha_{s,l}, X_{i,t} \right]. \tag{2}$$

In words, Equation (2) states that the LCR-induced increase in the GNMA premium does not coincide with unobserved shocks that affect nonbanks' propensity to deny FHA borrowers over conventional borrowers, relative to banks' propensity. Under this assumption, the parameter β

[14]Borrower controls are requested loan-to-income ratio, log income, and an indicator for whether the borrower is black or Hispanic.

in Equation (1) may be interpreted as the effect of the GNMA premium on nonbanks' relative FHA denial rate. Note that this effect is conditional on the various fixed effects and controls in Equation (2), which together make the assumption rather weak.[15]

We devote Section 7 to investigating the validity of Equation (2), but, as a first pass, Figure 6 inspects pre-trends by plotting the relative denial rate on FHA loans over conventional loans for banks and nonbanks over time. The relative denial rates for the two groups of lenders follow parallel trends leading up the introduction of the LCR, after which nonbank denial rates on FHA loans fall. This observation suggests that Equation (2) is not invalid because of a pre-trend.

Table 2 contains the results from estimating Equation (1) over the 2010–2015 period.[16] In the first column, we find that nonbanks are 2 pp less likely to deny an FHA loan over a conventional loan in the post-LCR period, relative to banks. To make the channel more precise, the second column implies that the increase in the FNMA-GNMA spread due to the introduction of the LCR lowers nonbanks' relative denial rate by 0.8 pp. We obtain a similar result when considering the FHLMC-GNMA spread in the third column. All results are significant at the 1% level, per the p-values that we report in parentheses.

Collectively, these results imply that higher GNMA prices due to the introduction of the LCR lower nonbanks' relative FHA denial rates by 1–2 pp, or roughly 40% of the difference between the unconditional FHA denial rate (i.e., 13.8%) and conventional denial rate (i.e., 10%). The next section traces this effect through to nonbanks' market share.

[15]The fixed effects $\alpha_{l,t}$, $\alpha_{s,t}$, and $\alpha_{m(i),t}$ subsume the direct effect of $Premium_t$.

[16]We cluster standard errors by lender-year bins and report p-values in parentheses.

4 Effect on Market Share

We estimate the effect of the LCR-induced increase in GNMA prices on nonbanks' market share through a zip-code-level regression. Then, we use our zip-code-level estimates and methods from the applied macroeconomics literature (e.g., Chodorow-Reich 2014) to calculate nonbanks' counterfactual aggregate market share in the absence of the LCR shock.

4.1 Zip-code-level specification

We estimate the effect of the LCR shock on nonbanks' market share by aggregating to the ZIP code level. ZIP codes contain roughly 8,000 people, and so they are still granular enough to control for confounding geographic effects, yet large enough to capture alternative margins through which nonbanks increase credit supply, besides lowering denial rates (e.g., larger loans). We then estimate the following cross-sectional regression equation:

$$\Delta \mathit{Nonbank\ market\ share}_z = \beta \left(\mathit{FHA\ app\ share}_z \times \mathit{Nonbank\ app\ share}_z \right) + \ldots \qquad (3)$$

$$\ldots + \gamma X_z + \alpha_{c(z)} + u_z,$$

where $\Delta \mathit{Nonbank\ market\ share}_z$ is a measure of the 2013–2015 change in the share of the mortgage volume that is originated by nonbanks, which can be defined in terms of either FHA market share or *overall* market share; $\mathit{FHA\ app\ share}_z$ is the 2013 share of mortgage applications for FHA loans; $\mathit{Nonbank\ app\ share}_z$ is the 2013 share of FHA applications to nonbanks; and $\alpha_{c(z)}$ is a county fixed effect, where the notation $c(z)$ denotes the county to which ZIP code z belongs.

All specifications control for $\textit{FHA app share}_z$ and $\textit{Nonbank app share}_z$, as standard.[17]

Under an identification assumption discussed shortly, the parameter β in Equation (3) represents the effect of the LCR shock on nonbanks' market share in ZIP codes marginally more exposed to this shock. Building on the core analysis from Section 3, more-exposed ZIP codes are those where borrowers historically sought a large share of their mortgage credit (a) in the form of FHA loans and (b) from nonbanks. As standard, we control for both the initial FHA application share ($\textit{FHA app share}_z$) and the nonbank share ($\textit{Nonbank app share}_z$), which account for features of FHA-prevalent or non-bank-prevalent markets that correlate with changes in nonbanks' market share. Moreover, the county fixed effect $\alpha_{c(z)}$ limits variation to within the same county, which accounts for changes in nonbanks' market share due to county-level unobservables, such as ease of construction (e.g., Saiz 2010). We identify the effect of the LCR-induced increase in MBS prices on nonbanks' market share (i.e., β) using the previously documented finding that nonbanks loosened standards among FHA loans.

For the parameter β in Equation (3) to recover the effect of the LCR shock on nonbanks' market share, we make an identification assumption similar to Equation (2),

$$0 = \mathbb{E}\left[\textit{FHA app share}_z \times \textit{Nonbank app share}_z \times u_z \middle| X_z, \alpha_{c(z)}\right]. \tag{4}$$

In words, we assume that the treatment exposure variable, given by the product of FHA and nonbank shares in 2013, is conditionally uncorrelated with unobserved drivers of nonbanks'

[17]Additional ZIP-code-level controls are the 2013–2015 changes in the average requested loan-to-income ratio; share of applications from black or Hispanic borrowers; and the average applicant's log income. We weight ZIP codes in Equation (3) by 2013 mortgage origination volume so that we can use the point estimates to calculate an aggregate market share, as described in Section 4.2, and we control for nonbanks' share of origination volume in 2013.

market share over 2013–2015, which are subsumed by the residual term u_z. Internet Appendix Table A3 shows how the treatment exposure variable is indeed conditionally uncorrelated with various well-known drivers, such as changes in the capital adequacy of local banks, growth in mortgage applications from minorities, or changes in the average applicant's loan-to-income ratio, a proxy for credit risk. This lack of correlation supports the validity of the assumption in Equation (4).

The results in columns 1 and 2 of Table 3 show how ZIP codes more exposed to nonbanks' expansion in the FHA market see a significant increase in nonbanks' *overall* mortgage market share. Quantitatively, the point estimate implies that a 1 standard deviation increase in the treatment exposure variable increases the growth rate in nonbanks' market share by 40% of its average value. We supplement this calculation with a more formal aggregation exercise in the next subsection, which accounts for the fact that we obtain identification from the cross-section and therefore need a more precise definition of treated and control ZIP codes. That the results are similar after including additional controls in column 2 suggests a relatively small scope for bias based on unobservables. We verify this conjecture through an Oster (2017) correction.[18] In columns 3 and 4, we study nonbanks' FHA market share and obtain substantially larger point estimates, as expected.

Finally, Internet Appendix Table A4 complements Table 3 by showing how the sum of bank and non-bank-intermediated mortgage origination volume is significantly higher in ZIP codes more exposed to the LCR shock. Therefore, the increase in nonbanks' market share documented in Table 3 comes from an increase in overall credit supply, rather than a substitution between

[18] Oster (2017) proposes an additive correction to the point estimate that depends on the difference in R^2 between specifications with and without control variables. The corrected point estimate in column 2 is 0.15, based on a maximum R^2 parameter of .60.

bank and nonbank lending.

Collectively, the results in Table 3 imply that the LCR shock increases nonbanks' market share, both overall and within the FHA market. In the next subsection, we use these results to calculate a counterfactual growth rate in nonbanks' market share absent the LCR shock.

4.2 Counterfactual aggregate market share

Using the estimates from Table 3, we perform an aggregation exercise to calculate nonbanks' counterfactual market share in the absence of the LCR-induced increase in GNMA prices. In particular, we ask what share of nonbanks' observed growth in market share over 2013–2015 can be attributed to the LCR shock, which we denote by η. To make progress on this question, we perform a similar exercise as Chodorow-Reich (2014), which, in our setting, requires two additional assumptions. Both assumptions reflect the fact that we obtain identification from the cross-section.

Assumption 1 (Control group) The LCR-induced increase in MBS prices does not affect the 2013–2015 change in the share of mortgage volume that is originated by nonbanks in ZIP codes where nonbanks' FHA application share in 2013 was less than or equal to the *Bth* percentile across ZIP codes. Therefore, the effect of the LCR shock on ZIP code z is

$$\beta_z = \beta \times FHA\ app\ share_z \times ... \tag{5}$$
$$... \times \max\left\{Nonbank\ app\ share_z - P_B\left(Nonbank\ app\ share_z\right), 0\right\},$$

where $P_B\left(Nonbank\ app\ share_z\right)$ denotes the B^{th} percentile of $Nonbank\ app\ share_z$ across ZIP codes.

We introduce Assumption 1 because we have a continuous measure of treatment exposure, and we therefore need to define some minimum threshold for this measure below which a ZIP code is considered unexposed to the shock (i.e., in the "control group"). Assumption 1 defines the control group as ZIP codes where nonbanks' share of FHA applications in 2013 was less than the B^{th} percentile across ZIP codes. This assumption is conservative, since nonbanks have an incentive to respond to LCR regulation by entering markets where they have historically received a small share of applications. For example, even ZIP codes in the bottom centile of $\textit{Nonbank app share}_z$ (i.e., $B = 0.01$) saw growth in nonbanks' overall market share of 4.6 pp over 2013–2015. Nevertheless, we find similar results for various definitions of the control group, as parameterized by B, and we report results for B up to 0.10.

Assumption 2 (Partial equilibrium) The effect of the LCR-induced increase in MBS prices on the aggregate 2013–2015 change in the share of mortgage volume that is originated by nonbanks is equal to the average of ZIP-code-level effects, β_z, weighting by the size of each ZIP code's mortgage market in dollars in 2013, w_z. In particular, the share of nonbanks' observed growth in market share that is due to the LCR shock is

$$\eta = \frac{\sum_z \beta_z \times w_z}{\sum_z \Delta \textit{Nonbank market share}_z \times w_z}. \tag{6}$$

Table 4 summarizes the results of this aggregation exercise. Each row considers a separate definition of control group (i.e., B), per Assumption 1. Focusing on the first row of the table, our baseline estimates and set of assumptions imply that 22% of nonbanks' observed growth in overall market share over 2013–2015 is due to the LCR shock. To provide some context, nonbanks' market share grew by 5.4 pp over 2013–2015, so that 1.2 pp is attributable to the shock (i.e., $1.2 = 5.4 \times 0.22$). The results are of a similar magnitude under alternative assumptions about the control group, shown in the second row. Turning to the rightmost column of the table, the LCR shock accounts for between 37% and 48% of nonbanks' observed growth in FHA market share. Per Assumption 2, all of the results in Table 4 should be interpreted as partial equilibrium effects.[19]

In summary, nonbanks' relaxation of lending standards in response to the LCR-induced increase in GNMA prices increases their share of the FHA market and, consequently, of the overall mortgage market. This increase in the size of the shadow banking system has implications for financial stability, to which we turn in the next section.

5 Implications for Financial Stability

The LCR-induced increase in nonbanks' market share affects financial stability through two channels: credit risk and funding fragility. First, nonbanks' relaxation of lending standards among FHA borrowers (i.e., lower denial rates) increases aggregate credit risk. This credit risk is borne by the U.S. government, which insures FHA loans. Second, the increase in nonbanks'

[19]We introduce Assumption 2 because the LCR shock affects nonbanks' market share through general equilibrium forces, but these forces are subsumed by the fixed effects in Equation (3). It is theoretically unclear how accounting for general equilibrium forces would affect our results. In one direction, the increase in homeownership rates documented in Section 6 may raise starter house prices within a county. This force would discourage would-be FHA borrowers from applying for a loan, thus dampening nonbanks' growth in market share across ZIP codes within the county. We cannot identify this channel because it is captured by the county fixed effect, leading us to overstate the effect of the LCR shock on nonbanks' overall market share. In the opposite direction, the reduction in interest rates documented in Section 7.8 would encourage borrowers to seek FHA credit. By the same logic used in the previous example, this channel would lead us to understate the effect of the LCR shock. Since it is beyond the scope of this paper to quantify general equilibrium forces, we follow the custom in the applied macroeconomics literature and present our results as partial equilibrium effects.

overall mortgage market share increases average funding fragility, since, as mentioned in Section 1, nonbanks rely on short-term funding arrangements which are more prone to runs than traditional deposits. This section directly studies implications of the LCR regulatory spillover for credit risk and funding fragility.

5.1 Credit risk

Based on the logic of standard credit rationing models, lower denial rates correspond to an increase in average credit risk because riskier borrowers enter the market. In our setting, higher credit risk ex ante can also lead to higher default costs ex post because securitization reduces the ability of distressed borrowers to renegotiate their loan terms (e.g., Piskorski, Seru, and Vig 2010).

We estimate the effect of the LCR-induced increase in nonbanks' credit supply on credit risk at the ZIP code level. Mirroring Equation (3), we estimate

$$\Delta Y_z = \beta\,(FHA\ app\ share_z \times Nonbank\ app\ share_z) + ... \tag{7}$$

$$... + \gamma X_z + \alpha_{c(z)} + u_z,$$

where the outcome ΔY_z is a measure of the 2013–2015 change in a measure of credit risk. The two outcomes we study are the 2013–2015 change in the FHA mortgage application denial rate; and the 2013–2015 change in the 30+ day mortgage delinquency rate for the surrounding county, since default rates are only observed at the county level as described in Internet Appendix A.

Table 5 reports the results from estimating equation (7), and it reveals an increase in credit risk in ZIP codes more exposed to the LCR shock. The estimates in columns 1 and 2 imply that more-exposed ZIP codes experience a significant reduction in FHA mortgage denial rates over 2013–2015. To assess the broader impact of this finding, we next replace the outcome variable with the 2013–2015 change in the mortgage delinquency rate across all loan types. The corresponding results in columns 3 and 4 show how more-exposed ZIP codes also experience an overall increase in mortgage default.

Nonbanks' relaxed lending standards affect default rates through two channels: an increase in the size of the FHA market, and an increase in the riskiness of FHA borrowers. First, FHA loans are riskier by design, since they are targeted toward first-time homeowners. For example, in 2017 the delinquency rate on FHA loans was 3.8 times the conventional delinquency rate (Urban Institute 2017). Therefore, the increase in the relative supply of FHA credit increases overall credit risk, even if the riskiness of FHA borrowers does not change.

Second, a reduction in FHA denial rates ushers in riskier borrowers into the FHA market, thereby raising credit risk among FHA loans. For example, Figure 7 shows how the median relative FICO score on non-bank-originated FHA loans tracks that of bank-originated loans until the introduction of the LCR, after which the FICO score on non-bank-originated loans falls substantially.[20] By contrast, the median FICO scores on bank and non-bank-originated loans in the conventional market track each other closely throughout the observation window, as shown in Internet Appendix Figure A4. To substantiate this point further, Internet Appendix Table A5 shows that nonbanks lower denial rates by an additional 25% (0.3 pp) for FHA

[20]Our raw data only begin in August 2013, and so in Internet Appendix Figures A3 and A4 we extend the time series using a standard imputation procedure described in Internet Appendix A. The result confirms the parallel trends intuition of Figure 7.

borrowers with a 1 standard deviation higher loan-to-income ratio, a proxy for credit risk.[21]

5.2 Funding fragility

A number of papers have highlighted how nonbanks' reliance on short-term, uninsured warehouse credit makes them more prone to run-like withdrawals, such as those seen during the 2008 Financial Crisis (e.g., Kim et al. 2018; Drechsler, Savov, and Schnbal 2019; D'Avernas, Vandeweyer, and Pariès 2020; Hanson et al. 2015). By contrast, banks can fund mortgage originations through deposits, which are more stable during periods of financial crisis. Building on this literature, we assess how the LCR shock affects funding fragility by reestimating the ZIP-code-level regression Equation (3) and replacing the outcome variable with the 2013–2015 change in the average lender's noncore funding ratio. Explicitly, we estimate

$$\Delta \textit{Noncore funding ratio}_z = \beta \left(\textit{FHA app share}_z \times \textit{Nonbank app share}_z \right) + ... \tag{8}$$

$$... + \gamma X_z + \alpha_{c(z)} + u_z .$$

The results in Internet Appendix Table A6 imply that ZIP codes more exposed to the LCR shock experience an increase in funding fragility, as proxied for by the average noncore funding ratio.

[21] Explicitly, the table estimates a variant of the difference-in-differences equation introduced in Section 7.4 after interacting the treatment variable with the average requested loan-to-income ratio (LTI) in the applicant's MSA of residence. While FHA borrowers are subject to debt-to-income ceilings, lenders can increase this ceiling by invoking "compensating factors," such as cash reserves or residual income.

5.3 Discussion

The previous two sets of results suggest that the LCR-induced increase in nonbanks' market share reduces financial stability. However, we lack a sufficiently long time series to directly test this prediction, and nonbanks' growth might improve financial stability through margins that we cannot test empirically. For example, risk may be more dispersed in a financial system with both banks and nonbanks.[22] In addition, recent work by Jiang et al. (2020) suggests that nonbank mortgage lenders are better capitalized than their bank counterparts. Outside the mortgage market, Bernstein, Lerner, and Mezzanotti (2019) argue that private equity improves financial stability. Therefore, the overall welfare content of our results is unclear, as we discuss in our conclusion.

6 Implications for Homeownership

By the same token that the LCR shock increases credit risk, it may also enable borrowers constrained by credit frictions to become homeowners. Most of our analysis occurs in the context of the FHA market, which caters to households on the margin of homeownership. Thus, it is natural to ask whether the relaxation in nonbanks' lending standards affects homeownership rates.

We estimate a regression equation of the same form as equation (7), after replacing the outcome variable with the 2013–2015 change in the ZIP code's homeownership rate. Explicitly,

[22] We thank an anonymous referee for bringing this possibility to our attention.

we estimate

$$\Delta Homeownership\ rate_z = \beta\left(FHA\ app\ share_z \times Nonbank\ app\ share_z\right) + ... \tag{9}$$

$$... + \gamma X_z + \alpha_{c(z)} + u_z.$$

The results in Table 6 imply that ZIP codes more exposed to nonbanks' increase in credit supply see significantly higher growth in homeownership. Quantitatively, the point estimate implies that a one-standard-deviation increase in the treatment exposure variable leads to a 4.2-pp higher homeownership rate in 2015. This effect is substantial given that national homeownership rates fell 5.3 pp between 2004 and 2015, according to the Census Bureau's HVS survey (2019).

7 Robustness

In this section, we perform a variety of robustness tests to evaluate our primary identification assumptions, the exclusion restrictions in equations (2) and (4). The results of these tests support the validity of the assumptions.

To provide a brief outline, we check that the key mechanism is reliance on an originate-to-securitize funding model (7.1); explicitly control for regulatory arbitrage incentives (7.2); study how the shock affects the size of non-bank-originated loans (7.3); test for crowding-out effects among non-FHA loans (7.4); perform a placebo test to evaluate the role of pre-trends in our ZIP-code-level and loan-level analyses (7.5); perform a placebo test in the conventional loan market (7.6); reperform our analysis after excluding large lenders and nonbanks to check that litigation risk does not drive the results (7.7); study interest rates on FHA loans (7.8); demonstrate robustness to the effects of quantitative easing (7.9) or the net stable funding ratio (7.10); reperform the analysis using the option-adjusted spread to account for changes in prepayment risk (7.11); test the mechanism over the precrisis period (7.12); and check that the results are robust to a monthly frequency (7.13).

7.1 Testing the mechanism

The mechanism through which MBS prices increase nonbanks' lending is their originate-to-securitize funding model: nonbanks do not have access to stable deposit funding, and so their lending capacity is more dependent on demand from MBS investors. Consequently, nonbanks' lending behavior responds more elastically to MBS prices than banks'. This conjecture motivates us to estimate a more general variant of equation (1),

$$Denial_{i,l,s,t} = \beta\left(F_l \times Premium_t \times FHA_s\right) + \gamma X_{i,t} + \alpha_{l,t} + \alpha_{m(i),t} + \alpha_{m(i),l} + ... \tag{10}$$
$$... + \alpha_{s,t} + \alpha_{s,l} + u_{i,l,s,t},$$

where F_l is a measure of lender l's reliance on an originate-to-securitize funding model, in contrast to a deposit funding model. Our first measure is the lender's ratio of securitized loans to total originations in 2010, which we call the lender's "securitization rate." This variable is meant to proxy for technological specialization in an originate-to-distribute model, which might arise from a lack of access to stable deposit funding, as discussed in Section 1. Our second measure, called "noncore funding," is one minus the ratio of total deposits to total assets in 2010. By definition, nonbanks have noncore funding equal to one. Internet Appendix Table A1 summarizes the distribution of these two measures and other lender-level variables. Note, in particular, the substantial variation in banks' securitization rates, so that Equation (10) can be estimated among the subsample of bank lenders, as we do in subsequent tests.[23]

Table 7 contains the results of the more general equation in (10). The estimates in the first column suggest that lenders who rely entirely on securitization respond to the LCR-induced GNMA premium by denying 1.2 pp fewer FHA loan applicants than lenders who do not securitize. We obtain a similar result in terms of noncore funding in the rightmost two columns. Collectively, these findings support our theory by showing how the baseline effect works through variation in lenders' funding models.

7.2 Regulatory arbitrage

As documented by Buchak et al. (2018), regulatory arbitrage has been a key driver of nonbanks' increasing market share. Thus, our baseline analysis may capture differential costs of regulation across lenders rather than a response to LCR-induced changes in MBS prices. To evaluate this possibility, we reestimate our more general triple difference-in-differences regression equation (10) on the set of bank lenders after including the triple interaction between $Premium_t$, FHA_s, and an indicator for whether the bank has a high regulatory arbitrage incentive. The additional triple interaction term captures changes in relative denial rates stemming

[23]The securitization rate does not equal one for all nonbanks for two reasons. The principal reason is that our core HMDA data only record a loan as securitized if it was sold within the same calendar year, per Internet Appendix A. Since it takes around 50 days to securitize a mortgage (Echeverry, Stanton, and Wallace 2016), the observed securitization rate understates the true securitization rate. However, because our identification comes from the cross-section, this measurement error does not produce biased estimates and, if anything, leads to attenuation bias toward zero. Second, a small minority (i.e., 5%) of nonbanks' originations are booked as held-for-investment mortgages, which are funded by long-term debt (Mortgage Bankers' Association 2014).

from the possibility that FHA loans are more capital-intensive, subject to greater putback risk, or otherwise less attractive to originate for risk averse lenders seeking to minimize their regulatory exposure.

Our first measure of a high regulatory arbitrage incentive is an indicator for whether l's capital ratio in 2010 is below the median across banks. In principle, such banks should raise more equity capital to satisfy Basel III regulatory capital requirements. In practice, they often respond to these requirements by reducing their holdings of relatively risky assets (e.g., Buchak et al. 2018), which, in our setting, would correspond to an increase in the relative denial rate on FHA loans. We also use an indicator for whether the change in l's capital ratio is below the median, which captures the bank's deterioration in capital. Finally, we use an indicator for whether l's ratio of mortgage servicing rights (MSRs) to equity in 2010 is above the median across banks. Since regulators penalize MSRs with a high regulatory risk weight, such banks also have an incentive to reduce their holdings of relatively risky assets, as pointed out by Buchak et al. (2018) who use an analogous MSR-based measure. Internet Appendix Table A1 summarizes the distribution of the capital and MSR ratios.

Table 8 contains the results of this test. Across all columns, the estimated coefficients of interest are similar to their analogs from Table 7. In columns 2–4, we assess whether our measures of a high regulatory arbitrage incentive simply proxy for bank size by including an additional triple interaction between $Premium_t$, FHA_s, and an indicator for whether the bank's assets are above the median. We continue to obtain similar estimates for the coefficient of interest.[24]

[24]We measure the capital ratio using the ratio of total equity to total assets (i.e., economic capital ratio), because we observe this statistic for a larger share of the sample than the ratio of tier 1 equity to net risk-weighted assets (i.e., regulatory capital ratio). However, in untabulated results, we obtain a significant point

The regulatory arbitrage hypothesis begins with the idea of time variation in regulatory burdens. If $Premium_t$ covaries with such regulatory burdens in the time series, then the regulatory arbitrage hypothesis would predict a positive coefficient on the triple interaction term associated with regulatory arbitrage incentives. Consistent with this logic, we estimate a positive and significant coefficient on the measures of a high regulatory arbitrage incentive, even after accounting for the possibility that these measures proxy for bank size.

In Internet Appendix Table A7, we perform a similar exercise at the ZIP code level. Explicitly, we reestimate Equation (3) after controlling for various measures of local banks' regulatory arbitrage incentives, which we aggregate to the ZIP-code-level weighting by the bank's mortgage origination volume in the ZIP code. As in Section 4, ZIP codes more exposed to the LCR shock experience significant growth in nonbanks' mortgage market share, and the magnitude of the effect is almost the same as those in Table 3.

Collectively, the results of this robustness test provide strong evidence that variation in funding models, rather than other differences among lenders, is the key mechanism through which higher MBS prices lower nonbanks' lending standards and increase their market share. These findings also suggest that nonbanks' growth has multiple roots, including both a retreat of bank lending because of regulatory arbitrage and an expansion of nonbank lending because of the LCR-induced increase in MBS prices.

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estimate of -0.016 for the coefficient of interest when using the regulatory capital ratio.

7.3 Loan volume

We study denial rates as our primary loan-level outcome given our interest in lending standards. However, lower denial rates may not increase nonbanks' market share if nonbanks compensate by originating smaller loans. We study the effect of the LCR-induced increase in the GNMA premium on the size of originated loans in Table 9. The positive point estimates imply that nonbanks respond to the GNMA premium by increasing the relative size of FHA loans that they originate. This expansion in the intensive margin of credit amplifies our baseline findings on the extensive margin, and it helps explain the large effect on market share documented in Tables 3 and 4.

7.4 Crowding-out effects

Nonbanks may respond to higher GNMA prices by reallocating internal funds away from other forms of credit to FHA loans, leading to crowding out of non-FHA credit. Note that such a reallocation must occur within the mortgage market, since nonbanks are highly specialized lenders, and, in contrast to banks, there are no relevant nonbanks in both the mortgage and corporate credit markets. That said, if nonbanks respond to an increase in the relative MBS price of FHA loans by originating fewer conventional loans, then it is unclear why their market share should increase.

We evaluate the scope for reallocation through the following difference-in-differences regression equation,

$$Denial_{i,l,t} = \beta \left(F_l \times Premium_t \right) + \gamma X_{i,t} + \alpha_{m(i),t} + \alpha_{m(i),l} + u_{i,l,t}, \tag{11}$$

which we estimate on the subsample of either FHA or conventional loans.[25] As in equation (10), F_l is a measure of lender l's reliance on an originate-to-securitize funding model, in contrast to a deposit funding model.

Columns 1 and 2 of Table 10 contain the results from estimating equation (11) on the subsample of FHA loans. Consistent with our baseline results in Tables 2 and 7, we find that nonbanks and, more generally, originate-to-securitize lenders deny fewer FHA borrowers in response to the LCR-induced increase in the GNMA premium. However, we find no effect when restricting the sample to conventional loans in columns 3 and 4. This lack of tightening in the conventional market suggests that nonbanks respond to higher MBS prices for FHA loans by obtaining warehouse credit to originate more of these loans, rather than by reallocating internal funds away from other forms of credit.

Collectively, the results in Table 10 imply that nonbanks do not reallocate internal funds away from conventional borrowers to FHA borrowers, and so the large estimated effect of the LCR shock on nonbanks' market share is plausible. However, this finding does not imply that reallocation does not occur, but, rather, that nonbank lenders do not reallocate. In Internet Appendix Table A8, we find that poorly capitalized banks who rely on securitization do indeed tighten the supply of credit for conventional borrowers following the increase in

[25]Our baseline triple difference-in-differences analysis is uninformative about whether nonbanks reduce their supply of conventional loans, since, for the sake of internal validity, it obtains identification from the difference between FHA and conventional loans.

the GNMA premium. This apparent reallocation within the set of bank lenders is consistent with Chakraborty, Goldstein, and MacKinlay (2018, 2020), and it may stem from the fact that banks face overall leverage constraints (e.g., capital requirements), whereas nonbanks face loan-by-loan leverage constraints (e.g., repo haircuts).

7.5 Placebo test over the pre-LCR period

The treatment exposure measure in our ZIP-code-level analysis is based on the product of initial exposure to the FHA market and to nonbanks. We evaluate the measure's validity by performing a placebo test over the 2011–2013 period, which precedes the LCR shock but follows the post-crisis regulatory overhaul. If the measure is valid, it should not explain growth in nonbanks' market share over this period. We test this hypothesis by reestimating Equation (3) over 2011–2013. The results in columns 1 and 2 of Table 11 show that the measure of exposure cannot explain nonbanks' growth in market share over 2011–2013. However, consistent with the validity of assumption (4), it explains a large share of nonbanks' growth in market share over 2013–2015, as we showed in Table 3.

Likewise, we evaluate our loan-level results by reestimating equation (1) and its more general variant, equation (10), over the 2011–2013 period. If the main results indeed stem from the LCR-induced increase in GNMA prices, the results should be insignificant when replacing $Premium_t$ with a linear time trend.[26] Consistent with this this prediction, the point estimates in columns 3 and 4 of Table 11 are insignificant. Together, the results in Table 11 suggest that the baseline ZIP code and loan-level estimates are not biased because of a pre-trend in

[26] We remove any effect of MBS prices by residualizing the trend against the FNMA spread.

nonbanks' market share.

7.6 Placebo test on conventional loans

Similar in spirit to the previous test, we assess the robustness of the ZIP-code-level results through a placebo test on conventional loans over the post-LCR period. Since conventional loans represent our control loan type, as substantiated by columns 3 and 4 of Table 10, we should not expect to see an effect on nonbanks' market share or default rates among such loans. We test this prediction by reestimating equation (7) after replacing the outcome with ZIP-code-level variables that describe the conventional loan market. The results in columns 1 and 2 of Internet Appendix Table A9 show how more-exposed ZIP codes do not experience an increase in nonbanks' conventional market share. Such ZIP codes do not experience an increase in default rates on conventional loans, as shown in columns 3 and 4. This finding suggests that the LCR-induced increase in GNMA prices is the relevant mechanism behind the main results in Tables 3 and 5, rather than increased risk-taking by nonbanks across the board.

7.7 Litigation risk by lender size

Beginning with a 2011 suit against Deutsche Bank, the U.S. Department of Justice sued a number of large banks over 2011–2015, alleging that their FHA lending behavior violated the False Claims Act. To the extent that an increase in expected litigation activity coincided with the introduction of the LCR, the baseline results may reflect heightened legal risk rather than a higher GNMA premium. However, litigation risk is an unlikely source of bias for two reasons. First, large nonbank lenders, such as Quicken Loans, were also subject to lawsuits related to their lending in FHA markets. Second, the Department of Justice also sued large lenders over their behavior in conventional mortgage markets.[27] Thus, if litigation risk is a significant source of bias, one would expect to see similar results among conventional loans. However, as discussed in Section 7.4, we find no evidence of relaxed standards in the conventional market.

To more directly address bias from large lenders' litigation risk, we reestimate our baseline specification on the set of lenders with less than 2% of the total mortgage market in 2010, measured by origination share. The results in columns 1 and 2 of Internet Appendix Table A10 imply that, within this restricted subsample, nonbanks respond to the increase in the GNMA premium by lowering their relative denial rate on FHA loans. This finding suggests that our baseline result is not driven by differences in litigation risk between large and small lenders.

In columns 3 and 4 of Internet Appendix Table A10, we perform a similar exercise after dropping nonbanks from the sample, and, consistent with our results in Table 7, we find that banks with an originate-to-securitize funding model respond to a higher GNMA premium by lowering their relative denial rate on FHA loans. This finding confirms the key mechanism: MBS prices disproportionately increase nonbanks' credit supply because nonbanks rely on an originate-to-securitize funding model, rather than a deposit funding model.

7.8 Interest rates

We now study how higher GNMA prices affect the relative interest rate charged by nonbanks on FHA loans, using data from HUD's FHA Single Family Portfolio Snap Shot. To do

[27]For example, in 2012, the Department of Justice alleged that Bank of America violated the Financial Institutions Reform, Recovery, and Enforcement Act of 1989 by selling low-quality loans to Fannie Mae and Freddie Mac.

so, we estimate a similar equation as equation (11) over the 2012–2015 period,

$$Rate_{i,l,t} = \beta \left(Nonbank_l \times Premium_t \right) + \gamma Z_{i,t} + \alpha_{m(i),t} + \alpha_{m(i),l} + u_{i,l,t}, \tag{12}$$

where i, l, and t index borrowers, lenders, and months; each observation is an originated loan; and $Rate_{i,l,t}$ is the interest rate on the loan. Unlike in our baseline analysis, we do not normalize $Premium_t$ by the implied effect of the LCR shock, since our outcome variable is now an interest rate. The controls in $Z_{i,t}$ are log loan size and an indicator for whether the loan is a fixed-rate mortgage. The remaining notation is the same as that used in prior equations.[28]

Mortgage interest rates typically fall when the GNMA premium rises, measured using either total return or option-adjusted spreads. Thus, the parameter β captures nonbanks' rate of pass-through from higher MBS prices to lower mortgage rates, relative to banks' rate of pass-through. The first two columns of Internet Appendix Table A11 show that nonbanks' rate of pass-through is 5 pp greater than banks'. To place this number in perspective, the unconditional pass-through of the FNMA-GNMA spread to mortgage interest rates is 30%, so that nonbanks have a 17% (i.e., 0.05/0.30) higher pass-through rate. The rightmost columns obtain a similar result when using the option-adjusted spread to measure $Premium_t$.

The results from this exercise imply that nonbanks disproportionately lower interest rates following an increase in MBS prices. In fact, this finding understates the true reduction in the price of credit, since we have already seen that nonbanks also originate riskier loans, which would tend to increase interest rates.

[28] We classify lenders as nonbanks if their parent company's name does not contain "Bank," "Credit Union," or a variant spelling of these terms.

7.9 Quantitative easing

The third round of MBS purchases by the Fed overlapped with the introduction of the LCR, as it lasted from 2012 to 2014. The Fed bought MBS sponsored by the GSEs (i.e., FNMA and FHLMC) and by GNMA, with a tilt toward GSE MBS per the report by the Board of Governors (2016). In particular, Internet Appendix Figure A5 shows that the ratio of the Fed's purchases was weighted against GNMA MBS. Therefore, assuming the price elasticity of demand is the same for both GNMA MBS and GSE-backed MBS, these purchases are unlikely to account for the increase in the GNMA premium and nonbanks' substitution toward FHA lending.[29]

7.10 Net stable funding ratio

The Basel III accords involved not only a LCR but also a complementary net stable funding ratio (NSFR). The NSFR aims to ensure that banks "maintain sufficient levels of stable funding, thereby reducing liquidity risk in the banking system." However, the NSFR was not proposed in the United States until May 2016, more than 2 years after the LCR shock. Therefore, our results do not confound spillovers related to the NSFR.

7.11 Prepayment risk

In Internet Appendix Table A12, we reestimate equation (11) using the option-adjusted spread (OAS) to measure $Premium_t$ and find similar results. Since the OAS strips out changes

[29]We thank an anonymous referee for pointing out that this argument requires the price elasticity of demand to be the same.

in the prepayment risk premium, this finding suggests that the baseline results are not driven by either spurious correlation or changes in the relative prepayment risk of GNMA versus non-GNMA MBS.[30]

7.12 Testing the mechanism before the crisis

In Internet Appendix Table A13, we reestimate equation (11) and document a strong link between the GNMA premium and the supply of FHA credit by nonbanks over the 2000–2006 period, before the post-crisis regulatory overhaul. On one hand, the point estimates from the 2000–2006 period are less informative because this period lacks an exogenous source of variation in the cross-section of MBS prices. On the other hand, higher MBS prices should, in principle, affect the relative supply of credit by nonbanks in periods outside the 2010–2015 window. Indeed, the results obtained over 2000–2006 are both qualitatively and quantitatively consistent with those obtained in our baseline analysis. This similarity suggests that the baseline estimates are not biased because of spurious correlation between the LCR shock and unobserved time-series dynamics, such as an increase in incentives for regulatory arbitrage.

7.13 Monthly frequency

Our core data set, HMDA, is only available at a yearly frequency. We evaluate how this data restriction affects our results by performing a similar exercise at the monthly frequency using data from HUD's FHA Single Family Portfolio Snap Shot. The results, discussed in

[30]Option-adjusted spreads are computed by Bloomberg. For purely interpretive purposes, we normalize the FNMA and FHLMC OAS spreads by 13 bps, which is the estimated effect of LCR regulation as discussed in Section 2.1.

Section 7.8, are similar to those from our baseline analysis.

8 Conclusion

We document spillover effects from liquidity regulation, and, in so doing, we identify a novel, price-based channel through which securitization affects financial stability. In particular, we find that changes in MBS prices can significantly affect the size of the shadow banking system and the amount of credit risk in the primary mortgage market. Our empirical strategy used exogenous variation in the cross-section of MBS prices induced by the introduction of the U.S. LCR to identify the effect of MBS prices on the supply of nonbank credit. We showed that LCR regulation, designed to prevent runs in secondary mortgage markets, has inadvertently attracted nonbanks to the FHA market and lowered their lending standards. Thus, as an unintended consequence, LCR regulation has increased the market share of lenders with a fragile funding model, and it has increased the credit risk borne by U.S. taxpayers who insure FHA loans.

However, it is unclear how this unintended, LCR-induced increase in nonbanks' market share affects welfare. On one hand, the financial system may have become more unstable. On the other hand, the expansion in nonbank credit appears to have bolstered homeownership during a period when the U.S. homeownership rate has approached a historic low.

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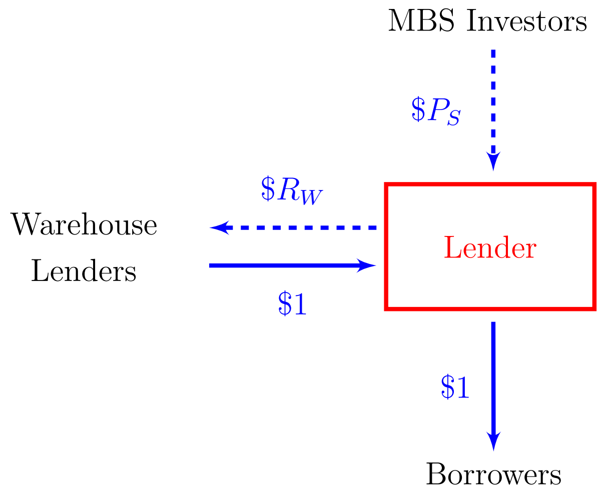
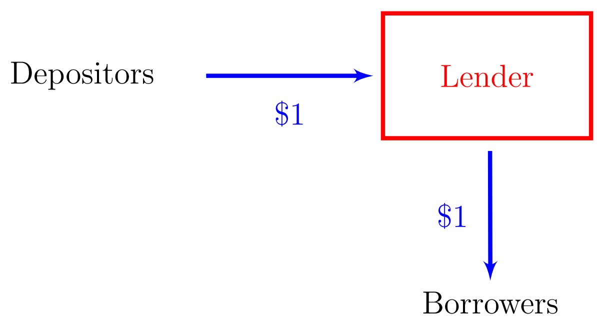
Figures and Tables



(A) Funding through securitization



(B) Funding with Deposits

Figure 1. Financing a \$1 loan by lender's funding model This figure presents a simple diagram contrasting lenders' funding models. In panel A, the lender borrows \$1 in warehouse credit to originate a \$1 loan, sells the loan to MBS investors at price P_S, and repays the warehouse lender at the gross interest rate R_W. In panel B, the lender raises \$1 in deposits to originate a \$1 loan.

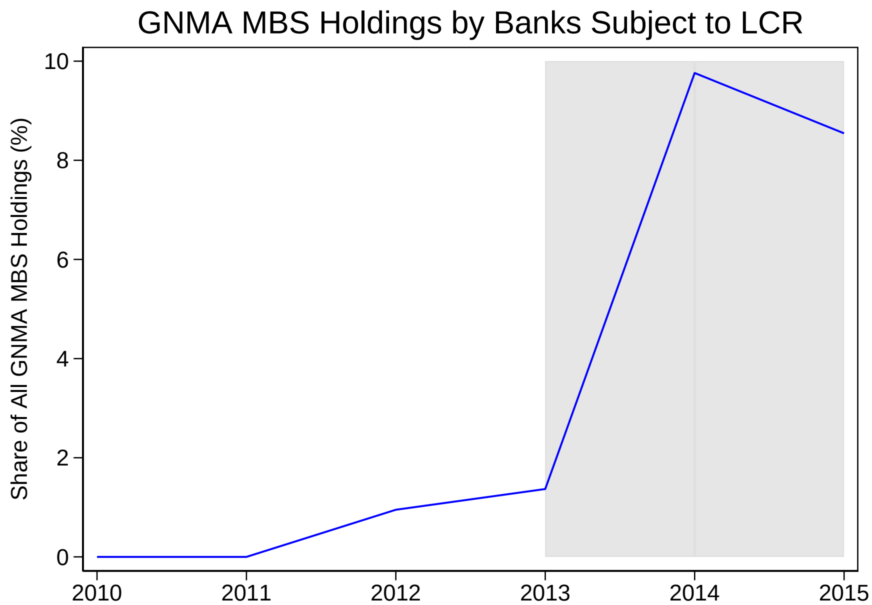


Figure 2. GNMA MBS holdings by banks subject to liquidity regulation This figure plots average Ginnie Mae (GNMA) MBS held by banks subject to the LCR policy divided by the sum of average GNMA MBS held by such banks plus average GNMA MBS held by banks not subject to the LCR. The shaded region corresponds to the period after LCR rules were proposed on October 24, 2013. Data come from the Call Reports (FRY-9C).

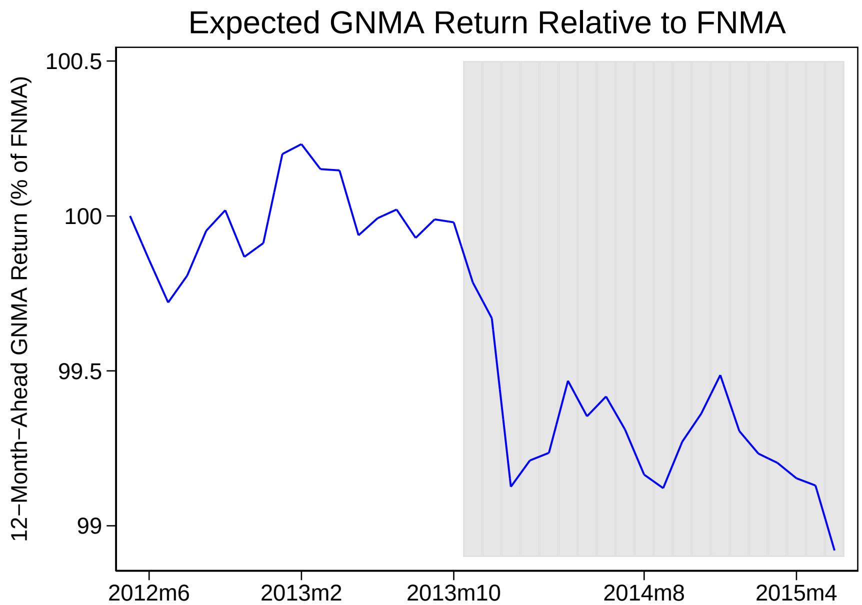


Figure 3. Expected MBS return This figure plots the ratio of the 12-month-ahead total gross return for GNMA relative to FNMA MBS, measured using the Bloomberg-Barclays Total Return Index. A drop in the relative return means that GNMA prices have increased more than FNMA prices. The shaded region corresponds to the period after LCR rules were proposed on October 24, 2013. Data come from Bloomberg.

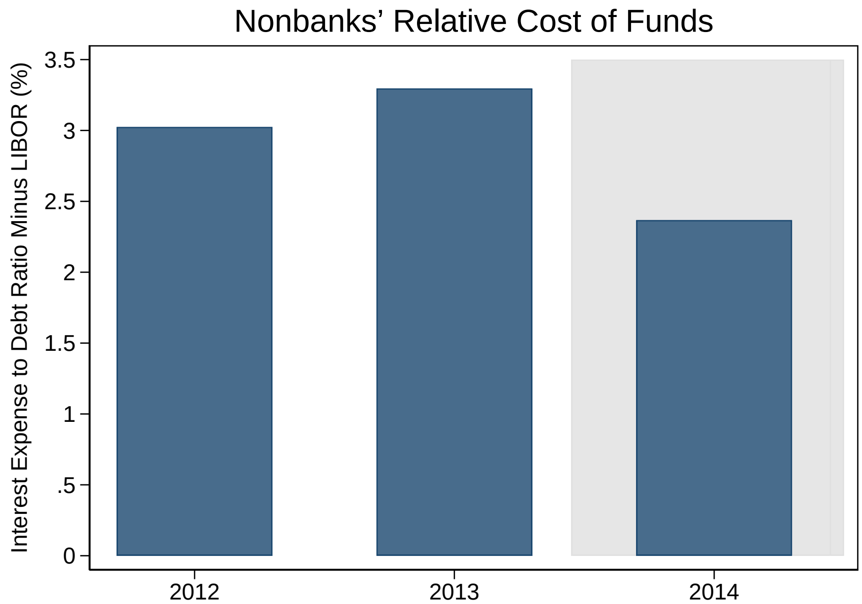


Figure 4. Nonbanks' cost of warehouse funding. This figure plots the difference between the ratio of the average nonbank's warehouse interest expense to the value of its credit lines and the 3-month LIBOR rate. The shaded region corresponds to the period after LCR rules were proposed on October 24, 2013. Data come from the Mortgage Bankers' Association (2014) and are available for the 2012–2014 period.

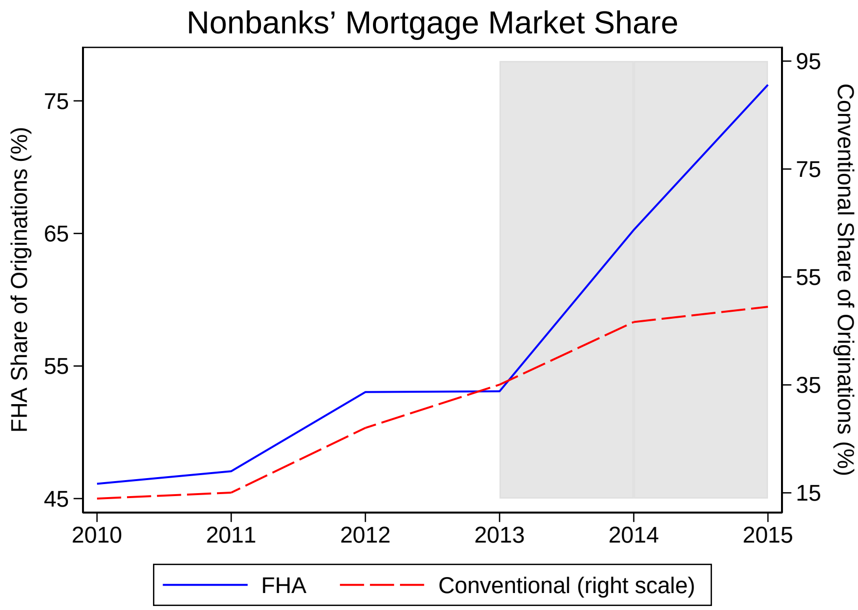


Figure 5. Nonbank market share This figure plots the share of loans originated by nonbank lenders. The blue and red curves plot this ratio for the FHA and conventional mortgage markets, respectively. The shaded region represents the period after LCR rules were proposed on October 24, 2013. Data come from Buchak et al. (2018).

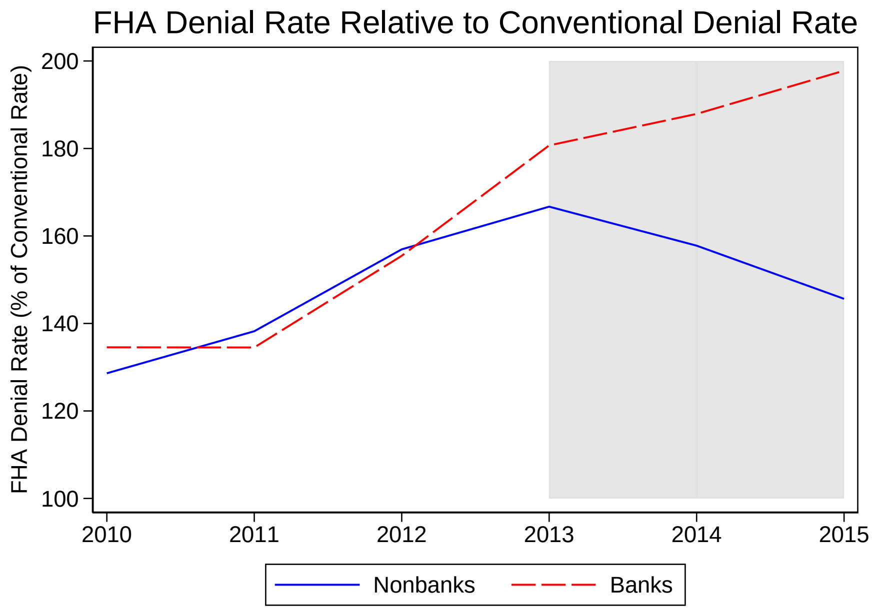


Figure 6. Relative denial rate on FHA loans for banks and nonbanks This figure plots the ratio of the average lender's denial rate on FHA loan applications to the average lender's denial rate on conventional loan applications, which assesses the scope for pre-trends in our baseline regression equation (1). The blue and red curves plot this ratio for nonbank and bank lenders, respectively. The shaded region corresponds to the period after LCR rules were proposed on October 24, 2013. Data come from HMDA.

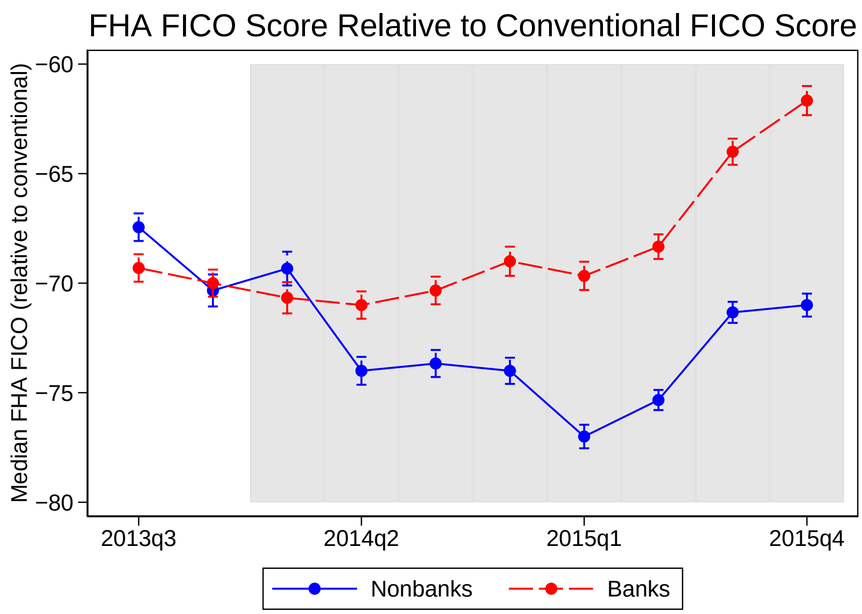


Figure 7. Relative FICO score on FHA loans for banks and nonbanks This figure plots the difference in the median lender's FICO score between FHA and conventional loan originations. The blue and red curves plot this difference for nonbank and bank lenders, respectively. Bars correspond to a 95% confidence interval. Data are from Recursion and begin in August 2013.

Table 1: Summary statistics

Variable	Number of observations	Mean	Standard deviation
Loan-level variables:			
All loans:			
Denial indicator	13,114,592	0.112	0.316
Nonbank indicator	13,114,592	0.495	0.500
FHA indicator	13,114,592	0.320	0.467
Securitization rate	10,409,953	0.828	0.263
Noncore funding ratio	10,646,461	0.723	0.351
Loan-to-income ratio	13,114,592	2.786	2.361
Minority indicator	13,114,592	0.176	0.381
FHA loans:			
Denial indicator	4,199,495	0.138	0.345
Nonbank indicator	4,199,495	0.621	0.485
Loan-to-income ratio	4,199,495	3.011	1.870
Minority indicator	4,199,495	0.303	0.460
Conventional loans:			
Denial indicator	8,915,097	0.100	0.300
Nonbank indicator	8,915,097	0.435	0.496
Loan-to-income ratio	8,915,097	2.680	2.553
Minority indicator	8,915,097	0.116	0.619
Zip-code-level variables:			
ΔNonbank market share, 2013–2015	4,506	0.054	0.125
Nonbank app share, 2013	4,545	0.695	0.186
FHA app share, 2013	4,545	0.241	0.145
Time-series variables:			
GNMA total return (pp)	16	5.012	2.739
FNMA spread (pp)	16	0.075	0.559
FHLMC spread (pp)	16	0.035	0.624

In the loan-level panels, each observation is a loan application for the purchase of an owner-occupied single-family dwelling over 2010–2015, and the variables are defined as follows: Denial indicates if the application was denied; Nonbank indicates if the lender is a nondepository institution; FHA indicates if the application is for an FHA loan; Securitization rate is the lender's ratio of securitized mortgages to total originations in 2010; Noncore funding ratio is one minus the ratio of total deposits to total assets in 2010, which equals one for nonbanks by definition; Loan-to-income is the ratio of the applicant's requested loan to her reported annual income; and Minority indicates if the applicant is black or Hispanic. In the Zip-Code-Level panel, each observation is a ZIP code weighted by 2013 origination volume, and the variables are defined as follows: Δ*Nonbank market share* is the change in the share of mortgage volume originated by nonbanks; *FHA app share*$_z$ is the share of mortgage applications for FHA loans; and *Nonbank app share*$_z$ is the share of FHA applications to nonbanks. In the time-series panel, each observation is a year over the 2000–2015 window, and the variables are defined as follows: GNMA Total Return is the average 12-month-ahead total return to Ginnie Mae (GNMA) MBS, where total returns are measured using the Bloomberg Barclays MBS Total Return indices; FNMA Spread is the difference between Fannie Mae (FNMA) Total Return and GNMA Total Return; and FHLMC Spread is analogously defined in terms of Freddie Mac (FHLMC) Total Return.

Table 2: LCR and nonbanks' lending standards

Outcome:	$Denial_{i,l,s,t}$		
$Nonbank_l \times Premium_t \times FHA_s$	-0.021	-0.008	-0.007
	(.000)	(.000)	(.000)
Premium measure	Post-LCR	FNMA spread	FHLMC spread
Lender-MSA FE	Yes	Yes	Yes
MSA-year FE	Yes	Yes	Yes
Lender-year FE	Yes	Yes	Yes
Loan type-lender FE	Yes	Yes	Yes
Loan type-year FE	Yes	Yes	Yes
Borrower controls	Yes	Yes	Yes
R-squared	.116	.116	.116
Number of observations	3,267,670	3,267,670	3,267,670

p-values appear in parentheses. This table estimates Equation (1), which is our baseline triple difference-in-differences equation. Subscripts i, l, s, and t index borrower, lender, loan type, and year, respectively. Each observation is a loan application. The regression equation takes the following form:

$$Denial_{i,l,s,t} = \beta\left(Nonbank_l \times Premium_t \times FHA_s\right) + \gamma X_{i,t} + \alpha_{l,t} + \alpha_{m(i),t} + \alpha_{m(i),l} + ...$$
$$... + \alpha_{s,t} + \alpha_{s,l} + u_{i,l,s,t},$$

where $Denial_{i,l,s,t}$ indicates if the loan application is denied; $Nonbank_l$ indicates if the lender is a nonbank; $Premium_t$ is a measure of the GNMA premium; FHA_s indicates whether the loan's type is FHA, where the possible types are FHA and Conforming non-FHA, which we call "conventional" in the text; and $\alpha_{l,t}$, $\alpha_{s,t}$, $\alpha_{s,l}$, $\alpha_{m(i),t}$, and $\alpha_{m(i),l}$ are lender-year, type-year, type-lender, MSA-year, and MSA-lender fixed effects. Each column interacts Nonbank with a different measure of the Ginnie Mae (GNMA) premium: Post-LCR indicates whether $t \geq 2014$, the first full year after LCR regulation was announced; FNMA spread is the difference in expected total return between Fannie Mae (FNMA) and GNMA MBS; and FHLMC spread is the analogous difference between Freddie Mac (FHLMC) and GNMA MBS. Expected total return is measured using the average 12-month-ahead total return in year t, where total returns are measured using the Bloomberg Barclays MBS Total Return indices. Borrower controls are requested loan-to-income ratio, log income, and an indicator for whether the borrower is black or Hispanic. The sample consists of applications for FHA or conventional loans for the purchase of an owner-occupied single-family dwelling. The sample period is 2010–2015. Standard errors are clustered by lender-year bins.

Table 3: Effect on nonbanks' share of origination volume

Outcome:	Δ*Nonbank market share*$_z$			
FHA app share$_z$ $\times$ *Nonbank app share*$_z$	0.207	0.202	0.688	0.690
	(.001)	(.002)	(.000)	(.000)
Market	All mortgages		FHA mortgages	
County FE	Yes	Yes	Yes	Yes
ZIP code controls	No	Yes	No	Yes
R-squared	.541	.545	.369	.370
Number of observations	4,069	4,025	4,069	4,025

p-values appear in parentheses. This table estimates Equation (3), which assesses how the LCR shock affects nonbanks' market share at the ZIP code level. Subscript z indexes ZIP codes. The regression equation takes the following form:

$$\Delta \textit{Nonbank market share}_z = \beta\,(\textit{FHA app share}_z \times \textit{Nonbank app share}_z) + ...$$
$$... + \gamma X_z + \alpha_{c(z)} + u_z,$$

where Δ*Nonbank market share*$_z$ is a measure of the 2013-2015 change in the share of mortgage volume that is originated by nonbanks; *FHA app share*$_z$ is the 2013 share of mortgage applications for FHA loans; *Nonbank app share*$_z$ is the 2013 share of FHA applications to nonbanks; and $\alpha_{c(z)}$ is a county fixed effect. The outcome in columns 1 and 2 is the change in nonbanks' share of all mortgage volume, and the outcome in columns 3 and 4 is the change in their share of FHA mortgage volume. All specifications control for *FHA app share*$_z$, *Nonbank app share*$_z$, and nonbanks' share of origination volume in 2013. ZIP code controls are the 2013–2015 changes in the average requested loan-to-income ratio; share of applications from black or Hispanic borrowers; and the average applicant's log income. Observations are ZIP codes weighted by 2013 mortgage origination volume.

Table 4: Share of nonbanks' growth due to the LCR shock

Market:	All mortgages	FHA mortgages
Share of growth due to shock, $B = 0.01$	22%	48%
Share of growth due to shock, $B = 0.10$	17%	37%
Source of point estimate (β):	Table 3, column 1	Table 3, column 3

This table shows the implied contribution of LCR regulation to the aggregate 2013-2015 change in the share of the mortgage volume that is originated by nonbanks, denoted by η in the text and defined in Equation (6). The implied contribution is based on Assumption 1 (Control group) and Assumption 2 (Partial equilibrium). Each row makes a different assumption about which ZIP codes are not affected by LCR regulation, denoted B. The first and second rows respectively assume LCR regulation has no effect on ZIP codes where nonbanks' share of FHA applications in 2013 (i.e., $\textit{Nonbank app share}_z$) is below the first or tenth percentile across ZIP codes, respectively. The first column summarizes this calculation for the overall mortgage market, and the second column summarizes this calculation for the FHA market.

Table 5: Implications for credit risk

Outcome:	Δ*Denial rate*$_z$		Δ*Default rate*$_{c(z)}$	
FHA app share$_z$ $\times$ *Nonbank app share*$_z$	-0.288	-0.277	0.006	0.006
	(.011)	(.014)	(.028)	(.042)
County FE	Yes	Yes	No	No
ZIP code controls	No	Yes	No	Yes
R-squared	.154	.166	.057	.058
Number of observations	2,707	2,659	3,963	3,925

p-values appear in parentheses. This table estimates equation (7), which assesses how nonbanks' expansion in the FHA market affects various measures of credit risk. Subscripts z and $c(z)$ index ZIP code and county. The regression equation takes the following form:

$$\Delta Y_z = \beta \left(\textit{FHA app share}_z \times \textit{Nonbank app share}_z \right) + ...$$
$$... + \gamma X_z + \alpha_{c(z)} + u_z,$$

where the outcome ΔY_z is a measure of the 2013–2015 change in a measure of credit risk. The outcomes in columns 1–2 and 3–4 are, respectively: the 2013–2015 change in the FHA mortgage application denial rate; and the 2013–2015 change in the 30+ day mortgage delinquency rate for the surrounding county, which includes all loan types. Default (i.e., delinquency) rates are only observed at the county level as described in Internet Appendix A. All specifications control for *FHA app share*$_z$ and *Nonbank app share*$_z$. ZIP code controls are those from Table 3. Observations are ZIP codes.

Table 6: Implications for homeownership

Outcome:	Δ*Homeownership rate*$_z$	
Nonbank app share$_z$ $\times$ *FHA app share*$_z$	0.314	0.319
	(.014)	(.013)
County FE	Yes	Yes
ZIP code controls	No	Yes
R-squared	.352	.357
Number of observations	4,536	4,460

p-values are in parentheses. This table estimates a variant of Equation (7), which assesses how nonbanks' expansion in the FHA market affects homeownership. Subscript z indexes ZIP codes. The regression equation is the same as that used in Table 5, replacing the outcome with the 2013–2015 change in the homeownership rate. The remaining notation, notes on specification, and sample are the same as those in Table 5.

Table 7: Variation in funding models as the mechanism

Outcome:	$Denial_{i,l,s,t}$			
$Securitization\ rate_l \times Premium_t \times FHA_s$	-0.012	-0.011		
	(.000)	(.000)		
$Noncore\ funding_l \times Premium_t \times FHA_s$			-0.013	-0.012
			(.000)	(.000)
Premium Measure	FNMA spread	FHLMC spread	FNMA spread	FHLMC spread
Lender-MSA FE	Yes	Yes	Yes	Yes
MSA-year FE	Yes	Yes	Yes	Yes
Lender-year FE	Yes	Yes	Yes	Yes
Loan type-lender FE	Yes	Yes	Yes	Yes
Loan type-year FE	Yes	Yes	Yes	Yes
Borrower controls	Yes	Yes	Yes	Yes
R-squared	.115	.115	.113	.113
Number of observations	2,594,800	2,594,800	2,652,502	2,652,502

p-values appear in parentheses. This table estimates Equation (10), which allows us to test whether the baseline effect works through variation in lenders' funding models. Subscripts i, l, s, and t index borrower, lender, loan type, and year, respectively. Each observation is a loan application. The regression equation is of the form

$$Denial_{i,l,s,t} = \beta\left(F_l \times Premium_t \times FHA_s\right) + \gamma X_{i,t} + \alpha_{l,t} + \alpha_{m(i),t} + \alpha_{m(i),l} + ...$$
$$... + \alpha_{s,t} + \alpha_{s,l} + u_{i,l,s,t},$$

where F_l is a measure of lender l's reliance on an originate-to-securitize funding model: Securitization rate is the lender's ratio of securitized mortgages to total originations in 2010, and Noncore funding is one minus the ratio of total deposits to total assets in 2010, which equals one for nonbanks by definition. The remaining notation, sample period, and standard errors are the same as those used in Table 2.

Table 8: Robustness to regulatory arbitrage incentives

Outcome:	$Denial_{i,l,s,t}$			
$Securitization\ rate_l \times Premium_t \times FHA_s$	-0.012	-0.013	-0.017	-0.015
	(.034)	(.024)	(.004)	(.012)
$High\ incentive_l \times Premium_t \times FHA_s$	0.007	0.007	0.008	0.010
	(.007)	(.014)	(.003)	(.000)
$Large_l \times Premium_t \times FHA_s$		0.001	0.004	0.002
		(.763)	(.139)	(.471)
Incentive measure	Low initial Capital	Low initial Capital	Low change in Capital	High initial MSR
Lender-MSA FE	Yes	Yes	Yes	Yes
MSA-year FE	Yes	Yes	Yes	Yes
Lender-year FE	Yes	Yes	Yes	Yes
Loan type-lender FE	Yes	Yes	Yes	Yes
Loan type-year FE	Yes	Yes	Yes	Yes
Borrower controls	Yes	Yes	Yes	Yes
R-squared	.110	.110	.110	.110
Number of Observations	1,331,695	1,331,695	1,331,695	1,331,695

p-values appear in parentheses. This table estimates a variant of equation (10), which allows us to assess whether the results are robust to correlation between reliance on an originate-to-securitize funding model and measures of regulatory arbitrage incentives. Subscripts i, l, s, and t index borrower, lender, loan type, and year, respectively. Each observation is a loan application to a bank lender. The regression equation takes the following form:

$$\begin{aligned} Denial_{i,l,s,t} &= \beta\left(Securitization\ rate_l \times Premium_t \times FHA_s\right) + ... \\ &... + \gamma_0\left(High\ Incentive_l \times Premium_t \times FHA_s\right) + ... \\ &... + \gamma_1\left(Large_l \times Premium_t \times FHA_s\right) + ... \\ &... + \gamma_2 X_{i,t} + \alpha_{l,t} + \alpha_{m(i),t} + \alpha_{m(i),l} + \alpha_{s,t} + \alpha_{s,l} + u_{i,l,s,t}, \end{aligned}$$

where $Securitization\ rate_l$ is the lender's ratio of securitized loans to total originations in 2010; and $High\ Incentive_l$ is an indicator for whether l has a strong regulatory arbitrage incentive based on some measure: columns 1 and 2 use an indicator for whether l's ratio of total equity to total assets is below the asset-weighted median across bank lenders in 2010; column 3 uses an indicator for whether the 2010–2015 change in this ratio is below the asset-weighted median across bank lenders; and column 4 uses an indicator for whether l's ratio of mortgage servicing rights to total equity is above the asset-weighted median across bank lenders in 2010. Columns 2–4 include the interaction between $Securitization\ rate_l$, $Premium_t$, and an indicator for whether l's average assets over 2010–2015 exceed the asset-weighted median across banks, denoted $Large_l$. All columns measure $Premium_t$ using the FNMA spread, defined in Table 2. The remaining notation, sample period, and standard errors are the same as those used in Table 2.

Table 9: Robustness to the effect on nonbanks' loan volume

Outcome:	$\log\left(Loan\ size_{i,l,s,t}\right)$		
$Nonbank_l \times Premium_t \times FHA_s$	0.027	0.010	0.009
	(.000)	(.000)	(.000)
Premium measure	Post-LCR	FNMA spread	FHLMC spread
Lender-MSA FE	Yes	Yes	Yes
MSA-year FE	Yes	Yes	Yes
Lender-year FE	Yes	Yes	Yes
Loan type-lender FE	Yes	Yes	Yes
Loan type-year FE	Yes	Yes	Yes
Borrower controls	Yes	Yes	Yes
R-squared	.600	.600	.600
Number of observations	2,385,666	2,385,666	2,385,666

p-values appear in parentheses. This table estimates a variant of equation (1), which allows us to assess implications for financial stability by testing whether nonbanks respond to higher secondary market prices by approving larger loans. Subscripts i, l, s, and t index borrower, lender, loan type, and year, respectively. Loan Size is the size of the loan, in dollars. The sample consists of originated FHA or conventional loans for the purchase of an owner-occupied single-family dwelling. The remaining notation, sample period, and standard errors are the same as those used in Table 2.

Table 10: Effect on nonbanks' lending standards by loan type

Outcome:	$Denial_{i,l,t}$			
$Nonbank_l \times Premium_t$	-0.014		-0.001	
	(.000)		(.760)	
$Securitization\ rate_l \times Premium_t$		-0.014		0.000
		(.008)		(.878)
Sample	FHA loans		Conventional loans	
Lender-MSA FE	Yes	Yes	Yes	Yes
MSA-year FE	Yes	Yes	Yes	Yes
Borrower controls	Yes	Yes	Yes	Yes
R-squared	.118	.118	.101	.099
Number of observations	1,046,532	845,352	2,219,363	1,747,923

p-values appear in parentheses. This table estimates Equation (11) in the FHA and conventional loan markets, which allows us to assess whether lenders who increase credit among FHA loans also tighten credit among conventional loans. Subscripts i, l, and t index borrower, lender, and year, respectively. Each observation is a loan application. The regression equation takes the following form:

$$Denial_{i,l,t} = \beta \left(F_l \times Premium_t\right) + \gamma X_{i,t} + \alpha_{m(i),t} + \alpha_{m(i),l} + u_{i,l,t},$$

where F_l is a measure of lender l's reliance on an originate-to-securitize funding model: Nonbank indicates if the lender is a nonbank, and Securitization rate is the lender's ratio of securitized mortgages to total originations in 2010. All columns measure $Premium_t$ using the FNMA spread, defined in Table 2. The sample in columns 1 and 2 consists of applications for FHA loans for the purchase of an owner-occupied single-family dwelling, and the sample in columns 3 and 4 consists of similar applications for conventional loans. The remaining notation, sample period, and standard errors are the same as those used in Table 2.

Table 11: Robustness to placebo test over the pre-LCR period

Specification:	ZIP code level		Loan level	
Outcome:	Δ*Nonbank market share*$_z$		$Denial_{i,l,s,t}$	
$FHA\ app\ share_z \times Nonbank\ app\ share_z$	0.045	0.077		
	(.494)	(.266)		
$Nonbank_l \times Trend_t \times FHA_s$			-0.003	
			(.429)	
$Securitization\ rate_l \times Trend_t \times FHA_s$				0.001
				(.858)
County FE	Yes	Yes		
ZIP code controls	No	Yes		
Lender-MSA FE			Yes	Yes
MSA-year FE			Yes	Yes
Lender-year FE			Yes	Yes
Loan type-lender FE			Yes	Yes
Loan type-year FE			Yes	Yes
Borrower controls			Yes	Yes
R-squared	.554	.551	.126	.124
Number of observations	3,145	2,873	1,557,821	1,204,518

p-values appear in parentheses. This table estimates equations (3) and (7) over the 2011–2013 period, which serves as a placebo test for our zip-code-level and loan-level results. Columns 1 and 2 contain the results of regressions similar to those in the first two columns of Table 3, except that 2013 is replaced with 2011 and 2013–2015 are replaced with 2011–2013 in all variable definitions. Columns 3 and 4 contain the results of regressions similar to those in Tables 2 and 7, except that the sample period is 2011–2013 and $Premium_t$ is measured by a linear time trend, denoted $Trend_t$ and residualized against the FNMA spread. The remaining notation and notes on specification in columns 1–2, 3, and 4 are the same as those used in Tables 3, 2, and 7, respectively.