How Quantitative Easing Works:
Evidence on the Refinancing Channel[*]

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Abstract

We document the transmission of large-scale asset purchases by the Federal Reserve to the real economy using rich borrower-linked mortgage-market data and an identification strategy based on mortgage market segmentation. We find that central bank QE1 MBS purchases substantially increased refinancing activity, reduced interest payments for refinancing households, led to a boom in equity extraction, and increased aggregate consumption. Relative to QE-ineligible jumbo mortgages, QE-eligible conforming mortgage interest rates fell by an additional 40 bp and refinancing volumes increased by an additional 56% during QE1. We estimate that households refinancing during QE1 increased their durable consumption by 12%. Our results highlight that the transmission of unconventional monetary policy to the real economy depends crucially on the composition of assets purchased and the degree of segmentation in the market.

1   Introduction

In recent years, many central banks have undertaken unconventional monetary policy to stimulate their economies, mainly through large-scale asset purchase programs (LSAPs) often referred to as Quantitative Easing (QE).[1] Owing to the newfound popularity of LSAPs worldwide, their effectiveness and the channels through which they affect the real economy have been at the center of a vigorous policy and academic debate. The existing literature has focused on the reaction of asset prices to the announcements of these new monetary tools—see Krishnamurthy and Vissing-Jørgensen (2011 and 2013). We contribute to this debate by investigating effects on both prices and quantities at the household level, quantifying the importance of the refinancing channel in LSAP transmission to the real economy, and studying how LSAPs affect aggregate debt issuance and consumption through the mortgage market.

Identifying the effects of macroeconomic policies is particularly challenging given that such policies intentionally respond to current and anticipated aggregate shocks, making it difficult to disentangle the effects of LSAPs from shocks that might also affect households' decisions. For traction, most of the literature has used event studies of short-run asset-price changes immediately surrounding central bank policy announcements. Given that the origination process takes time, estimating effects on debt issuance requires a different approach. Ideally, an empiricist would compare market segments subject to similar demand shocks but differentially affected by LSAPs, e.g., the Fed intervening in one but not another. Contrasting effects would provide a credible vehicle to examine downstream transmission of the purchases.

Our identification strategy exploits the segmentation of the U.S. mortgage market and

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[1]A common feature of these programs is their significant size; the Federal Reserve, for example, increased the size of its balance sheet more than fivefold ($3.6 trillion) from 2008 to 2015 (see Figure 1). LSAPs have varied significantly in the type of assets purchased by central banks, e.g., from Treasuries and government-guaranteed Mortgage-Backed Securities (MBS) in the U.S. to Exchange-Traded Funds and corporate bonds in Japan.

the legal restriction that the Fed can only purchase mortgages guaranteed by the Government Sponsored Enterprises (GSEs).[2] With limited exceptions, GSE-guaranteed loan sizes must be less than Conforming Loan Limits and must have loan-to-value ratios (LTVs) at or below 80 percent.[3] This allows us to estimate the importance of the composition of assets included in each QE round by contrasting how each QE round affected refinancing activities in the conforming and jumbo segments. Specifically, QE1 and QE3 involved MBS and Treasury purchases, QE2 purchases consisted entirely of Treasuries, and the Maturity Extension Program involved MBS principal reinvestment and funding long-term Treasury purchases with sales of short-term Treasuries. We refer to the announcement that the flow of QE3 MBS purchases would begin to decline as the Tapering.[4]

We first examine changes in mortgage interest rates and refinancing volumes in response to each QE event. Figure 2 shows graphical evidence of interest rate effects, and Figure 3 plots mortgage refinancing volumes across mortgage segments. Consistent with existing work, we find that interest rates decreased by more than 100 basis points around the beginning of QE1. However, interest rates for QE-ineligible "jumbo" loans (whose balances exceed GSE limits) decreased significantly less, increasing the interest-rate spread between jumbo and conforming loans by more than 40 basis points. Moreover, despite the jumbo securitization market shutting down well before our estimation sample begins, Figure 2 and Appendix Figure 1 show conventional monetary stimulus did not have this differential impact across segments. By contrast, other QE events that didn't involve mortgage purchases or occurred when the banking sector was much healthier were associated with overall effects on mortgage rates of 17-42 basis points and similar effects across conforming and jumbo segments.

We then examine how unconventional monetary policy affected the volume of mortgage refinancing. In the months immediately following the announcement of QE1, Figure 3 shows

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[2]Section 14 of the Federal Reserve Act mandates that, with limited exceptions, the Federal Reserve purchase only government-guaranteed debt.

[3]Loans with LTVs above 80% are permissible if enhanced by private mortgage insurance, a scarce option during the crisis (Bhutta and Keys, 2017).

[4]Panel II of Figure 1 and Appendix A provide an overview of the QE timeline and the mix of assets purchased.

that the monthly origination of mortgages that were eligible for purchase by the Fed more than doubled, while the origination of loans above the conforming loan limits increased by less than 10%. Similar to our interest-rate results, other QE events show relatively similar reactions across segments, with some evidence of (much smaller) differential effects around QE3 and Tapering. Using newly constructed disaggregated data on bank lending, we further show that a key factor in the contrasting effects of QE1 and QE3 was the impairment of the banking sector. In particular, we find that banks with higher mortgage-related losses in the crisis originated significantly less jumbo mortgages (but not conforming mortgages) during QE1 but not during QE3 when banking-sector health had improved.

Because of the large number of potential confounds when trying to identify effects of macroeconomic policies, we detail several robustness checks that are each consistent with the differential reaction to QE1 across these two market segments being attributable to Fed MBS purchases. We then follow the transmission of the purchases to the household sector through the refinancing channel. We estimate that Fed mortgage purchases increased refinancing by over 56%, substantially reducing interest payments for refinancing households. Our back-of-the-envelope calculations suggest that refinancing increased by $102 billion over the first six months of QE1, increasing mortgagors' consumption by $13.5 billion over the same horizon. Contrasting the effects on mortgage volumes across QE episodes, we again find that QE events after QE1 had smaller overall effects and much smaller differential impacts across mortgage-market segments, consistent with this heterogeneity in the mortgage market response to LSAPs being a function of the type of debt the central bank purchased and banking-sector health.

Our analysis of interest rates, origination volumes, and durable consumption makes several contributions relative to studies that rely on high-frequency event studies of yields. The identification advantage of such papers is that asset prices responding almost instantaneously to central bank announcements are unlikely to have been affected by other shocks in narrow windows around QE announcements. However, market price reactions in the immediate

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short run might be different from the programs' effects in the longer run.[5] Second, most event-study papers seek to estimate changes in secondary-market yields.[6] Because the slope of the credit demand curve changes over time and the pass-through of MBS yields to interest rates is imperfect during this period (Fuster et al., 2013 and Scharfstein and Sunderam, 2013), papers inferring LSAP effectiveness from high-frequency yields may overstate their real effects. Third, because interest rates are observed only conditional on origination, estimating the effects of unconventional monetary policy purely from interest-rate changes will be an overstatement by assuming perfect availability of credit.[7] Finally, because points and fees are generally unobservable to the econometrician and reflect the time-varying price of financial intermediation (Fuster, Lo, and Willen, 2017), changes in interest rates may not be a sufficient statistic for changes in the cost of mortgages. This motivates our focus on the direct detection of real effects, specifically the effects of credit easing on the primary mortgage market induced by LSAPs as distinct from effects on financial variables like asset prices and interest rates.

To identify the impacts of LSAPs, we account for time-varying credit demand and supply shocks that might otherwise confound our results. Our loan-level dataset combines agency and non-agency mortgages, allowing us, for example, to compare observationally similar loans in different segments: those that are plausibly exposed to the same demand shocks and fundamentals but that are above and below the GSE conforming-loan limits. Event studies support this parallel trends assumption, particularly within our specifications' three-month windows around policy dates.[8] Further steps to address demand shocks include

[5]This could be either because of partial segmentation of asset classes as in Greenwood et al. (2018) or because investors' understanding of the effects of LSAPs changes over time relative to what was already priced in at announcement.

[6]Notable exceptions are Fuster and Willen (2010) and Hancock and Passmore (2011), who look for effects on mortgage applications, originations, underwriting, and primary-market mortgage rates. See section 2 for further discussion.

[7]The preponderance of fixed-rate mortgages in the United States means that most households need to qualify for a new refinance mortgage to benefit from monetary stimulus, preventing underwater fixed-rate borrowers (and fixed-rate borrowers who cannot qualify for new refinance mortgages) from the direct benefits of QE (DeFusco and Mondragon, 2018).

[8]For example, the reaction of both segments to the Federal Funds Rate and 5- and 10-year Treasury yields suggests a similar sensitivity to risk-free rates.

focusing on refinance mortgages, demand for which is mainly driven by changes in interest rates as opposed to changes in the demand for housing, and accounting for regional shocks to fundamentals (income, house prices, expectations, etc.) by controlling for county × month effects. Estimates using several additional segment-specific controls suggest that our results are not driven by segment-specific credit supply shocks. We focus our analysis on the post-2008 period to avoid making inferences off of the asset-backed securities market disruptions that differentially affected the jumbo-lending market as it transitioned away from being heavily reliant on private securitization (Chernenko, Hanson, and Sunderam, 2014).

We proceed as follows. Section 2 briefly contextualizes our work in the relevant academic literature on monetary policy transmission, and section 3 details the data sources used in our analysis and our empirical design. Section 4 presents graphical evidence and outlines our main empirical strategy. Section 5 reports our core results on interest rates and aggregate debt origination, robustness exercises, and additional findings on the importance of banking-sector health in QE transmission. Section 6 estimates effects on household-level refinancing and consumption behavior to estimate the aggregate effects of QE1. We conclude in section 7 with a summary and discussion of policy implications. An appendix provides additional robustness checks, further background on the Federal Reserve's Quantitative Easing program, a simulation of the potential effectiveness of QE-complementing GSE policy, and evidence on the differential allocation of QE credit across regions.

2 Related Literature

This paper contributes to the empirical literature on LSAPs that has generally focused on effects on equilibrium rates of return, including Baba et al. (2006), Gagnon et al. (2010, 2011), Sarkar and Shrader (2010), Ashcraft et al. (2011), Hancock and Passmore (2011), Joyce et al. (2011), Swanson (2011, 2015), Stroebel and Taylor (2012), D'Amico and King

(2013), Kandrac and Schlusche (2013), and Koijen et al. (2018).[9] In addition to providing evidence corroborating the results highlighted by these papers on the effects of QE on asset returns, we document how LSAPs shaped refinancing activities in the aftermath of the crisis and trace the transmission of LSAPs to the real economy through the refinancing channel.[10]

The key studies we build on are Krishnamurthy and Vissing-Jørgensen (2011, 2013), who, among other things, illustrate that QE1 MBS purchases affected MBS yields more than QE2 Treasury purchases did and that QE3's effect on MBS yields was much smaller than QE1's effect. Our main innovation with respect to these studies is to examine how credit supply reacted by analyzing changes in both mortgage origination and mortgage prices paid by borrowers in response to Fed's purchases of MBS and Treasuries. While the previous literature has implicitly relied on a tight connection between prices and quantities, the analysis of secondary-market mortgage yields may overestimate the effectiveness of these policies when intermediaries are constrained. Our paper contributes to this literature by showing how LSAPs caused a substantial increase in both refinancing and aggregate consumption. We also provide direct evidence in a new disaggregated panel of bank lending outcomes for the role of bank capital constraints in the transmission of QE to the real economy.

Also closely related to our work is Fuster and Willen (2010), which documents how aggregate loan application and origination volumes responded to QE1, emphasizing the differential changes in refinancing opportunities across the spectrum of borrower income and creditworthiness. Consistent with our findings, they find a significant increase in the number of mortgage applications and originations in response to QE1. Our paper builds on

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[9]For the international channel of unconventional monetary policy transmission, see Bauer and Neely (2014), Temesvary, Ongena and Owen (2018) and Caballero et al. (2016). Christensen and Krogstrup (2018) study the role of central-bank reserves in QE transmission. See also Chodorow-Reich (2014) and Di Maggio and Kacperczyk (2017), who study the impact of unconventional monetary policy on different sectors of the financial markets, such as pension funds, insurance companies, and money-market funds.

[10]Our results also inform the growing theoretical literature studying the importance of financial-market segmentation in the transmission of unconventional monetary policy. For example, see Curdia and Woodford (2011), Brunnermeier and Sannikov (2016), Drechsler et al. (2017), Del Negro et al. (2017), Gertler and Karadi (2011), Greenwood et al. (2018), and Farmer and Zabczyk (2016). Krishnamurthy and Vissing-Jørgensen (2011) provide a comprehensive treatment of transmission channels along with empirical evidence from yields on the relative importance of each channel.

these results in several ways. We look across mortgage-market segments using regional- and individual-level multivariate models of refinancing, consumption, and deleveraging. Contrasting the impact of each QE program on loans eligible and ineligible for the Fed purchase supports our identification of the transmission channel of QE to the real economy and allows us to trace QE transmission from aggregate quantities to households' consumption and savings decisions. By exploiting our microdata's ability to track borrowers across loans and cross-sectional differences in available equity, we document heterogeneity in equity-extraction behavior around refinancing decisions. Overall, our results highlight frictions that limit the pass-through of programs like LSAPs across geographies and borrower segments.[11]

A related empirical literature studies the effect of monetary policy on credit conditions. For example, Maddaloni and Peydró (2011) find that low short-term interest rates for an extended period eventually soften lending standards for household and corporate loans.[12] Chakraborty, Goldstein, and MacKinlay (2017) and Rodnyansky and Darmouni (2017) look explicitly at the effect of QE on bank lending, with the former showing that the banks that are more active in the MBS market increased their mortgage market share following the LSAPs and the latter finding that MBS purchases have important implications for which banks are able to increase credit supply in response to QE3. Kurtzman, Luck, and Zimmermann (2017) follow a similar strategy to show that overall lending standards declined following QE1 and QE3. Our measure of bank distress, combined with bank-level origination by county-segment-month allows us to provide direct evidence on the importance of banking-sector health in QE transmission. We also add to these studies evidence on the impact of unconventional monetary policy on individual household refinancing decisions, highlighting the heterogenous allocation of unconventional monetary stimulus.

Finally, we also contribute to the literature investigating the redistributional consequences

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[11]Our paper is also related to several papers investigating ECB programs, including Brown, Kirschenmann, and Ongena (2014), Avdjiev, Subelytė and Takáts (2016), Crosignani, Faria-e-Castro and Fonseca (2017), and Carpinelli and Crosignani (2018).

[12]See also Bernanke and Blinder (1988), Christiano and Eichenbaum (1992), Gomez et al. (2017), Stein (2012), and Williamson (2012).

of monetary policy, e.g., Doepke and Schneider (2006), Fuster and Willen (2010), Coibion et al. (2017), and Sterk and Tenreyro (2016). Particularly relevant to our study is Beraja et al. (2018), who use the same borrower-linked refinance data we do to show that the heterogeneous regional effects of QE1 covary with regional economic conditions and amplify existing regional disparities. While Beraja et al. (2018) focus on LTV distributions across space, much of our work contrasts behavior above and below the conforming loan limit within a given county-month for identification to account for local credit-demand shocks. We study individual-level prepayment behavior, consumption effects, five QE events, show how these effects aggregate up to affect aggregate consumption, and conduct counterfactual policy exercises to highlight the complementary role GSE policy can play in monetary stimulus.

3   Data

Our workhorse data source is the Equifax's Credit Risk Insight Servicing McDash (CRISM) dataset, which covers roughly 65 percent of the mortgage market during our sample period (2008-2014), also used to examine the effects of QE1 by Beraja et al. (2018). One of the features of this dataset is that it merges mortgage-servicing records from McDash Analytics with credit bureau data from Equifax. This provides us with information about the characteristics of each mortgage at origination, such as the mortgage type, the size of the loan, the monthly payments, the interest rate, the borrower's credit score (FICO), as well as their behavior over time. The ability to link multiple loans by the same borrower together allows us to track individual borrowers as they refinance into a new mortgage, in addition to observing each borrower in the credit registry data six months before any covered mortgage origination.

Panels I and II of Table 1 report loan-level summary statistics on conforming and jumbo loans from the CRISM database. Our sample includes more than six million loans below the conforming loan limit and about 155,000 jumbo loans. On average, conforming (jumbo)

borrowers in our sample have a 752 (762) FICO score and an LTV of 66% (64%). Average interest rates are higher for conforming loans in our sample only because of the popularity of adjustable-rate mortgages in the jumbo segment.[13] The average balance of conforming and jumbo loans in our data are $207,000 and $1,033,000, respectively.

Panel III reports summary statistics for time series controls used in robustness checks. For conforming mortgages to be guaranteed by the GSEs, the originator must pay a guarantee fee that changes over time. As this factor affects mortgage market segments differentially, we use a quarterly measure from Fuster et al. (2013) of the cost of having the GSE guarantee in our robustness checks. Over our six year time period, the average guarantee fee was 34 bps. As a measure of banking-sector health, we calculate bank-size weighted average five-year credit-default swap (CDS) spreads from Markit. Over our sample, these spreads ranged from a 10th percentile of 17 bps to a 90th percentile of 241 bps. Finally, as the GSEs guarantee the timely repayment of principal in the event of borrower default, default risk is uniquely a jumbo segment concern. We construct a measure of mortgage market risk premium by calculating a measure of credit spreads in the jumbo segment that we refer to as FICO Credit Spread. This variable is constructed as the negative of the slope coefficients from a regression of interest rates in the jumbo segment on FICO scores interacted with month dummies.[14] Below, we show that jumbo mortgage rates are more affected when credit risk is priced more intensively in the mortgage sector. For the average month in our data, one standard deviation increase in FICO credit spreads increases jumbo interest rates by an additional 26 basis points.

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[13]When we compare the average spread over the current ten-year Treasury yield of fixed-rate conforming and jumbo mortgages, jumbo mortgage spreads are indeed higher than conforming mortgage spreads.

[14]We estimate these credit spreads using a sample of 30-year jumbo mortgages with LTVs less than or equal to 80% and FICO scores of at least 560, and control for product-type by month interactions and LTV-bin by month interactions.

4 Empirical Strategy and Graphical Evidence

In this section we present our main empirical strategy to identify the effects of LSAPs on interest rates and refinancing volumes. After discussing the corresponding estimation results in section 5, we discuss richer specifications to deal with potential confounding shocks in section 5.1 and address the endogeneity of mortgage segment choice in section 5.3.

4.1 The Effect of QE on Interest Rates

We begin by comparing the interest-rate reaction to LSAPs for loans above and below the conforming loan limit. As changes in borrower composition over time limit the usefulness of simple time-series comparisons of interest rates, we account for equilibrium rate changes due to changing borrower characteristics. For example, some of the decrease in interest rates that we observe is due to stricter credit standards at the end of our sample—later mortgages feature both higher average FICO scores and lower LTVs. To facilitate graphical comparisons of composition-adjusted interest rates over time, we estimate the following regression separately for 30-year fixed-rate loans above and below the conforming loan limit

$$r_{it} = \alpha_t + \beta_1(FICO_i - 760) + \beta_2(LTV_i - 0.75) + \varepsilon_{it}, \tag{1}$$

where r_{it} is the interest rate of loan i at time t measured in basis points. We control for the difference between the FICO score and loan-to-value ratio of loan i and benchmark FICO and LTV ratios such that estimated time effects α_t capture "rate-sheet–adjusted" interest rates—interest rates for a representative borrower with a FICO score of 760 and an LTV ratio of 75%.[15] Figure 2 plots these composition-adjusted interest rates $\hat{\alpha}_t$ for loans above and below the conforming loan limit. Overall, rates for the two types of loans follow each

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[15]Note that our data do not allow us to measure closing costs (fees) or any payments made by the borrower to reduce the mortgage coupon (points), which Fuster, Lo, and Willen (2017) show to covary with interest rates in important ways (and in particular around QE1). To the extent that charged fees or points taken by the borrower or the risk-pricing coefficients in equation (1) respond differently to QE in the two segments, this further motivates our primary focus on origination volumes.

other closely.[16] However, there is a visible change in interest rates as QE1 begins, when mortgage interest rates declined markedly and conforming-loan rates declined almost 50% more than prime jumbo mortgage rates.

A potential alternative explanation for these results is that the decline in jumbo securitization led the jumbo segment to be relatively insensitive to monetary policy. This argument is insufficient to explain the time-series behavior of both segments. While the jumbo segment has historically relied on the private securitization market, this market shut down in mid-2007 (see Chernenko, Hanson, and Sunderam, 2014). However, we see parallel changes in conforming and jumbo interest rates before QE1 throughout 2008, during the entirety of which the securitization market was collapsed. To further examine this concern, Appendix Figure 1 plots jumbo and conforming interest rates alongside the Federal Funds Rate. Despite the collapsed jumbo securitization market, jumbo mortgage rates fell together with conforming interest rates during the beginning and end of 2008 prior to QE1 in response to conventional monetary policy stimulus. The gap between jumbo and conforming interest rates increased only after the onset of QE1, during which time the short rate was not changing.[17]

To quantify the differential reaction of jumbo and conforming interest rates (measured in basis points) to purchase-program events, we report results in section 5 of estimating the following loan-level specification separately around the beginning of each monetary policy event

$$r_{icst} = \theta_0 QE_t + \theta_1 Jumbo_s + \theta_2 QE_t \cdot Jumbo_s + X_i'\beta + \gamma W_t \cdot Jumbo_s + \varphi_{cs} + \eta_{ct} + \varepsilon_{icst} \quad (2)$$

where QE_t is an indicator for month t being in the three months immediately after the institution of a specific monetary policy program (e.g., QE1) and $Jumbo_s$ is an indicator variable for whether loan i in segment $s \in \{Jumbo, Conforming\}$ is a jumbo mortgage. In

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[16]These results are robust to a non-parametric specification of (1) that controls for 10-point FICO bin indicators and 5-point LTV bin indicators.

[17]While the dynamics of 10-year Treasury yields are most consistent with a causal effect of QE1, inference would be more complicated if other factors also caused a contemporaneous-with-QE decline in yields.

robustness checks, we also control for a vector X_i of flexible loan-level controls, aggregate factors W_t potentially affecting each segment differentially, county $\times$ segment fixed effects φ_{cs}, and county $\times$ month fixed effects η_{ct} (which absorb θ_0). Loan-level controls include indicators for 5-point LTV bins, 20-point FICO bins, missing FICO scores, and interactions of product type (interest-only, balloon, or prepayment penalty) and maturity (15-, 20-, or 30-year indicators). As introduced in section 3, the time-series controls W_t include guarantee fees charged to investors in GSE-guaranteed mortgages, average bank credit-default swap spreads, and a measure of credit spreads in the private mortgage market. County-segment and county-month fixed effects purge interest rates in both segments of static and time-varying regional shocks to credit demand or supply, including differences in house price growth and changes in expectations. We consider the three quantitative easing programs as well as the Maturity Extension Program in September 2011 and the announcement of QE3 purchase tapering in June 2013.

The coefficient θ_0 represents the basis points by which average interest rates for LSAP-eligible conforming mortgages changed in the three months immediately following the beginning of each QE campaign relative to the period immediately prior. The coefficient θ_1 on the jumbo indicator measures the initial difference in jumbo and conforming interest rates (known as the jumbo-conforming spread). The coefficient θ_2 tracks the differential interest-rate response to QE of the directly affected conforming segment relative to ineligible jumbo mortgages (i.e. changes in the jumbo-conforming spread). We interpret θ_0 with some degree of caution because it combines the effect of LSAPs with any contemporaneous national shock to mortgage rates, even those effects not caused by the monetary policy events in question. The identifying assumption behind our causal interpretation of θ_2 is that conditional on our time-series controls, time-varying shocks do not affect the jumbo and conforming segments differently. In other words, our ability to provide evidence on the pass-through of LSAPs to retail mortgage markets relies on a conditional parallel-trends assumption. While difficult to validate in a macroeconomic setting, below we discuss evidence that this identifying

assumption seems plausible over the short horizons used to estimate equation (2).

To facilitate interest rate comparisons across these two segments, we restrict the sample to a set of relatively homogenous mortgage products by considering fixed-rate first-lien mortgages secured by single-family homes with non-missing LTV values and the most common maturities (15, 20, or 30 years). We also drop FHA mortgages for this exercise, which require mortgage insurance and have more flexible lending requirements than those for conventional loans. To ensure that we have enough variation in slow-moving macro factors to estimate γ precisely, we first estimate equation (2) over the entire sample period of 2008–2014 without the QE coefficients and impose this $\hat{\gamma}$ when we estimate (2).[18] Finally, we cluster our standard errors at the month $\times$ segment level to account for the correlation between contemporaneous shocks to each segment across geographies at a given time.

4.2 The Effect of QE on Mortgage Origination Volumes

As discussed above, inferring the impact of unconventional monetary policy from changes in interest rates tends to overstate policy effectiveness by assuming perfect availability of credit. Considering the volume of debt issuance in response to the LSAPs is a more direct characterization of the transmission of unconventional monetary policy through the refinancing channel.

Figure 3 plots monthly origination volumes in our data for mortgages with loan sizes above and below the GSE conforming loan limit (CLL). The jumbo and conforming segments trend similarly in origination counts (panel I) and total volume (panel II) prior to the beginning of QE1, bolstering our identifying assumption of parallel trends. Again, although the jumbo segment experienced a negative supply shock from disruptions to the private securitization market in 2007, origination trends moved together throughout 2008. Right at the commencement of QE1, the amount of conforming refinance origination increases by a factor of three (counts) or four (dollar volume). The sudden increase and subsequent fad-

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[18]Our results are robust to estimating (2) on the full 2008-2014 sample and controlling for $W_t \cdot Jumbo_s$ contemporaneously.

ing of conforming refinance originations coincides closely with the dynamics of Fed MBS purchases. By contrast, refinance origination above the conforming loan limit is fairly flat until a modest increase in April 2009. In other words, while the increase in the conforming spread indicates a differential response of rates depending on GSE eligibility, loan origination suggests an even deeper relationship between the allocation of credit supply and QE1 MBS purchases.

Section 5 reports results quantifying these effects on refinance origination volumes Q_{sct} at the county c × mortgage-market segment s × month t level by estimating

$$\log Q_{cst} = \psi_0 QE_t + \psi_1 Jumbo_s + \psi_2 QE_t \cdot Jumbo_s + X'_{cst}\beta + \kappa W_t \cdot Jumbo_s + \delta_{cs} + \alpha_{ct} + u_{cst}. \quad (3)$$

For each policy event, we again provide baseline results without any controls. Subsequent robustness checks use the full specification in (3), controlling for the local composition of each segment's borrowers in each month X_{sct} (average FICO scores, the fraction missing FICO scores, and average LTV ratios for a county × month × segment), allowing for county × segment and county × month fixed effects δ_{cs} and α_{ct}, and time-series controls W_t interacted with the jumbo indicator, with κ estimated as described above for γ in (2).[19] We also restrict attention to counties where we observe an active jumbo market by limiting the sample to counties that have at least one jumbo refinance origination each month.

The coefficient ψ_0 tells us by how many log points conforming origination volumes increased in the average county in the months following a QE event relative to the months immediately preceding that event. We focus our attention on ψ_2, which is an estimate of the degree to which jumbo origination volumes responded differently than conforming origination volumes. Again, the identifying assumption required for ψ_2 to be an unbiased estimate of the differential allocation of LSAP credit across mortgage market segments is that there were no other shocks, say ζ_{st}, occurring coincident with QE events that affected the jumbo market more (or less) than the conforming market. While our interest in refinance origina-

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[19]Our results are robust to estimating (3) on the full 2008-2014 sample and controlling for $W_t \cdot Jumbo_s$ contemporaneously.

tion volumes instead of purchase originations helps us abstract away from local shocks to credit demand including beliefs about future house prices, county $\times$ month fixed effects α_{ct} absorb any remaining local shocks to credit demand or supply.

The requirement for conditional parallel trends across conforming and jumbo segments motivates using three-month windows around each event, as well as focusing on the conforming loan limit since underwriting standards in the conforming and prime jumbo market are more similar than looking across prime and non-prime segments. For unrelated and coincidental changes in borrower composition to explain our results, it would need to be that jumbo lending standards disproportionately tightened in the three months following the beginning of QE1 relative to the three months prior. The graphical evidence discussed above also supports this parallel-trends assumption, as do results below on default risk and specifications that control for macroeconomic factors W_t interacted with the jumbo indicator to allow for several potential segment-specific shocks.

5 The Effect of LSAPs on the Primary Mortgage Market

Using the data and empirical strategies outlined above, in this section we present our core results on the response of mortgage rates and refinancing origination volumes to QE, as well as several robustness exercises. To facilitate comparison with prior literature, we first present interest-rate results. Extending our analysis to effects on actual debt issuance, however, is crucial for establishing the validity of comparing conforming and jumbo segments in tracing the transmission of unconventional monetary policy through the refinancing channel.

To quantify the average magnitude of the effects seen in Figure 2 over each monetary policy event, Table 2 reports estimates of equation (2) for the interest-rate response of LSAP announcements for jumbo and non-jumbo loans. In panel I, we find the most significant reaction to the announcement of QE1 with an interest-rate reduction of more than 120 basis points. Consistent with Figure 2, jumbo-mortgage interest rates also decline after QE1 but by

55 basis points less than LSAP-eligible conforming interest rates. Rates declined by 36 basis points around the announcement of QE2 without any differential effect across segments. As QE2 did not entail any MBS purchases, we find this consistent with our interpretation that the reason QE1 had differential effects is because the Fed purchased MBS at a time of strong segmentation in the mortgage market. The Maturity Extension Program was associated with a reduction of about 47 basis points, but we again fail to reject that there was no differential effect for conforming and jumbo segments.[20] Interest rates in both segments also fell by about 20 basis points following the announcement of QE3.[21] Finally, the beginning of the Fed tapering saw a statistically insignificant increase in interest rates concentrated in the conforming loan segment.

Table 3 reports results from estimating equation (3). Column 1 of panel I shows that overall mortgage refinancing activity increased by 102 log points (177%) during QE1, with almost all of the effect concentrated in the conforming loan segment. Might the conforming segment have responded so much more strongly than the jumbo segment even if the Fed had not purchased MBS during QE1? The response to both QE2 and the MEP, estimated in columns 2 and 3, suggests that MBS purchases were a key component of QE1's relative effectiveness at stimulating conforming mortgage origination. The overall increase in both conforming and jumbo originations was 80% and 70% (60 and 55 log points) during QE2 and the MEP, respectively, with no detectable differential effect across loan segments. Column 4 reports an insignificant origination response to the announcement of QE3, which we discuss more in section 5.2 below. In the aftermath of the Fed's tapering announcement (column 5), refinancing activities in the conforming segment fell around 30% but increased in the jumbo segment.

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[20]While these results confirm the findings of Krishnamurthy and Vissing-Jørgensen (2011) that QE2 had smaller effects on mortgage yields because it did not include MBS purchases, note, too, that the health of the banking sector improved between QE1 and QE2.

[21]While our identification strategy's comparative advantage is not in the causal interpretation of main effects, the small overall effects that we estimate for QE2 and QE3 are consistent with results on yields in Krishnamurthy and Vissing-Jørgensen (2013) suggesting that the size of those programs (and market expectations about them) muted their overall effects.

As discussed in section 4, while contemporaneous aggregate shocks could confound our estimates of the main effect of each LSAP event, under our identifying assumption of no mortgage segment-specific shocks, the QE Program × Jumbo coefficients reflect the differential impact of each QE program on origination volumes. We now present specifications with additional controls to bolster our interpretation of these results, followed by further analysis to understand the reasons for the differences between QE1 and QE3 results in section 5.2. Finally, before examining QE1's effect on monthly payments and household consumption, we address the endogeneity of segment choice in section 5.3.

5.1 Robustness and Conditional Parallel Trends

Our identification strategy takes advantage of the natural segmentation in the mortgage market and employs a differences-in-differences approach to compare the differential refinancing response of the conforming and jumbo segments in a narrow window around the policy events. This allows us to limit the role of contemporaneous common shocks affecting both segments at the same time (such as conventional monetary policy shocks) and control for local changes in credit demand. The parallel trends in Figures 2 and 3, especially in the period after the private securitization crunch but during ongoing conventional monetary policy stimulus before QE1 (2008), support this strategy. However, one potential limitation of this approach is the possibility that unobserved aggregate credit supply shocks occurring at about the same time of the policy announcements might differentially impact the conforming and jumbo loan segments and confound our results.[22] For example, because jumbo mortgage investors bear default risk that GSE mortgage investors do not, lenders might face different shocks to funding constraints in the two segments of the market.

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[22]A related concern is that these differences among loans below and above the conforming loan limit could be an artifact of the January 2008 change in the limit itself. The relevant chronology is not consistent with this particular explanation—see Appendix B for further analysis. Our findings are also unlikely to be related to TARP. The majority of lending to banks through TARP occurred in October and November of 2008, which, if supportive of mortgage origination, would work against our finding a strong reaction to the beginning of QE1. Likewise, improvement in bank health would likely encourage jumbo origination more than conforming loans, as the latter rely more heavily on market liquidity.

We examine three such factors introduced in section 3 and labeled W_t in equations (2) and (3) (guarantee fees, average premia on bank credit-default swaps, and mortgage market risk premia). After demonstrating that they are jointly and individually significant predictors of differential movements in the jumbo and conforming segments, we verify that our results are robust to the inclusion of these three factors as time-series controls measuring aggregate shocks to funding availability in these two segments. First, the guarantee fee ("g-fee") originators must pay to the GSEs to insure a loan's default risk has varied over time from just 20 basis points before the crisis to the 50 basis points in 2015, naturally affecting non-GSE and GSE-eligible loans differently. Second, we collect data on the credit-default swaps for all major financial institutions in the U.S. and construct an index by value weighting the corresponding five-year contract premia. FICO spreads measure the interest-rate premia charged borrowers of varying levels of credit-worthiness. Intuitively, the credit spread captures the expected level and cost of default, the g-fee reflects the price to avoid default risk, and the CDS index captures the overall financial health of the banking sector, each of which might influence the relative market supply of jumbo-mortgage credit. When g-fees increase, we expect the jumbo-conforming spread to decrease, and when credit spreads or CDS premia rise, we expect the jumbo segment to be hit harder than the conforming segment. These factors do play an important role in the mortgage market, together explaining 80% of the variation in the jumbo-conforming spread.

In order to have a stable estimate of the contribution of these three factors to interest rates and quantities not coincident with the policy change itself, we use the entire 2008–2013 sample period to estimate the coefficients on g-fee, FICO spread, and the CDS index in a linear regression following equations (2) and (3) with a jumbo indicator, other controls X, and county-segment and county-month fixed effects (omitting QE controls).[23] These coefficients are reported in Appendix Table 1; each factor is individually predictive of interest rates

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[23]The time fixed effects and six-month periods we use in estimating (2) and (3) are useful for identification of θ and ψ but pose a challenge for estimating segment-specific coefficients on the time-series factors W_t. Instead, we discipline these factor loadings by using the full time sample where we have sufficient variation to estimate precise relationships to estimate the coefficients γ and κ on the time-series controls.

and quantities. We then partial out these three factors, by subtracting the contribution of contemporaneous mortgage-credit spreads, g-fees, and CDS index from current interest rates and quantities to reestimate the specifications in (2) and (3), imposing $\hat{\gamma}$ estimated in Appendix Table 1. Testing whether our estimates $\hat{\theta}_2$ and $\hat{\psi}_2$ of the differential effect of LSAPs on the conforming and jumbo segments are sensitive to potential segment-specific shocks through the $W_t \times Jumbo_s$ terms provides a useful gauge of the validity of the conditional parallel-trends identifying assumption.

Panel II of Table 2 controls for county-month fixed effects, county-segment fixed effects, loan characteristics, and time-series controls interacted with the jumbo indicator. Loan-level controls ensure that the differential response of jumbo and conforming mortgages is not driven by time-varying borrower composition. Geography $\times$ time fixed effects absorb time-varying regional heterogeneity to account for any local shocks to fundamentals or beliefs that affect credit demand or supply. Geography $\times$ segment fixed effects allow for heterogeneity across counties in factors that affect the baseline level of jumbo mortgage issuance, such as the level of house prices, which determines the set of homes that are potential candidates as jumbo-mortgage collateral. These additional controls absorb the main program and jumbo indicator variables but allow us to focus on our main coefficient of interest.

Comparing the top and bottom panels of Table 2, we find that our conclusions based on the effect θ_2 of QE on the jumbo-conforming spread are relatively similar with and without the additional controls. The QE1 direct effect on conforming mortgage rates is 44 basis points stronger than its indirect effect on jumbo mortgages. QE2 and the MEP still have small and at most marginally significant differential effects across segments. Accounting for time-series controls, borrower composition, and local shocks does affect the QE3 and Tapering interest rate effects. Although small in magnitude, precision improves using the specification of panel II such that we can detect a 6 basis point increase and 16 basis point decrease in the jumbo-conforming spread in response to QE3 and the Tapering, respectively. We view these interest-rate results as supporting our strategy of using the segmentation in

the mortgage market as a natural experiment to trace the transmission of LSAPs through the refinancing channel.[24]

Panel II of Table 3 examines robustness to potentially confounding segment-specific shocks by additionally controlling for county-segment-time attributes, county-month fixed effects, county-segment fixed effects, and interactions of time-series factors W_t with a jumbo-segment indicator.[25] As before with interest rates, the estimated QE1 and QE2 effects are similar between panels I and II, and the MEP effect is again statistically insignificant and imprecise. The estimated differential effect of QE3 on jumbo quantity is significantly negative in panel II, albeit 20% of the magnitude of the QE1 coefficient in column 1. The Tapering effect is smaller and insignificant with controls, likely driven by a contemporaneous increase in GSE guarantee fees that Appendix Table 1 shows would be expected to spur a large relative increase in jumbo origination.

We conclude that QE1 MBS purchases had significantly different effects across mortgage market segments, particularly visible in quantities. Overall, these results demonstrate that, especially during QE1, the conforming segment was much more affected by Fed LSAPs than the jumbo segment. We next explore reasons for the differential impact of QE1 MBS purchases and QE3 MBS purchases as they are informative about how the transmission of monetary policy takes place; this same logic can be applied to explain the relatively small effects of Tapering.

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[24]We also investigate whether our results are confounded by differences in the sensitivity of jumbo and conforming mortgage rates to short and long interest rates. For example, it might be that jumbo mortgages are more sensitive to the short end of the yield curve, which might make them less responsive to policies that aim to decrease the long end of the yield curve. As reported in Appendix Table 2, we find that this is not the case. We perform univariate regressions relating mortgage rates to 5-year and 10-year Treasury rates in columns 1-4, and then we regress mortgage rates on both Treasury rates in columns 5 and 6. The univariate regressions show a very similar sensitivity of both types of mortgage rates to Treasury rates, as highlighted by the similar coefficients in columns 1-4. If anything, columns 5 and 6 show that the conforming rates seem to be more sensitive to the short end of the yield curve, ruling out this alternative explanation for our findings.

[25]Because the quantity regressions in Table 3 are at the county-segment-time level, this fixed effect structure nearly saturates the model, resulting in high R^2 measures and limiting the scope for omitted variables bias. Results with an intermediate set of fixed effects are similar.

5.2 Explaining the Differential Response Across QE Events

What drives the differential response to QE1 and QE3 given that both campaigns involved MBS purchases? The empirical findings and reasoning of Krishnamurthy and Vissing-Jørgensen (2013) suggest one explanation: as financial intermediaries were in greater distress during QE1 than QE3, LSAP funding should reallocate more across segments during QE3 than QE1 resulting in a stronger jumbo response during QE3 than QE1.[26] For example, the model of Greenwood, Hanson, and Liao (2018) predicts that upon the arrival of a large supply shock, the directly impacted market (e.g., the conforming market) initially overreacts to the supply shock, while related asset classes (e.g., jumbo MBS), underreact. Such a model suggests that a reduced presence of generalists due to financial constraints (or equivalently a more severe market segmentation such as the one experienced during QE1) leads to slower moving capital and a lower reaction of the jumbo market. In our context, it is plausible that both specialists (GSE MBS investors such as mortgage REITs) and generalists (banks that originate both GSE and jumbo mortgages) were capital constrained such that the neutrality results of Wallace (1981) and Eggertsson and Woodford (2003) did not hold during QE1. Comparing an environment in which banks are constrained versus an environment in which banks are less constrained, we should observe that there will be less spillover from MBS purchases to jumbo mortgage origination when banks are more capital constrained.

In practice, distressed banks may find it more difficult to fund jumbo-segment lending with proceeds from conforming-segment lending for several reasons. For example, with the jumbo securitization market largely frozen during QE1, jumbo origination was much more capital intensive than securitized conforming lending. Another reason is regulation; the capital a bank is required to hold against a jumbo mortgage is 2.5 times the amount it must hold against a GSE-guaranteed mortgages.[27]

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[26]Three commonly used metrics of banking-sector health each show dramatic improvements by the time QE3 began. Between QE1 and QE3, the capital ratios of stress-tested bank-holding companies increased from 6% to 11% (Federal Reserve Board, 2016), the average premia on 6-month CDS on the largest 50 banks decreased from 7.5 bp to 0.3 bp, and the TED Spread decreased from over 100 bp to under 25 bp.

[27]Under Basel I capital requirements for risk-weighted assets, the risk weight for a GSE mortgage is 0.2

To test whether time-series variation in bank distress can account for the differential impact of QE1 and QE3 on mortgage origination across market segments, we construct a novel panel dataset of mortgage origination volume at the bank-county-month level to compare the debt-issuance response to QE1 and QE3 of banks with varying levels of distress. We use Dataquick microdata on mortgage origination sourced from deeds records merged with Y-9C bank holding company data to aggregate refinance origination volumes to bank-county-month observations and estimate

$$\log Q_{bcst} = \beta_{0,s} QE_t + \beta_{1,s} QE_t \cdot Distress_b + \delta_{bs} + \alpha_{cst} + \varepsilon_{bcst}$$

where Q_{bcst} is the total dollar volume of refinance origination by lender b in county c in month t. We estimate this equation separately by segment s (jumbo and conforming), clustering by bank to allow for bank-specific errors to be arbitrarily correlated across segments, time, and geography.[28] Controls include a measure of bank distress (the amount of real-estate loans charged off in 2007-2008 normalized by total assets in 2008 quarter 4 as reported in Call Report data) interacted with the QE indicator variable, bank fixed effects (that absorb the main effect of bank distress) to capture any time-invariant heterogeneity among banks, and county-month fixed effects to absorb heterogeneity in bank exposure to distressed areas.[29]

We present the results of this estimation in Table 4. Panel I reports results for jumbo origination volumes. The coefficient of interest is the Program × Bank Distress interaction term. Columns 1 and 2 show that banks with higher real-estate losses originated substantially less credit on jumbo loans in response to QE1, a result that is robust to county × month fixed effects in column 2 showing that any potential sorting of distressed banks to distressed areas cannot explain the results. Columns 3 and 4 show that the origination pattern across lenders was different during QE3; a formal test that the QE1 and QE3 × Bank Distress coefficients are equal rejects at the 1% level.[30] Whereas distressed banks had been too impaired to take

and the risk weight for a prime whole loan held on balance sheet is 0.5.

[28] Our results are robust to a triple-interaction specification that pools jumbo and conforming loans.

[29] Our results are robust to bank-county-segment fixed effects and other measures of bank distress including CDS spreads, loan-loss provisions, and capital ratios.

[30] Our sample restriction to only include county-bank pairs that have at least 10 jumbo originations in

advantage of QE1 interest-rate changes and originated fewer jumbo loans, they were extra responsive to QE3, as these same banks ramped up their jumbo origination as seen in the QE3 indicator coefficient in column 3. Consistent with the hypothesis that intermediary distress and tighter capital constraints explain the difference between QE1 and QE3 effects, panel II shows that there is no significant differential reaction as a function of bank health to QE1 and QE3 in the conforming segment where capital requirements are the lowest.

Using cross-sectional variation in bank health, Table 4 provides a unifying rationale for the pattern of coefficients in Tables 2 and 3. Even though both QE1 and QE3 involved MBS purchases, distressed banks did not reallocate capital across mortgage-market segments during QE1 and did reallocate during QE3 when bank health had improved.[31] Overall, this helps explain why we find much smaller effects following the QE3 and Tapering announcements than during QE1 even though each event involved MBS transactions. These findings further support contrasting the jumbo and conforming mortgage segments in our analysis—especially during QE1—and provide further evidence that central-bank mortgage purchases were particularly effective during QE1 when the banking sector was impaired. Of course, our empirical strategy's bounding of causal effects by measuring differential impacts across segments is less informative during QE3 because we rely on mortgage market segmentation and the mortgage market was less segmented during QE3. However, we note that given the lack of differential response to QE3 despite QE3 including MBS purchases, at a minimum we can conclude that the composition of assets purchased matters less when the banking sector is readily reallocating capital. Moreover, similar to our estimated small main effects of QE3, other work (Krishnamurthy and Vissing-Jørgensen, 2013) found that QE3 had smaller announcement effects, potentially because it was anticipated or because asset

the given QE event window results in a larger number of available counties for QE3 than QE1. Our results are robust to restricting the sample to the county-bank pairs common to both samples.

[31]These findings are in line with the evidence recently provided by Rodnyansky and Darmouni (2017), who argue that the additional lending observed in the aftermath of QE3 was driven by the banks' improved financial conditions. Luck and Zimmermann (2018) show that during QE3 there were significantly more spillovers from the mortgage segment to the commercial and industrial (C&I) segment than during QE1. This external evidence corroborates our explanation for the differential effects of QE1 and QE3.

composition mattered less when the market wasn't as segmented as during QE1.

5.3   Addressing Endogeneity of Mortgage Segment Choice

One drawback to the empirical approach outlined above is that borrowers may endogenously respond to market conditions by switching to the GSE segment when refinancing (for example, by paying down their mortgages to be below the conforming loan limit or splitting their jumbo mortgage into a conforming first mortgage and a second mortgage). If so, the volume of jumbo origination may be artificially depressed in a way that complicates our cross-sectional comparison and prevents us from using the difference between conforming and jumbo borrowers as a lower bound on the effects of QE. We address this concern in several ways.

First, we reconsider our aggregate time series evidence excluding loans near the conforming loan limit (90-140% of the CLL) that are most able to endogenously switch segments.[32] Appendix Figure 2 shows that the strong differential in origination volumes persists even when considering only loans with an expensive and unlikely option to switch from the jumbo to the conforming segment. Second, we show that "debt relabeling," the practice of using a second mortgage to reduce first-mortgage size in refinancing, cannot account for the relatively sluggish jumbo origination response during QE1. Appendix Figure 3 plots the change in second-lien balance for refinancing borrowers reducing their balance. For both types of second mortgages, the vast majority of borrowers cash-in refinancing do not seem to be splitting a jumbo first mortgage into two smaller mortgages.[33]

Still, panel I of Appendix Figure 3 shows that switching from the jumbo to conforming

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[32]The fraction of loans in our data above 140% of the CLL that switch from the jumbo to conforming segments at refinancing is less than 2% during QE1, meaning that restricting the sample to this group effectively shuts down the option to switch segments.

[33]A related compositional concern is that loose QE credit might have led to mortgage origination to riskier borrowers in the LSAP-eligible segment and not the jumbo segment. Appendix Figure 4 plots the percent of loans delinquent within one year (panel I) and within four years (panel II) of origination for different types of refinances: GSE, FHA, cash-out and non-GSE. While rising overall property values and tighter credit are improving successive cohorts' loan performance (consistent with Palmer, 2015), default trends are smooth and unabated throughout QE1, suggesting that parallel trends in underwriting continued.

segment via cash-in refinancing wasn't uncommon among jumbo mortgagors who refinanced during QE1. To address the potential of this mortgage segment selection to confound our results, here we condition on ex-ante original mortgage status instead of ex-post (endogenous) new mortgage status, using the individual-level panel structure of our data to test whether each QE campaign altered the likelihood that a given mortgage was prepaid. Whereas a loan's segment as used in equations (2) and (3) is an outcome potentially affected by QE itself, individual-level regressions solve this selection problem by looking at results based on the *initial* loan segment and characteristics.[34] We estimate linear probability models

$$Prepay_{ict} = X'_{it}\theta + QE1_t \cdot X'_{it}\beta + A'_{it}\rho + \omega_{ct} + \zeta_{cs} + v_{ict} \tag{4}$$

where $Prepay_{it}$ is an indicator for whether loan i was paid off in time period $t \in \{\text{pre, post}\}$ denoting either the three months preceding or following QE1's announcement. The indicator $QE1_t$ is equal to one for the three months immediately following the announcement of QE1. Covariates X_{it} consist of loan-level controls including log loan size and indicators for jumbo categories and current LTV categories imputed using CBSA-level CoreLogic repeat-sales home-price indices to mark collateral values to market; and borrower-level controls including FICO, DTI, and a missing DTI indicator. We also control for a cubic A_{it} of loan age to flexibly capture the standard life-cycle of mortgage prepayment. The coefficients β of interest are multiply the QE1 indicator interacted with loan- and borrower-level controls. Estimated $\hat{\beta}$ can be interpreted as percentage-point effects on the likelihood of prepayment, and we have demeaned all continuous variables to facilitate interpretation of constant terms.

In Table 5, we report results from estimating (4), clustering at the loan level to allow for arbitrary correlation in shocks for a given borrower pre- and post-QE1. Column 1 shows that conforming mortgages saw a 5.6 percentage-point (295%) increase in individual prepayment likelihood following the beginning of QE1 MBS purchases. Jumbo loans and high-LTV loans

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[34]For this exercise, we construct a stratified random sample of 500,000 conforming and 250,000 jumbo fixed-rate first-lien mortgages secured by single-family homes originated between January 2002 and January 2008 and outstanding as of September 2008.

(LTV ratio exceeding 80%) were slightly less likely to prepay than conforming mortgages even before QE1, as shown by the negative main effects for the jumbo and over-80% LTV indicators in column 1, but this gap widened considerably following QE1. While the prepayment of non-conforming mortgages did increase in absolute terms following QE1, the response of outstanding high-LTV and jumbo mortgages was 60% and 40% smaller, respectively, than conforming mortgages.

Column 2 of Table 5 adds the full set of borrower and loan controls interacted with QE1 indicators, and column 3 adds county-segment and county-time fixed effects and finds similar results. Column 4 demonstrates that these findings are particularly concentrated among super-jumbo mortgages (balances over 140% of the CLL) and those with current loan-to-value ratios exceeding 90%. While borrowers with 80-90% current LTV ratios and loan balances 100-140% of the CLL are also less responsive to QE1, the super-jumbo segment and the over-90% LTV segment both have only cash-intensive ways to refinance to a conforming mortgage and saw little increase in prepayment behavior following QE1.[35] Columns 5 and 6 show that these effects are robust to our borrower-level controls, county-time, and county-segment fixed effects to absorb local credit demand shocks and fixed differences across segments that drive heterogeneity in county-level responses. These results have strong implications for the spatial distribution of unconventional monetary stimulus. QE1 credit seems to have benefitted the hardest hit areas—with the highest share of underwater homeowners—the least during the Great Recession (see Appendix D and Beraja et al., 2018).

The results of this section show that the strong differential results of QE1 on quantities seen in Table 3 is robust to dealing with the endogeneity of mortgage segment choice during QE1 in several ways: adjusting for bunching, checking for the prevalence of debt relabeling refinances, and estimating individual-level models conditioning on ex-ante mortgage choice and not ex-post.

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[35]Note that the net effect of QE1 for super-jumbo loans is slightly different than the QE1 × Super-Jumbo interaction term because of QE1 interactions with demeaned loan and borrower controls.

6 Real Effects on Refinancing and Consumption

6.1 Consumption Evidence

To test the extent to which refinancing results in an increase in refinancing activity and borrowers' consumption, we plot event studies around the origination of a refinance mortgage in Figure 4. Specifically, we examine the relationship between refinancing and durable goods purchases during QE1, measured using borrower-level credit-bureau data on auto loans linked with data on mortgage refinancing.[36] We restrict our attention to those borrowers who refinanced their mortgage during QE1 (December 2008 to March 2010) and leverage the panel structure of our data by following each borrower 12 months before and after refinancing with event studies of the form

$$y_{ikt} = \sum_{\tau \neq 0} \delta_\tau 1(t - \text{refinance month}_i = \tau) + \alpha_{kt} + \varepsilon_{ikt}$$

where y_{ikt} is either the monthly interest payment or an indicator for whether borrower i in balance quartile k and calendar-month t purchased a car (and financed at least some of that purchase to be observed by Equifax). The balance quartile k is the quartile of the loan size of borrower i's new refinance mortgage to allow for income-segment specific shocks and eliminate pre-trends. The parameters of interest are the event study coefficients δ_τ, which range from $\tau = -12$ to $\tau = 12$ and turn on in the month during which borrower i was τ months away from refinancing. The omitted category is borrowers in the same month their refinance mortgage was originated ($\tau = 0$), meaning that δ_2, for example, is the estimated average change in y for borrowers that refinanced two months ago compared with the level of y in the month they refinanced. Calendar-month $\times$ balance-quartile fixed effects α_{kt} capture any time-varying aggregate shocks to outcomes. An important consideration in interpreting these results is that the timing of interest-rate savings is not random, in contrast to the

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[36]These conditional-on-refinancing results are nearly identical when we instead restrict the sample to the QE3 period.

setting of Di Maggio et al. (2017), whose consumption response functions are identified by exogenous interest rate changes that do not require refinancing. To the extent that the borrowers refinance in response to (or in anticipation of) idiosyncratic shocks correlated with their demand for auto loans, our estimates of δ_τ are biased. The presence of α_{kt}, however, accounts for common shocks to credit demand.

Figure 4 plots estimated $\hat{\delta}_\tau$ for the effect of refinancing on monthly mortgage interest payments (panel I) and the probability of taking on a new auto loan (panel II, measured as an increase in outstanding auto-loan debt of more than \$5,000) along with 95% confidence intervals. Immediately after refinancing, there is a large and persistent decline in monthly mortgage interest payments, averaging \$250/month over the first year after refinancing. These interest savings appear to be spent at least partially on durable consumption. The coefficients plotted in panel II show an increase in the monthly probability of purchasing a car (a fairly rare event at baseline) of 12% in the year following refinancing relative to the year preceding. The effects start two months after refinancing and staying statistically significant with no signs of reversal over the following year. These results highlight the transmission of central-bank LSAPs to the household sector's debt origination and durable consumption through the refinancing channel.

6.2 Equity Extraction and Deleveraging

The extent to which households increase or decrease their leverage through refinancing has important implications for the transmission of LSAPs to aggregate consumption. In this section, we exploit the longitudinal dimension of our data to study the intensive margin of household leverage decisions during QE. We measure cash-in refinancing by linking each new refinance loan to the unpaid balance on the borrower's prior loan.[37] One key advantage of our panel data is that it allows us to observe loan amounts before refinancing and to estimate

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[37]We allow for \$3,000 closing costs to be rolled into the new loan without being classified as cash-in refinancing. See http://bankrate.com/finance/mortgages/closing-costs/closing-costs-by-state.aspx for data on average closing costs by state.

LTVs prior to refinancing using local home-price indices to impute current valuations for each property.[38]

Panel I of Figure 5 plots three kernel densities estimating the LTV distribution for three groups: (1) all outstanding loans with an LTV between 80% and 90%, (2) loans with an LTV between 80% and 90% that were refinanced during the period, and (3) the new refinance mortgages for borrowers who started with an original loan with an 80–90% LTV. Comparing the three curves, we find strong evidence that the differential availability and price of GSE eligible and GSE ineligible mortgages resulted in significant deleveraging. Whereas the LTV distribution for all outstanding mortgages (dotted green line) in this LTV range is relatively uniform, those that refinance (dashed red line) are much more likely to start near 80%, consistent with the results of section 5.3 and further evidence of the frictions associated with non-GSE–eligible refinancing during this time period. Comparing the original LTV distribution and the new LTV distribution for refinancers (solid blue line), it is clear that the GSE cutoffs were very important during this time period. Around 34% of households who prepay a mortgage that was 80–90% LTV at the time (and thus ineligible for a GSE-guaranteed refinance) deleverage and take out an 80% LTV mortgage, paying down their mortgage debt. For this (relatively small) subgroup of borrowers, the effect is economically meaningful: conditional on deleveraging to 80% or below, borrowers cashed-in about $12,300; conditional on deleveraging to 80%, borrowers cashed-in about $9,000. These results highlight the tightness of credit for a mortgage market segment that was not directly stimulated by Fed MBS purchases.

We can also measure the expansionary effects of LSAPs by looking at cash-out refinancing. Panel II of Figure 5 shows a bunching rate of about 22% with the average borrower cashing out $4,000. That is, about 22% of the refinances with an imputed LTV between 70 and 80% before refinancing decide to cash out from their mortgages by refinancing to a loan

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[38]To account for the introduction of the Home Affordable Refinance Program (HARP) by the FHFA in March 2009, which aimed to help underwater homeowners to refinance their mortgages, the results in this section focus on the pre-HARP period.

with an 80% LTV. This household balance-sheet response to interest rate changes and its dependence on current home equity highlights how accommodative monetary policy may be ineffective at helping distressed regions with less equity to extract compared to areas with lower outstanding LTVs, as discussed in Appendix D.[39] Overall, the aggregate effects of cash-out refinancing significantly outweigh the effects of cash-in refinancing, as we detail in section 6.3.

The extent of bunching around GSE cutoffs and the amount of cash-in and cash-out refinancing lead to several conclusions. First, segmentation in the mortgage market is particularly strong during banking-sector turmoil, as revealed by the leveraging and deleveraging decisions of borrowers during the crisis. Second, policies that allow for negative-equity refinancing (e.g., HARP) could reduce the effects of segmentation by expanding the segment with the largest credit supply and enabling refinancing among borrowers otherwise ineligible for GSE loans. Third, these findings highlight that central bank mortgage purchases can be more effective when paired with accommodating GSE policy. Countercyclical macroprudential tools such as loosening LTV constraints during a crisis could complement LSAPs and further stimulate the economy—see Appendix C for further discussion.

6.3   Aggregate Effects

In this section, we quantify the stimulative effects of QE by estimating the amount of additional refinancing and consumption attributable to QE1 MBS purchases. There were many contemporaneous sources of mortgage-market stimuli during this time period, including declines in the Federal Funds Rate and long-maturity Treasury yields. Nevertheless, the empirical model presented in Table 5 provides a strategy for characterizing the contribution of MBS purchases to aggregate refinancing volumes using jumbo mortgages as a counterfactual

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[39]In addition to the LTV bunching of Figure 5, Appendix Figure 5 plots cash-in and cash-out decisions in relation to the CLL. Consistent with the results on bunching to the 80% LTV, we find that about 43% of households who prepay a GSE-ineligible jumbo mortgage delever to the CLL, with an average cash-in amount of about $31,000 ($73,000 for mortgagors bunching around 100% of the CLL). Panel II shows some but less bunching from below the CLL, with only about 10% of the borrowers bunching at the CLL using the new mortgage to cash out an average of $2,000.

for conforming mortgages. The parallel trends of Figures 2 and 3 except when the Fed is purchasing MBS and banks are impaired support this identifying assumption. Moreover, when the Fed is not purchasing MBS, jumbo and conforming interest rates respond similarly to changes in Treasury rates (see Appendix Table 2).

Based on the similar sensitivity across segments to other sources of aggregate shocks to the mortgage market (including, for example, to QE2), we can interpret the difference in refinancing likelihood observed in Table 5 as an impact Fed MBS purchases. Of course, it need not be the case that the effect of QE1 on jumbo origination is zero. However, attributing only the *difference* between conforming and jumbo refinancing likelihoods to QE1 MBS purchases is conservative and allows for the possibility that there were some spillovers from MBS purchases to jumbo origination. While our identification strategy can only identify relative differences in origination (i.e. that the conforming segment responded much more to QE1), we note that spillovers would bias down our estimates of QE1 MBS effects: in the absence of spillovers, the difference between jumbo and conforming refinancing (our measure of the causal effect of QE1 MBS purchases) would have been larger.

The coefficients in Table 5 suggest that there was 43% more conforming-segment refinancing during the first three months of QE1 than if conforming loans had instead responded to QE1 similarly to jumbo loans.[40] Combining this estimate with the relevant total amount of refinancing implies that QE1 MBS purchases led to an additional 220,000 households refinancing and an additional $50 billion in refinancing volume during the first three months of QE1.[41] A similar calculation yields an estimate of 454,000 additional households refinancing during the first six months of QE1 because of QE1 MBS purchases, totaling $102 billion additional refinancing.[42] Over the entire QE1 period, we find 1.088 million additional loans

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[40]The predicted refinancing rate for conforming mortgages during QE1 is the sum of the constant and QE1 indicator coefficients, 7.3% in column 2. The QE1 × Jumbo coefficient of -2.2% then implies that the actual response was 7.3/(7.3-2.2) = 143% of the response if conforming mortgages had responded to QE1 similarly to jumbo mortgages.

[41]Our 43% estimate implies that there would have been 30% less refinancing but for QE1, which we apply to our estimates that there were 734,000 fixed-rate conforming mortgages refinanced (totaling $166 billion) during the first three months of QE1.

[42]To estimate effects over the first six months of QE1, we re-estimate Table 5 with the QE1 period

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totaling $237 billion in mortgage origination attributable to QE1 MBS purchases, although admittedly the conditional parallel-trends assumption may be less likely to hold over this longer estimation horizon.[43]

An increase in refinancing increases consumption through multiple channels. First, many borrowers cash out equity while refinancing, providing cash on hand to support new expenditures. Second, by refinancing their loans, borrowers are able to secure lower interest rates, and increase their monthly disposable income. On average, the net amount of equity cashed out is about 11% of the total volume of refinancing (accounting for closing costs and the small fraction of borrowers who cash-in refinance). As commonly assumed in the MPC literature (e.g., Mian and Sufi, 2011), we assume the marginal propensity to consume out of cashed-out equity is 1 (the assumption being that borrowers cash-out equity to spend it). This translates into an increase in mortgagors' consumption of about $11.22 billion over the first six months of QE1. We also compute the increase in consumption due to interest savings. Specifically, households refinancing in 2009 saved on average $3,000 per year due to the lower interest rates (see discussion in section 6.1). Per Di Maggio et al. (2017), the average marginal propensity to consume out of a decrease in monthly interest payments is about 0.75. This implies an additional $1.02 billion in consumption from MBS purchases through lower interest payments (454,000 loans × $3,000 × 0.75) due to interest rate savings. Moreover, under the same identifying assumptions above, panel II of Table 2 implies that MBS purchases resulted in 44 bps lower rates for those who refinanced to a conforming mortgage during QE1. This means that for the inframarginal $309 billion of GSE refinancing, borrowers realized $309 billion × 44 bps = $1.36 billion of marginal savings on their annual interest payments, resulting in an additional $1.0 billion of consumption.[44] Putting these

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ranging from December 2008 to May 2009, finding a 34% refinancing effect. We estimate that 1.818 million households refinanced fixed-rate conforming mortgages over the first six months of QE1, totaling $409 billion in origination volume.

[43]Estimating equation (4) with the usual three-month pre-period and a post period spanning all of QE1 leads to an estimated 38% increase in conforming versus jumbo mortgages, which we apply to a base of 3.886 million loans and $846 billion refinanced during QE1.

[44]There was $409 billion of fixed-rate conforming refinancing during the first six months of QE1, of which we estimate $100 billion was caused by QE1 MBS purchases (instead of an equivalent amount of Treasuries),

channels together, we estimate that the decision to purchase MBS instead of exclusively Treasuries during QE1 increased aggregate consumption through the refinancing channel by approximately $13.5 billion.

Our results on the allocation of credit to the conforming segment have straightforward policy implications—if the Central Bank can only purchase government-guaranteed mortgages, then countercyclical policy could expand the relevant eligibility requirements. In Appendix C, we illustrate the stimulative potential of unconventional monetary policy–complementary GSE policy by modeling the additional refinancing that would have occurred in response to QE1 had the GSEs increased maximum allowable LTV ratios from 80 to 90%. We find that such a policy could have increased overall refinancing by 9% and equity extraction by 28%. While the extensive-margin refinancing increase is driven most by borrowers with LTVs before refinancing of 80-90%, we estimate that the largest consumption response would come from borrowers with 60-70% LTVs, who would choose to cash-out additional equity in under a 90% LTV cap, further increasing aggregate effects.

7   Conclusion

Prior to the fall of 2007, the Fed had largely held Treasury securities on its balance sheet. However, in response to the financial crisis, the Fed started several new programs including targeted purchases of trillions of dollars of long-term Treasuries and GSE-guaranteed mortgage-backed securities. The ultimate impacts of these unconventional monetary policies—whether and how they affect the real economy—have been the subject of ongoing debate.

In this paper, we focus on demonstrating and quantifying the pass-through of unconventional monetary policy to the real economy through the mortgage market. We show that Quantitative Easing works through a refinancing channel by improving credit availability

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meaning approximately $309 billion of refinancing would have happened irrespective of the assets purchased during QE1.

and lowering interest rates for affected households. Especially during QE1, declining long-run interest rates passed through to borrowers who were able to refinance into mortgages bundled into MBS purchased by the Fed but significantly less to borrowers who didn't qualify for a GSE-eligible mortgage. Our results imply that during the first three months of QE1, MBS purchases increased refinancing by 56%. Over the first six months of QE1, MBS purchases increased origination by $102 billion, leading to a boom in equity extraction, reducing interest payments, and increasing aggregate in consumption by $13.5 billion.

A key channel through which Quantitative Easing works is by improving credit availability and lowering interest rates for affected households. Aggregate demand increases from refinancing households consuming much of their cashed-out equity and monthly mortgage payment savings. An implication of the targeted nature of the Fed's MBS program is that the borrowers who benefitted the most from monetary stimulus during the recession had relatively high levels of home equity or cash-on-hand and disproportionately lived in the least hard-hit areas. Our counterfactual exercise finds that a simple countercyclical macro-prudential policy lever raising the GSE LTV eligibility cutoff from 80% to 90% would have resulted in a 9% increase in refinanced loans and a 28% increase in equity cashed out during QE1, further supporting aggregate demand.

There are several implications of these findings for designing effective unconventional monetary policy. First, Federal Reserve Act provisions that restrict Fed purchases to government-guaranteed debt have real consequences in allocating credit to certain sectors (i.e., housing) and particular segments within those sectors (i.e., conforming mortgages). Even operating within the legal constraints that govern Federal Reserve purchases, it appears preferable for LSAPs to purchase MBS directly instead of Treasuries during times when banks are reluctant to lend on their own. Relatedly, central-bank interventions could be more effective by providing more direct funding to banks for lending to credit-constrained small business and households. Finally, we demonstrate a strong interaction between GSE policy and the effectiveness of MBS purchases. Tight GSE-eligibility requirements and the segmented response

to LSAPs likely dampened the multiplier effects of lower interest rates, suggesting that countercyclical macroprudential policy could enhance the effectiveness of MBS purchases. In particular, our counterfactual simulations show that relaxing LTV caps during the crisis would have benefitted economically distressed areas proportionally more by enabling more households to refinance and by reducing household deleveraging.

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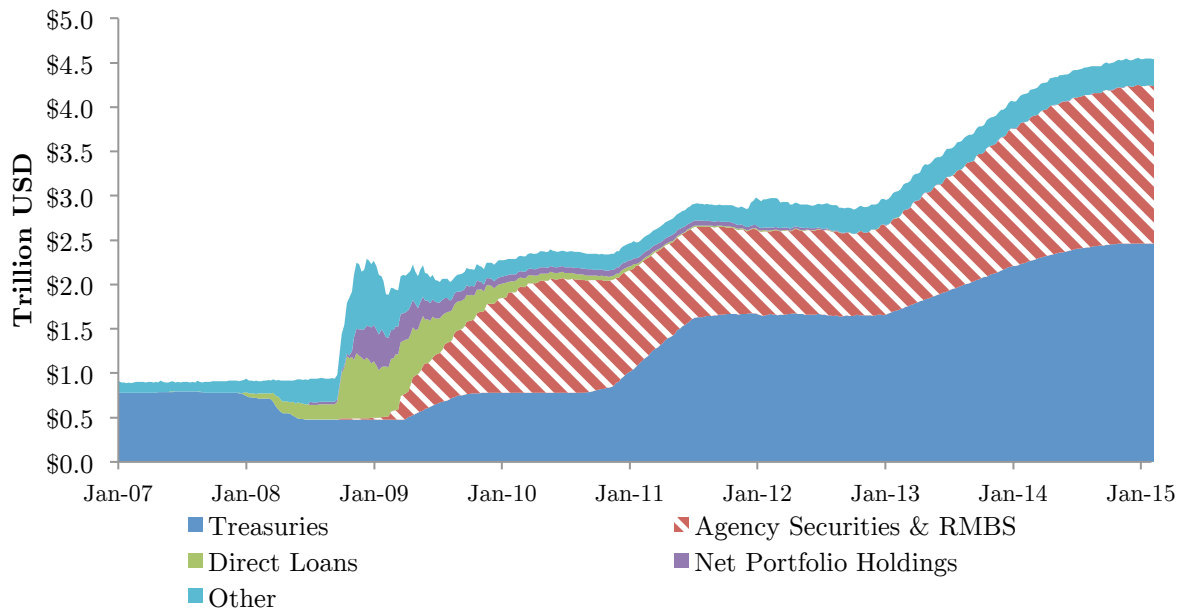
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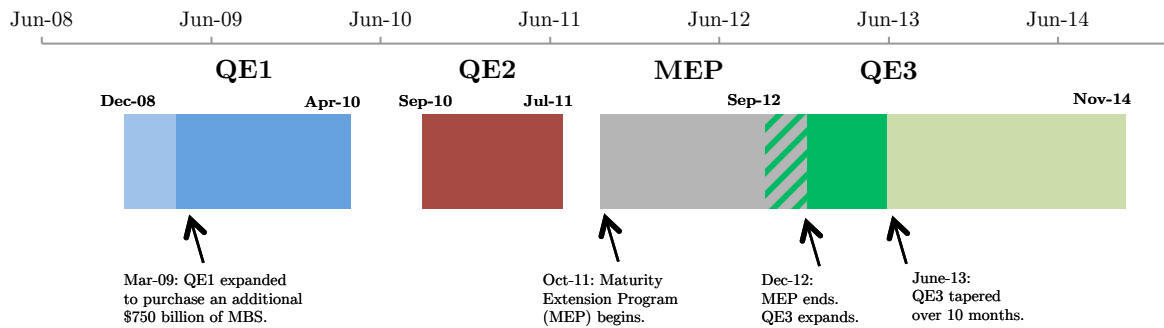
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Figure 1. Federal Reserve Balance Sheet and QE Timeline

Panel I. Federal Reserve Balance Sheet: Assets

Legend:
- Treasuries
- Agency Securities & RMBS
- Direct Loans
- Net Portfolio Holdings
- Other

Panel II. Quantitative Easing Timeline

QE1 — Dec-08 to Apr-10
Mar-09: QE1 expanded to purchase an additional $750 billion of MBS.

QE2 — Sep-10 to Jul-11

MEP — Sep-12
Oct-11: Maturity Extension Program (MEP) begins.

Dec-12: MEP ends. QE3 expands.

QE3 — Nov-14
June-13: QE3 tapered over 10 months.
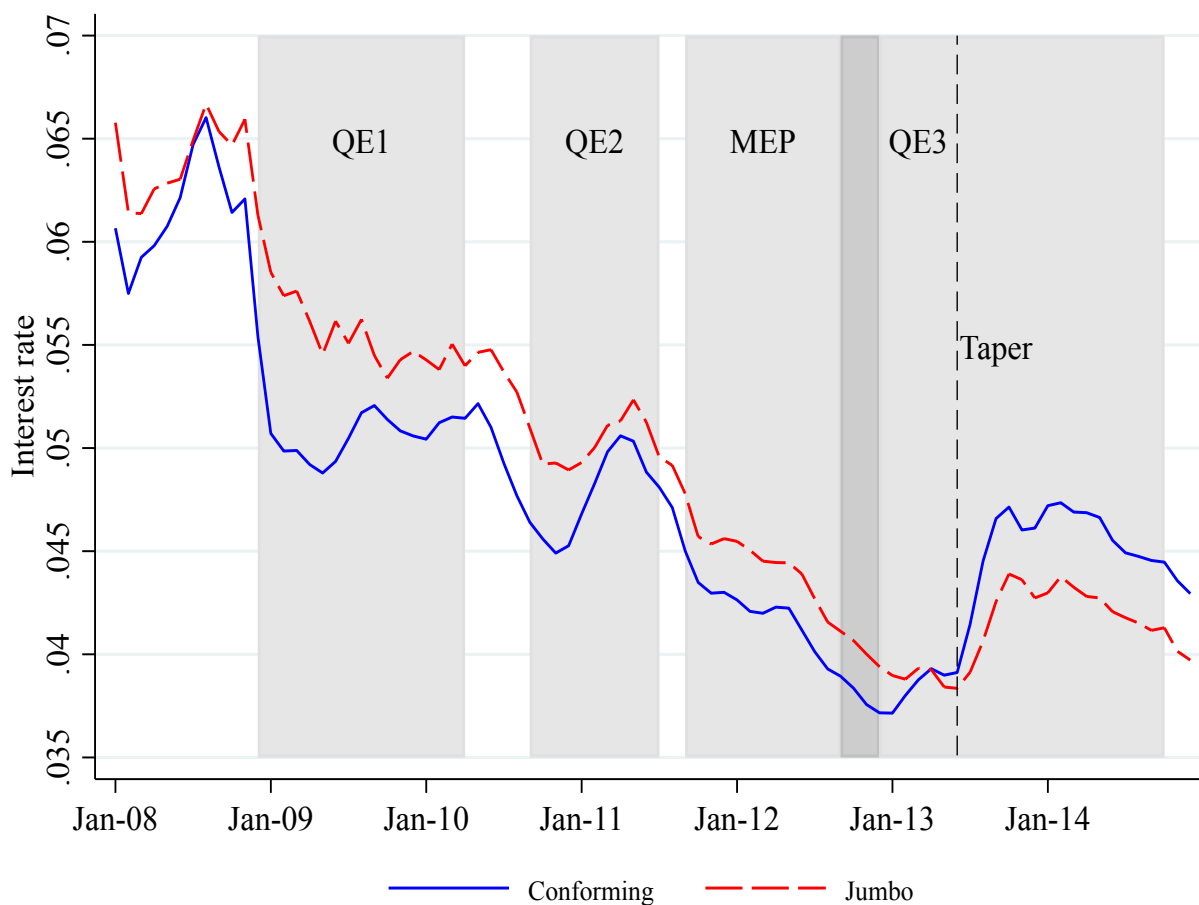
Notes: Panel I graphs the size and the composition of assets on the Fed Balance sheet from 2007-2015 sourced from Fed H4.A1041 weekly reports. Panel II plots the timeline of the main LSAPs implemented after the financial crisis using Krishnamurthy and Vissing-Jorgensen (2011, 2013) dates.

Figure 2. Interest Rates for Conforming and Jumbo Refinance Loans

Notes: Figure plots the estimated monthly interest rates for 30-year fixed-rate refinance loans above and below the confoming loan limit with a loan-to-value ratio of 75% and a FICO score of 760. The estimates are based on non-FHA first-lien refinance loans backing single-family homes without prepayment penalties, balloon features, or interest-only periods in LPS with LTV less or equal to 80% and adjusted for the LTV and credit score of the borrower. See Section 4.1 for more details.

Figure 3. Refinance Origination Volume

Panel I. Number of Originated Mortgages
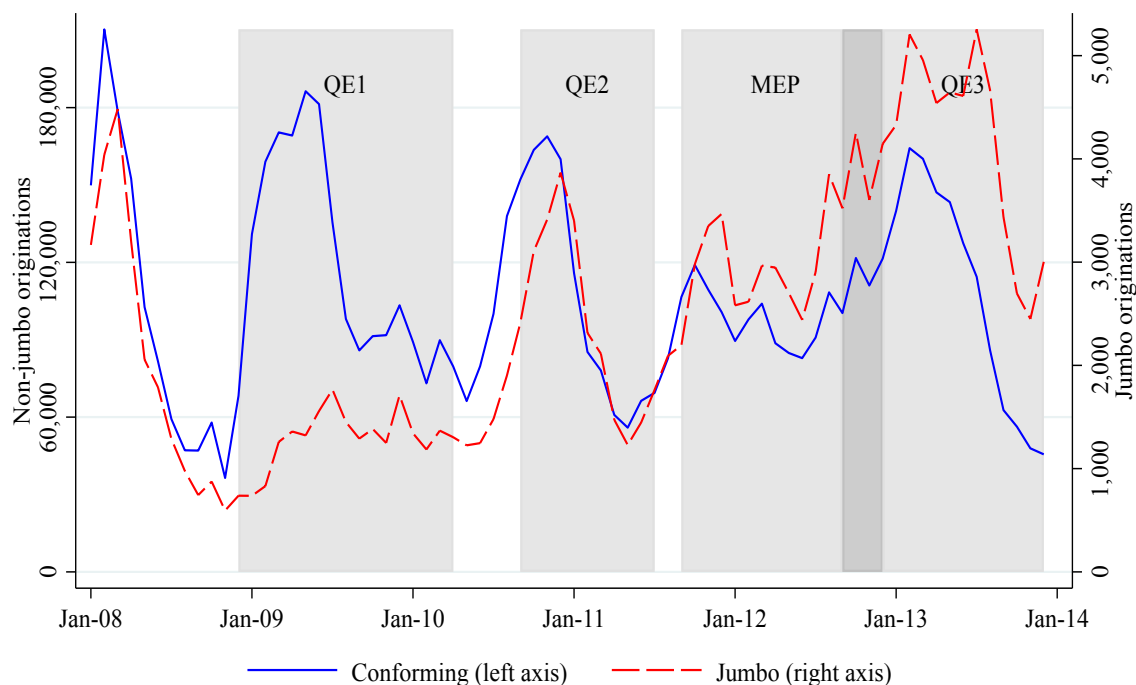
Panel II. Origination Volume
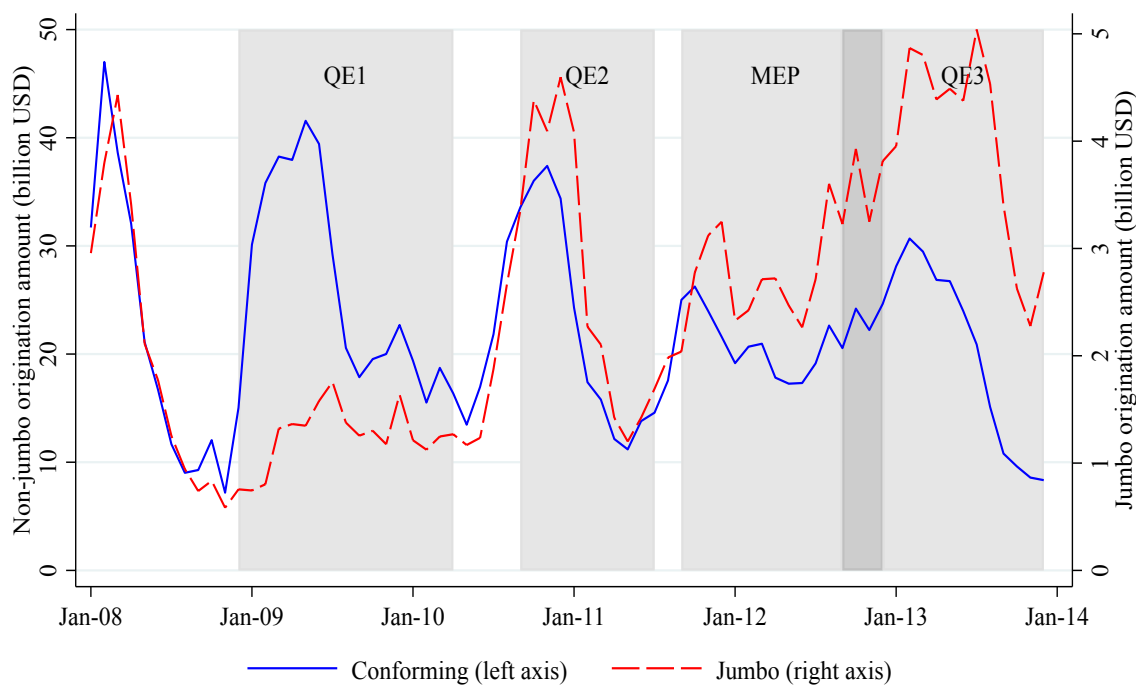
Notes: Figure plots the number of originations (top panel) and the origination volume (bottom panel) of refinance first-lien non-FHA mortgages securing single-family homes below the conforming loan limit and above the conforming loan limit as recorded by LPS. FHA loans are excluded from the data.

Figure 4. Refinance Event Studies of Interest Payments and Car Purchases

Panel I. Effect of Refinancing on Mortgage Interest Payments
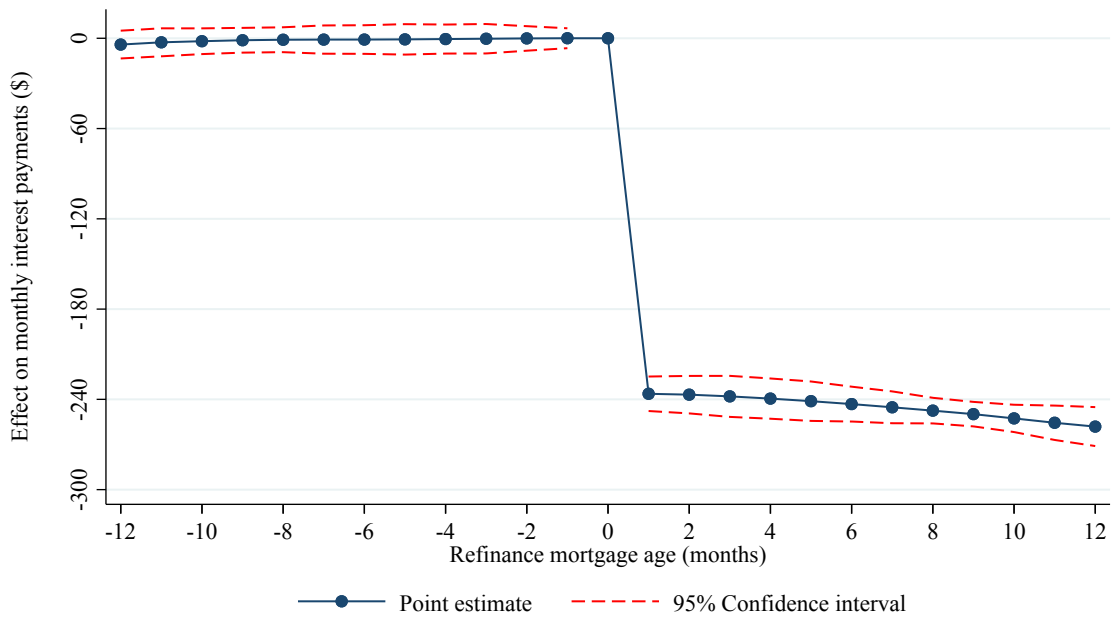
Panel II. Effect of Refinancing on Car Purchases
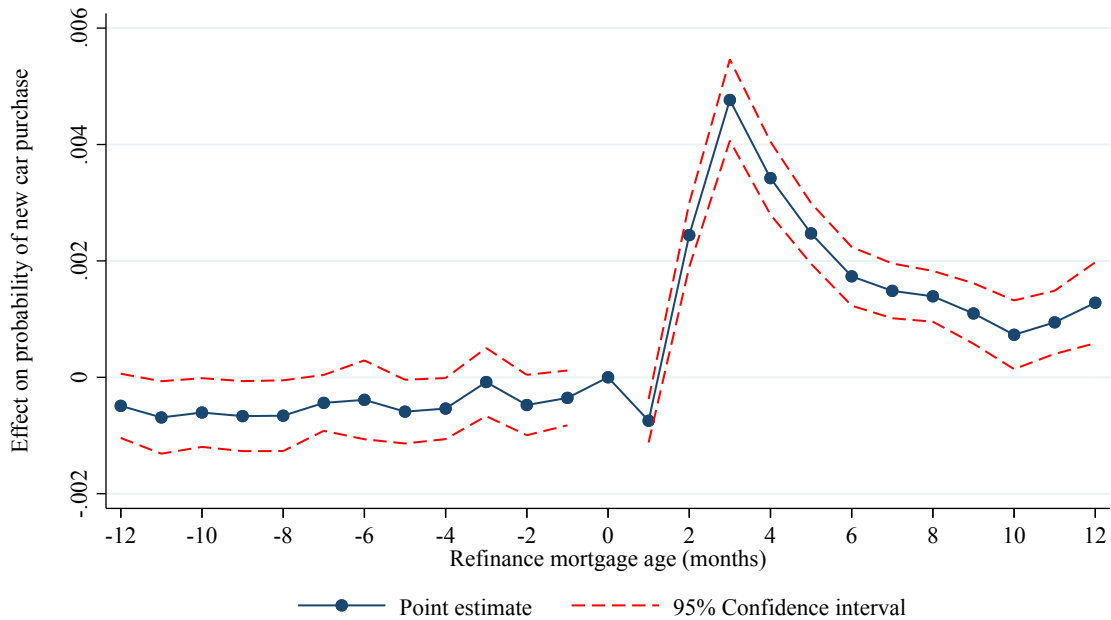
Notes: Figure plots event study coefficients of monthly mortgage interest payments (panel I) and the likelihood of purchasing a car (panel II, measured as increasing outstanding auto-related debt by more than $5,000) on time until (or since) refinancing. The sample is restricted to borrowers who refinanced during the QE1 period. See Section 6.1 for more details.

Figure 5. Loan-to-Value Ratio Bunching

Panel I. LTV Distribution for Original LTVs 80-90%

Average cash-in: $2k, bunching rate: 34%, bunching average cash-in: $9k

- – – – LTV before refinancing
- ——— LTV after refinancing
- ·········· LTV on outstanding loans (Dec. 2008)

Panel II. LTV Distribution for Original LTVs 70-80%

Average cash-out: $4k, bunching rate: 22%, bunching average cash-out: $9k

- – – – LTV before refinancing
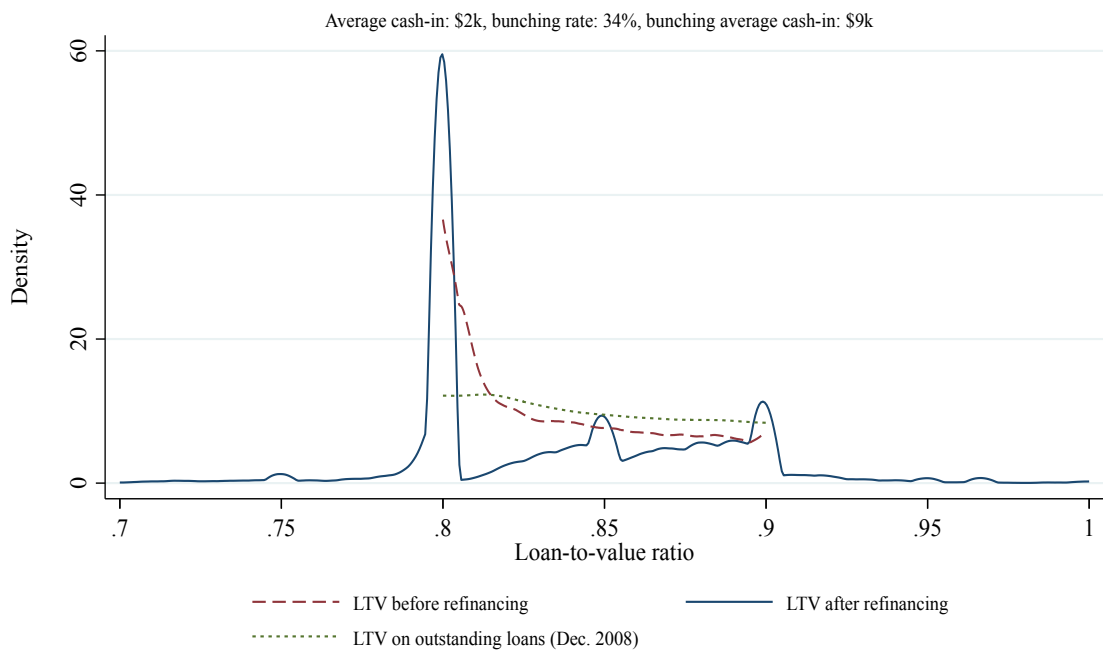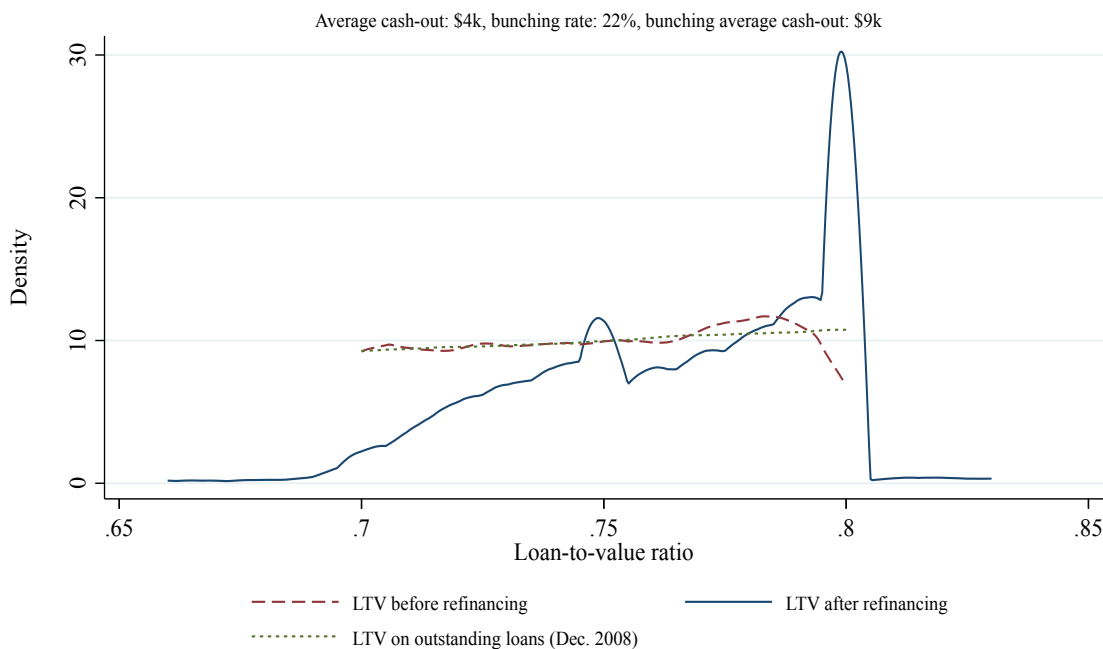- ——— LTV after refinancing
- ·········· LTV on outstanding loans (Dec. 2008)

Notes: Figures plot the distribution of borrower LTV ratios during QE1 before the start of the Home Affordable Refinance Program (Dec. 2008-May 2009). Panels I and II include loans where the imputed LTV on the predecessor loan was 80-90% and 70-80%, respectively. Dotted lines plot the LTV distribution of all outstanding loans (not conditional on refinancing). Dashed lines plot the LTV distribution for mortgages refinanced during the period. Solid blue lines plot the distribution of actual LTV ratios for originated refinance mortgages. Bunching rate is share of refinanced loans with LTV 79.5-80.5% at origination. Reported average cash-in (out) is average amount borrowers refinancing provide (extract) at the closing of their new refinance mortgage, accounting for rolling $3,000 of closing costs into new balance.

Table 1. Summary Statistics

	Mean	Std. Dev.	10th	50th	90th
		Panel I. Conforming Loans			
LTV (%)	65.90	22.18	36.83	68.38	88.96
FICO Score	752.19	51	681	766	805
Interest Rate (%)	4.54	0.97	3.38	4.50	5.75
Balance ($)	206,958	118,172	79,000	180,000	385,000
Observations			6,684,123		
		Panel II. Jumbo Loans			
LTV (%)	64.49	24.10	40.86	66.84	80.00
FICO Score	761.59	36.22	709	770	801
Interest Rate (%)	4.09	1.15	2.75	3.88	5.63
Balance	1,033,282	1,208,891	535,000	815,000	1,494,000
Observations			155,787		
		Panel III. Time-Series Controls			
Guarantee Fees (bp)	34.05	11.03	25.70	28.92	56.07
CDS Spread (bp)	115.62	122.29	16.41	80.51	240.61
FICO Credit Spread (bp)	6.24	3.33	1.03	6.58	9.59
Observations			72		

Notes: Panel I & Panel II report loan-level summary statistics on conforming and jumbo loans from Equifax's CRISM database which merges McDash Analytics mortgage servicing records from Lender Processing Services with Equifax credit bureau data. Panel III reports summary statistics for time series controls used in robustness checks, including guarantee fees from Fuster et al. (2013), CDS index spreads from Markit, and the FICO credit spread as calculated by the authors using CRISM.

Table 2. Effect of QE Commencement on Interest Rates by QE Program

Program	(1) QE1	(2) QE2	(3) MEP	(4) QE3	(5) Tapering
Panel I. Without Controls					
Program Indicator	-120.607***	-36.271***	-47.290***	-19.874***	18.711
	(14.341)	(9.808)	(7.045)	(5.568)	(11.642)
Jumbo Indicator	26.246***	45.060***	33.398***	12.668*	-4.955**
	(8.029)	(12.810)	(7.835)	(7.033)	(2.161)
Program x Jumbo	55.188**	-5.143	6.051	2.467	-14.532
	(18.762)	(13.640)	(10.457)	(7.955)	(12.765)
Observations	466,831	604,596	450,059	527,983	674,959
R-squared	0.382	0.151	0.176	0.041	0.029
Panel II. With Controls					
Program x Jumbo	43.916***	-6.611*	-5.002	6.392***	-15.649**
	(5.337)	(3.187)	(2.961)	(1.648)	(5.945)
Controls:					
Loan-level	Yes	Yes	Yes	Yes	Yes
Time-Series x Jumbo	Yes	Yes	Yes	Yes	Yes
County-Month FEs	Yes	Yes	Yes	Yes	Yes
County-Segment FEs	Yes	Yes	Yes	Yes	Yes
Observations	466,831	604,596	450,059	527,983	674,959
R-squared	0.616	0.599	0.684	0.614	0.615

Notes: The table reports regression coefficients relating loan-level mortgage interest rates (in basis points) to indicated unconventional monetary policy programs. QE, MEP ("Maturity Extension Program"), and Tapering Indicators are dummy variables equal to one after the introduction of each program. The sample includes single-family, first-lien, 15/20/30-year term, fixed-rate, non-FHA refinance mortgages with nonmissing LTVs. Jumbo Indicator is a dummy equal to one for jumbo loans. Program x Jumbo is the interaction between the program dummies and Jumbo Indicator. The event window includes the three months before/after the beginning month of each program period (e.g. QE1 sample is Sep2008-Feb2009). Specifications in Panel II control for 5-point LTV bins, 20-point FICO bins, a categorical interaction of interest rate type, interest-only indicator, and original term, an indicator for missing FICO, county-month fixed effects, and county-segment fixed effects. The contribution of interactions of the Jumbo indicator with GSE guarantee fees, mortgage credit spreads, and bank credit default swaps on interest rates over the entire sample period were also subtracted before the Panel II specifications were run. (See Appendix Table 1 for the coefficients of these time-series controls.) Standard errors are clustered at the month-segment level. Asterisks denote significance levels (***=1%, **=5%, *=10%).

Table 3. Effect of QE Commencement on Log Refinance Origination Volumes by QE Program

Program	(1) QE1	(2) QE2	(3) MEP	(4) QE3	(5) Tapering
	Panel I. Without Controls				
Program Indicator	1.019***	0.597***	0.544***	0.122	-0.346**
	(0.279)	(0.164)	(0.075)	(0.080)	(0.139)
Jumbo Indicator	-2.138***	-2.169***	-1.757***	-1.543***	-1.435***
	(0.156)	(0.188)	(0.116)	(0.098)	(0.036)
Program x Jumbo	-0.831**	0.067	-0.057	0.060	0.416**
	(0.289)	(0.208)	(0.143)	(0.114)	(0.146)
Observations	492	492	492	492	492
R-squared	0.637	0.560	0.466	0.355	0.292
	Panel II. With Controls				
Program x Jumbo	-0.810***	0.073	0.231	-0.151**	0.230
	(0.197)	(0.104)	(0.154)	(0.066)	(0.157)
Controls:					
Loan Composition	Yes	Yes	Yes	Yes	Yes
Time-Series x Jumbo	Yes	Yes	Yes	Yes	Yes
County-Month FEs	Yes	Yes	Yes	Yes	Yes
County-Segment FEs	Yes	Yes	Yes	Yes	Yes
Observations	492	492	492	492	492
R-squared	0.975	0.991	0.988	0.988	0.994

Notes: Table reports regression coefficients relating county x month x mortgage segment log refinancing volumes to unconventional monetary policy programs. The left-hand side variable is the log dollar volume of refinanced mortgages at the county-month-segment level as reported in the CRISM data. QE, MEP ("Maturity Extension Program"), and Tapering Indicators are dummy variables equal to one after the introduction of each program. Jumbo Indicator is a dummy equal to one for jumbo loans. Program x Jumbo is the interaction between the program dummies and Jumbo Indicator. The sample includes single-family, first-lien, non-FHA refinance mortgages with nonmissing LTV values. Counties are included in the sample if they have a positive number of jumbo originations in every sample month. The event window includes the three months before/after each QE period (e.g. QE1 sample is Sep2008-Feb2009). Specifications in Panel II include controls for average FICO, average LTV, the fraction not missing a FICO score for all mortgages originated in that county-month-segment, county-month fixed effects, and county-segment fixed effects. The contribution of interactions of the Jumbo indicator with GSE guarantee fees, mortgage credit spreads, and bank credit default swaps on log refinancing volumes over the entire sample period were also subtracted before the Panel II specifications were run. (See Appendix Table 1 for the coefficients of these time-series controls.) Standard errors are clustered at the month-segment level. Asterisks denote significance levels (***=1%, **=5%, *=10%).

Table 4. Bank-level Effects of QE1 and QE3 on Refinance Origination Volumes

	(1)	(2)	(3)	(4)
Program	QE1		QE3	
Panel I. Log(Jumbo Volume)				
Program x Bank Distress	-0.667**	-0.643***	0.208	0.276
	(0.249)	(0.176)	(0.199)	(0.174)
Program Indicator	0.024		0.828***	
	(0.527)		(0.152)	
Bank Fixed Effects	Yes	Yes	Yes	Yes
County-Month Fixed Effects		Yes		Yes
Observations	510	510	1,314	1,314
R-squared	0.063	0.350	0.092	0.372
Panel II. Log(Conforming Volume)				
Program x Bank Distress	-0.099	-0.166	0.034	0.026
	(0.256)	(0.277)	(0.065)	(0.062)
Program Indicator	0.847*		0.092*	
	(0.409)		(0.051)	
Bank Fixed Effects	Yes	Yes	Yes	Yes
County-Month Fixed Effects		Yes		Yes
Observations	510	510	510	510
R-squared	0.346	0.612	0.346	0.612

Notes: Table reports regression coefficients relating a bank-level measure of bank distress to county-bank level log quantities of jumbo and conforming loans issued in QE1 and QE3. The bank distress measure is equal to the sum of net charge-offs for loans related to real-estate across all quarters in 2007 and 2008, normalized by total assets in the last quarter of 2008. Program is a dummy equal to one for the introduction of each program. The event window includes the three months before/after each QE period (e.g. QE1 sample is Sep2008-Feb2009). Counties are included in the sample if they have a positive number of jumbo originations in every month between 2008 and 2013. Furthermore, county-bank pairs are included in the sample if they have at least 10 jumbo refinance originations in the given QE event window. Standard errors are clustered at the bank level. Asterisks denote significance levels (***=1%, **=5%, *=10%).

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Table 5. Loan-level Estimates of QE1 Effect on Prepayment Indicator

	(1)	(2)	(3)	(4)	(5)	(6)
QE1 Indicator	0.056***	0.053***		0.058***	0.055***	
	(0.001)	(0.001)		(0.001)	(0.001)	
QE1 x Jumbo	-0.022***	-0.022***	-0.023***			
	(0.001)	(0.001)	(0.001)			
QE1 x 100-140% CLL				-0.009***	-0.009***	-0.010***
				(0.001)	(0.001)	(0.001)
QE1 x Super-Jumbo				-0.059***	-0.058***	-0.063***
				(0.001)	(0.001)	(0.001)
QE1 x Over 80% LTV	-0.033***	-0.027***	-0.019***			
	(0.001)	(0.001)	(0.001)			
QE1 x 80-90% LTV				-0.027***	-0.022***	-0.018***
				(0.001)	(0.001)	(0.001)
QE1 x Over 90% LTV				-0.047***	-0.039***	-0.027***
				(0.001)	(0.001)	(0.001)
Jumbo Indicator	-0.002***	-0.002***				
	(0.001)	(0.001)				
100-140% CLL Indicator				-0.002***	-0.002***	
				(0.001)	(0.001)	
Super-Jumbo Indicator				-0.003***	-0.003***	
				(0.001)	(0.001)	
Over 80% LTV Indicator	-0.011***	-0.012***				
	(0.000)	(0.000)				
80-90% LTV Indicator				-0.009***	-0.009***	
				(0.000)	(0.000)	
Over 90% LTV Indicator				-0.016***	-0.017***	
				(0.000)	(0.000)	
Constant	0.019***	0.020***		0.019***	0.020***	
	(0.000)	(0.000)		(0.000)	(0.000)	
Loan Controls x QE1	Yes	Yes	Yes	Yes	Yes	Yes
Borrower Controls x QE1		Yes	Yes		Yes	Yes
County-Segment Fixed Effects			Yes			Yes
County-Time Fixed Effects			Yes			Yes
Observations	1,219,140	1,219,140	1,219,140	1,219,140	1,219,140	1,219,140

Notes: Table reports loan-time-level linear probability model estimates of a prepayment indicator on loan and borrower characteristics and QE1 indicator interactions. Sample includes first-lien 30-year, fixed-rate mortgages with nonmissing LTVs for two time periods: three months before/after QE1 announcement. Loan controls are log loan balance, current LTV bin, and non-super jumbo and super-jumbo segment indicators (current balance 100-140% and 140%+ of conforming loan limit). Borrower controls are DTI, missing DTI dummy, and FICO bin. All continuous variables are demeaned; all columns control for a cubic in loan age. Standard errors clustered by loan.

Online Appendix

A Quantitative Easing Background

In this section, we provide a brief summary of the Federal Reserve's Quantitative Easing program and discuss how its MBS purchases were conducted on the secondary mortgage market. For reference, panel II of Figure 1 provides a timeline of the various Fed LSAP programs and panel I of Appendix Figure 6 shows the time series of asset purchases and sales.

In late November 2008, the Fed announced its mortgage-buying program (known as QE1) with the intent to purchase about $500 billion in mortgage-backed securities, consisting of mortgages guaranteed by Fannie Mae, Freddie Mac, and to a lesser extent, Ginnie Mae. In March 2009, the Fed announced an expansion to this program, subsequently purchasing an additional $750 billion in mortgage-backed securities with 50-70% of agency originations each month ending up on the Fed's balance sheet (Appendix Figure 6). QE1 lasted until March 2010 with a total of $1.25 trillion in purchases of mortgage-backed securities and $175 billion of agency debt purchases. QE2 was first announced in mid-August 2010, ran from November 2010 to June 2011, and exclusively focused on Treasuries. We consider QE2 to have begun in September 2010 when the Fed signaled that it was considering a second round of monetary stimulus.[45] In September 2011, the Fed began a program known as the Maturity Extension Program (MEP) or Operation Twist. Under the MEP, the Federal Reserve began reinvesting repaid MBS principal and reduced the supply of longer-term Treasury securities in the market by selling and redeeming about $600 billion in shorter-term Treasury securities and using the proceeds to buy longer-term Treasuries. QE3 was announced in September 2012, and was roughly equally weighted between Treasuries and MBS. We treat the beginning of the Fed's tapering its MBS purchases as June 2013, following Bernanke's tapering announcement on May 22, 2013.

As Appendix Figure 6 shows, a greater fraction of each QE campaign's MBS purchases have occurred at the beginning of each program, with purchases slowly declining over the course of each LSAP campaign.[46] Panel II of Appendix Figure 6 shows the relative magnitude of GSE MBS net purchases compared with the total size of the GSE-guaranteed mortgage market. During QE1, the volume of Fed purchases was similar in magnitude to the volume of new issuance of GSE-guaranteed MBS. During QE3, Fed net GSE MBS purchases were roughly half of the GSE market.

To comply with the Federal Reserve Act, Fed MBS purchases had to consist of government-guaranteed debt. Contrary to popular perception, Fed MBS purchases did not involve buying legacy (and underperforming) MBS from banks. Instead, Fed MBS purchases were forward contracts on the TBA (To-be Announced) mortgage market, consisting mostly of newly orig-

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[45]Krishnamurthy and Vissing-Jørgensen (2013) find that most of the market reaction to QE2 was when it was first signaled in September 2010. Interest rates actually increased after the official announcement in November 2010 as it failed to live up to market expectations.

[46]Note that the policy of the Fed to reinvest principal prepaid on its MBS holdings into new MBS purchases results in non-zero MBS purchases even after QE3 officially ends.

inated GSE-eligible mortgages (see Appendix Figure 7). The strict eligibility rules for GSE guarantees allow us to compare origination volumes by loan size. Specifically, GSE guarantees require loan sizes to be beneath published conforming loan limits (CLLs).[47] Mortgages with a loan size exceeding geographically and time-varying CLLs (known as jumbo mortgages) are essentially ineligible for inclusion in GSE MBS. Many of our results test for a deviation in mortgage origination volume for loans below the CLL, which should be directly affected by Fed purchases because of their TBA eligibility, and loans above the CLL, which should only be indirectly affected by Fed MBS purchases.

B   Robustness to Increasing Conforming Loan Limits

In this appendix, we investigate the concern that the stronger response of GSE-eligible originations relative to jumbo originations around QE event dates simply reflects the establishment of high-cost area designations. Ultimately, this initial increase in conforming loan limits happened much too early to explain the differential response of mortgage market segments to QE1, and the eventual decrease in conforming loan limits (September 2011) did not coincide with any particular LSAP window (see Appendix Figures 8 and 9).

The conforming loan limit increased from about $400,000 to about $700,000 for certain high-cost areas over time (see panel I of Appendix Figure 8). As mapped in panel II of Appendix Figure 8, areas with the high-cost designation are mainly counties on the coasts with higher land values. Although this increase occurred nearly a year before the beginning of QE1, expanding the size of the conforming market by increasing the CLL in certain areas should tilt originations from the jumbo segment to the GSE-eligible segment. To address this concern, we construct an estimation sample using only non-high-cost counties whose conforming loan limits last changed in 2006 (and even then only incrementally). Appendix Figure 9 shows that even when we restrict attention to these areas, we observe a significant and differential increase in the origination of conforming loans around QE1. As before, origination volumes in the jumbo and conforming segments track each other closely except during the QE1 period, confirming that changes in the conforming loan limit cannot explain the differential origination pattern we observe in response to QE1.

C   Counterfactual Simulation of Countercyclical LTV Caps

Using our statistical model of the relationship between the LSAPs, debt origination, and equity extraction, we can demonstrate the importance of the interaction between QE and an oft-proposed macroprudential tool: countercyclical leverage caps. A key implication of our main results is that when the banking sector is impaired, Fed MBS purchases have significant effects on the mortgage market and the wider economy. Given that Fed MBS purchases are restricted to conforming mortgages, there is significant potential to enhance the effectualness of these purchases by temporarily expanding the definition of conforming mortgages in a crisis. Specifically, we analyze the complementarity between the maximum loan-to-value

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[47]See Adelino et al. (2013) and DeFusco and Paciorek (2017) for studies of the consequences of the sharp change in GSE eligibility at the conforming loan limit.

ratio allowed by the GSEs (that is, the maximum allowable without credit enhancements such as PMI) and QE by investigating what would have been the effects of an increase in the LTV cap from 80% to 90%. This exercise highlights the degree to which seemingly unrelated GSE policy can be an important factor in modulating the effectiveness of LSAPs. While low-downpayment loans have been maligned as an contributor to the housing crisis, ideally leverage ratios would be tight during credit expansions and loose during contractions to smooth macroeconomic shocks. Our bunching and loan-level prepayment model results highlight the importance of this LTV cap in determining the effectiveness of MBS purchases.

Adopting a countercyclical LTV policy might have several effects. First, it might allow borrowers with LTV higher than 80% that would not have been able to qualify for a new mortgage to refinance their current mortgages. Second, it might enable borrowers with lower LTVs to cash-out additional equity, supporting their spending behavior during the downturn. This policy intervention is substantially different from HARP; whereas HARP relaxed the requirements to qualify for a refinance loans, it prohibited borrowers from extracting any equity out of their homes (see Amromin and Kearns, 2014 and Agarwal et al. 2017). Third, borrowers with LTV higher than 80% that might liquidate accumulated wealth to cash-in refinance could do so without deleveraging as much.

Appendix Table 3 reports the results of this exercise. For each of several LTV bins, we measure in our data the number of loans, the fraction of borrowers that refinanced, and the average amount cashed-out (or cashed-in) again, allowing for $3,000 in closing costs in columns 1–3. To estimate the counterfactual prepayment rate in column 4, we estimate hazard models following Palmer (2015) to simulate what would have happened if the maximum allowable LTV were 90% instead of 80% (see Appendix Table 4 for these results). Specifically, we shift the coefficients for the LTV bins between 60–100% LTV up by one bin, effectively assuming that with a maximum LTV of 90% instead of 80%, for example, it would be as easy for an 85% current LTV loan to refinance as it had been for a 75% LTV loan to refinance in the factual world of an 80% LTV cap. Conservatively, we do not shift the coefficients for borrowers in the lowest (current LTV below 60%) and highest (current LTV above 120%) bins, as the elasticity of refinancing with respect to equity is near zero for these groups with high levels of positive or negative equity. Likewise, we perform a similar exercise for the counterfactual amount of equity cashed out by estimating an OLS regression of the actual amount cashed out on a similar set of controls (reported in Appendix Tables 5 and 6). In addition to shifting the LTV bin coefficients in the counterfactual, we also replace the amount of available equity to be cashed out (actual equity minus 20% of property value) with actual equity minus 10% of the property value. Columns 4 and 5 of Appendix Table 3 report the predicted fraction of borrowers who would be able to refinance and the predicted average cash out under the counterfactual policy. Columns 6 and 7 report the increase in the number of refinances and the increase in aggregate equity cashed-out as the difference between the actual number and the predicted ones.

We find that the biggest increases in the number of refinances come from the 80% to 90% bin, as these borrowers were not able to refinance before without deleveraging to get their current LTV ratio under 80%, and from the 90% to 100% bin, as these borrowers now have to delever much less. However, we also find that there is a significant increase in the aggregate amount cashed out, a result that is mainly driven by the borrowers with current LTVs below 90%, as these borrowers are now able to extract equity in their houses and

still be able to refinance. Are such borrowers with substantial equity actually likely to be affected by a change in the maximum allowable LTV from 80% to 90%? Appendix Figure 10 shows that the 80% cutoff is relevant even for those with very low LTVs that are refinancing. The effects from the below 60% LTV bin are particularly important because, although only a small fraction of these borrowers cash out, it is the group with the largest number of borrowers. We find in our data that about 5.6% of borrowers in the below 60% LTV cash out by bunching at the 80% threshold, so it is plausible that we would see an increase of a comparable magnitude for people refinancing to 90% LTV.[48]

Can this policy have large effects? Yes. Accounting for the 48% coverage ratio of our data (that is, we estimate it covers 48% of nationwide 2009 mortgage origination as measured by HMDA), our estimates show that this simple policy would have increased the number of refinancing households by over 380,000. If we multiply this number by the average mortgage size (i.e., $224,262), this policy would have resulted in a $86 billion increase in refinance mortgage origination, including a 28% increase in equity cashed-out ($22 billion) with potentially important effects on aggregate demand. One way to benchmark the magnitude of these numbers is to contrast them with estimated effects in the literature of the effect of the HARP program that explicitly supported the refinancing of high LTV mortgages owned by the GSE. We find that the change in the LTV requirement would have been more effective in terms of refinances and aggregate volume than HARP, partly because our policy experiment would have had a significant impact on cash-out activities, and thus on the consumption expenditures, of these borrowers.

Overall, an important implication of our findings is the complementarity between unconventional monetary policy and mortgage-market policies that may play a key role in supporting aggregate demand through their effects on borrowers' ability to cash out equity from their houses.

# D   Allocation of Credit Across Regions

Given our evidence that unconventional monetary stimulus from QE1 was not distributed evenly across the mortgage market, what implications did this have for the geography of credit allocation? We investigate this by analyzing where 2009 refinancing activity was concentrated (see Beraja et al., 2018 for a full treatment of regional heterogeneity in the effects of QE). To ensure full coverage of the mortgage market, we use Home Mortgage Disclosure Act data, which reports the universe of mortgage originations by institutions large enough to be regulated by the act. In Appendix Figure 11, we plot the state-level percentage of outstanding mortgage balance refinanced in 2009 against two lagged measures of state-level economic health: 2006-2008 home price appreciation (top panel) and 2006-2008 real GDP growth (bottom panel).

Panel I shows that even though a clear objective of QE1 was to stimulate distressed housing markets, there is a strong positive relationship between past home price appreciation and new refinancing activity, suggesting that the QE1-induced increased availability of refinancing credit may not have reached the areas that arguably needed it the most. In

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[48]We note that this equity-extraction channel is not present with other policy interventions such as HARP that target exclusively on high-LTV loans and prohibit cash-out refinancing.

particular, note that the states most affected by the housing bust (the so-called "sand states" of California, Florida, Arizona and Nevada) were the states with the lowest refinancing activity. Note that while the correlation between *purchase* mortgage credit growth could also be driven by shocks to fundamentals that simultaneously reduced demand for mortgage credit and lowered home prices, this is less of a concern for the refinancing activity measure shown here. Panel II of Appendix Figure 11 repeats this exercise, relating refinancing activity to state-level growth in real GDP from 2006–2008. Again, there is a clear positive relationship with contracting states benefiting less from QE1. Taken together, these figures provide evidence that mortgage market segmentation and contemporaneous banking sector stress allocated credit to the regions with the most potential GSE-eligible refinances (areas with fewer underwater borrowers and correspondingly stronger local economies). These across-region correlations highlight the important interplay between GSE mortgage-market policy and the effectiveness of monetary stimulus at reaching the local economies that would benefit the most.

Appendix Figure 1. Federal Funds Rate and Mortgage Interest Rates Around QE1
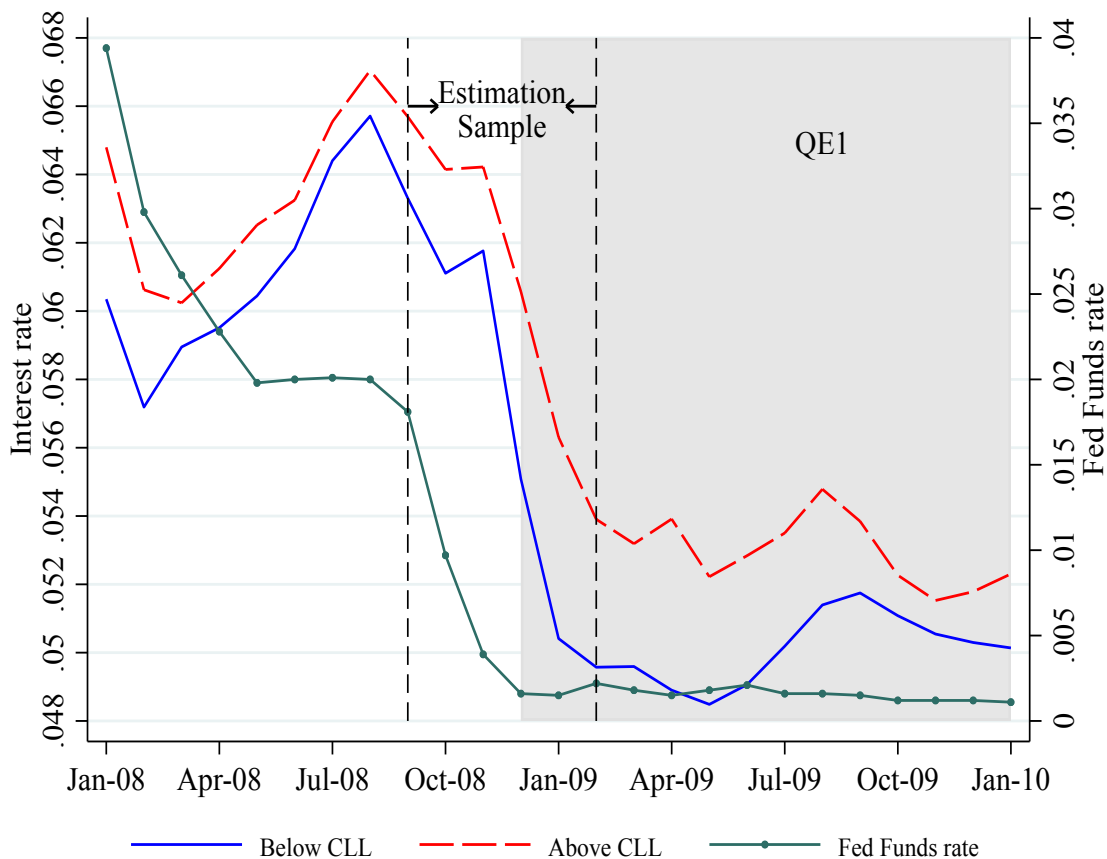

Notes: Figure plots estimated monthly interest rates for 30-year fixed-rate refinance loans above and below the conforming loan limit (CLL) on the left axis against the Federal Funds Rate on the right axis obtained from FRED. See notes to Figure 2.

Appendix Figure 2. Refinance Volume Excluding Loans Near CLL

### *Panel I. Number of Originations*
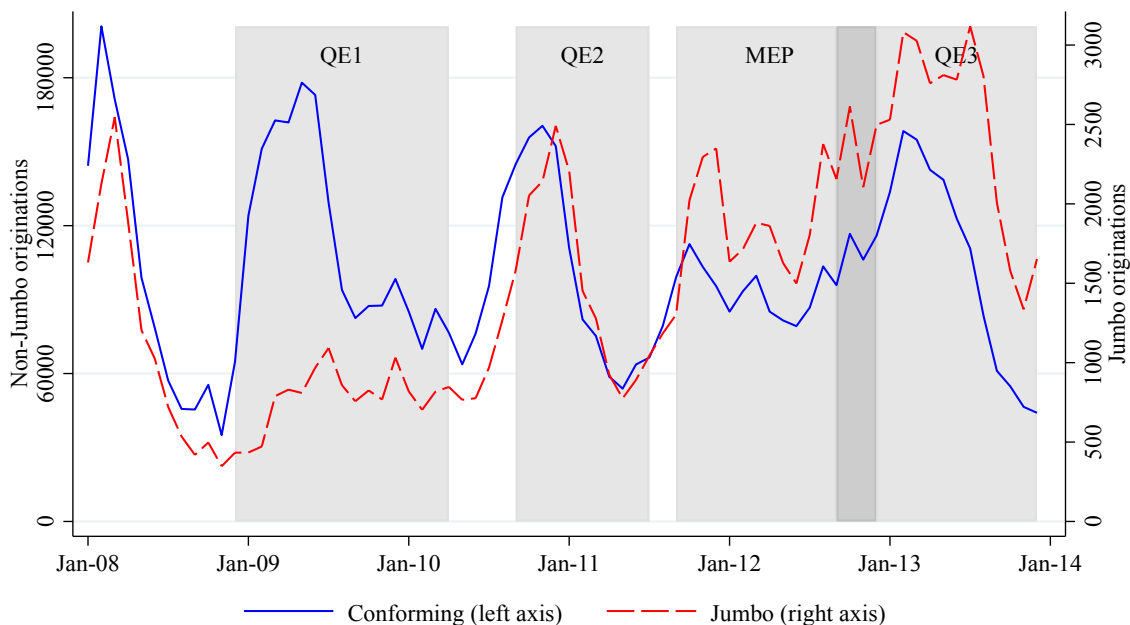

Panel II. Origination Volume
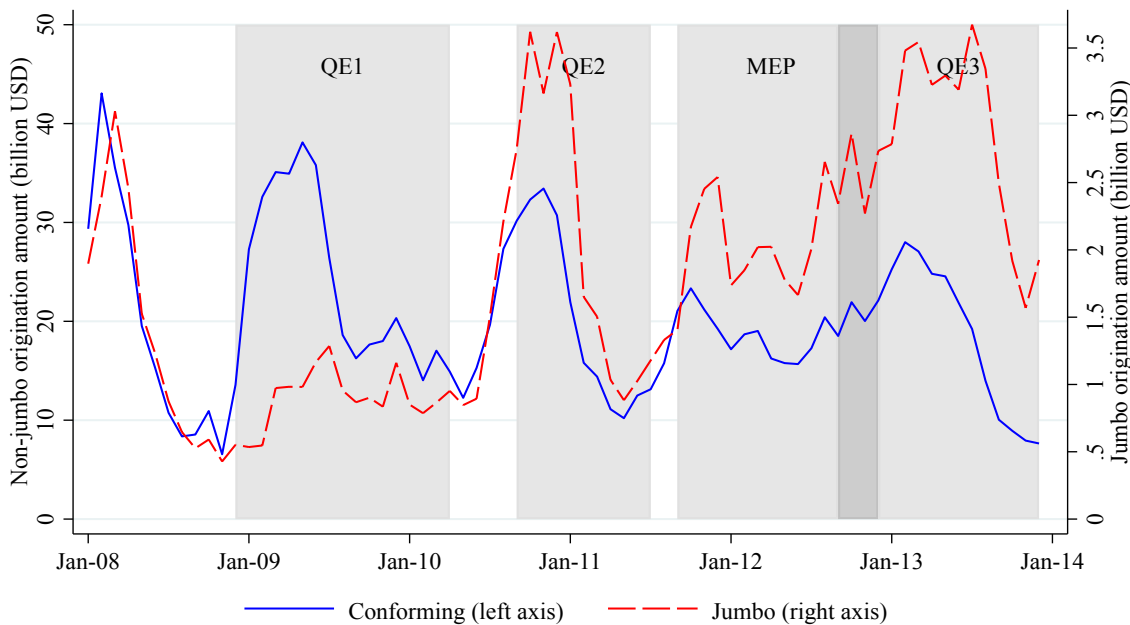
Notes: Figure plots the number of originations (top panel) and the origination volume (bottom panel) of refinance mortgages below the conforming loan limit (CLL) and above the CLL as recorded by LPS after dropping all loans within [90%, 140%] of the CLL. See notes to Figure 3.

Appendix Figure 3. Changes in Second- and First-lien Balances

Panel I. Closed-end Second Mortgages
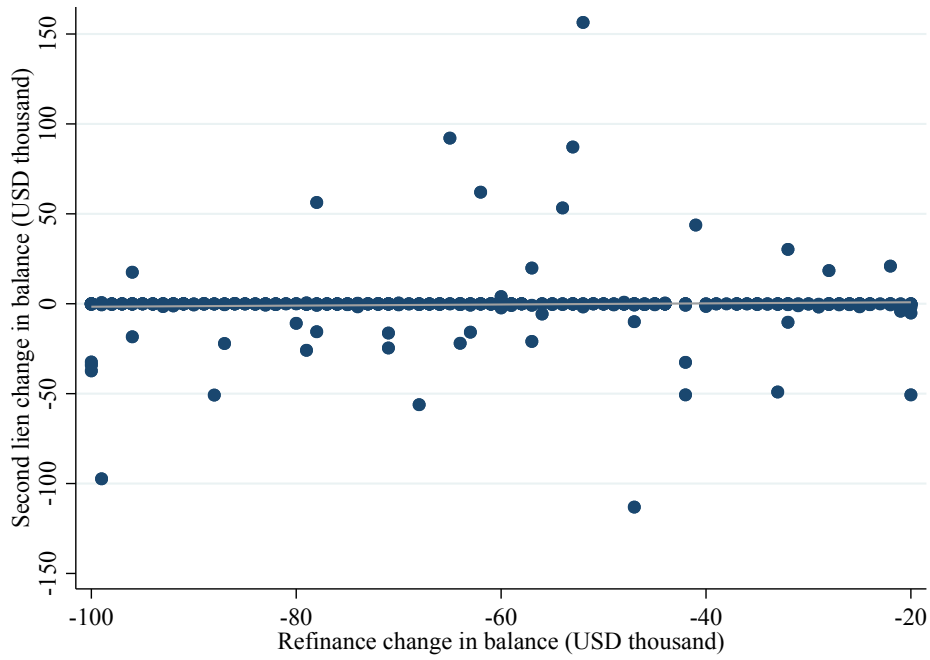
Panel II. Home Equity Lines of Credit
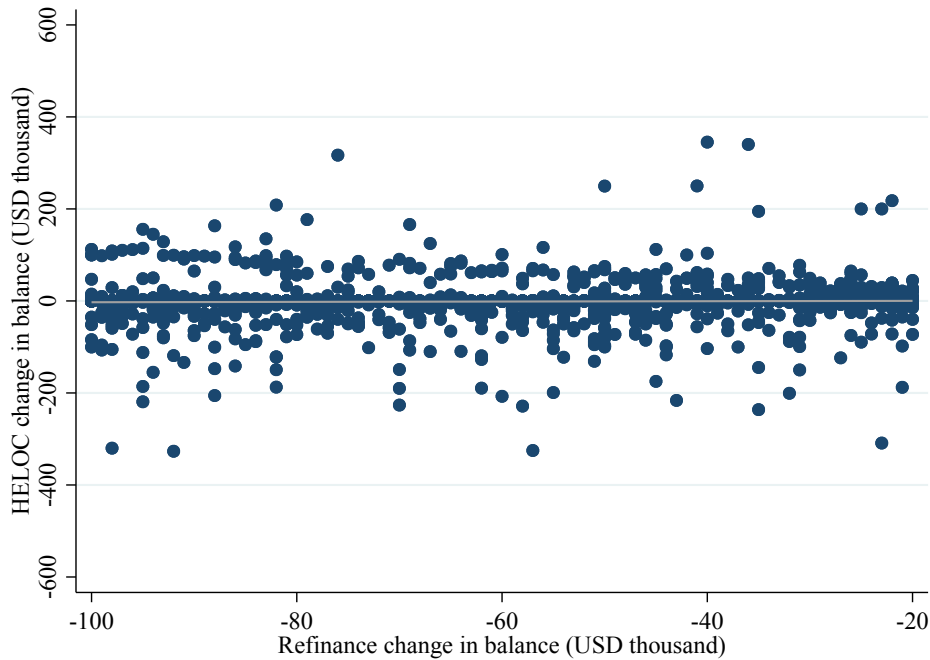
Notes: Figure plots the change in second-lien balances at refinancing as a function of change in first-lien balances with closed-end second mortgages (top panel) and home-equity lines of credit (bottom panel) for refinance mortgages that cashed in between $20,000 and $100,000 during QE1 along with bivariate regression lines. Debt relabeling or mortgage splitting would appear as an increase in second-lien balance to finance lowering first-lien balance.

Appendix Figure 4. Default Rates by Origination Date and Mortgage Segment
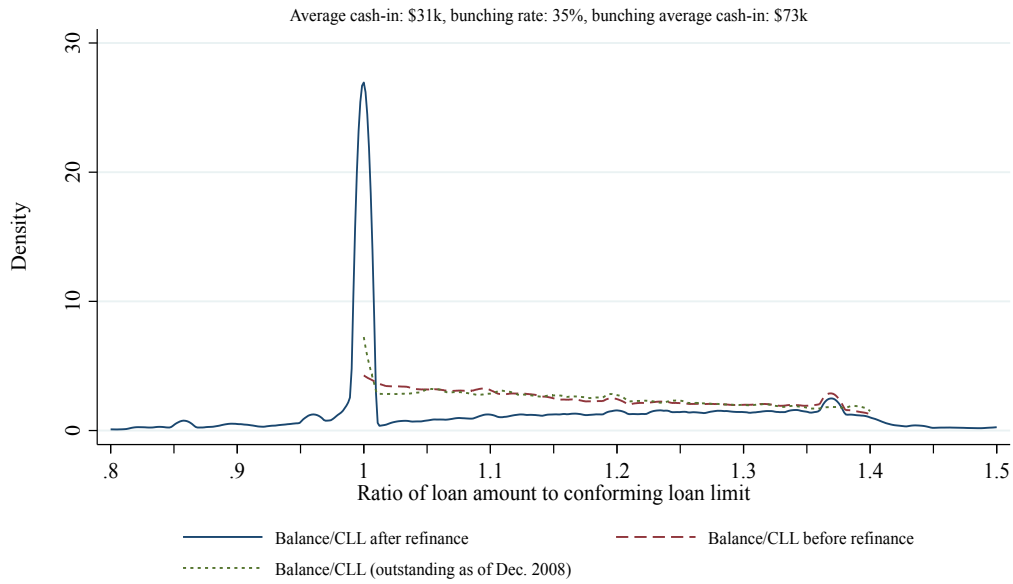
Panel I. Delinquent within 1 year
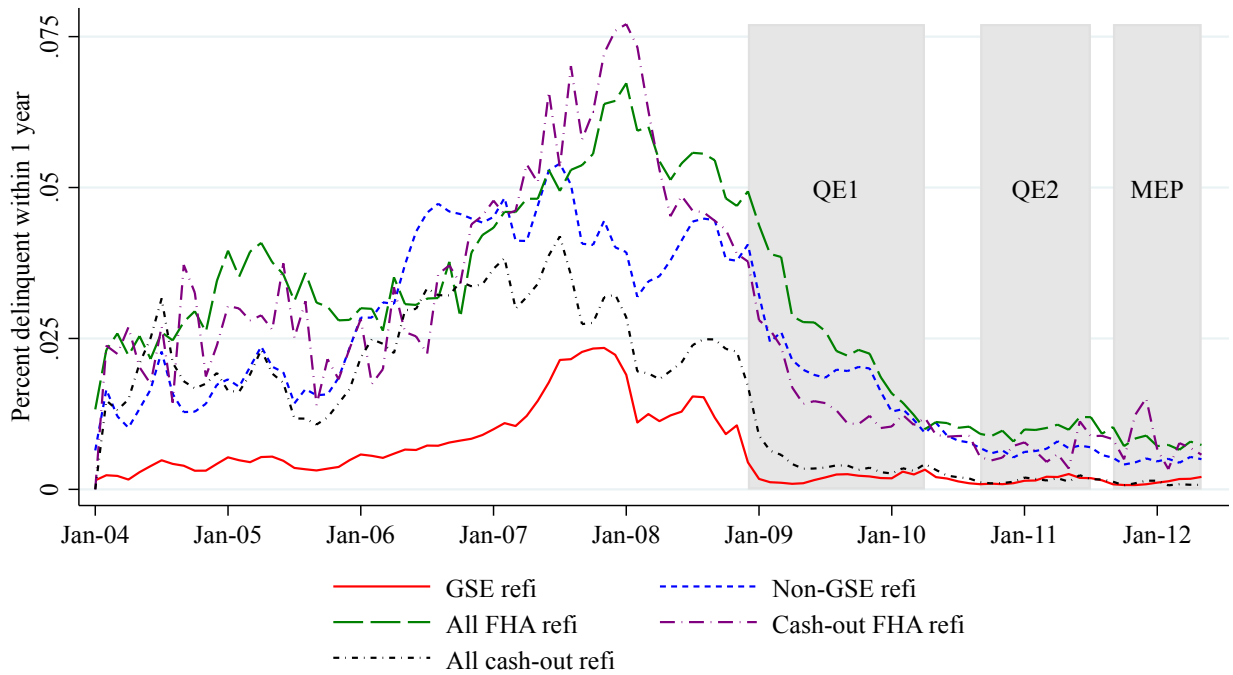
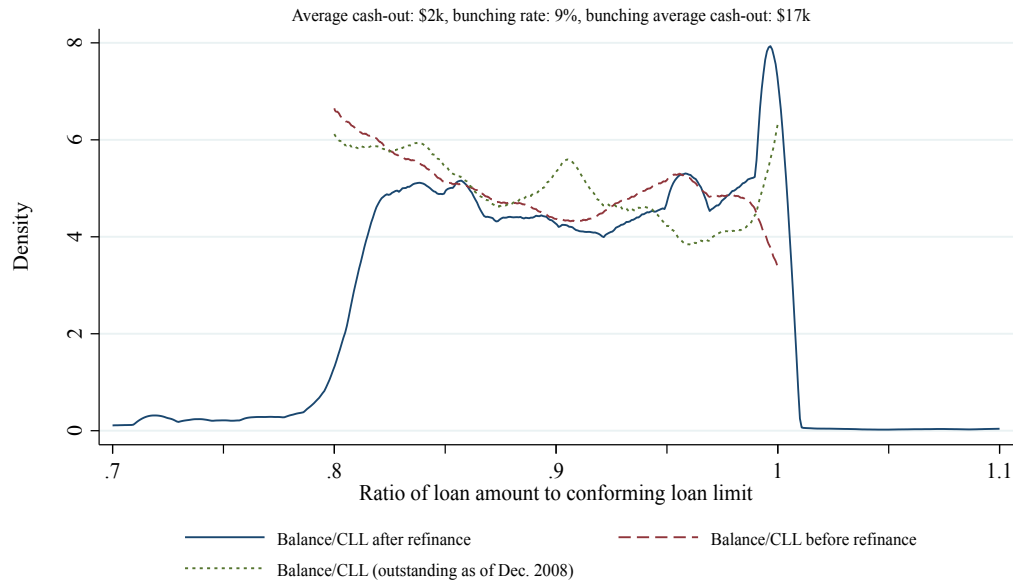## *Panel II. Delinquent within 4 years*
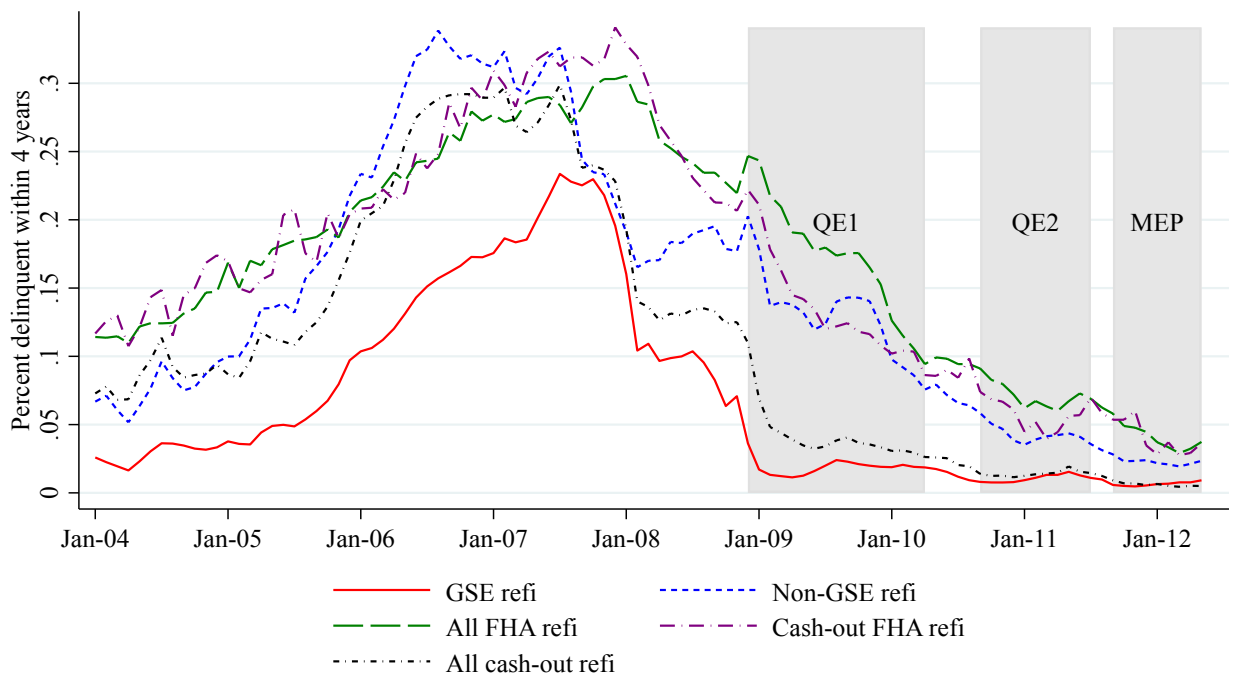
Notes: Figure plots the percent of loans delinquent within 1 year (top panel) and within four years (bottom panel) from refinancing for different types of refinances: GSE, FHA, cash-out and non-GSE as recorded by LPS.

Appendix Figure 5. Conforming Loan Limit Bunching

## *Panel I. Distribution of Loan Size/CLL for Original Balances 100-140% of CLL*

Average cash-in: $31k, bunching rate: 35%, bunching average cash-in: $73k

Density — Ratio of loan amount to conforming loan limit

- —— Balance/CLL after refinance
- – – – Balance/CLL before refinance
- ········ Balance/CLL (outstanding as of Dec. 2008)

Panel II. Distribution of Loan Size/CLL for Original Balances 80-100% of CLL

Average cash-out: $2k, bunching rate: 9%, bunching average cash-out: $17k

Density — Ratio of loan amount to conforming loan limit

- —— Balance/CLL after refinance
- – – – Balance/CLL before refinance
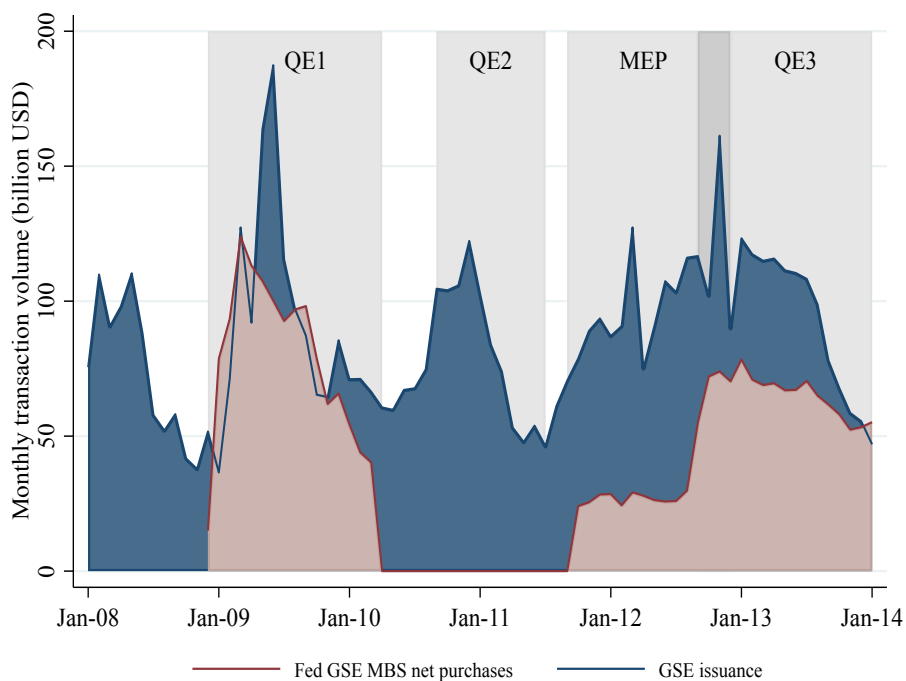- ········ Balance/CLL (outstanding as of Dec. 2008)

Notes: Figures plot distribution of loan sizes relative to the local conforming loan limit (CLL). Dotted lines plot the LTV distribution of all outstanding mortgages in the given LTV range. Dashed lines plot the distribution of predecessor normalized loan sizes (measured three months before refinancing) for refinancing borrowers. Solid lines shows the distribution of normalized loan sizes for that group's new refinance mortgages. Panel I includes loans for which we observe the predecessor loan with outstanding principal between 100 and 140% of the local CLL and whose origination balance (adjusted for expected refinancing costs) is between 80 and 150% of the CLL. Panel II includes loans for which we observe the predecessor loan with outstanding principal between 80 and 100% of the local CLL and whose outstanding balance is between 70 and 110% of the CLL. Bunching rate is share of new refinance loans in each sample that have are between 99.5% and 100.5% of the CLL at origination.

Appendix Figure 6. Federal Reserve Asset Purchases
Panel I. Federal Reserve Asset Purchases and Sales (Gross)

Legend: Gross treasury purchases · Net agency purchases · Gross treasury sales
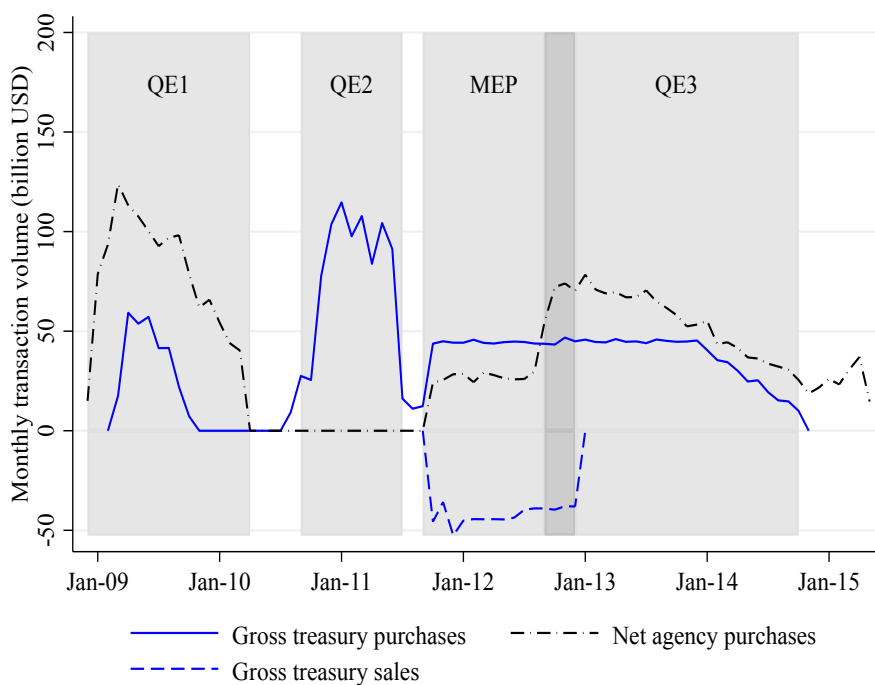
Panel II. Fed GSE MBS Net Purchases vs. Monthly GSE Issuance

Legend: Fed GSE MBS net purchases · GSE issuance

Notes: Panel I plots monthly gross Fed purchase and sale amounts for mortgage-backed securities (in black) and Treasuries (in blue) during the each QE operation. MEP shading represents the period of the Matury Extension Program that involved the swapping of short- and long-term Treasuries. Panel II plots the transaction amounts for Fed purchases of mortgage-backed securities and the issuance of GSE securities during the three quantitative easing operations. Source: NY Fed Open Market Operations Data, Fannie Mae, and Freddie Mac.

Appendix Figure 7. Share of GSE Origination Owned by Federal Reserve by Issuance Quarter
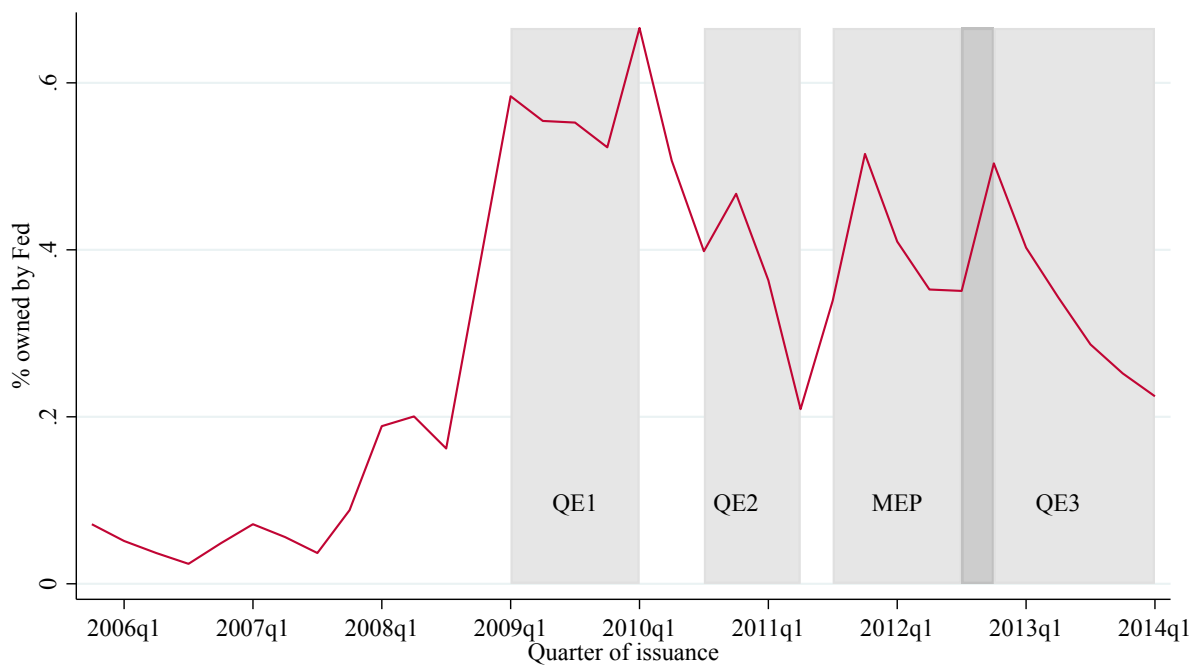
Notes: Figure plots the percentage of GSE MBS volume issued in each quarter that was ultimately owned by the Federal Reserve. Shaded regions indicate QE programs. Source: Fannie Mae, Freddie Mac, and New York Federal Reserve Open Market Operations data.

Appendix Figure 8. Conforming Loan Limits

Panel I. National and Maximum High-Cost Area Conforming Loan Limits

Legend: – – – National CLL ——— County-varying max CLL
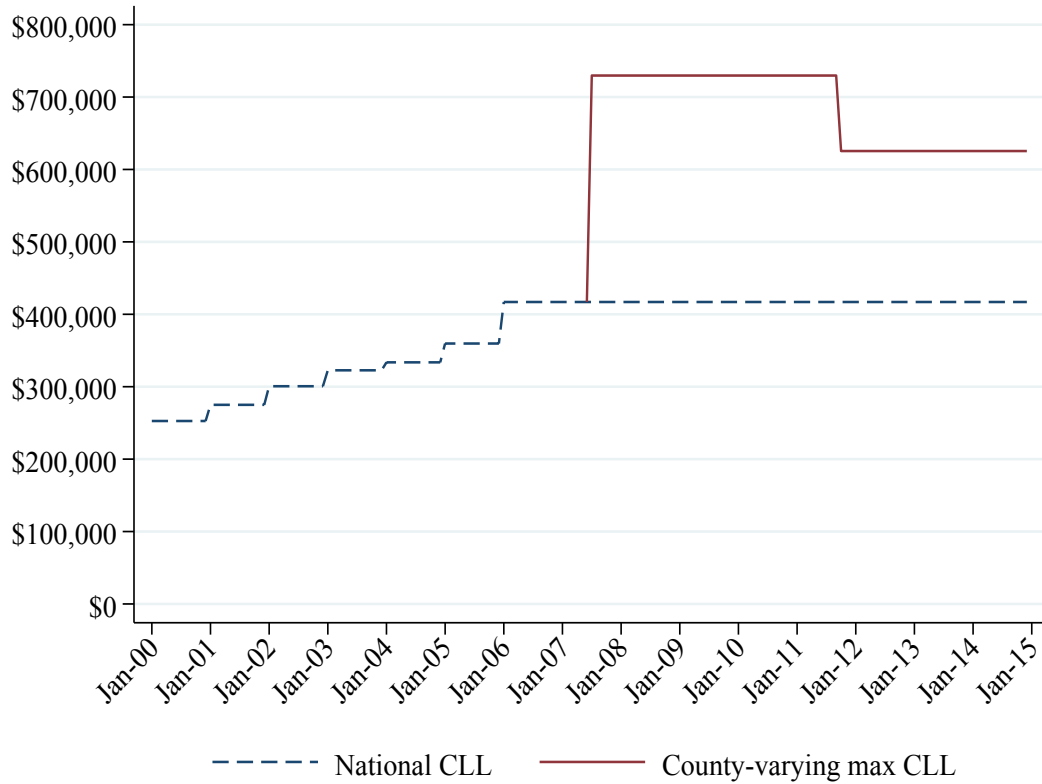
Panel II. The Geography of High-Cost County Designation

High cost (>$417k)
Non-high cost
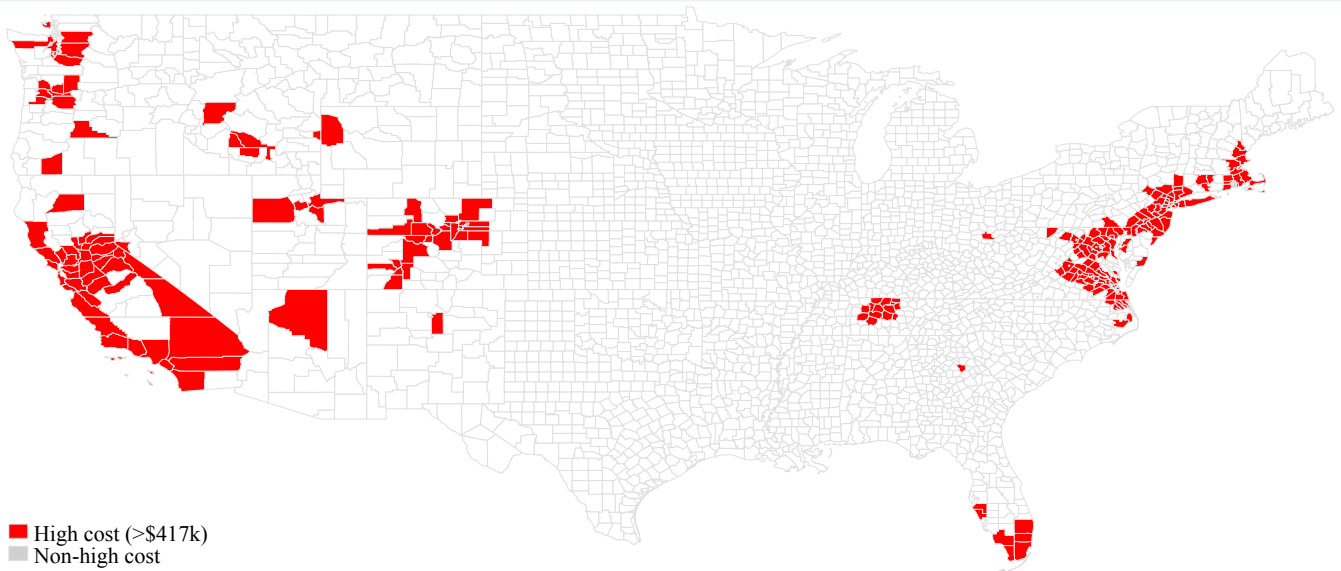
Notes: Panel I plots the national conforming loan limit over time and its maximal increase among certain high-cost counties in early 2008. Some of these temporary high-cost exemptions expired on October 1, 2011. Panel II plots all counties in the contiguous state. Darkly shaded areas indicate counties designated as high cost, defined as counties with conforming loan limits greater than $417,000.

Appendix Figure 9. Refinance Origination Count in Low-Cost Areas
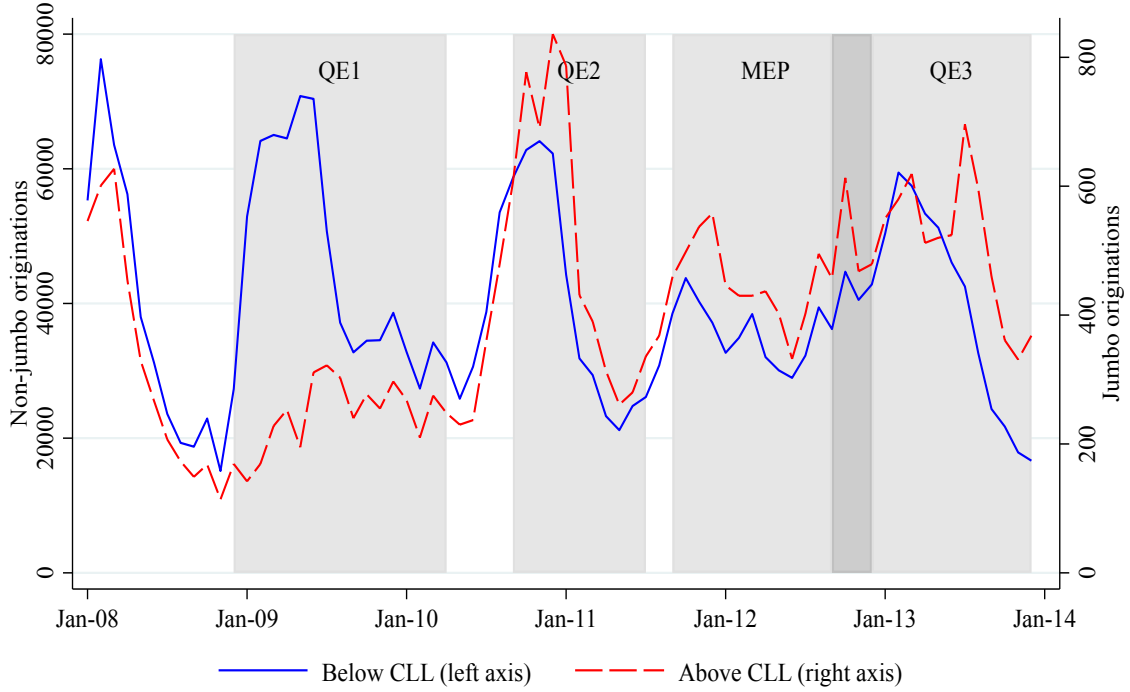
Notes: Figure plots the count of refinance mortgage originations in low-cost areas recorded by LPS for loans below and above the GSE conforming loan limit (CLL). See notes to Figure 3.

Appendix Figure 10. Loan-to-Value Ratio Bunching for Original Loans Below 70%

Panel I. Distribution of LTV for Original LTVs 0-60%

Average cash-out: $37k, bunching rate: 4%, bunching average cash-out: $93k

Panel II. Distribution of LTV for Original LTVs 60-70%

Average cash-out: $13k, bunching rate: 11%, bunching average cash-out: $40k
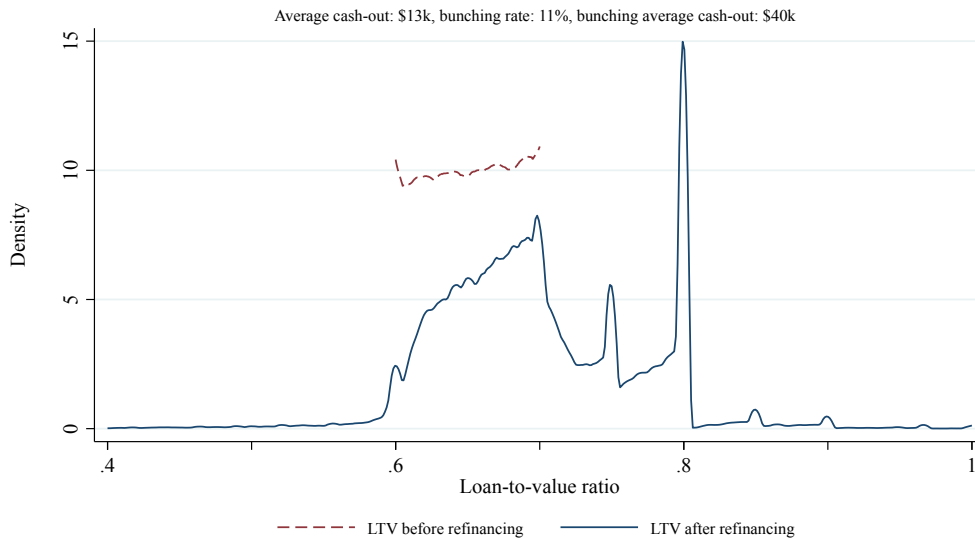
Notes: Figures report the distribution of borrower LTV ratios during QE1 but before the start of the Home Affordable Refinance Program (HARP) (Dec. 2008 to May 2009). Panel I includes loans for which we observe the predecessor loan with imputed LTV 0-60%. Panel II incl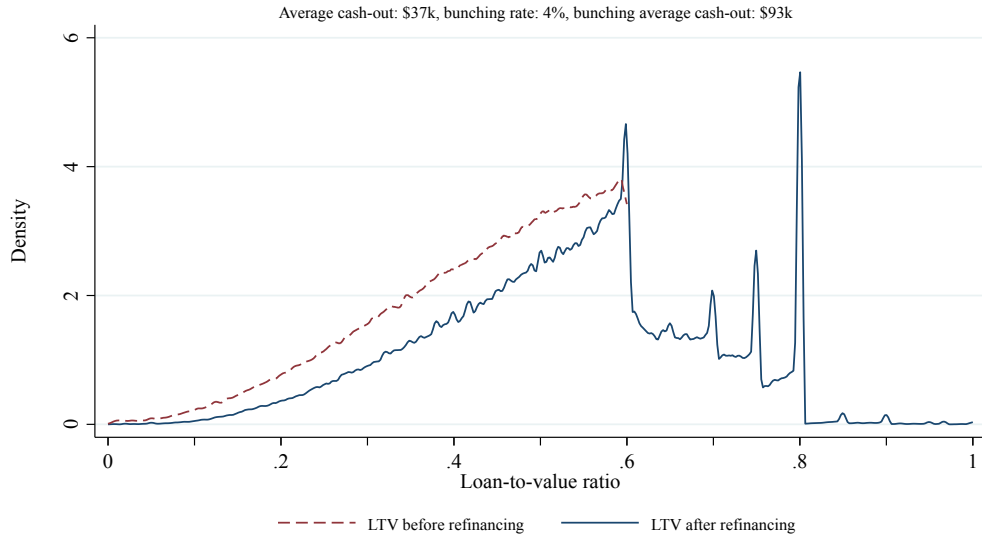udes loans for which we observe the predecessor loan with imputed LTV below 60-70%. Dashed lines represent the imputed LTV distribution for mortgages that will be refinanced during the time period. To account for rolling closing costs into the balance of the new loan, we add average refinancing costs ($3,000) to the loan balance before the refinance. The solid blue lines report the distribution of actual LTV ratios for originated refinance mortgages. The bunching rate is the number of refinance mortgages with an LTV ratio between 79.5% and 80.5% at origination divided by the total number of loans in the given original LTV range that refinance. Bunching average cash-out is the average amount borrowers refinancing provide at the closing of their new refinance mortgage.

Appendix Figure 11. State-level Refinancing Activity and Economic Conditions

Panel I. Refinancing Activity and House-Price Growth

Panel II. Refinancing Activity and Real GDP Growth

Notes: Figures plot the state-level percentage of 2009 outstanding mortgage balances that were refinanced in 2009 against state-level 2006-2008 Zillow Home Price Index percentage changes (top panel) and 2006-2008 state-level real GDP growth (bottom panel) from the BEA, along with the corresponding bivariate regression line. The robust t-stats are 4.7 for panel I and 4.2 for panel II.

Appendix Table 1. Coefficients on Time-Series Controls

	(1) Interest Rates	(2) Log(Refinance Origination Volume)
G-Fee × Jumbo	-1.523***	2.678***
	(0.117)	(0.286)
FICO Spread × Jumbo	-0.264	0.006
	(0.503)	(0.011)
Bank CDS × Jumbo	0.070***	-27.526***
	(0.019)	(5.136)
Controls	Yes	Yes
County-Month FEs	Yes	Yes
County-Segment FEs	No	Yes
Observations	6,740,267	5,904
R-squared	0.816	0.963

Notes: Table estimates the relationship between interest rates (column 1) and origination volumes (column 2) to guarantee fees, mortgage credit spreads, and bank credit default swap spreads between 2008-2013 (see Section 3 for details). The sample for column 1 includes loans with nonmissing LTVs, controlling for 5-point LTV bins, 20-point FICO bins, a categorical interaction of interest rate type, interest-only indicator, and original term, an indicator for missing FICO, and county by month fixed effects. The sample for column 2 has two observations for each county-month: log of total refinance origination below the conforming loan limit and above the conforming loan limit for counties that had non-zero refinancing activity in both the jumbo and non-jumbo segments throughout the sample period. Column 2 controls include mean FICO, mean LTV, share of loans not missing FICO, and county by month fixed effects. Standard errors clustered by month-segment are reported in parentheses. Asterisks denote significance levels (***=1%, **=5%, *=10%).

Appendix Table 2. OLS Estimation of Treasury Rates and Predicted Mortgage Rates

	(1)	(2)	(3)	(4)	(5)	(6)
	Predicted Jumbo Rate	Predicted Conforming Rate	Predicted Jumbo Rate	Predicted Conforming Rate	Predicted Jumbo Rate	Predicted Conforming Rate
5-year Treasury Yield	0.643***	0.635***			0.113	0.199***
	(0.023)	(0.018)			(0.085)	(0.070)
10-year Treasury Yield			0.854***	0.836***	0.711***	0.585***
			(0.028)	(0.019)	(0.117)	(0.093)
Constant	0.038***	0.037***	0.025***	0.024***	0.027***	0.028***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
Observations	156	156	156	156	156	156
R-squared	0.790	0.862	0.823	0.883	0.824	0.888

Notes: Table estimates the relationship between interest rates to constant maturity treasury yields. The odd columns consider the jumbo rates, while the even columns analyze the conforming rates. Columns 1 and 2 consider the relation between mortgage rates and the treasury rate with 5 year maturity, while Columns 3 and 4 investigate the relation to the 10 year maturity. Columns 5 and 6 include both treasury rates in the specification. Robust standard errors are reported in parentheses. Asterisks denote significance levels (***=1%, **=5%, *=10%).

Appendix Table 3. Higher LTV Cap Simulation during QE1 (Dec. 2008—May 2009)

Current LTV Bin	Number of Mortgages in Bin (1)	Without LTV Change		Counterfactual Higher LTV Cap		Counterfactual Increase	
		Baseline Percent Prepaid (2)	Actual Average Cash-Out (In) (3)	Predicted Prepaid (4)	Predicted Average Cash-Out (In) (5)	Increase in Number of Refinances (6)	Increase in Aggregate Equity Cashed-Out (7)
LTV ≤ 60%	10,058,221	7.8%	$39,176	7.8%	$40,371	0	$937,367,289
60% < LTV ≤ 70%	4,319,690	7.6%	$17,752	7.5%	$32,076	(4,320)	$4,564,050,321
70% < LTV ≤ 80%	8,155,314	7.1%	$9,316	7.6%	$14,580	40,777	$3,642,277,178
80% < LTV ≤ 90%	3,577,874	5.6%	$2,700	7.5%	$7,982	67,980	$1,600,836,844
90% < LTV ≤ 100%	3,523,964	3.5%	$2,170	5.7%	$501	77,527	($167,051,775)
100% < LTV ≤ 110%	152,520	2.0%	($3,796)	3.5%	$2,391	2,288	$24,342,056
110% < LTV ≤ 120%	11,842	1.0%	($89,126)	2.0%	($12,855)	118	$7,509,850
120% < LTV	15,483	0.5%	($144,764)	0.5%	($144,184)	0	$44,875
Totals	29,814,908	6.8%	$18,787	7.4%	$23,204	184,370	$10,609,376,637
Total Adjusting for Data Coverage	62,114,392	6.8%	$18,787	7.4%	$23,204	384,104	$22,102,867,994

Note: Baseline percent of loans prepaying over Dec. 2008—May 2009 are predicted values from column 2 of Appendix Table 4. The number of mortgages includes all single family, first-lien mortgage as of December 2008 in LPS. Counterfactual Predicted Prepaid percent also uses column 2 of Appendix Table 4 along with modified LTV bins as described in Appendix C. Actual average cash-out and predicted average cash-out are from Columns 1

Appendix Table 4. Prepayment Probability during QE1 using Refinance Hazard Model Results

	(1)	(2)
Ln(Original Loan Amount)	-0.168***	-0.178***
	(0.005)	(0.005)
Current Balance > CLL + 60,000	-0.458***	-0.545***
	(0.023)	(0.023)
60%< LTV <=70%	-0.004	0.047**
	(0.022)	(0.023)
70%< LTV <=80%	-0.084***	-0.018
	(0.021)	(0.021)
80%< LTV <=90%	-0.386***	-0.304***
	(0.022)	(0.022)
90%< LTV <=100%	-0.942***	-0.800***
	(0.031)	(0.031)
100%< LTV <=110%	-1.570***	-1.377***
	(0.048)	(0.048)
110%< LTV <=120%	-2.213***	-2.012***
	(0.094)	(0.094)
120%< LTV	-2.943***	-2.693***
	(0.159)	(0.159)
Loan Controls	Yes	Yes
Borrower Controls		Yes
Observations	2,035,027	2,035,027

Notes: Table reports the results of a hazard model estimating the propensity to refinance. All specifications include a cubic function of loan age as a non-parametric baseline hazard. Loan-level controls include current LTV bin, loan size at origination, and current balance over the conforming loan limit. Borrower controls include DTI, a missing DTI indicator, and FICO bins. Jumbo is an indicator for loan amount at least $60,000 above the CLL to account for cash-in refinancing. Robust standard errors in parentheses. Asterisks denote significance levels (***=1%, **=5%, *=10%).

Appendix Table 5. Cash-Out Estimate during QE1 (Dec. 2008—May 2009)

Current LTV Bin	(1) Actual Average Cash-Out	(2) Cash-Out Model (Loan & Equity Controls)	(3) Prediction for LTV Increase & Equity Decrease (Col. 3)
[0%, 60%]	$39,176	$39,176	$40,371
(60%, 70%]	$17,752	$17,752	$32,076
(70%, 80%]	$9,316	$9,316	$14,580
(80%, 90%]	$2,700	$2,700	$7,982
(90%, 100%]	$2,170	$2,170	$501
(100%, 110%]	($3,796)	($3,796)	$2,391
(110%, 120%]	($89,126)	($89,126)	($12,855)
Above 120%	($144,764)	($144,764)	($144,184)

Note: Table reports the actual cash-out amounts over Dec. 2008—May 2009 in column 1, and the predicted cash-out amounts using an OLS model with loan and equity characteristic controls in column 2. Column 3 predicts cash-out amounts by shifting the LTV bins down by one bin for loans with current LTV between 60% and 120%, and changing the equity calculation from 20% to 10%. Equity calculated as property value minus unpaid balance on original loan minus 20% of property value.

Appendix Table 6. Cash-Out Regression Estimates during QE1

	(1)	(2)	(3)
Equity Available (20%)		0.025***	0.023***
		(0.006)	(0.006)
Unpaid Balance > 80% LTV X Equity Available (20%)			0.451***
			(0.098)
Log Unpaid Balance	-5,942.455***	-7,736.501***	-5,584.112***
	(293.676)	(526.326)	(699.385)
Unpaid Balance > CLL+$60,000	-89,972.064***	-94,673.259***	-89,991.239***
	(2,765.606)	(2,889.990)	(2,808.333)
60%<LTV<=70%	-17,270.706***	-13,462.585***	-14,157.241***
	(404.527)	(1,017.086)	(968.142)
70%<LTV<=80%	-22,716.783***	-17,938.175***	-18,871.462***
	(412.408)	(1,246.156)	(1,186.598)
80%<LTV<=90%	-27,876.773***	-22,551.291***	-19,801.435***
	(353.535)	(1,361.538)	(1,341.236)
90%<LTV<=100%	-26,181.303***	-20,318.167***	-6,619.607**
	(328.689)	(1,487.384)	(3,150.314)
100%<LTV<=110%	-31,690.277***	-26,046.732***	-10,656.271***
	(353.255)	(1,439.077)	(3,467.823)
110%<LTV<=120%	-110,364.624***	-101,578.779***	-60,053.387***
	(3,989.665)	(4,443.665)	(9,589.823)
120%<LTV	-166,534.843***	-156,179.071***	-86,735.836***
	(5,896.883)	(6,131.483)	(12,833.365)
R-squared	0.035	0.038	0.040
Observations	820,653	820,653	820,653

Notes: Table reports regression estimates of cash-out refinances. Left-hand side variable is cash-out amount. To account for rolling closing costs into the balance of the new loan, we add average refinancing costs ($3,000) to the loan balance before the refinance. All specifications include a cubic function of loan age as a non-parametric baseline hazard. Equity Available is calculated as property value minus unpaid balance on the borrower's previous mortgage, minus a 20% down payment (calculated as 20% of the original loan amount). Loan-level controls include current LTV bin, and loan size at origination. Jumbo is an indicator for loan amount at least $60,000 above the CLL to account for cash-in refinancing. Robust standard errors in parentheses. Asterisks denote significance levels (***=1%, **=5%, *=10%).